Retail Investor Trading and Market Reactions to Earnings Announcements*

Henry L. Friedman Zitong Zeng

UCLA Anderson School of Management
110 Westwood Plaza
Los Angeles, CA 90095

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Abstract

We use trade-level data and exploit retail brokerage outages to examine how retail investors affect the pricing of public earnings information. Retail trading is associated with stronger price responses to earnings news during the earnings announcement (EA) window and greater post-earnings announcement drift (PEAD), but only the EA-window effect is robust to identification using plausibly exogenous outages. Prior to earnings announcements, retail activity is associated with less informative returns, but retail buy-sell imbalance during EAs is not associated with price responses to earnings news. We discuss several potential theories of retail trade in light of our evidence. Overall, results are consistent with a theory of retail traders as noise traders who provide liquidity that can help or hinder the impounding of public earnings news into price, but inferences are complicated because retail traders select to trade on earnings characterized by PEAD, potentially due to attention or overconfidence.

Keywords: Retail investors; Earnings announcements; Stock returns; ERC; PEAD
JEL codes: G11; G14; G51; M41

*Author emails: henry.friedman@anderson.ucla.edu; and zitong.zeng.phd@anderson.ucla.edu. For helpful comments, we thank, Ed DeHaan, Carla Hayn, Elsa Juliani (discussant), Jing Pan (discussant), Russell Jame (discussant), Stanimir Markov (discussant), Estelle Sun (discussant), David Veenman, Regina Wittenberg-Moerman, and workshop participants at UCLA, UC Berkeley, the 2021 London Business School Trans-Atlantic Doctoral Conference, the 2021 Conference on Financial Economics and Accounting, the 2022 Hawaii Accounting Research Conference, the 2022 AAA FARS Conference, and the 2022 London Business School Accounting Research Symposium. Any remaining errors are ours.

1 Introduction

Stock prices often fail to reflect any and all public information, despite the intuitive appeal of semi-strong market efficiency. Retail investors, as relatively unsophisticated capital market participants, are often viewed as the source of trades that impound noise into prices and slow or stop prices from reacting efficiently to information (see, e.g., the evidence reviewed by Barber and Odean, 2013). However, retail traders over the last several years have broadly gained access, via web-based technologies, to real-time public information as well as inexpensive means to trade on that information. Additionally, their trading activity can provide liquidity to other market participants who help impound informative signals into prices. There are several competing theories and narratives describing retail investors' activity in capital markets, ranging from views of retail traders as pure noise traders to considerations of retail traders as relatively informed. There are also intermediate narratives that describe retail investors as over- or under-reacting to some signals (e.g., due to overconfidence or inattention) while responding appropriately (i.e., rationally) to others.

In this study, we use novel retail trade indicators developed by Boehmer et al. (2021) to examine how retail trading affects stock market pricing of corporate information. Our efforts are aimed at providing evidence that can help distinguish between theories and narratives regarding retail investor activity. We focus specifically on how retail activity affects the pricing of earnings news in the days before the announcement, during the earnings announcement window, and in the weeks and months following the earnings announcement. To derive plausibly causal estimates of the effects of retail trade on the pricing of earnings news, we exploit data on outages at retail brokerages, which serve as random shocks to retail traders' ability and/or cost to trade (Barber et al., 2021; Eaton et al., 2022).

We proceed chronologically around the earnings announcement (EA), starting with the pre-EA period. Prior studies suggest that informed trading prior to announcements can impound information about earnings into price (e.g., Baruch et al., 2017), though biases such as optimism can make pre-announcement returns noisy and lead to reversals (Aboody et al., 2018; Trueman et al., 2003). On average, returns in the 10 days prior to an earnings announcement are strongly associated with the realized earnings surprise in our sample. Germane to our research questions, this positive association is significantly weaker when there is more retail activity during the pre-announcement window, suggesting that retail trade tends to reduce the amount of (predictive) earnings information in pre-announcement returns.

During the EA window, stock returns are more responsive to earnings surprises when retail traders are more active in the stock. That is, earnings response coefficients (ERCs) are higher when there is more retail activity. We then turn to the predictability of post-earnings returns, which are often used to capture departures from semi-strong market efficiency (e.g., DellaVigna and Pollet, 2009; Even-Tov, 2017; Hirshleifer et al., 2009). We find that retail activity during the earnings announcement window is associated with greater post-EA drift (PEAD) in the direction of the earnings surprise, consistent with retail traders contributing to PEAD. This initial evidence contrasts with the finding of Hirshleifer et al. (2008) that retail activity plays little role in driving PEAD. Notably, our samples cover vastly different periods (early 1993-1996 versus 2010-2021), and capture retail trade using different methodologies.

Of course, retail trading is endogenous in the earnings announcement setting because investors choose whether and how much to trade based on beliefs about earnings, stock prices, other market participants, and features both observable and unobservable empirically. To identify a plausibly causal effect, we exploit retail brokerage outages whose timing is seemingly random relative to the timing and information content of firms' earnings announcements. These brokerage outages increase the trading frictions faced by retail traders and lead to less activity (Barber et al., 2021; Eaton et al., 2022). Exploiting the exogeneity of the brokerage outages, we show that retail frictions are associated with lower ERCs. This effect is stronger for earnings announcements expected to garner high retail activity. However, outages are not significantly associated with heterogeneity in PEAD. Overall, this suggests a causal effect of retail trade on ERCs, and an association between retail trade and

PEAD that may be driven by selection.[1]

We interpret our results in light of several theories discussed in further detail below. Notably, retail trade could enhance ERCs if retail investors are relatively informed, or if they are noise traders that provide liquidity that facilitates others' sophisticated trades. We find that retail order imbalance in the EA window is insignificantly associated with returns or ERCs, inconsistent with the interpretation of retail trade as informed. Instead, our evidence favors a liquidity provision via noise trade interpretation. Furthermore, our PEAD results suggest that retail traders may select to trade on earnings surprises whose information is impounded into price over a long horizon, potentially because of greater information content or processing difficulty. Such selection may be due to underlying attention or overconfidence effects, which could generate noise trade. The noise trade interpretation is also consistent with retail trade weakening the association between pre-EA returns and announced earnings information. In the pre-EA window, the lack of a public signal may imply fewer opportunities for sophisticated traders to exploit the liquidity provided by retail activity.

We discuss related literature and theories in Sections 2 and 5, but note here two studies that are particularly close to ours. First, Hirshleifer et al. (2008) use the 1991-1996 discount brokerage data (see also Barber and Odean, 2000) to examine whether individual investors' trades help explain PEAD.[2] Hirshleifer et al. (2008) conclude that PEAD is not caused by individual investors: individual investors are net buyers after both positive and negative earnings surprises, and their net trades are not associated with PEAD. This is consistent with our inference that retail trade may have a selection-based rather than a causal association with PEAD. Despite the similarities, our study differs in several important respects from Hirshleifer et al. (2008). First, our research questions are substantively different. We focus

[1]Kumar and Lee (2006) find that retail investors are more active in trading small firms, lower-priced firms, firms with lower institutional ownership, and high book-to-market (B/M) firms. Interestingly, these are the same categories of firms where the PEAD has traditionally been more pronounced. This could mean that the association between retail trading and PEAD might be driven, at least in part, by the types of firms that retail investors choose to trade, rather than the trading activity itself influencing PEAD.

[2]Bartov et al. (2000) show that institutional holdings are negatively associated with PEAD, but note that institutional holdings are an imperfect proxy for investor sophistication. Their results do not speak directly to the question of retail traders' potential effects on the pricing of earnings news.

on how retail investor activity affects market responses to earnings announcements during the announcement window as well as before and after. Second, retail trading behavior is plausibly different in our 2010-2021 sample compared to their 1991-1996 sample, due to factors including changes in information availability, processing costs, and trading technology. Third, retail brokerages experienced multiple outages during our sample frame that made it more difficult for retail investors to trade. We exploit these outages as exogenous increases to retail frictions that allow us to better identify the effects of retail trading per se. Fourth, while Hirshleifer et al. (2008) base their findings on a random sample of individual investors from a single brokerage firm, our sample consists of retail order flows sourced from an extensive set of retail brokerages largely spanning the market. The biggest inferential difference from Hirshleifer et al. (2008) is that our results suggest that retail investors on average help impound public information around earnings announcements, while potentially reducing the degree to which pre-announcement returns reflect earnings information.

Second, the concurrent study by Michels (2023) exploits data on Robinhood holdings to examine retail activity around earnings announcements.[3] However, Michels (2023) focuses on changes in holdings around and *following* earnings announcements, and differential effects of positive versus negative earnings surprises with a focus on investor attention. In contrast, we exploit plausibly exogenous outages and the Boehmer et al. (2021) measure of retail trade to develop our inferences and show that they generalize beyond Robinhood holdings per se.

Our study contributes to the understanding of retail traders' potential effects on market reactions to earnings news. We focus on earnings announcements because they are salient, pre-scheduled public information releases. Our results provide the most consistent support for a narrative in which retail traders provide liquidity allowing reactions to and incorporation of public information but also can impound noise into prices. Our pattern of results tends not to support simpler narratives in which retail traders are either always (on average) impounding noise into returns or always pushing prices towards efficient incorporation

[3]A previous version of our paper also used Robinhood holdings data.

of public information. Notably, this suggests a conceptualization of retail traders that is potentially context-dependent, relying on the availability of public information, transaction costs, and other traders' responses to retail trade.

The next section provides background and institutional detail regarding retail trade. Section 3 describes the data, and Section 4 presents our main results. Section 5 discusses several prominent theories and narratives for how retail trade would affect the pricing of earnings information, with attention to which are supported or rejected by our findings in Section 4. Section 6 describes additional subsample and robustness tests. Section 7 concludes.

2 Background and related literature

Retail trading refers to trading by households and non-professional investors, in contrast to trading by professionals including institutional investors, hedge funds, financial institutions, and asset managers. Prior studies (reviewed in Barber and Odean (2013) and Blankespoor et al. (2020)) have found substantial evidence of retail investors underperforming relative to low-cost benchmarks, buying and selling at disadvantageous times, under-diversifying, and being subject to behavioral factors such as the disposition effect.

Several studies have empirically examined the role of retail traders in capital markets. Barber and Odean (2000) use a now-popular data set from a large discount brokerage in the early to mid 1990s to show that households make poorly-performing stock trades on average. Grinblatt and Keloharju (2000), using data on trades made by Finnish households, reach a similar conclusion. Given the sparse access to data on individuals' portfolios and trades, many researchers have used low-latency Trade and Quote (TAQ) data to study the performance of retail investors. Prior studies interpreted small trades (less than $5,000) as coming from retail rather than institutional investors (e.g., Hvidkjaer, 2008).

Intermediaries are important conduits of retail trade. Retail investors access capital markets information and trade through securities brokers and investment management firms, which historically have generated revenue through trading commissions and fees on assets under management. The recent emergence of technology firms in the financial space (fintech) has disrupted these revenue streams. Robinhood's no-commission trading quickly attracted a large number of retail investors, and was followed in 2019 by the elimination of trading commissions at other large brokerages popular among retail traders (Osterland, 2019).[4]

Instead of commissions, retail brokerages now generate revenue through payment for order flow, margin lending to traders, lending of securities to short sellers, and net interest on investors' cash positions. Payment for order flow refers to the practice of wholesale market makers (e.g., Citadel Securities) offering rebates to retail brokerages for routing their order flow to the wholesaler for execution. Often, the orders are executed at prices that are fractions of a cent better than the National Best Bid and Offer (NBBO) available on public exchanges. Following Boehmer et al. (2021), we exploit these sub-penny price improvements observable in TAQ data as indicators of payment for order flow and thus retail trade.

Numerous channels, including brokerage platforms themselves, provide retail investors with information about earnings realizations, expectations, and interpretations both by professionals and peers (e.g., Farrell et al., 2020). Several retail brokerage houses provide push notifications about upcoming and recent earnings announcements (Moss, 2022), make it easy for users to listen to earnings calls, and incorporate data from multiple vendors and markets.[5] Because public information and context has become easily accessible, retail investors may now contribute to the impounding of earnings information into price, even if they hindered it in the past (i.e., in studies using earlier samples).

Interestingly, retail brokerages occasionally experience outages, which can limit their

[4] As of April 1, 2021, TD Ameritrade, E*Trade, Charles Schwab, Vanguard, Fidelity, Bank of America (Merrill Edge) and J.P. Morgan Chase (J.P. Morgan Self-Directed Investing) all offer zero-commission equity trading. See Nerdwallet's list at `https://www.nerdwallet.com/best/investing/free-stock-trading`. Even-Tov et al. (2022) provide empirical evidence on the implications of no-fee trading for retail investors.

[5] Robinhood provides the following information about their data sources: "Certain fundamental, market data, and other information is provided by FactSet Research Systems, Inc. ..., by Xignite (xignite.com), ICE Data Services, and/or other third party providers." Accessed at `https://cdn.robinhood.com/assets/robinhood/legal/RHF\%20Product\%20Features\%20Disclosures.pdf` on May 13, 2021.

users' ability to trade. Outages can be due to technical problems with the broker's system or periods of heightened market stress (e.g., around March 2-3, 2020). Eaton et al. (2022) document retail broker outages using complaints histories available from downdetector.com. We use their outage and complaints data as a source of variation exogenous to other factors influencing retail trading around earnings announcements (particularly after controlling for market stress as reflected in volume). The conditionally random nature of outages allows us to attribute changes in market pricing of earnings information around outages to the effects of (a reduction in) retail trade. In particular, the outages appear to be randomly timed relative to corporate earnings news. Inspired by Eaton et al. (2022), we develop a predictive model for retail trade, further illustrating how outage effects fluctuate based on anticipated retail trading patterns.

Although individual retail investors tend to be small, their impact on markets can be large. As of mid-2020, retail trading accounted for roughly 20% of market activity (Winck, 2020), partly facilitated by low-cost platforms such as Robinhood. In individual stocks, retail flows can cause large price movements. As examples, note recent episodes involving volatility in Gamestop, AMC Entertainment Holdings, Blackberry and Nokia, discussed in Lyócsa et al. (2021). Boehmer et al. (2021) find that retail order flow predicts returns over the subsequent week, though Eaton et al. (2022) find no evidence of Robinhood holdings predicting stock returns.

In our main analysis, we focus on how retail trade affects our three empirical outcomes of interest. The first outcome of interest is the degree to which earnings information is incorporated into returns before the earnings announcement (EIPAR: earnings information in pre-announcement returns). EIPAR can be the result of some traders having private information or processes that allow them to predict the earnings surprise. The second is the earnings response coefficient (ERC), which captures the sensitivity of returns during the announcement window to the news contained in the earnings announcement, captured by the earnings surprise. The third is post-earnings announcement drift (PEAD), which represents systematic trends in returns associated with earnings surprises following earnings announcements.

3 Sample and variables

3.1 Earnings announcements sample

The sample consists of quarterly earnings announcements for U.S. common stocks, at the intersection of the TAQ, CRSP, Compustat, and IBES databases, from January 2010 to December 2021. We select this period due to the widespread adoption of internalization and price improvement practices for retail investors by brokerage firms and wholesalers by 2010 (Boehmer et al., 2021).

We detail our sample selection process in Table 1. We start with announcements available in Compustat and CRSP. The initial dataset is comprised of 139,870 firm-quarter earnings announcement (EA) observations. We exclude observations missing from the IBES database or those with multiple earnings timestamps within a single quarter, reducing the dataset to 131,966 firm-quarters. We further exclude announcements (firm-quarters) with missing returns, which resulted in a total of 82,031 firm-quarters.[6] Additionally, we exclude firms missing from TAQ (i.e., firms that were not listed on Nasdaq, NYSE, or AMEX), and those presenting multiple observations for the same ticker-date, bringing our total to 79,857 firm-quarters. Lastly,we eliminate observations with missing control variables. This leaves us with a sample of 65,321 unique earnings announcements from 3,979 unique firms.[7] Concise definitions of variables, discussed below, are provided in Appendix B.

Prior studies have shown a substantial number of earnings announcements occur outside

[6]We retain only observations with stock returns available from 10 days before the EA through the next EA, to maintain a constant sample across specifications with different return windows. Observations where earnings are announced more than 100 days after the fiscal period end, and those with extremely high or low estimated earnings persistence (abs(EPersistence) > 100) were also dropped.

[7]In our regressions, we drop an additional 127 singleton observations, i.e., observations where the fixed effects leave no identifying variation, yielding regression samples of 65,194 in our main panel analyses.

market hours (e.g., Bochkay et al., 2020). To account for earnings released post-market closure, for earnings released after 4 pm Eastern Time based on IBES timestamps, we adjust the earnings announcement date to one trading day after.[8]

3.2 Retail trading

Our methodology for measuring retail trading is based on Boehmer et al. (2021). We obtain transaction data from the daily TAQ database,[9] and identify trades as retail buys or sells in the TAQ data if they have an exchange code D and the trade was executed at a price just below (for buys) or above (for sales) a round penny.[10] Using the identified retail trades, we compute retail volume as follows:

$$\text{Retail Volume}_{it} = \text{Retail Buys}_{it} + \text{Retail Sales}_{it} \tag{1}$$

Here, $Retail\ Buys_{it}$ and $Retail\ Sales_{it}$ represent buying and selling volumes initiated by individuals during interval t for stock i.[11] In our analyses, t refers to a one-day window on the adjusted earnings announcement day.

To address skewness, we use the natural log of $Retail\ Volume$ when employing it as a retail trade measure. We define nonretail trade as total market volume minus $Retail\ Volume$, and we control for the log of nonretail volume in our tests to help mitigate concerns that our

[8]For after-hour (pre-market) announcements, our earnings announcement window return, i.e., BHAR[0,1] based on close-to-close returns, captures the post-market (pre-open) market activities.

[9]We follow the data cleansing steps described in Holden and Jacobsen (2014) for daily TAQ data.

[10]Trades with a tenth-of-a-cent digit between 0 and 4 (6 and 9) are classified as retail sales (buys). This classification reflects the price improvement relative to round penny prices favoring the seller (or buyer). Most market orders initiated by retail investors are either internalized by brokers or routed to wholesalers. These orders usually do not occur on registered exchanges and are recorded in TAQ with an exchange code D. Orders routed to wholesalers are typically filled at prices slightly better than the national best bid and offer, with price improvements usually less than a penny. Institutional investors are prohibited from receiving sub-penny price improvements by Regulation NMS.

[11]Retail trades identification follows Boehmer et al. (2021) and focuses on marketable retail orders. As documented by Kelley and Tetlock (2013), retail trade tends to be aggressive, and data from various retail brokerages confirm this. For instance, in their 2003-2007 sample, retail investors primarily submit market orders to meet their trading needs, with the number of retail market orders surpassing nonmarketable limit orders by over 35%.

retail volume measure simply acts as a proxy for overall trading volume.

Figure 1 shows quarterly log market volume over our sample period (vertical bars) separately for small and big firms, based on a market capitalization median split. Unsurprisingly, big firms tend to have larger volume. The average quarterly % Retail Volume (Retail Volume scaled by total market volume) for each subset of firms is also displayed in Figure 1 (solid and dashed curves). Retail trade is more prominent in small firms, hovering around 12% for much of the sample period, relative to values around 4-6% for big firms. Overall, except for a rise in % Retail Volume for small firms during 2013, there do not seem to be significant secular patterns either for the total market volume or % Retail Volume.

Figure 2 displays the retail and nonretail trading volume trends across a 60-day window surrounding earnings announcements. As in Figure 1, we present averages separately for small and large firms, based on market capitalization. In line with the findings from Figure 1, the average nonretail volume surpasses the retail volume in both subsamples, and this holds true for each day around the earnings announcement. Both small and large firms experience surges in both retail and nonretail volume, starting a few days before the earnings announcement and subsiding thereafter. Despite differences in the magnitudes (i.e., levels) across trade types and firm sizes, the time series patterns appear broadly similar.

3.3 Brokerage outages

Data regarding retail brokerage platform outages is obtained from Downdetector.com, compiled and kindly provided by the authors of Eaton et al. (2022). Downdetector.com tracks user-reported outages on websites broadly. In their work, Eaton et al. (2022) gathered data on outage complaints for various retail brokerages, including Charles Schwab, E-Trade, Fidelity, TD Ameritrade, and Robinhood, and monitored these at 5-minute intervals. We omit outages that are potentially misreported, excluding those with fewer than 200 outage complaints as per Eaton et al. (2022).

We construct an indicator, *Outage*, at the earnings announcement level. This indicator takes a value of one if the aggregate outage complaints during a firm's adjusted earnings announcement date belong to the top quintile of daily complaints (at least 526) in the timeframe encompassed by Eaton et al. (2022). To mitigate concerns about outages being driven by market-wide factors, we exclude outages experienced by all brokers simultaneously within the same 5-minute interval, following the approach suggested in Eaton et al. (2022).[12] In our analyses exploiting outages, we control for total market volume to prevent erroneous inferences that may arise from outages specifically driven by high-volume days.

3.4 Market reactions to earnings announcements and controls

For our analyses of market reactions to earnings announcements, we measure the earnings surprise, denoted as SUE, following Livnat and Mendenhall (2006):

$$\mathrm{SUE}_{it} = \frac{\mathrm{X}_{it} - \mathbb{E}[\mathrm{X}_{it}]}{\mathrm{P}_{it}}$$

where i represents the firm, t refers to the quarter, X_{it} denotes the actual earnings reported by IBES, $\mathbb{E}[X_{it}]$ represents the expected earnings, taken as the latest median forecast from the IBES summary file, and P_{it} indicates the share price at the end of quarter t.[13] As in prior studies (e.g., DellaVigna and Pollet, 2009), when SUE acts as an independent variable, we use 11 SUE quantiles (five quintiles of negative surprises, five quintiles of positive surprises, and a no-surprise quantile at $SUE = 0$) based on calendar-quarter sorts.[14]

We compute daily excess returns each day as firm-specific returns after subtracting the returns on a size and book-to-market matched portfolio.[15] Earnings announcement returns

[12] While Eaton et al. (2022) examine the effect of different retail investor clienteles on stock market liquidity, our study concentrates on aggregate retail trading and its impact on the pricing of earnings news. As such, we do not conduct separate analyses for traditional brokers and Robinhood.

[13] Many variable definitions are similar to those in the earnings announcement sample in Andrei et al. (2023). The earnings surprise calculation follows the WRDS guidance as described in Dai (2020).

[14] Our main findings remain robust when using raw SUE (untabulated).

[15] We apply 25 matching portfolios based on the intersections of 5 portfolios formed on market capitalization and 5 portfolios created on book-to-market (i.e., independent sorts), following a similar method to that described at `http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/Data_Library/tw_5_ports.html`. Portfolios are constructed annually and assigned to firms on June 30 according to firm size (market

used for earnings response coefficient (ERC) tests are computed as buy-and-hold excess (abnormal) returns from the earnings announcement day through the following day (a two-day window). Additionally, we examine the pre-earnings announcement window from 10 days before the earnings announcement to the day before for pre-earnings returns, as well as several post-earnings announcement windows from 2 days after the earnings announcement to 5 days, 22 days, 45 days, and the subsequent earnings announcement to capture post-earnings returns over the week, month, 2 months, and quarter following the earnings announcement, respectively.

We use the following additional variables, mostly as controls, in line with previous literature (e.g., Hirshleifer et al., 2009): compound excess returns from ten to one day before the earnings announcement (PreRet); the market value of equity on the earnings announcement day (Size); the ratio of book value of equity to market value of equity at the quarter end (Book-to-Market); earnings persistence estimated based on quarter-to-quarter auto-correlation (EPersistence); institutional ownership represented as a fraction of total shares outstanding at the quarter end when the earnings are announced (IO); earnings volatility (EVOL); the reporting lag expressed as the number of days from quarter end to the earnings announcement (ERepLag); analyst following quantified as the number of analysts making quarterly earnings forecasts according to the IBES summary file (# Estimates); average monthly share turnover over the previous 12 months (TURN); an indicator variable for negative earnings (Loss); and the number of other firms making earnings announcements on the same day (# Announcements).[16]

3.5 Descriptive statistics and correlations

Descriptive statistics are provided in Table 2. Raw earnings surprises are on average near zero, but with wide dispersion, indicating wide variability in earnings surprises across dif-

capitalization) and book-to-market ratios.

[16]When our coefficient of interest is on an interaction between our retail measures and SUE, we also interact all control variables (excluding fixed effects) with SUE.

ferent firms and quarters. Our quantile-based SUE measure addresses the dispersion and ranges from 1 to 11. Values of 1-5 are the negative quintiles; 6 represents no surprise, and 7-11 capture the positive quintiles. Average buy-and-hold abnormal returns (BHAR) over the various windows examined are also close zero.

The average log-transformed retail and non-retail trading volumes during the announcement period (Log(Retail) and Log(Nonretail)) are approximately 14 and 17, respectively. These numbers suggest that non-retail trading volumes tend to be higher than retail volumes on average during the announcement window, consistent with Figure 2. Most earnings announcements are made on days with no complaints about retail brokerage outages on Downdetector.com, though the distribution is unsurprisingly right-skewed.

Table 3 shows raw pairwise correlations. The earnings surprise quantile, SUE, is positively and significantly correlated with abnormal returns in all relevant windows. SUE and announcement-window returns are also both strongly associated with the volume attributable to retail investors and non-retail investors. The log of retail volume is also significantly correlated with the announcement-firm controls, consistent with there being several plausible factors contributing to the trading mix around earnings announcements.

4 Results

We proceed chronologically around the earnings announcement, beginning with results focusing on retail trade prior to the earnings announcement. These analyses help demonstrate whether retail trade facilitates or impedes the revelation of upcoming earnings information in pre-announcement returns (EIPAR). We next present tests relating retail trading activity to price responses to earnings surprises, i.e., ERCs. We then examine the association between retail trade during the earnings announcement window and post-earnings returns, focusing on potential drift and reversal. Given the endogenous nature of trading decisions that inherently depend on available information and incentives, we next introduce brokerage outages as a plausible shock to retail trade. We show that outages lead to less retail trade, then examine how outages during earnings announcement windows affect ERCs and PEAD. We then examine whether outage effects vary with the amount of expected retail trade. After interpreting our results in light of various theories and narratives around retail trade in Section 5, we present additional analyses in Section 6.

4.1 Earnings information in pre-announcement returns (EIPAR)

This section explores the impact of retail trade on the degree of earnings information that is reflected in prices before the earnings announcement. This analysis is based on the ability of pre-announcement returns to predict the earnings surprise. Specifically, we estimate the following regression to investigate potential effects of retail trade on EIPAR:

$$\begin{aligned} \text{SUE}_{it} = &\beta_0 + \beta_1 \text{PreRetail}_{it} + \beta_2 \text{PreRet}_{it} + \beta_3 \text{PreRetail}_{it} \times \text{PreRet}_{it} \\ &+ \sum \beta_k X_{k,it} + \epsilon_{it}. \end{aligned} \tag{2}$$

Here, PreRet refers to the buy-and-hold abnormal return (relative to the size and book-to-market matched portfolio) from days -10 through -1, relative to the earnings announcement (EA) day 0. Likewise, PreRetail is the log of retail volume for trading days -10 through -1, preceding the EA date. We use estimates from equation (2) to test whether returns prior to an earnings announcement help predict the earnings surprise, and how retail trading during this period affects this predictive relation.

The results for the primary coefficients of interest from estimating equation (2) are presented in Table 4. The first column shows estimates with fixed effects but without controls. The coefficient on PreRet is positive and statistically significant ($\beta = 0.209$, $p < 0.01$), indicating the predictive value of pre-EA returns for the earnings surprise. Furthermore, the coefficient on PreRetail × PreRet is negative and significant ($\beta = -0.163$, $p < 0.01$), suggesting that retail activity attenuates the extent to which pre-EA returns predict the earnings surprise. We interpret this as a negative association between retail trade and EIPAR. The coefficient estimate of interest nearly halves (to -0.086) but remains statistically significant when controls are included in column 2. Inclusion of controls mitigates but cannot fully allay concerns about potential confounding effects, such as retail traders opting to trade more in anticipation of harder-to-predict earnings.

4.2 Earnings announcement returns and retail activity

Turning to the association between retail trading and ERCs. We estimate the following regression at the firm-quarter level:

$$\begin{aligned} \text{BHAR}[0,1]_{it} = \beta_0 + \beta_1 \text{SUE}_{it} + \beta_2 \text{Log(Retail)}_{it} + \beta_3 \text{SUE}_{it} \times \text{Log(Retail)}_{it} \\ + \sum \beta_I \text{X}_{I,it} + \sum \beta_k \text{SUE}_{it} \times \text{X}_{k,it} + \epsilon_{it}. \end{aligned} \tag{3}$$

In the above model, the dependent variable is the announcement-window abnormal stock return. Log(Retail)_{it} captures retail trading activity, and X_{it} is a set of controls, described in Section 3.4. The coefficient β_1 is an estimate of the average ERC. Our primary interest is the coefficient β_3 on SUE $\times \text{Log(Retail)}_{it}$, which captures the incremental effects of retail trade on the ERC. To simplify the interpretation of the non-interacted main effects and the economic magnitudes of interactions, all variables are standardized to have a mean of zero and a unit variance.

The coefficient estimates are presented in Table 5. Column 1 presents a specification without control variables, with the exception of year-quarter, day-of-week, and firm fixed effects. In this specification, the SUE coefficient is 0.366 ($p < 0.01$), implying a positive and both economically and statistically significant ERC. The coefficient on SUE $\times$ Log(Retail) is also positive and significant ($\beta = 0.266$, $p < 0.01$), suggesting a positive relation between retail trade activity and ERCs.[17]

Column 2 displays results for a specification including controls and their effects on ERCs,

[17] Of potential interest, the EA-window association between retail trade and abnormal returns is negative. However, this does not reflect a reaction to information per se, and is thus not our focus.

as represented by the interactions between each control and SUE. We regard this specification as more reliable because it facilitates the identification of the retail trade effect on ERCs, independent of potential confounding variables that can be included as controls (e.g., size, analyst coverage, prior turnover, and pre-EA returns). In this specification, the estimated average ERC is 0.313 ($p < 0.01$), and the coefficient of interest on SUE $\times$ log(Retail) remains nearly unchanged in magnitude and significance ($\beta = 0.247$, $p < 0.01$).

Before continuing, we comment briefly on economic magnitudes. Focusing on the estimate in column 2 of Table 5, and recalling that variables are standardized to mean-zero and unit-variance, our estimate implies that a one standard deviation increase in Log(Retail) is related to an ERC that is higher by approximately 0.247. This signifies more than 75% of the average ERC of 0.313, a substantial economic implication. However, it is smaller than the estimated 0.317 increase in ERC from a standard-deviation increase in non-retail trading volume, Log(Nonretail). Notably, this could reflect the effect of retail trading on ERCs as well as a selection effect whereby investors trade more around more informative earnings announcements.

4.3 Post-earnings announcement returns and retail activity

This section presents tests of the association between retail trade during the earnings announcement window and post-earnings returns, focusing specifically on potential drift and reversal. This analysis is conducted based on estimates from equation (3), where the earnings announcement window returns (BHAR[0,1]) are substituted with post-earnings returns (BHAR[2,5], BHAR[2,22], BHAR[2,45], and BHAR[2,next EA]) as the dependent variable.

Table 6 presents our findings. For comparison, column 1 repeats the coefficients computed with BHAR[0,1] as the dependent variable, as shown in Table 5, column 2. The remaining columns depict estimates for post-announcement windows spanning one week, one month, two months, and through the next earnings announcement, respectively.

In line with prior studies finding a positive post-earnings announcement drift (e.g., Bernard and Thomas, 1990), we find that the coefficients on SUE in columns 2 through 5 are positive and significantly different from zero at the 1% level. Our key coefficients of interest, those on the SUE $\times$ Log(Retail) interaction, are consistently positive and significant ($p < 0.01$), with the exception of the coefficient in column 2 for BHAR[2,5] ($p > 0.10$). This implies that retail trading activity during the earnings announcement window is associated with positive drift in post-EA returns in the direction of the earnings surprise for periods extending beyond the week following the earnings announcement.

Our findings suggest that retail trading during the earnings announcement window positively affects post-earnings announcement returns over the months following the announcement. On its own, this is consistent with retail traders slowing the speed at which earnings information is impounded into price, i.e., by introducing dampening noise during the EA window that subsequently corrects. Retail traders as this kind of noise traders, however, is inconsistent with the positive association between retail trade and ERCs in Table 5. A concern is that retail trade may be endogenously higher for earnings that both provide more information to the market (leading to higher ERCs) and take longer to process (leading to higher PEAD). This is effectively a selection problem that has the potential to bias coefficient estimates. Below, we exploit brokerage outages to generate plausibly exogenous variation in retail trade less susceptible to such selection effects.

Before proceeding to the outage-based analysis, we discuss two interesting patterns evident in Table 6. First, average PEAD spikes in the [2,22] window at 0.206, relaxing to roughly 0.020-0.025 in months 2 and 3 following the EA. Similarly, the coefficient on the SUE $\times$ Log(Retail) interaction is estimated to be 0.202 for the [2,22] window but only 0.051 (0.027) over the two-month (three-month) windows. These suggest reversal, both overall and related to EA-window retail trade, in the 2nd month after the EA.

Second, the coefficients on the SUE $\times$ Log(Nonretail) interaction in columns 4 and 5 are significantly negative. Non-retail trade in the EA window thus appears to be associated with return reversals over the two- and three-month horizons. While retail trade contributes consistently to PEAD, the implications of non-retail trade on PEAD depend on the time horizon examined. As our focus is on retail trade, and our empirical strategy below allows us only to obtain plausibly exogenous variation in retail trade, we leave the non-retail volume-PEAD association as a potentially interesting focus for future research.

Building on the patterns in Table 6, Table 7 presents regressions specified as in (3), but with the dependent variable replaced with BHAR over non-overlapping windows from 6 days after the EA through the next EA. Estimating these regressions allows us to test for drift and reversal over the [6,22] (weeks 2 through 4), [23,45] (month 2) and [46,next EA] (month 3) windows. In column 1, we find that PEAD continues through the first month after the EA, with a coefficient on SUE of 0.211 ($p < 0.01$). Coefficients on interactions between SUE and both retail and non-retail trade are also positive, suggesting that EA volume in general is associated with one-month drift. The return pattern turns to reversal in the 2nd month after the EA, however, with a coefficient on SUE of -0.185 ($p < 0.01$). Similarly, the interaction coefficients with SUE and both retail and non-retail trade in column (2) are negative, implying that greater EA window trade is associated with stronger post-earnings announcement *reversals* two months after the EA. In the third month, captured in column (3), we observe a small but significant ($\beta = 0.02$, $p < 0.01$) PEAD coefficient on SUE, but insignificant coefficients on the SUE interactions with trading volumes. These patterns, to our knowledge, are novel to the PEAD literature, and deserve further investigation. However, our focus remains on the effects of retail trade, and these patterns may be affected by important selection effects. Rather than investing here in interpreting the time series pattern, we proceed to tests using brokerage outages as shocks to retail trade, as these are plausibly less susceptible to selection effects noted above.

4.4 Earnings announcement returns and retail frictions from platform outages

Our results thus far suggest that retail traders play a crucial role in incorporating publicly available earnings information into stock prices during the EA window, while, perhaps surprisingly, facilitating return drift following the announcement (i.e., PEAD). However, retail trading—and trading in general—is endogenous to the earnings response setting, as investors, including retail investors, can choose whether and how much to trade based on information about earnings, stock prices, and other signals both observable and unobservable to researchers. Due to this endogeneity, we interpret our results above as indicative of interesting associations, rather than a causal effect of retail trading on price responses to earnings information.

To tease out more causal evidence, we capitalize on randomly timed outages experienced by retail brokerages. These outages, which substantially impede trading on the affected platforms, introduce an exogenous source of variation in retail trading frictions. Importantly, these frictions are plausibly independent of any given firm's earnings announcement and its informational content. To verify the negative effect of outages on retail volume, we estimate the following regression:

$$\text{Log(Retail)}_{it} = \beta_0 + \beta_1 \text{Outage}_t + \sum \beta_k \text{X}_{it} + \epsilon_{it} \tag{4}$$

where Log(Retail)_{it} is the retail trading volume during 5-minute interval t for stock i. Outage is an indicator for top-quintile complaints during a 5-min interval, and the controls vector, X, includes firm and time-of-day fixed effects and firm-time non-retail trade. We estimate equation (4) on a sample including 5-min intervals during outages, matched with intervals 1 trading day before and after for the same stock and time of the day. In these regressions, we do not focus on earnings announcements. Table A.1 in the Appendix presents our estimates of equation (4). We find that outages are negatively associated with retail volume, as expected, and in line with Barber et al. (2021) and Eaton et al. (2022).

Having demonstrated a negative association between outages and retail trade, we exploit outages as shocks whose timing is random with respect to earnings announcements to provide more plausibly causal evidence on the effects of retail trading on market reactions to earnings information. We show in Table A.2 in the Appendix that earnings announced on days with significant outages are broadly similar to earnings announced on other days, consistent with random assignment with respect to SUE, returns, and trading activity. We estimate specifications similar to (3), replacing Log(Retail) with a proxy for significant brokerage outages:

$$\begin{aligned}\mathrm{BHAR[a,b]}_{it} = &\beta_0 + \beta_1 \mathrm{Outage}_t + \beta_2 \mathrm{SUE}_{it} + \beta_3 \mathrm{SUE}_{it} \times \mathrm{Outage}_t + \sum \beta_I \mathrm{X}_{I,it} \\ &+ \sum \beta_k \mathrm{SUE}_{it} \times \mathrm{X}_{k,it} + \epsilon_{it}\end{aligned} \tag{5}$$

where $Outage_t$ is an indicator variable for earnings-announcement dates featuring top-quintile brokerage outage complaints. As before, our ERC tests use BHAR[0,1] as the dependent variable. Our PEAD tests use BHAR[2,b], $b \in \{5, 22, 45, \text{next EA}\}$. We include the same fixed effects, controls, and controls interacted with SUE as in Table 6, as well as a control for Log(MktVol) and its interaction with SUE, given the potential for volume-related stress to jointly affect returns and the probabilities of brokerage outages. For our tests using brokerage outage complaint data, we limit the sample to the January 2019 - June 2021 period covered by the Eaton et al. (2022) outage data. Table A.2 provides descriptive statistics for this subsample.

Results of estimating equation (5) are shown in Table 8. The ERC and PEAD coefficients in the first row follow a pattern similar to that in Table 6. Turning to the coefficient of interest on Outage × SUE, its estimate of -0.060 ($p < 0.05$) in column 1 implies that retail brokerage outages are associated with smaller ERCs. This implies a plausibly causal role of retail trading in shaping immediate market reactions to earnings announcements, consistent with the positive association between retail trade and ERCs shown in Table 5.

Results linking retail trading frictions to PEAD, however, are inconsistent with our association-based results. We do not find evidence of a statistically significant relationship between retail outages and incremental PEAD in column 2-5 of Table 8. This weakens our ability to infer a positive causal effect of EA retail trade on PEAD, as might be implied by the associative analysis presented in Table 6. As indicated above, selection effects may explain the difference in coefficient patterns. Although retail trade can have a causal positive effect on the impounding of earnings information during the EA window, EA retail trade may also be attracted to earnings announcements characterized by greater PEAD, e.g., due to properties of the information that is announced or trading frictions that slow the impounding of information. Since these are plausibly orthogonal to the timing of retail brokerage outages, their effects would not be captured by the Outage $\times$ SUE interaction coefficients in columns 2-5 of Table 8.

4.5 Analysis using outages and predicted retail trade

In this section, we provide additional evidence supporting our inferences from Table 8 by exploiting predictable heterogeneity in retail trade across earnings announcements. This aids identification and mitigates concerns of spurious inference from our outage analysis presented thus far, as retail brokerage outages are most likely to affect announcements where retail trade would have otherwise been more active. This also helps avoid concerns that outages capture variation in market-wide news or activity, which should have systematic effects rather than effects that vary with expected retail trade at the firm-day level.

Following Eaton et al. (2022), we examine the incremental effects of outages on stocks with high versus low expected retail activity, where expected retail activity is derived from a regression model. However, we modify their methodology to better align with our research context and empirical setting, given their focus on daily market quality and contrasting Robinhood to other retail investors and ours on reactions to earnings information for retail trading generally. First, we do not include WallStreetBets mentions as predictors. Such mentions, though salient for meme stocks and Robinhood trading, do not necessarily encapsulate the trading dynamics of the broader market. Our emphasis is on comprehensively understanding retail trading spanning various platforms, beyond just Robinhood.[18] Second,

[18]Most of the explanatory power in predicting retail volume in Eaton et al. (2022) comes from lagged

we exclude the preceding overnight returns (i.e., close-to-open) from our predictive model because they coincide with the period when earnings are typically announced. Their inclusion would inevitably overlap with our [0,1] close-to-close earnings announcement window returns, leading to potentially spurious estimates. Third, we focus on retail trading orthogonal to lagged return levels. Although Eaton et al. (2022) incorporate lagged returns in days [-5,-2] and [-2,-1] windows as predictors, Table 4 demonstrates that they contain information about upcoming earnings surprises. By orthogonalizing retail trade, we ensure our model remains uncontaminated by predictors that might inadvertently signal the magnitude of an impending SUE. This precaution further shields our analysis from misleading correlations. Finally, we exclude date fixed effects from our predictive model, since these are collinear with date-level indicators for outages and could spuriously incorporate the effects of outages into our model that is supposed to predict retail trade absent outages.[19]

Specifically, we leverage a three-step methodology. We first isolate the component of retail trading volume that is orthogonal to recent returns. For this, we regress $Log(Retail)_{i,t}$ on returns in the [-5,-2] and [-2,-1] windows, $Return_{t-2\ to\ t-1}$ and $Return_{t-5\ to\ t-2}$.[20] We estimate the following regression for all firm-days:

$$\text{Log(Retail)}_{i,t} = \beta_0 + \beta_1 \text{Return}_{t-2\ to\ t-1} + \beta_2 \text{Return}_{t-5\ to\ t-2} + \epsilon_{i,t}. \tag{6}$$

From this regression, we extract the residuals, $\hat{\epsilon}_{i,t}$, which capture variation in $Log(Retail)$ that is not explained by lagged returns. We then regress the residuals on the remaining

retail trade, rather than their indicators for WallStreetBets (WSB) quintiles. This can be seen by comparing the R^2 in the third and fourth columns of their Table 4, which rise from 0.24 to 0.86 with the inclusion of lagged retail trade (highly significant, t-stat = 106.6) and lagged Robinhood trade (insignificant, t-stat = -1.32). Additionally, the WSB variables alone explain less than 10% of the variation in retail order imbalance explained jointly by the WSB variables and controls (compare the third and fourth columns in Table 3). Second, one focus of Eaton et al. (2022) was specifically on Robinhood investors. As indicated by Barber et al. (2021), Robinhood investors display tendencies to herd into certain stocks, and are conjectured to be more significantly influenced by financial social media compared to other retail investors. WSB has more limited implications for aggregate retail trade, which is our focus.

[19] Eaton et al. (2022) do not explicitly list firm and date fixed effects as included in their Table 4 prediction regressions, but their inclusion can be inferred from the replication package (available at `https://data.mendeley.com/datasets/5mjd8kbvbd/1`.

[20] We use these windows because they were included in the prediction model of Eaton et al. (2022).

predictors from Eaton et al. (2022), estimated at the stock-day level (i.e., not restricted to EA days):

$$\text{Residualized Log(Retail)}_{i,t} = \beta_0 + \lambda' \text{X}^{firm}_{i,t-1} + \gamma \text{Log(Retail)}_{i,t-1} + \delta \text{Log(Mkt)}_{i,t-1} + \epsilon_{i,t}, \quad (7)$$

where Residualized $\text{Log(Retail)}_{i,t}$ is $\hat{\epsilon}_{i,t}$ from equation (6), $\text{X}^{firm}_{i,t-1}$ is a vector of lagged firm-level control variables, and $Log(Retail)_{i,t-1}$ and $Log(Mkt)_{i,t-1}$ are lagged retail trading volume and lagged market-wide volume, respectively. We also include firm fixed effects. The fitted values from this regression represent our measure of expected orthogonal retail trading activity, which we denote as $E[Retail]$.

Table 9.A presents results from estimating equation (6). Both lagged return variables are positively associated with Log(Retail). This suggests that retail trading volume tends to increase following positive returns, consistent with retail trade on average following return momentum. Estimates of equation (7) are shown in Table 9.B. Retail trade, orthogonalized against recent lagged returns, is positively associated with the returns range, recent retail trade, and recent market volume, and negatively associated with return skewness. The adjusted regression R^2 is 0.874, implying a high degree of predictability for retail trading volume based on our predictors. This is comparable to the regression R^2 of 0.860 reported in the predictive model of Eaton et al. (2022, Table 4, column 4).

As in Eaton et al. (2022), we focus on differences between high and non-high expected retail trade, where high is defined as top-quintile. Specifically, we take $E[Retail]$ from the regression estimated in Table 9.B, drop non-EA days, sort into quintiles, and define $ERetail^{TQ}$ as an indicator for top-quintile $E[Retail]$ within EA days. Interacting this indicator with the variables of interest from equation (5) yields an approach similar to a difference-in-differences design. The estimate of interest in this analysis involves, first, the difference in ERCs and PEAD for firms with retail brokerage outages on EA days versus those without, and, second, the difference across firms with high versus low expected retail trade. If retail trade is causally associated with higher ERCs, we expect to see incrementally lower ERCs for announcements on outage days for firms with high expected retail trade. Specifically, we estimate the following regression equation:

$$\begin{aligned}\text{BHAR[a,b]}_{it} = \beta_0 &+ \beta_1\text{Outage}_t + \beta_2\text{SUE}_{it} + \beta_3 ERetail^{TQ} + \beta_4\text{SUE}_{it} \times \text{Outage}_t \\ &+ \beta_5 ERetail^{TQ} \times \text{Outage}_t + \beta_6\text{SUE}_{it} \times ERetail^{TQ} \\ &+ \beta_7\text{SUE}_{it} \times \text{Outage}_t \times ERetail^{TQ} \\ &+ \sum \beta_k \text{X}_{k,it} + \sum \beta_k \text{SUE}_{it} \times \text{X}_{k,it} + \epsilon_{it},\end{aligned}$$

where $ERetail^{TQ}$ is an indicator for top-quintile expected retail trading volume. Other variables are the same as in equation (5).

Table 9.C shows the incremental effects on ERCs and PEAD of brokerage outages on firms with high predicted retail trading activity. The coefficients of interest are on the triple interaction between SUE, Outage, and $ERetail^{TQ}$. In column 1, this coefficient is negative and significant ($\beta = -0.060$, $p < 0.05$). This implies that for firms with higher expected retail trading, the positive market reaction to earnings surprises is reduced during brokerage outages. This result confirms the plausibly causal role of retail trading in increasing EA-window price reactions to earnings news. In line with the results in Table 8, the coefficients of interest on the triple interactions in columns 2-4 are insignificantly different from zero, though there is a positive and marginally significant ($\beta = 0.050$, $p < 0.10$) coefficient for the [2,next EA] window. This is consistent the under-reaction related to the outage during the EA window for high expected retail trade announcements reversing around the subsequent EA. Additionally, we find a positive coefficient in column 1 on the SUE $\times$ $ERetail^{TQ}$ interaction, consistent with a positive association between high expected retail trade and ERCs, which could be driven by retail traders' selection to trade around more informative earnings.

Overall, our evidence presented thus far implies that EA-window retail trade increases market reactions to earnings news, resulting in higher ERCs. Associative evidence suggests that retail trade also facilitates PEAD, but this is not confirmed by a natural experiment exploiting retail brokerage outages.

4.6 Retail noise or informed trade: buy-sell imbalance tests

We next test whether the average direction of trade for retail investors during earnings announcements is associated with EA returns and ERCs. This helps distinguish between retail-as-noise and other possible interpretations discussed further in Section 5. Retail order imbalance during the EA window is defined as:

$$Retail\ OIB_{i,t} = \frac{\text{Retail Buys}_{i,t} - \text{Retail Sales}_{i,t}}{\text{Retail Buys}_{i,t} + \text{Retail Sales}_{i,t}}.$$

If retail trade is reacting to the EA, either in an informed or overconfident way, then we expect retail order imbalance to be positively associated with returns, and market reactions to earnings surprises to be stronger when the surprises are aligned with retail OIB. If retail trade is mostly noise, then retail OIB should be roughly independent from returns.[21] To distinguish these, we re-estimate our ERC regressions from Table 5 replacing log retail volume with Retail OIB as the independent variable of interest.

Table 10 shows that retail order imbalance is not generally associated with returns during the earnings announcement window, which suggests that retail traders are not generally trading in a directional manner that supports ERCs. This is further corroborated by Figure 3, which shows a similar pattern of retail buy-sell imbalance for both positive and negative earnings surprises. Except for a notable increase leading up to the earnings announcement, the retail buy-sell imbalance across the two series plotted in is slightly negative.

[21] In many theoretical microstructure studies, building on Kyle (1985), market makers set stock prices based on expectations of fundamental value conditional on aggregate order flow. In such a setting, price is responsive to noise trade because the market maker cannot distinguish noise from informed trade. If they could, then price would be independent of noise trade. In our setting, and inherent in the Boehmer et al. (2021) identification of retail trade, market makers (i.e., wholesalers paying for order flow) *can* distinguish retail trade from other order flows. As such, in our setting retail-as-noise should yield effectively no association between net order flow (i.e., Retail OIB) and returns.

The evidence presented in Table 10 and Figure 3 suggests that retail traders are not generally responding to the information in public earnings surprises. Instead, our findings indicate that the nature of retail order flow is more akin to noise trade, unrelated to fundamentals. This comports with wholesalers' practice of offering price improvements (i.e., payments for order flow) to largely noisy retail order flow (e.g., Easley et al., 1996).

5 Theories and narratives of retail trade in light of our results

Prior studies have developed several narratives and sets of related hypotheses relating to different ways in which retail traders might affect market prices and the information content of prices. We discuss several of these in this section, aiming to be reasonably comprehensive. Nonetheless, we acknowledge the likelihood of unintentional omissions. Our primary goal is to compare and assess competing theories in light of our evidence. The general pattern of our evidence is that retail trade leads to higher ERCs and, in association tests, lower EIPAR and higher PEAD. Retail trade is also directionally independent from earnings surprises.

5.1 Retail traders are no different

The most common models of capital markets typically assume investor homogeneity or, equivalently, that investors' effects on prices and returns can be summarized by a single representative investor. Even if investors are heterogeneous, their differences are idiosyncratic and wash out in the aggregate.[22] In these models, retail investors would have no effect on the pricing of corporate earnings. Under the *no difference* narrative, more retail activity would not be associated with EIPAR, ERCs, or PEAD. Our findings are inconsistent with the narrative/theory that retail trade is not systematically different from non-retail trade.

[22]The homogeneous or representative investor model is consistent with but not equivalent to semi-strong market efficiency, which implies that prices should respond fully and completely to public information, regardless of the composition of traders. Investors can be homogeneous but still face frictions that cause prices to react slowly to information releases. The key is whether the departure from semi-strong efficiency is related to or driven by the composition of traders.

5.2 Retail traders are noise traders

A common convenience assumption in capital market models is that there is a set of traders who trade for reasons orthogonal to the information about an asset's value, such as idiosyncratic liquidity shocks, misinterpretation of signals, or mood (e.g., Black, 1986). These traders, particularly in noisy rational expectations models (e.g., Hellwig, 1982; Grossman and Stiglitz, 1981; Verrecchia, 1982; Diamond and Verrecchia, 1981) exist in part to prevent stock prices from fully revealing informed traders' information. Mechanically, noise traders move price away from fundamentals, weakening the link between fundamental information and asset values, while providing liquidity that can motivate other market participants to acquire relevant information (e.g., Kyle, 1985). Noise traders can also have a negative effect on liquidity, by increasing market makers' expected inventory costs (e.g., Ho and Stoll, 1981) or imposing risk on market participants in general, leading to greater price protection (e.g., De Long et al., 1990).[23] Overall noise trade around public information releases tends to weaken the price response to the information or the degree to which returns reflect private anticipatory information prior to the EA. If retail trades are primarily noise, then we would expect greater retail activity to be associated with lower EIPAR, weaker ERCs, and plausibly greater PEAD as prices slowly incorporate earnings news. The pattern of our results does not support the primarily noise narrative and related theories.

Retail traders, while acting as noise traders who potentially generate uninformative price pressure, can create liquidity that allows other traders to impound information. Increased liquidity and the exploitation of that liquidity by informed traders should be associated with prices that are more reflective of both private and public information. Note that retail noise trade does not directly imply liquidity provision. Retail noise trade can impose risk on other traders (e.g., De Long et al., 1990) or market makers (e.g., Ho and Stoll, 1981). If retail traders are primarily liquidity providers, then greater retail activity should be associated

[23] The evidence in Eaton et al. (2022) is consistent with some retail investors behaving as coordinated noise traders that market makers price protect against, while other retail investors are net liquidity providers.

with higher EIPAR and ERCs, and less PEAD.

5.3 Retail traders are particularly attention-driven

A large and growing literature focuses on investor attention. Naturally, for investors to react to information, they have to pay attention to it. Narratives focusing on investor attention can broadly be classified as either entirely behavioral or constrained-rational. On the behavioral side, studies have highlighted the potential for investor distraction to reduce attention to earnings announcements (e.g., DellaVigna and Pollet, 2009; Hirshleifer et al., 2009; Diamond et al., 2016). Similarly, excess attention can enhance trading activity (Cahill et al., 2021), but the attention can lead to trading (often buying) independent of the underlying information on average (e.g., Hirshleifer et al., 2008; Michels, 2023). Constrained-rational models, in contrast, explicitly allow investors to choose what to pay attention to, but impose costs of such attention (possibly opportunity costs) that limit the use of information in equilibrium (e.g., Hirshleifer and Teoh, 2003; Sims, 2003; Stijn and Veldkamp, 2010).[24]

Regardless of the underlying mechanisms (i.e., distraction or constrained-rationality), inattention to an earnings announcement will cause the announcement to have weaker effects on stock prices. Subsequent attention to the news can cause PEAD. DellaVigna and Pollet (2009) and Hirshleifer et al. (2009) show that prices respond more weakly to earnings surprises when investors are likely to be distracted. The underreaction they document is followed by PEAD, implying a subsequent correction of the underreaction. If retail investors are more subject to attention effects than other investors, then increased retail trade around an earnings announcement is likely to reflect increased attention to the earnings news. This would cause retail trade to be associated with stronger ERCs, less PEAD, and pre-EA returns that reflect more of the upcoming earnings information (higher EIPAR).

[24] See Blankespoor et al. (2020) for a review of literature discussing various costs investors face when obtaining and processing disclosures, and how these costs affect market outcomes.

5.4 Retail traders are overconfident

Traders can be overconfident about their trading abilities or, relatedly, the quality of the information they receive. Daniel et al. (1998) develop a model where overconfident investors view their private information as being better than it really is, leading to overreaction that is subsequently corrected.[25] In their model, investors rationally process public information, which corrects the initial overreaction over time. Overconfidence in some (i.e., private) signals can also lead investors to under-weight other (i.e., public) signals. Lihong (2003) finds that investors are insufficiently responsive to the quality of public signals. Overconfidence can also generally lead to more and worse trading. Barber and Odean (2001) find that men, who are more likely to be overconfident, trade more and earn lower returns than women.

The effects of retail investor overconfidence depend crucially on several other features (e.g., correlation between private signals, and the degree of over- or under-confidence with regard to public signals such as earnings forecasts and announcements). Retail traders could be overconfident about uninformative signals prior to the earnings announcement but correctly interpret publicly announced earnings information. Alternatively, they could be overconfident about private signals and under-weight public earnings information, or be overconfident about noisy but informative private signals. Given the potential for narratives building on overconfidence to provide differing predictions related to ERCs, PEAD, and EIPAR, we note that overconfidence among retail traders could be consistent with our results, but would require further refinement regarding what signals retail traders are overconfident about. We leave such refinements for future work, noting that our evidence is inconsistent at least with the most prominent theories of retail traders either being overconfident about public signals (implying higher ERC and lower PEAD) or private noisy signals (lower ERC and higher PEAD) around earnings announcements.

[25] They also discuss the attribution bias which can facilitate overconfidence when confirmatory signals are overweighted relative to contrary signals.

5.5 Retail traders are relatively informed

The narratives discussed so far portray retail investors, if anything, as prone to making trading decisions in ways that do not reflect fundamentals, either by assumption (i.e., as noise traders), due to inattention or excessive attention, or based on incorrect beliefs about signals. Alternatively, retail investors may be relatively informed. Using proprietary NYSE data that provides aggregate daily buy and sell orders originating from individuals, Kaniel et al. (2012) show that individual trades *before* the earnings announcement predict both earnings announcement window returns and drift. Kelley and Tetlock (2013) use proprietary data from two over-the-counter market centers to show that retail market orders predict both near-term returns and news, including earnings announcements, while retail limit orders provide liquidity.[26] Both Kaniel et al. (2012) and Kelley and Tetlock (2013) interpret their evidence as consistent with individuals having private information and providing liquidity, but they do not focus on retail investors' effects on the incorporation of public information.

If retail investors are relatively informed *prior* to the earnings announcement, we should expect lower ERCs and greater EIPAR when retail traders are more active. PEAD should also be lower when there is greater informed retail trade prior to and around the earnings announcement. This is inconsistent with our findings, suggesting that retail traders are not, on average, best treated as informed traders.

5.6 Overall inference

We view our evidence, overall, as consistent with a nuanced view of retail trade. Our best evidence, based on outages, with support from Retail OIB tests, suggests that retail trade around earnings announcements represents noise that facilitates higher ERCs, plausibly by providing liquidity to other traders. Our other evidence points to a negative association between retail trade and EIPAR, and a positive (non-causal) association between retail

[26] Aboody et al. (2010) find that price increases in the 12 months prior to an earnings announcement are associated with positive (negative) returns during (following) the earnings announcement. They attribute their result at least in part to individual investor attention attracted to high pre-announcement returns.

trade and PEAD. Both of these are consistent with a selection narrative in which retail trade is more likely around earnings announcements that are: (1) more difficult to predict, which leads to to lower EIPAR; and (2) takes more time to be incorporated into price, leading to higher PEAD. The negative association between retail trade and EIPAR is also consistent with retail trade as noise, although in the pre-announcement window, absent a salient public signal, the noise seems to reduce the information in price via a direct noise effect, rather than increasing it via a liquidity provision effect. Although we use the term selection, retail traders selecting to trade in certain stocks could be driven by attention or overconfidence effects. Retail attention may be attracted to earnings that are difficult to predict and interpret, potentially because they are overconfident about their ability to profit in such an environment. Nonetheless, in the EA window, their trading seems to provide liquidity to traders who impound earnings news.

6 Additional analyses

This section discusses additional analyses, presented in Appendix A, that support our earlier analysis and show robustness to alternative specifications or sample cuts. Table A.1, discussed above, supports our use of outages as a negative shock to retail trade using intraday data. Table A.2 shows that earnings announced on outage and non-outage days are largely similar, with the exception of a large mechanical difference in the number of outage complaints. Table A.3 presents PEAD regressions with EA-window retail trade replaced by PEAD-window retail trade, and finds results consistent with those in Table 6. Tables A.4-A.9 replicate our ERC and PEAD regressions in subsamples and, in Tables A.6-A.9, compare the coefficients of interest across subsamples. Table A.4 limits the sample to extreme earnings announcements (top 2 and bottom 2 quantiles). Table A.5 uses a subsample with single-penny bid-ask spreads where the Boehmer et al. (2021) methodology is more likely to identify retail trades via price improvement (see Barber et al. (2023) for further details).[27] Table A.6 presents estimates from subsamples of positive-only and negative-only SUE and shows that retail trade is associated with incrementally higher ERCs for positive earnings news. Consistent with this, Figure A.1 demonstrates that retail trading volumes (as well as non-retail volumes) tend to be higher around positive surprises compared to negative surprises. Table A.7 presents estimates split by firm size. Retail trade amplifies the ERC more in larger firms than in smaller firms. Table A.8 separates normal EA days from busy EA days, i.e., days without too many competing earnings announcements. Coefficients of interest are not significantly different across this split. Table A.9 shows that our result are similar for EAs falling on Fridays as for EAs falling on other days (e.g., DellaVigna and Pollet, 2009).

Table A.10 is a "reverse" regression of the specification presented in Table 4, with PreRet as the dependent variable and PreRetail interacted with SUE. The negative coefficient on the interaction implies that foreknowledge of the earnings surprise is less positively associated with pre-announcement returns when retail trade is greater. As in Table 4, retail trade weakens the association between pre-announcement returns and the earnings surprise.

Table A.11 shows that our outage-based results are robust to entropy balancing to address potential differences between earnings announced on outage days versus non-outage days and to using the EA day's number of complaints in lieu of the top-quintile complaint indicators. Table A.11 also provides a placebo test with insignificant coefficients of interest when the Outage indicator is replaced with a pseudo-indicator drawn randomly (repeated 1,000 times to generate coefficient distributions). Table A.12 shows that our inferences are insensitive to defining outage days based on other quantiles of the distribution of complaints.

[27] Barber et al. (2023) note the potential for signing errors due to fluctuating spreads, which could affect our OIB tests but are unlikely to affect our analysis of unsigned trade volumes.

7 Discussion and Conclusion

This study examines how retail trading affects market reactions to corporate earnings announcements using the recently-developed TAQ-based measure of retail trade based on price improvement offered to retail market orders and outages at retail brokerages. Importantly, retail brokerage outages, which make it harder for their users to trade, are random with respect to earnings news, so we can treat them as exogenous shocks to retail trading frictions and use them to identify plausibly causal effects of retail trade on the pricing of earnings news.

Our findings suggest that retail activity leads to stronger reactions to earnings announcements, which is consistent with information-based trade or liquidity provision. Furthermore, ERCs are lower for EAs which are affected by retail brokerage outages, and this effect is stronger for announcements expected to receive high retail trade. Retail traders potentially impound noise into prices, but can also trade on available information and provide liquidity to other market participants. Additional evidence aligns with liquidity provision, as retail order imbalance is not associated with EA window returns or their sensitivity to the earnings surprise. Additional evidence suggests that retail traders act as noise traders prior to earnings announcements, weakening the relation between pre-EA returns and the earnings news. Furthermore, although we find a relation between retail activity and PEAD in associative tests, a lack of support in outage-based tests implies that selection effects may explain the association-based evidence. Specifically, retail traders may trade more around earnings announcements that take longer to impound into price. In spite of this, their liquidity provision during the EA window leads to stronger market reactions and ERCs.

Our results suggest that whether retail traders' net effect is to impound noise or provide liquidity to informed trade may depend on the availability of public information. Prior to earnings announcements, retail traders and liquidity-takers are plausibly relatively uninformed, given their limited access to resources available to professional and institutional traders or the scarcity of private information about upcoming earnings. Once earnings are announced, traders can access substantial amounts of corporate information, including actual earnings, conference call transcripts, and analysis (e.g., via SeekingAlpha contributors). This information access can facilitate investors' processing of earnings information, and the use of retail-provided liquidity to conduct trades that impound earnings news into price.

Our results on the relation between retail trade and PEAD extend the findings of Hirshleifer et al. (2008) and Bartov et al. (2000) while complementing those of Michels (2023). Retail trade during the earnings announcement window is associated with higher PEAD, but not in tests exploiting a (lack of) retail trade exogenously driven by retail brokerage outages. Our inference is similar to Hirshleifer et al. (2008), in that we cannot reject the null hypothesis that retail trade has no causal effect on PEAD.

Our findings may differ from prior studies (e.g., our stronger *association* between retail trade and PEAD relative to Hirshleifer et al. (2008)) because of differences in sample time frame. Prior studies have largely used data from periods in which internet-based stock market information was harder to access and trade on (e.g., the early to mid-1990s). Broadly, our results contribute to the emerging non-hegemonic characterization of retail investors as market participants who can either help or hinder the degree to which prices reflect public information, potentially due to the multiple ways they can now access and trade on various signals (e.g., Aboody et al., 2010; Barber and Odean, 2000; Kaniel et al., 2008, 2012; Kelley and Tetlock, 2013; Ozik et al., 2020; Welch, 2022).

We close by suggesting some potentially interesting avenues for future research. Studies are already using multiple measures of retail activity, including the Boehmer et al. (2021) proxy and proxies based on Robinhood holdings to examine reactions to non-earnings announcements (e.g., Moss et al., 2023), but could examine whether and how retail traders facilitate information transfer across firms. Additionally, the effects of retail trading are likely to be contextual, differing across heterogeneous institutional environments. We believe further attention is merited to identify features that separately encourage or discourage noise, liquidity provision, or information-based trade. Theoretical studies might provide insight into how the differing effects of retail traders in different settings affect aggregate efficiency and incentives to acquire information. Additional theory and evidence on how firms optimize their disclosures in the face of retail investors who are not just one-dimensional noise traders seems warranted.

Figures

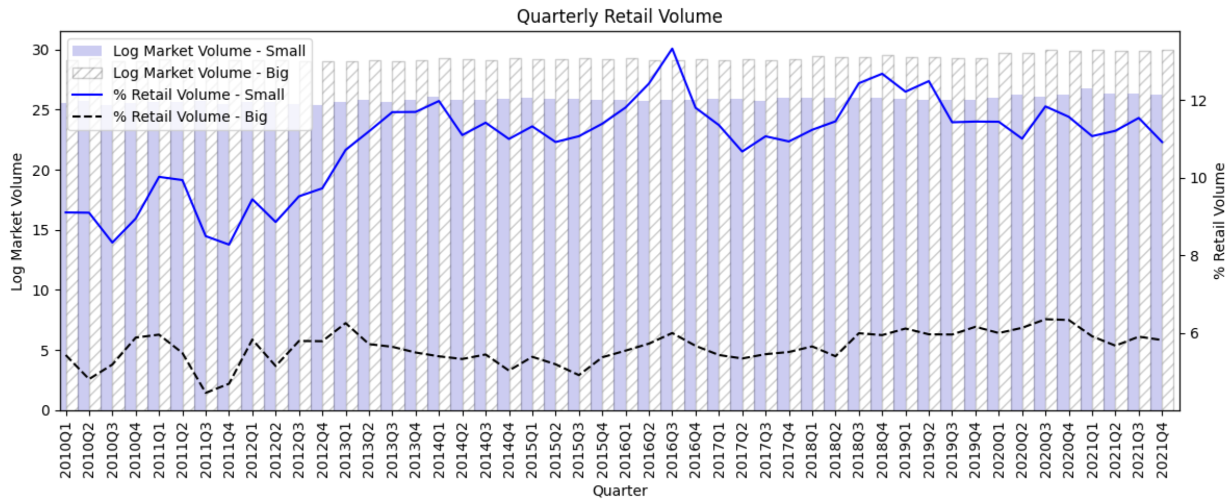

Figure 1: Quarterly Market Volume (share value traded) and % Retail Volume for small and big firm subsamples. Small and big designations are based on a median split on market value of equity.

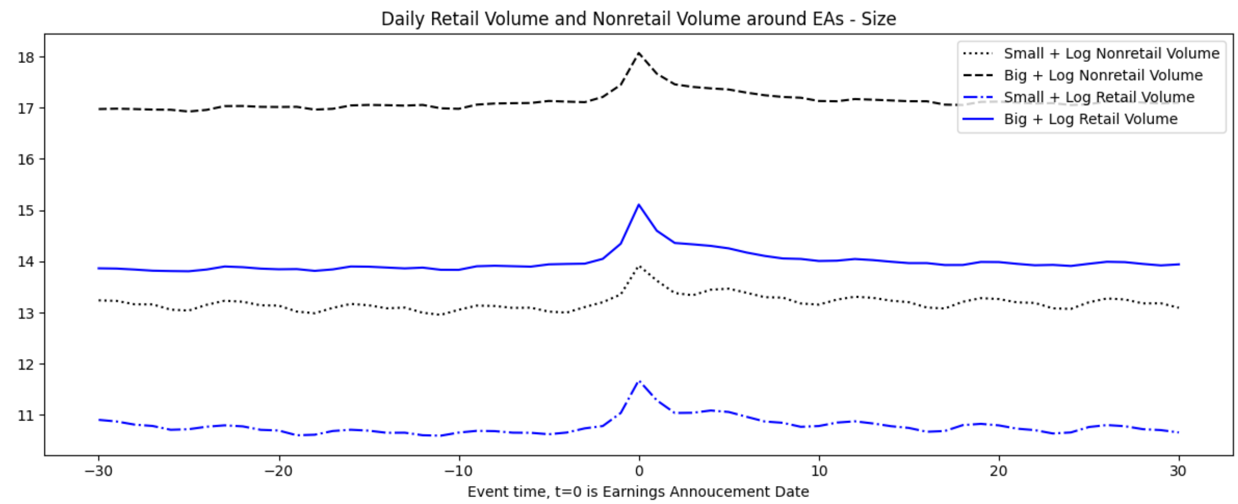

Figure 2: Daily Retail and Nonretail Volume around earnings announcements for small and big firm subsamples. Small and big designations are based on a median split on market value of equity.

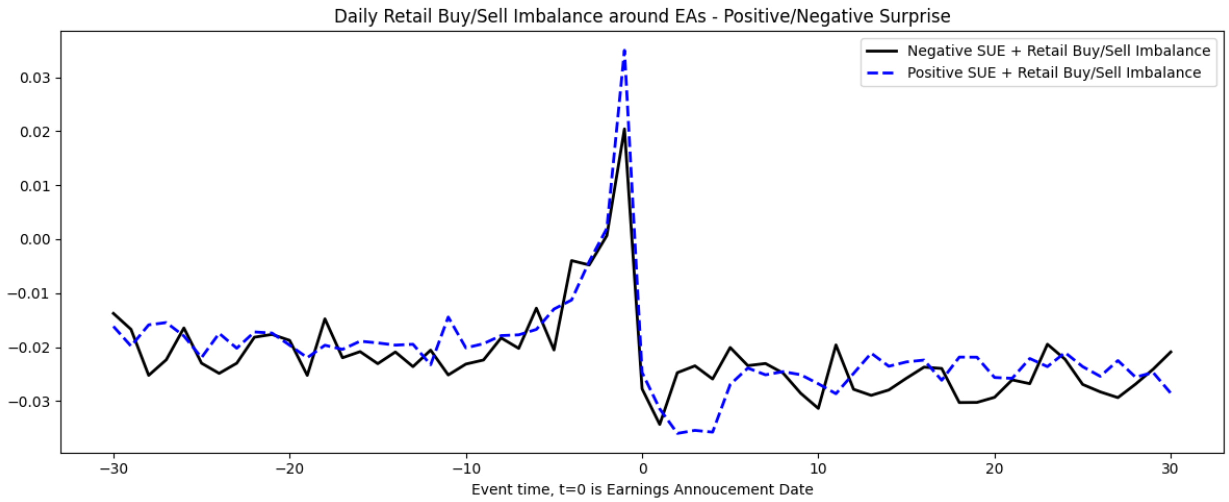

Figure 3: Daily Retail buy-sell imbalance around earnings announcements for positive and negative earnings surprises.

Tables

Table 1: Sample selection

	Earnings Announcements
Compustat and CRSP sample	139,870
Less: missing IBES data or with multiple earnings timestamps per quarter	7,904
Less: missing returns	49,935
Less: missing TAQ data, i.e., not listed on Nasdaq/NYSE/AMEX or with multiple ticker-date observations	2,174
Less: missing controls	14,536
	Final Sample
Unique firms	3,979
Unique earnings announcements	65,321

Table 2: Descriptive Statistics

	mean	sd	min	max	N
SUE(raw)	-0.00	0.07	-13.20	1.95	65321
SUE	6.91	3.18	1.00	11.00	65321
BHAR[0,1]	-0.00	0.08	-0.25	0.26	65321
BHAR[2,5]	-0.00	0.05	-0.15	0.18	65321
BHAR[2,22]	0.00	0.14	-0.37	0.50	65321
BHAR[2,45]	-0.00	0.15	-0.41	0.59	65321
BHAR[2,next]	-0.00	0.17	-0.47	0.69	65321
#Complaints	233.59	1500.54	0.00	12598.00	65321
Log(Retail)	14.49	1.98	5.99	21.85	65321
Log(Nonretail)	17.18	2.16	7.50	23.58	65321
PreRetail	15.25	1.88	8.88	23.11	65321
PreNonretail	18.06	2.11	10.17	24.71	65321
Log(MktVol)	17.26	2.13	7.70	23.61	65321
PreRet	0.01	0.10	-0.79	2.22	65321
Log(Size)	7.15	1.78	0.89	13.56	65321
Book-to-Market	0.66	0.64	0.00	56.96	65321
EPersistence	0.15	0.34	-0.80	1.03	65321
EVOL	1.82	6.67	0.04	63.37	65321
ERepLag	35.94	13.37	-7.00	442.00	65321
#Estimates	8.41	6.71	1.00	45.00	65321
TURN	21.24	19.03	1.36	132.82	65321
Loss	0.29	0.45	0.00	1.00	65321
#Announcements	144.50	90.57	1.00	408.00	65321
IO	0.55	0.38	0.00	1.00	65321

This table presents descriptive statistics for the sample. Detailed definitions of all variables are included in Appendix A.

Table 3: Correlations

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
1 SUE	1.000																						
2 BHAR[0,1]	0.340***	1.000																					
3 BHAR[2,5]	0.034***	0.015***	1.000																				
4 BHAR[2,22]	0.233***	0.630***	0.371***	1.000																			
5 BHAR[2,45]	0.035***	0.027***	0.345***	0.539***	1.000																		
6 BHAR[2,next]	0.048***	0.028***	0.303***	0.480***	0.829***	1.000																	
7 Log(Retail)	0.110***	0.029***	0.019***	0.022***	-0.016***	0.002	1.000																
8 Log(Nonretail)	0.119***	0.045***	0.021***	0.033***	-0.012***	0.006	0.941***	1.000															
9 #Complaints	0.015***	-0.001	0.001	0.007*	0.002	0.001	0.029***	0.021***	1.000														
10 PreRetail	0.081***	-0.006	0.009*	-0.008*	-0.019***	-0.003	0.918***	0.892***	0.023***	1.000													
11 PreNonretail	0.096***	0.015***	0.014***	0.007*	-0.014***	0.002	0.888***	0.961***	0.012***	0.935***	1.000												
12 Log(MktVol)	0.121***	0.043***	0.021***	0.035***	-0.008*	0.008*	0.951***	0.999***	0.027***	0.899***	0.960***	1.000											
13 PreRet	0.184***	0.261***	-0.003	0.165***	0.011**	0.025***	0.057***	0.048***	0.005	0.043***	0.037***	0.052***	1.000										
14 Log(Size)	0.115***	0.037***	0.013***	0.023***	-0.009**	0.005	0.810***	0.901***	0.024***	0.834***	0.924***	0.896***	0.050***	1.000									
15 Book-to-Market	-0.047***	0.013***	0.007*	0.030***	0.032***	0.036***	-0.188***	-0.227***	0.009*	-0.163***	-0.210***	-0.232***	0.033***	-0.264***	1.000								
16 EPersistence	0.006	-0.010**	-0.000	-0.002	0.006	0.005	0.018***	0.006	-0.010**	0.022***	0.002	0.009**	0.006	-0.028***	-0.008*	1.000							
17 EVOL	-0.024***	-0.032***	-0.024***	-0.052***	-0.049***	-0.059***	-0.035***	-0.069***	-0.007*	-0.017***	-0.064***	-0.063***	-0.024***	-0.103***	0.094***	-0.041***	1.000						
18 ERepLag	-0.090***	-0.025***	-0.012***	-0.028***	-0.026***	-0.020***	-0.267***	-0.322***	0.039***	-0.257***	-0.319***	-0.320***	0.003	-0.347***	0.099***	-0.019***	0.060***	1.000					
19 #Estimates	0.073***	0.011**	0.013***	0.007	-0.003	-0.001	0.689***	0.711***	-0.016***	0.711***	0.724***	0.713***	0.004	0.693***	-0.116***	0.059***	-0.022***	-0.250***	1.000				
20 TURN	0.017***	-0.036***	-0.008*	-0.032***	-0.033***	-0.031***	0.369***	0.281***	0.041***	0.344***	0.237***	0.312***	-0.007	0.062***	0.024***	0.070***	0.172***	-0.010**	0.232***	1.000			
21 Loss	-0.215***	-0.131***	-0.023***	-0.088***	-0.019***	-0.041***	-0.219***	-0.289***	0.026***	-0.204***	-0.293***	-0.277***	-0.076***	-0.352***	0.114***	0.009*	0.135***	0.203***	-0.166***	0.164***	1.000		
22 #Announcements	0.024***	0.006	-0.011**	0.020***	0.033***	0.020***	-0.005	0.058***	-0.026***	0.011**	0.054***	0.066***	-0.025***	0.100***	-0.020***	0.008*	-0.016***	-0.288***	-0.008*	-0.009*	-0.009*	1.000	
23 IO	0.044***	0.023***	0.016***	0.032***	0.005	-0.007	0.301***	0.355***	0.061***	0.209***	0.301***	0.367***	-0.002	0.345***	-0.126***	-0.075***	-0.068***	-0.124***	0.150***	0.034***	-0.047***	0.078***	1.000

This table presents Pearson correlations. Detailed definitions of all variables are included in Appendix A. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 4: Earnings Surprises, Pre-Earnings Announcement Retail Trading Volume and Pre-Earnings Announcement Returns

	Dependent Variable: SUE	
	(1)	(2)
PreRetail	0.00708	0.0254
	(0.0269)	(0.0255)
PreRetail × PreRet	-0.163***	-0.0856***
	(0.0138)	(0.0150)
PreNonretail	-0.0284	-0.151***
	(0.0341)	(0.0373)
PreNonretail × PreRet	0.162***	0.0289
	(0.0140)	(0.0225)
PreRet	0.209***	0.211***
	(0.0119)	(0.0106)
Controls	No	Yes
Year-Quarter FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Firm FE	Yes	Yes
Observations	65194	65194
Adj.R2	0.133	0.168

This table presents estimates of $SUE_{it} = \beta_0 + \beta_1 PreRetail_{it} + \beta_2 PreRet_{it} + \beta_3 PreRetail_{it} * PreRet_{it} + \beta_4 PreNonretail_{it} + \beta_5 PreNonretail_{it} * PreRet_{it} + \sum \beta_k X_{k,it} + \epsilon_{it}$. All independent variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market , EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 5: Earnings Announcement Returns and Retail Trading Volume

	Dependent Variable: BHAR[0,1]	
	(1)	(2)
SUE	0.366***	0.313***
	(0.0109)	(0.00638)
Log(Retail)	-0.109***	-0.251***
	(0.0343)	(0.0327)
SUE × Log(Retail)	0.266***	0.247***
	(0.0220)	(0.0186)
Log(Nonretail)	0.124***	0.468***
	(0.0397)	(0.0471)
SUE × Log(Nonretail)	-0.142***	0.317***
	(0.0194)	(0.0270)
PreRet		0.239***
		(0.0127)
Controls	No	Yes
Controls*SUE	No	Yes
Year-Quarter FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Firm FE	Yes	Yes
Observations	65194	65194
Adj.R2	0.152	0.247

This table presents estimates of $BHAR[0,1]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The dependent variable, BHAR[0,1], is earnings announcement abnormal returns. The independent variable of interest is the interaction between the earnings surprise quantile (SUE) and log(Retail Volume) during the earnings announcement window. All independent variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market , EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors and coefficients are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 6: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.313***	0.0278***	0.206***	0.0195***	0.0247***
	(0.00638)	(0.00633)	(0.00583)	(0.00570)	(0.00588)
Log(Retail)	-0.251***	0.0100	-0.133***	0.00874	0.0263
	(0.0327)	(0.0244)	(0.0270)	(0.0223)	(0.0237)
SUE × Log(Retail)	0.247***	0.0286	0.202***	0.0510***	0.0372***
	(0.0186)	(0.0182)	(0.0186)	(0.0130)	(0.0135)
Log(Nonretail)	0.468***	0.0992**	0.340***	0.0343	0.0260
	(0.0471)	(0.0391)	(0.0399)	(0.0409)	(0.0433)
SUE × Log(Nonretail)	0.317***	-0.0122	0.138***	-0.0716***	-0.0411*
	(0.0270)	(0.0237)	(0.0233)	(0.0212)	(0.0237)
PreRet	0.239***	-0.00556	0.141***	0.00892	0.00139
	(0.0127)	(0.0105)	(0.00947)	(0.0114)	(0.0127)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	65194	65194	65194	65194	65194
Adj.R2	0.244	0.0236	0.134	0.0531	0.0613

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \beta_4 Log(Nonretail)_{it} + \beta_5 SUE_{it} * Log(Nonretail)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The dependent variables, BHAR[a,b], are abnormal returns from day a to day b around the earnings announcement day 0. The independent variable of interest is the interaction between the earnings surprise quantile (SUE) and log(Retail Volume) during the earnings announcement window. All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 7: Post-earnings Announcement Drift (Non-overlapping windows)

	BHAR[6,22]	*BHAR[23,45]*	*BHAR[46,next EA]*
	(1)	(2)	(3)
SUE	0.211***	-0.185***	0.0215***
	(0.00572)	(0.00650)	(0.00639)
Log(Retail)	-0.152***	0.226***	0.0660***
	(0.0281)	(0.0283)	(0.0214)
SUE × Log(Retail)	0.202***	-0.158***	0.0227
	(0.0174)	(0.0149)	(0.0151)
Log(Nonretail)	0.336***	-0.197***	0.00732
	(0.0406)	(0.0402)	(0.0379)
SUE × Log(Nonretail)	0.162***	-0.229***	-0.00456
	(0.0211)	(0.0220)	(0.0236)
PreRet	0.151***	-0.131***	-0.00229
	(0.00948)	(0.0115)	(0.0118)
Controls	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes
Observations	65194	65194	65194
Adj.R2	0.137	0.107	0.00674

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \beta_4 Log(Nonretail)_{it} + \beta_5 SUE_{it} * Log(Nonretail)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The dependent variables, BHAR[a,b], are abnormal returns from day a to day b around the earnings announcement day 0. The independent variable of interest is the interaction between the earnings surprise quantile (SUE) and log(Retail Volume) during the earnings announcement window. All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 8: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Frictions

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.311***	0.0425**	0.214***	0.0473***	0.0358*
	(0.0142)	(0.0175)	(0.0175)	(0.0145)	(0.0169)
Outage	-0.00434	-0.00562	0.0138	0.0579	0.0524
	(0.0247)	(0.0506)	(0.0588)	(0.0462)	(0.0466)
Outage × SUE	-0.0604**	-0.0157	0.00417	0.0318	0.0437
	(0.0250)	(0.0273)	(0.0330)	(0.0426)	(0.0483)
Log(MktVol)	0.298***	0.0718	0.276***	0.0469	0.0677
	(0.0639)	(0.0642)	(0.0640)	(0.0715)	(0.0792)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	21549	21549	21549	21549	21549
Adj.R2	0.212	0.0356	0.175	0.135	0.167

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 Outage_t + \beta_2 SUE_{it} + \beta_3 SUE_{it} * Outage_t + \beta_4 Log(MktVol)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{it} + \epsilon_{it}$ The dependent variables, BHAR[a,b], are abnormal returns from day a to day b around the earnings announcement day 0. The independent variable of interest is the interaction between the earnings surprise quantile (SUE) and Outage, an indicator for a top-quintile brokerage outage on the same day as the earnings announcement. All variables are standardized to be mean-zero and unit-variance. Control variables include Log(MktVol), PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 9.A: Regression to orthogonalize *Log(Retail)* against recent returns

	Log(Retail)
$\text{Return}_{t-2\ to\ t-1}$	1.278***
	(0.0167)
$\text{Return}_{t-5\ to\ t-2}$	1.069***
	(0.0106)
Firm FE	No
Date FE	No
Observations	7426261
Adj.R2	0.00327

This table presents estimates of $Log(Retail)_{it} = \beta_0 + \beta_1 Return_{t-2\ to\ t-1} + \beta_2 Return_{t-5\ to\ t-2} + \epsilon_{it}$, estimated at the stock-day level. All variables are standardized to be mean-zero and unit-variance. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 9.B: Predicting return-orthogonalized retail trading

	Residual *Log(Retail)*
Return Skewness	-0.00192***
	(0.000366)
Return Range	0.0608***
	(0.00107)
Price_{t-1}	0.0902***
	(0.00375)
Market Cap_{t-1}	0.0159***
	(0.00598)
Log(Retail)_{t-1}	0.389***
	(0.00302)
Log(MktVol)_{t-1}	0.403***
	(0.00304)
Firm FE	Yes
Date FE	No
Observations	7320202
Adj.R2	0.874

This table presents estimates of Residual $\text{Log(Retail)}_{i,t} = \beta_0 + \lambda' \text{X}_{i,t-1}^{firm} + \gamma \text{Log(Retail)}_{i,t-1} + \delta \text{Log(Mkt)}_{i,t-1} + \epsilon_{i,t}$, estimated at the stock-day level. The dependent variable is the regression errors from Table 9.A. Detailed definitions of independent variables are included in Appendix A. All variables are standardized to be mean-zero and unit-variance. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 9.C: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Frictions with Predicted Retail Trade

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.209**	0.195**	0.0951	0.0951	-0.0262
	(0.0854)	(0.0946)	(0.0947)	(0.0947)	(0.0888)
Outage	0.0133	0.0230	0.0726***	0.0726***	0.0490**
	(0.0207)	(0.0230)	(0.0230)	(0.0230)	(0.0216)
$ERetail^{TQ}$	-0.296***	-0.00423	-0.168***	-0.168***	0.113***
	(0.0229)	(0.0254)	(0.0254)	(0.0254)	(0.0238)
Outage × SUE × $ERetail^{TQ}$	-0.0592**	0.0509	-0.0280	-0.0280	0.0493*
	(0.0281)	(0.0312)	(0.0312)	(0.0312)	(0.0293)
Outage × SUE	-0.0166	-0.0146	0.0162	0.0162	0.0130
	(0.0188)	(0.0208)	(0.0208)	(0.0208)	(0.0195)
SUE × $ERetail^{TQ}$	0.0484**	0.0145	0.0181	0.0181	-0.00623
	(0.0196)	(0.0217)	(0.0218)	(0.0218)	(0.0204)
Outage × $ERetail^{TQ}$	-0.0432	0.0616*	0.0453	0.0453	0.00219
	(0.0289)	(0.0320)	(0.0320)	(0.0320)	(0.0300)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	20649	20649	20629	20629	20649
Adj.R2	0.152	0.00464	0.0743	0.0743	0.0184

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 Outage_t + \beta_2 SUE_{it} + \beta_3 ERetail^{TQ} + \beta_4 SUE_{it} * Outage_t + \beta_5 SUE_{it} * ERetail^{TQ} + \beta_6 Outage_t * ERetail^{TQ} + \beta_7 SUE_{it} * Outage_t * ERetail^{TQ} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{it} + \epsilon_{it}$. The dependent variables, BHAR[a,b], are abnormal returns from day a to day b around the earnings announcement day 0. The independent variable of interest is the triple interaction between the earnings surprise quantile (SUE), Outage, an indicator for a top-quintile brokerage outage on the same day as the earnings announcement, and $\text{Retail}^T Q$, an indicator for predicted top-quintile retail trade. All variables are standardized to be mean-zero and unit-variance. Control variables include Log(MktVol), PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table 10: Retail Order Imbalance

	Dependent Variable: BHAR[0,1]	
	(1)	(2)
SUE	0.373***	0.341***
	(0.00951)	(0.00702)
Retail OIB	-0.00913	-0.00293
	(0.00928)	(0.00838)
SUE × Retail OIB	0.00407	0.000757
	(0.00517)	(0.00493)
Controls	No	Yes
Controls*SUE	No	Yes
Year-Quarter FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Firm FE	Yes	Yes
Observations	65194	65194
Adj.R2	0.137	0.205

This table presents estimates of $BHAR[0,1]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 RetailOIB_{it} + \beta_3 SUE_{it} * RetailOIB_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The dependent variable, BHAR[0,1], is abnormal returns during the earnings announcement window. The independent variables of interest are Retail OIB (order imbalance) and its interactio with the earnings surprise quantile (SUE). All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

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A Additional analysis

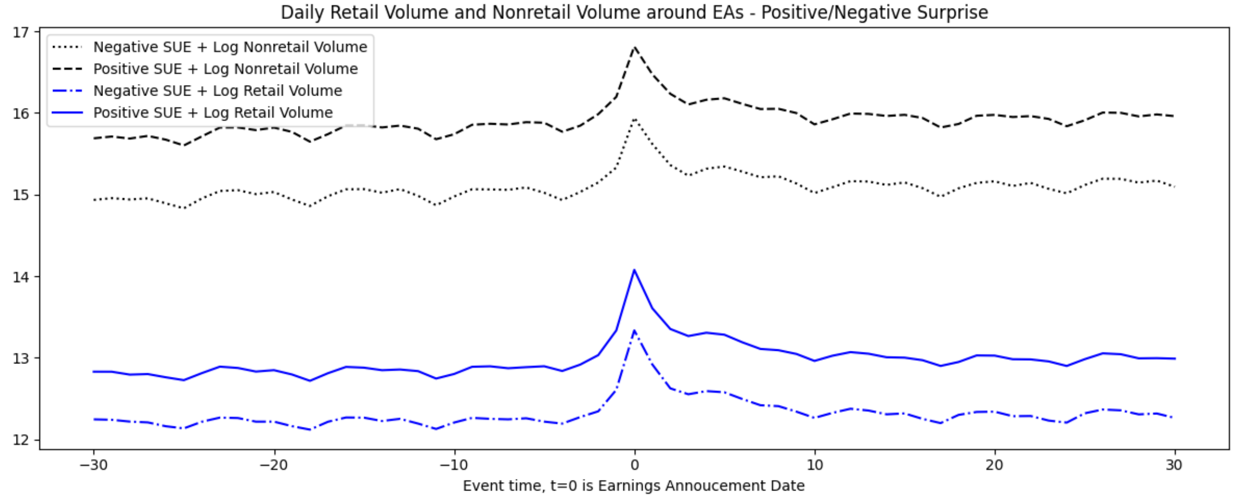

Figure A.1: Daily Retail and Nonretail Volume around earnings announcements for positive and negative earnings surprises.

Table A.1: Retail Trading Volume and Brokerage Outages

	Dep var:Log(Retail)	
	(1)	(2)
Outage	-0.00570**	-0.00412**
	(0.00281)	(0.00203)
Log(NonRetail)		0.283***
		(0.00667)
Time-of-Day FE	Yes	Yes
Firm FE	Yes	Yes
Observations	13823032	13823032
Adj.R2	0.571	0.595

This table presents estimates of $Log(Retail)_{it} = \beta_0 + \beta_1 Outage_t + \beta_2 Log(NonRetail)_{it} + \gamma_i + \delta_t + \epsilon_{it}$. The sample includes 5-min intervals during outages, matched with intervals 1 trading day before and after for the same stock and time of the day. In Column (2), we control for Log(NonRetail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and time of the day. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.2: Descriptive Statistics for Earnings Announcements with and without Outages

	Outage=0			*Outage=1*			
	N	mean	sd	N	mean	sd	Diff
SUE(raw)	21228	-0.00	0.10	321	0.00	0.04	-0.001
SUE	21228	7.07	3.23	321	7.29	3.23	0.145
BHAR[0,1]	21228	-0.00	0.09	321	-0.00	0.09	0.007
BHAR[2,5]	21228	-0.00	0.06	321	-0.01	0.06	-0.016**
BHAR[2,22]	21228	-0.00	0.16	321	0.00	0.18	0.007
BHAR[2,45]	21228	-0.01	0.18	321	0.01	0.21	0.023
BHAR[2,next]	21228	-0.01	0.20	321	0.02	0.23	0.022
#Complaints	21228	525.80	2097.16	321	12598.00	0.00	11,704.577***
Log(Retail)	21228	14.60	2.03	321	14.64	1.95	-0.071
Log(Nonretail)	21228	17.18	2.20	321	17.25	2.16	-0.063
PreRetail	21228	15.35	1.89	321	15.60	1.99	-0.024
PreNonretail	21228	18.10	2.13	321	18.24	2.18	-0.097
Log(MktVol)	21228	17.28	2.16	321	17.35	2.12	-0.060
PreRet	21228	0.01	0.11	321	0.00	0.17	-0.004
Log(Size)	21228	7.19	1.89	321	7.26	2.00	-0.054
Book-to-Market	21228	0.68	0.88	321	0.66	0.56	-0.016
EPersistence	21228	0.12	0.34	321	0.15	0.35	-0.002
EVOL	21228	1.80	6.01	321	1.41	3.19	-0.063
ERepLag	21228	37.58	13.55	321	42.11	25.04	-2.640
#Estimates	21228	7.65	6.10	321	7.58	6.04	0.137
TURN	21228	23.67	23.51	321	23.03	21.90	-1.246**
Loss	21228	0.36	0.48	321	0.37	0.48	0.022
#Announcements	21228	143.67	92.98	321	84.47	53.31	-1.737
IO	21228	0.71	0.25	321	0.65	0.23	-0.007

This table presents comparative descriptive statistics for the sample, distinguishing between earnings announcements that experienced outage incidents ($Outage = 1$) and those that did not ($Outage = 0$) within a two-day announcement window. Detailed definitions of all variables are included in Appendix A. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.3: Retail trading matched to various post-earnings announcement windows

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.313***	0.0247***	0.244***	0.00408	0.0118**
	(0.00638)	(0.00631)	(0.00677)	(0.00888)	(0.00562)
Log(Retail)	-0.251***				
	(0.0327)				
SUE × Log(Retail)	0.247***				
	(0.0186)				
Log(Nonretail)	0.468***				
	(0.0471)				
SUE × Log(Nonretail)	0.317***				
	(0.0270)				
Log(Retail)[2,5]		0.142***			
		(0.0236)			
SUE × Log(Retail)[2,5]		0.0628***			
		(0.0178)			
Log(Nonretail)[2,5]		0.499***			
		(0.0474)			
SUE × Log(Nonretail)[2,5]		-0.0116			
		(0.0223)			
Log(Retail)[2,22]			-0.0752*		
			(0.0414)		
SUE × Log(Retail)[2,22]			0.114***		
			(0.0223)		
Log(Nonretail)[2,22]			1.119***		
			(0.0666)		
SUE × Log(Nonretail)[2,22]			0.0975***		
			(0.0251)		
Log(Retail)[2,45]				-0.0223	
				(0.0559)	
SUE × Log(Retail)[2,45]				0.0562***	
				(0.0167)	
Log(Nonretail)[2,45]				1.312***	
				(0.108)	
SUE × Log(Nonretail)[2,45]				-0.0669**	
				(0.0273)	
Log(Retail)[2,next]					-0.0435
					(0.0633)
SUE × Log(Retail)[2,next]					0.0670***
					(0.0195)
Log(Nonretail)[2,next]					1.440***
					(0.128)
SUE × Log(Nonretail)[2,next]					-0.0651*
					(0.0329)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Observations	65194	64882	63657	62536	65192
Adj.R2	0.247	0.0592	0.200	0.178	0.183

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)[a,b]_{it} + \beta_3 SUE_{it} * Log(Retail)[a,b]_{it} + \beta_4 Log(Nonretail)[a,b]_{it} + \beta_5 SUE_{it} * Log(Nonretail)[a,b]_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$, regressions of earnings announcement returns over different windows on earnings surprise quantiles interacted with Log(Retail) over different windows. All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Controls are suppressed. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.4: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Extreme SUE)

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.296***	0.0280***	0.197***	0.0184**	0.0228***
	(0.00856)	(0.00871)	(0.00822)	(0.00694)	(0.00797)
Log(Retail)	-0.147***	-0.00102	-0.0890**	-0.0347	-0.0143
	(0.0326)	(0.0423)	(0.0409)	(0.0413)	(0.0378)
SUE × Log(Retail)	0.239***	0.0177	0.192***	0.0399**	0.0223
	(0.0210)	(0.0226)	(0.0214)	(0.0171)	(0.0192)
Log(Nonretail)	0.539***	0.153**	0.432***	0.0975	0.0899
	(0.0507)	(0.0641)	(0.0562)	(0.0620)	(0.0640)
SUE × Log(Nonretail)	0.294***	0.00930	0.131***	-0.0619**	-0.0301
	(0.0300)	(0.0268)	(0.0254)	(0.0249)	(0.0300)
PreRet	0.200***	-0.00702	0.121***	0.0161	0.0132
	(0.0126)	(0.0128)	(0.0104)	(0.0117)	(0.0127)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	26317	26317	26317	26317	26317
Adj.R2	0.290	0.0308	0.184	0.0998	0.119

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \beta_4 Log(Nonretail)_{it} + \beta_5 SUE_{it} * Log(Nonretail)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$ on a sample limited to earnings announcements in extreme SUE quantiles (1, 2, 10, and 11). All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.5: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Stocks with Penny Spreads)

	BHAR[0,1]	*BHAR[2,5]*	*BHAR[2,22]*	*BHAR[2,45]*	*BHAR[2,next EA]*
	(1)	(2)	(3)	(4)	(5)
SUE	0.242***	0.0300**	0.153***	0.00725	0.00513
	(0.0100)	(0.0121)	(0.0117)	(0.0104)	(0.0103)
Log(Retail)	-0.262***	0.0337	-0.117**	-0.0263	-0.0159
	(0.0520)	(0.0533)	(0.0571)	(0.0567)	(0.0510)
SUE × Log(Retail)	0.283***	0.00360	0.262***	0.0811***	0.0758**
	(0.0317)	(0.0332)	(0.0305)	(0.0250)	(0.0283)
Log(Nonretail)	0.516***	0.0132	0.308***	0.0355	0.0209
	(0.0765)	(0.0869)	(0.0724)	(0.0861)	(0.0809)
SUE × Log(Nonretail)	0.265***	0.0224	0.0993***	-0.0832**	-0.0344
	(0.0398)	(0.0383)	(0.0349)	(0.0327)	(0.0356)
PreRet	0.211***	-0.00292	0.122***	-0.00638	-0.00695
	(0.0182)	(0.0186)	(0.0176)	(0.0161)	(0.0156)
Controls	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	18555	18555	18555	18555	18555
Adj.R2	0.215	0.0303	0.142	0.0948	0.109

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$ in a subsample of stocks with bid-ask spreads below a penny on earnings announcement days. Bid-ask spread is the average time-weighted quoted bid-ask spread prior to trading. All variables are standardized to be mean-zero and unit-variance. Control variables include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.6: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Sign Split)

	BHAR[0,1]		*BHAR[2,5]*		*BHAR[2,22]*		*BHAR[2,45]*		*BHAR[2,next EA]*	
	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative	Positive	Negative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SUE	0.502***	0.348***	0.0627**	0.00804	0.333***	0.231***	0.0343	0.0169	0.0374	0.00491
	(0.0228)	(0.0278)	(0.0261)	(0.0257)	(0.0228)	(0.0281)	(0.0263)	(0.0286)	(0.0244)	(0.0294)
Log(Retail)	-0.344***	-0.448***	-0.0930**	0.0730	-0.237***	-0.225**	-0.0240	0.105	0.0127	0.0857
	(0.0553)	(0.0822)	(0.0397)	(0.0924)	(0.0455)	(0.0865)	(0.0465)	(0.0914)	(0.0426)	(0.0881)
SUE × Log(Retail)	0.481***	0.185***	0.0884	0.0582	0.367***	0.154**	0.0440	0.0857	0.0214	0.0457
	(0.0502)	(0.0622)	(0.0596)	(0.0721)	(0.0573)	(0.0720)	(0.0627)	(0.0740)	(0.0556)	(0.0680)
Log(Nonretail)	0.239***	-0.185	0.125**	0.0615	0.236***	-0.214*	0.103	-0.0935	0.0962	-0.0199
	(0.0717)	(0.113)	(0.0598)	(0.118)	(0.0667)	(0.115)	(0.0680)	(0.136)	(0.0605)	(0.141)
SUE × Log(Nonretail)	0.527***	-0.211**	0.0273	-0.0116	0.283***	-0.245**	-0.0779	-0.104	-0.0572	-0.0144
	(0.0739)	(0.0829)	(0.0851)	(0.0883)	(0.0782)	(0.0923)	(0.0788)	(0.101)	(0.0725)	(0.106)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39220	20835	39220	20835	39220	20835	39220	20835	39220	20835
Adj.R2	0.209	0.205	0.0467	0.0535	0.162	0.138	0.111	0.105	0.120	0.117
	Difference in SUE × Log(Retail) coefficients									
	0.296**		0.0302		0.2130*		-0.0417		-0.0243	
	0.0808		(0.1014)		(0.0929)		(0.1061)		(0.0929)	

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)[a,b]_{it} + \beta_3 SUE_{it} * Log(Retail)[a,b]_{it} + \beta_4 Log(Nonretail)[a,b]_{it} + \beta_5 SUE_{it} * Log(Nonretail)[a,b]_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The sample is split by the sign of earnings surprises. All variables are standardized to be mean-zero and unit-variance. Control variables (suppressed) include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively. Statistical significance for differences in coefficients across subsamples is evaluated based on the standard errors of the coefficient on the interaction of SUE × Log(Retail) $\times 1_{right}$ where 1_{right} is an indicator for the right subsample condition, in a fully interacted specification, i.e., we allow each variable, including the fixed effects, to vary by the partitioning variable and drop observations that do not fall into either partition if relevant.

Table A.7: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Size Split)

	BHAR[0,1]		*BHAR[2,5]*		*BHAR[2,22]*		*BHAR[2,45]*		*BHAR[2,next EA]*	
	Small	Big	Small	Big	Small	Big	Small	Big	Small	Big
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SUE	0.371***	0.334***	-0.000645	-0.00605	0.210***	0.197***	-0.0293*	0.00104	-0.0195	-0.00200
	(0.0149)	(0.0158)	(0.0166)	(0.0133)	(0.0159)	(0.0130)	(0.0168)	(0.0139)	(0.0175)	(0.0136)
Log(Retail)	-0.159***	-0.357***	-0.0198	0.0114	-0.0888***	-0.202***	-0.0176	0.00645	0.00792	0.0218
	(0.0348)	(0.0409)	(0.0385)	(0.0242)	(0.0309)	(0.0377)	(0.0307)	(0.0303)	(0.0344)	(0.0306)
SUE × Log(Retail)	0.222***	0.372***	-0.00235	-0.000631	0.180***	0.246***	0.0356*	0.0136	0.0201	-0.00543
	(0.0202)	(0.0309)	(0.0246)	(0.0249)	(0.0228)	(0.0245)	(0.0202)	(0.0182)	(0.0238)	(0.0202)
Log(Nonretail)	0.466***	0.427***	0.162***	0.0573	0.392***	0.257***	0.113**	-0.0153	0.0934*	-0.0243
	(0.0512)	(0.0599)	(0.0532)	(0.0398)	(0.0425)	(0.0513)	(0.0478)	(0.0415)	(0.0527)	(0.0419)
SUE × Log(Nonretail)	0.289***	0.283***	0.0472	0.0101	0.144***	0.148***	-0.0221	-0.0658**	0.00547	-0.0276
	(0.0299)	(0.0479)	(0.0326)	(0.0345)	(0.0274)	(0.0392)	(0.0256)	(0.0284)	(0.0319)	(0.0288)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26714	38336	26714	38336	26714	38336	26714	38336	26714	38336
Adj.R2	0.245	0.270	0.0403	0.0386	0.172	0.178	0.111	0.106	0.126	0.112
	Difference in SUE × Log(Retail) coefficients									
	-0.1500***		0.0017		-0.0660**		0.0220		0.0255	
	(0.0311)		(0.029)		0.0321)		(0.0305)		(0.0316)	

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)[a,b]_{it} + \beta_3 SUE_{it} * Log(Retail)[a,b]_{it} + \beta_4 Log(Nonretail)[a,b]_{it} + \beta_5 SUE_{it} * Log(Nonretail)[a,b]_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The sample split is based on firm size. All independent variables are standardized to be mean-zero and unit-variance. Control variables (suppressed) include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively. Statistical significance for differences in coefficients across subsamples is evaluated based on the standard errors of the coefficient on the interaction of SUE × Log(Retail) $\times 1_{right}$ where 1_{right} is an indicator for the right subsample condition, in a fully interacted specification, i.e., we allow each variable, including the fixed effects, to vary by the partitioning variable and drop observations that do not fall into either partition if relevant.

Table A.8: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Concurrent EAs Split)

	BHAR[0,1]		*BHAR[2,5]*		*BHAR[2,22]*		*BHAR[2,45]*		*BHAR[2,next EA]*	
	Normal	Busy	Normal	Busy	Normal	Busy	Normal	Busy	Normal	Busy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SUE	0.305	0.680***	0.529	0.0298	0.570	0.419***	0.617	-0.0269	0.963**	-0.0159
	(0.386)	(0.0935)	(0.375)	(0.118)	(0.350)	(0.0802)	(0.374)	(0.112)	(0.415)	(0.117)
Log(Retail)	-0.230***	-0.101	-0.00359	-0.0553	-0.0985	-0.0741	0.139*	0.0335	0.181**	0.0436
	(0.0735)	(0.0605)	(0.0620)	(0.0596)	(0.0781)	(0.0629)	(0.0758)	(0.0579)	(0.0784)	(0.0643)
SUE × Log(Retail)	0.445***	0.302***	0.00238	0.0496	0.324***	0.206***	0.0992	-0.0112	0.0791	-0.0207
	(0.0737)	(0.0541)	(0.0729)	(0.0567)	(0.0656)	(0.0469)	(0.0629)	(0.0442)	(0.0600)	(0.0555)
Log(Nonretail)	0.550***	0.154	0.167	0.253**	0.422***	0.181*	-0.106	0.0566	-0.102	-0.0251
	(0.128)	(0.0961)	(0.113)	(0.0970)	(0.115)	(0.0936)	(0.128)	(0.0992)	(0.122)	(0.0937)
SUE × Log(Nonretail)	-0.180**	-0.125**	-0.0400	-0.112	-0.177**	-0.0934*	-0.104	-0.00727	-0.0979	0.0287
	(0.0708)	(0.0529)	(0.0799)	(0.0734)	(0.0666)	(0.0525)	(0.0697)	(0.0566)	(0.0611)	(0.0641)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	31027	33812	31027	33812	31027	33812	31027	33812	31027	33812
Adj.R2	0.216	0.251	0.0363	0.0538	0.157	0.166	0.107	0.111	0.109	0.122
	Difference in SUE × Log(Retail) coefficients									
	0.1430		-0.04722		0.1180		0.1068		-0.0903	
	(0.0922)		(0.0951)		(0.0785)		(0.0806)		(0.0956)	

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)[a,b]_{it} + \beta_3 SUE_{it} * Log(Retail)[a,b]_{it} + \beta_4 Log(Nonretail)[a,b]_{it} + \beta_5 SUE_{it} * Log(Nonretail)[a,b]_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The sample split is based on busy versus non-busy EA days, where busy days are those with above-median number of concurrent announcements. All independent variables are standardized to be mean-zero and unit-variance. Control variables (suppressed) include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively. Statistical significance for differences in coefficients across subsamples is evaluated based on the standard errors of the coefficient on the interaction of SUE × Log(Retail) $\times 1_{right}$ where 1_{right} is an indicator for the right subsample condition, in a fully interacted specification, i.e., we allow each variable, including the fixed effects, to vary by the partitioning variable and drop observations that do not fall into either partition if relevant.
Regression model: $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)_{it} + \beta_3 SUE_{it} * Log(Retail)_{it} + \beta_4 Log(Nonretail)_{it} + \beta_5 SUE_{it} * Log(Nonretail)_{it} + \sum \beta_{k+6} X_{k,it} + \sum \beta_{k+6} SUE_{it} * X_{k,it} + \epsilon_{it}$

Table A.9: Earnings Announcement Returns, Post-earnings Announcement Drift, and Retail Trading (Friday versus Other days)

	BHAR[0,1]		*BHAR[2,5]*		*BHAR[2,22]*		*BHAR[2,45]*		*BHAR[2,next EA]*	
	Other days	Friday	Other days	Friday	Other days	Friday	Other days	Friday	Other days	Friday
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SUE	0.331***	0.314***	0.0294***	0.000333	0.219***	0.209***	0.0157**	0.0168	0.0227***	0.0298***
	(0.00860)	(0.0112)	(0.00629)	(0.0106)	(0.00804)	(0.00924)	(0.00764)	(0.0101)	(0.00746)	(0.00963)
Log(Retail)	-0.256***	-0.241***	0.00283	-0.0329	-0.122***	-0.140***	0.0328	-0.00323	0.0580**	0.00438
	(0.0413)	(0.0405)	(0.0258)	(0.0329)	(0.0338)	(0.0382)	(0.0254)	(0.0321)	(0.0265)	(0.0336)
SUE × Log(Retail)	0.365***	0.330***	0.0284	0.0418*	0.215***	0.230***	0.0317**	0.0473*	0.0321*	0.0418
	(0.0233)	(0.0341)	(0.0182)	(0.0248)	(0.0236)	(0.0336)	(0.0195)	(0.0245)	(0.0177)	(0.0285)
Log(Nonretail)	0.519***	0.415***	0.106***	0.144***	0.369***	0.302***	0.0402	0.0239	0.0217	0.0252
	(0.0625)	(0.0647)	(0.0388)	(0.0531)	(0.0557)	(0.0528)	(0.0461)	(0.0622)	(0.0465)	(0.0588)
SUE × Log(Nonretail)	-0.178***	-0.140***	-0.0479***	-0.0408	-0.181***	-0.129***	-0.0753***	-0.0842***	-0.0615***	-0.0710**
	(0.0223)	(0.0374)	(0.0167)	(0.0298)	(0.0222)	(0.0379)	(0.0215)	(0.0252)	(0.0190)	(0.0293)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls*SUE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39141	21657	39141	21657	39141	21657	39141	21657	39141	21657
Adj.R2	0.216	0.251	0.0363	0.0538	0.157	0.166	0.107	0.111	0.109	0.122

	Difference in SUE × Log(Retail) coefficients				
	0.0350	-0.0134	-0.0150	-0.0156	-0.0097
	(0.0367)	(0.0257)	(0.0361)	(0.0290)	(0.0327)

This table presents estimates of $BHAR[a,b]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Log(Retail)[a,b]_{it} + \beta_3 SUE_{it} * Log(Retail)[a,b]_{it} + \beta_4 Log(Nonretail)[a,b]_{it} + \beta_5 SUE_{it} * Log(Nonretail)[a,b]_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. The sample split is based on the weekday when earnings are announced. All independent variables are standardized to be mean-zero and unit-variance. Control variables (suppressed) include PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, IO, and Log(Nonretail). Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively. Statistical significance for differences in coefficients across subsamples is evaluated based on the standard errors of the coefficient on the interaction of SUE × Log(Retail) $\times 1_{right}$ where 1_{right} is an indicator for the right subsample condition, in a fully interacted specification, i.e., we allow each variable, including the fixed effects, to vary by the partitioning variable and drop observations that do not fall into either partition if relevant.

Table A.10: Earnings Surprises, Pre-Earnings Announcement Retail Trading Volume and Pre-Earnings Announcement Returns

	Dependent Variable: PreRet	
	(1)	(2)
PreRetail	0.215***	0.264***
	(0.0495)	(0.0493)
PreRetail × SUE	-0.0354**	-0.0451***
	(0.0160)	(0.0152)
PreNonretail	-0.164***	-0.656***
	(0.0555)	(0.0826)
PreNonretail × SUE	0.0642***	0.154***
	(0.0162)	(0.0292)
SUE	0.184***	0.185***
	(0.00730)	(0.00832)
Controls	No	Yes
Year-Quarter FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Firm FE	Yes	Yes
Observations	65194	65194
Adj.R2	0.127	0.164

This table presents estimates of $PreRet_{it} = \beta_0 + \beta_1 PreRetail_{it} + \beta_2 SUE_{it} + \beta_3 PreRetail_{it} * SUE_{it} + \beta_4 PreNonretail_{it} + \beta_5 PreNonretail_{it} * SUE_{it} + \sum \beta_k X_{k,it} + \epsilon_{it}$. All independent variables are standardized to be mean-zero and unit-variance. Control variables include Log(Size), Book-to-Market , EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.11: Robustness Tests: ERC and Brokerage Outages

	Dependent Variable: BHAR[0,1]			
	Original Sample	*Entropy Balanced Sample*	using log(#Complaints) as Outage	*Randomized Outages*
SUE	0.311***	0.317***	0.310***	0.304***
	(0.0142)	(0.0272)	(0.0143)	(0.0146)
Outage	-0.00434	0.00628	-0.000540	0.00363
	(0.0247)	(0.0343)	(0.00287)	(0.0210)
Outage × SUE	-0.0604**	-0.0553**	-0.00689**	-0.000150
	(0.0250)	(0.0246)	(0.00317)	(0.0229)
Log(MktVol)	0.298***	0.498***	0.298***	0.300***
	(0.0639)	(0.102)	(0.0640)	(0.0642)
Controls*SUE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes
Observations	21549	21549	21549	21549
Adj.R2	0.212	0.385	0.212	0.212

This table presents estimates of $BHAR[0,1]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Outage_t + \beta_3 SUE_{it} * Outage_t + \beta_4 Log(MktVol)_{it} + \beta_5 SUE_{it} * Log(MktVol)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$. Column (1) reports results using the original sample. Column (2) reports results using an entropy balanced sample. This sample is balanced on the means and standard deviations of SUE quantiles and control variables. Column (3) defines *Outage* as the number of complaints rather than an indicator for top-quintile outage complaint days. Column (4) reports means and standard deviations of coefficients from 1,000 regressions using randomized outage indicators. In each of these regressions, we assign a randomized outage indicator for each earnings announcement. All variables are standardized to be mean-zero and unit-variance. Control variables include Log(MktVol), PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

Table A.12: Robustness Tests: Various Definitions of Brokerage Outages

	Dependent Variable: BHAR[0,1]			
Indicator for EA days with outages:	*above-median*	*top-tercile*	*top-quartile*	*top-quintile*
SUE	0.305***	0.306***	0.305***	0.311***
	(0.0146)	(0.0147)	(0.0143)	(0.0142)
Outage	0.0189	0.0202	0.0525	-0.00434
	(0.0274)	(0.0300)	(0.0388)	(0.0247)
Outage × SUE	-0.0662*	-0.0765*	-0.0721*	-0.0604**
	(0.0393)	(0.0387)	(0.0403)	(0.0250)
Log(MktVol)	0.299***	0.299***	0.299***	0.298***
	(0.0642)	(0.0643)	(0.0642)	(0.0639)
Controls*SUE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes
Observations	21549	21549	21549	21549
# of observations where Outage = 1	1149	745	350	321
Adj.R2	0.211	0.211	0.211	0.212

This table presents estimates of $BHAR[0,1]_{it} = \beta_0 + \beta_1 SUE_{it} + \beta_2 Outage_t + \beta_3 SUE_{it} * Outage_t + \beta_4 Log(MktVol)_{it} + \beta_5 SUE_{it} * Log(MktVol)_{it} + \sum \beta_I X_{I,it} + \sum \beta_k SUE_{it} * X_{k,it} + \epsilon_{it}$ with *Outage* defined based on different top-quantiles in each column. All variables are standardized to be mean-zero and unit-variance. Control variables include Log(MktVol), PreRet, Log(Size), Book-to-Market, EVOL, EPersistence, ERepLag, #Estimates, TURN, Loss, #Announcements, and IO. Detailed definitions of all variables are included in Appendix A. Standard errors are clustered by firm and quarter. ***, **, and * indicate statistical significance at two-sided 1%, 5%, and 10% levels, respectively.

B Variable definitions

Variable	Description
BHAR[s,t]	Size and book-to-market adjusted return cumulated from trading day s through day t relative to the earnings announcement date, calculated as $BHAR[s,t]_{i,q} = \prod_{d=s}^{t}(1+ret_{i,d}) - \prod_{d=s}^{t} t(1+ret_{p,d})$, where $ret_{i,d}$ is the daily stock return of firm i and $ret_{p,d}$ is the return on the size and book-to-market matching portfolio on day d. Source: CRSP.
SUE	Earnings surprise relative to analyst consensus forecasts deflated by quarter-end share price. Measured as intra-quarter quantile as in DellaVigna and Pollet (2009). Source: IBES, CRSP.
Outage	A dummy variable set to one if the total number of retail brokerage outage complaints in the earnings-announcement window or a 5-minute trading interval is in the top quintile of complaints for that time window. Source: Downdetector.com (Eaton et al., 2022).
Log(Retail)	Log of one plus the dollar volume of shares traded by retail investors over the earnings announcement window. Retail trades are identified following the Boehmer et al. (2021) approach. Source: TAQ.
Log(NonRetail)	Log of one plus dollar market volume less retail trading volume over the earnings announcement window. Source: TAQ.
# Complaints	Total number of retail brokerage outage complaints in the earnings-announcement window or a 5-minute trading interval. Source: Downdetector.com (Eaton et al., 2022).
PreRetail	Pre-earnings announcement retail activity. The aggregate retail trading volume (scaled by market volume) for earnings announcement trading date -10 to -1 relative to announcement date 0. Source: TAQ.
PreRet	Pre-earnings announcement returns. Compound excess return over the size portfolio for earnings announcement trading date -10 to -1 relative to announcement date 0. Source: CRSP.
Size	Market value of equity on the earnings announcement date in $M. Source: CRSP.
Book-to-Market	Book to market ratio at the end of the quarter for which earnings are announced. Source: Compustat.
EPersistence	Earnings persistence based on AR(1) regression with at least 4, up to 16 quarterly earnings. Source: Compustat.
IO	Institutional ownership as a fraction of total shares outstanding. Values greater than 1 are set to 1. Source: WRDS SEC Analytics.
EVOL	Standard deviation of seasonally differenced quarterly earnings over the prior 16 (at least 4) quarters. Winsorized at 1st and 99th percentiles. Source: Compustat.
ERepLag	Days from quarter-end to earnings announcement. Source: Compustat.
#Estimates	Number of analysts making quarterly earnings forecasts. Source: IBES Summary File.

Variable definitions (continued)

Variable	Description
TURN	Average monthly turnover for the 12 months preceding the earnings announcement. Source: CRSP.
Loss	Indicator for negative earnings. Source: Compustat.
#Announcements	Number of same-day earnings announcements. Source: IBES.
Log(MktVol)	Log value of market volume during the 2-day earnings announcement window in $M. Source: CRSP.
Retail OIB	Marketable retail order imbalance for each firm i over the two-day window around an earnings announcement, calculated as $RetailOIB_i = \frac{RetailBuy_i - RetailSell_i}{RetailBuy_i + RetailSell_i}$, where $RetailBuy_i$ ($RetailSell_i$) is dollar volume of shares bought (sold) by retail investors. Retail investors' directional trades are identified based on the Boehmer et al. (2021) approach. Source: TAQ.
$\text{Return}_{s,t}$	Firm i's return from trading day s through day t. Source: CRSP.
Return Skewness	Skewness of a firm's daily returns over the past 60 days, calculated as the third central moment about the mean of daily returns, following Harvey and Siddique (2000). Winsorized at 1st and 99th percentiles. Source: CRSP.
Return Range	The range in stock value over the preceding 60-day period, calculated as the difference between the highest and lowest closing prices during this timeframe. Source: CRSP.
Price_{t-1}	The price of the stock on day $t-1$. Source: CRSP.
Market Cap_{t-1}	Firm i's total market value on the preceding day $(t-1)$, calculated by multiplying the firm's stock price by its total number of outstanding shares. The resultant market equity is log transformed and lagged by one day. Winsorized at 1st and 99th percentiles. Source: CRSP.

Response memorandum

We would like to begin by thanking the review team for their very thoughtful, insightful, and constructive comments. We have revised the manuscript based on your feedback, and believe that doing so has significantly improved it.

As we understand them, the biggest concerns with our original submission relate to the dynamic provision theory and casual claims. The following list summarizes the major changes made in this revision, which are detailed further below:

1. De-emphasized the dynamic liquidity narrative. We no longer interpret our results as consistent with this theory and do not discuss it in detail.

2. Added results using expected retail trade around brokerage outages, following Eaton et al. (2022)

3. Added additional discussion of the retail order imbalance results

4. Added additional robustness checks given concerns around the Boehmer et al. (2021) measure

In the remainder of this response document, we present our responses to the editor and reviewers' comments, one-by-one. The review team's comments appear, verbatim, in blue font (summaries omitted). Our responses appear beneath them, indented, in black font.

Response to Comments from the Editor

The reviewer is concerned that the dynamic liquidity provision theory does not fit your institutional setting. You suggest that when retail trade falls due to a brokerage outage, sophisticated investors can observe this and infer that liquidity has dried up, and so they are less willing to pay attention to the earnings surprise. However, as the reviewer points out, if institutional investors pay enough attention to recognize that there has been a change in liquidity in the firm's stock, they should be also aware of the earnings surprises as they require much less attention. I was also puzzled by this apparent inconsistency in your arguments. The reviewer and I may be missing something obvious here and if so, clarifications would be welcome.

Thank you (and the reviewer) for helping us think through the plausibility of the dynamic liquidity interpretation. In light of your feedback, we have removed this interpretation of the results. We now interpret our ERC results as consistent with retail traders providing liquidity to sophisticated investors who impound earnings information into price. Prior to the earnings announcement, retail investors also act as noise traders, but absent the public signal, their trade instead reduces the (earnings) information in price. Our PEAD results are consistent with a selection mechanism, rather than a causal effect of retail trade. This selection mechanism could be due to retail attention or overconfidence effects. We discuss the overall inference in Section 5.6.

In terms of your empirical analyses, the reviewer suggests that your evidence in Tables 6 and 8 is inconsistent with the dynamic liquidity provision theory. Furthermore, s/he finds that the implementation of brokerage outages is less rigorous in your study compared to prior studies in finance.

We have pivoted the paper away from dynamic liquidity, so inconsistency of our evidence with that theory should no longer be a concern.

In our response to the reviewer below and in footnote 21 on page 25 of the manuscript, we discuss how a lack of an association between retail order imbalance and returns (shown in Table 10) is consistent with interpreting retail trade as noise, given that our setting is characterized by market makers who are able to distinguish retail trade from other order flows.[i]

Following the reviewers' guidance, we have implemented more rigorous tests following Eaton et al. (2022). These are presented in Section 4.5 and described in greater detail in the response to the reviewer. These tests, which exploit predicted retail trade, support our overall inferences. Specifically, the effect of outages on ERCs is more negative for earnings announcements with greater expected retail trade.

In closing, although I share the reviewer's concerns, I also agree with her/him that you are exploring an important topic. Therefore, as I noted above, I am open to a resubmission, even though I realize that the revision will require the paper's overhaul, with substantial uncertainty regarding the new findings. In other words, the nature of the concerns is such that the paper will go forward at JAR only with a more reasonable economic framework and a substantially restructured set of analyses. But you know your data and setting best and are better equipped to assess whether such a revision is feasible.

Thank you for being open to a resubmission. We believe we have addressed the reviewers' concerns about the economic framework and empirical analyses, and we look forward to your thoughts on our revised draft.

[i]Tables 6 and 8 in the prior version correspond, respectively, to Tables 6 and 10 in the current version.

Response to Comments from the Reviewer

I found the empirical evidence in the paper that both ERCs and short-window PEAD climb when retail trade is elevated to be interesting. I like the idea of utilizing retail brokerage outages to help address the endogeneity of retail trade in past studies. However, I unfortunately feel the paper has several major problems.

Thank you for the positive feedback and for pointing out issues whose addressing can help improve our inferences and contribution. Below, we provide detail on the changes we made to address the problems you highlighted.

First, the "dynamic liquidity theory" that the authors use to explain the results appears to have major flaws when applied to this setting. In addition, the evidence in the latter part of the paper appears to invalidate this hypothesis as a potential explanation, and so I was left puzzled by the collective evidence in the paper.

Thank you for helping us think through the plausibility of the dynamic liquidity interpretation. In light of your feedback, we have removed this interpretation of the results. We now interpret our ERC results as consistent with retail traders providing liquidity to sophisticated investors who impound earnings information into price. Prior to the earnings announcement, retail investors also act as noise traders, but absent the public signal, their trade instead reduces the (earnings) information in price. Our PEAD results are consistent with a selection mechanism, rather than a causal effect of retail trade. This selection mechanism could be due to retail attention or overconfidence effects. We discuss the overall inference in Section 5.6.

Furthermore, Barber et al. "A (Sub)penny For Your Thoughts: Tracking Retail Investor Activity in TAQ" (working) argue that the Boehmer et al. measure is a very noisy metric of retail trade, and likely noisier than the brokerage house metrics used in existing work such as Hirshleifer et al. (2008). As such, the application of this measure does not seem to be a major empirical contribution, which leaves the application of brokerage outages as the main empirical contribution. However, the identification strategy surrounding outages the paper uses is weak compared to the published work that develops the brokerage outage methodology and is potentially confounded by an association between macro-level trading activity and outages.

Barber et al. (2023) submitted thousands of retail trades, and checked whether the Boehmer et al. (2021) method captured these trades. In their Figure 2.A, they show that only about 40% of their trades on stocks with 1-cent bid-ask spreads were identified as retail trades, with this fraction falling to around 30% for trades on stocks with spreads greater than 10 cents. Note that this does not imply that the Boehmer et al. (2021) method misclassifies nonretail trade as retail, only that it misses retail trades (i.e., those executing at round or half-cent increments). It seems reasonable that capturing roughly 30-40% of retail trade in the 2010-2021 period is a vast improvement over brokerage-specific proxies that date from the early/mid-1990s, particularly for considering how the potential effects of retail trade have changed since then.

Noting the evidence in Barber et al. (2023) that the Boehmer et al. (2021) measure performs better when bid-ask spreads are at one cent rather than greater than one cent, we re-estimate our Table 6 results in a subsample of earnings announced by firms with one-cent spreads. Results, shown in Table A.5, are substantially similar. These results are discussed briefly, starting on page 31.

Based on your recommendations, we have added additional analysis jointly exploiting outages and expected retail trade, presented in Section 4.5. Following Eaton et al. (2022), we use a regression to predict retail trade (Table 9.B), then interact our variables (and interactions) of interest in the outage tests with an indicator for high (top-quintile) predicted retail trade (Table 9.C). We find that the effect of outages on ERCs is more negative for announcements expected to have high retail trade. This, while consistent with our evidence in Table 8 using only outages, helps provide confidence that our inferences are not confounded by, as you wrote above, "an association between macro-level trading activity and outages."

Conceptual issues with the dynamic liquidity provision story

I struggled to make sense of the dynamic liquidity provision theory as an explanation for the results at a conceptual level. The story (as explained in Section 5.2.2) is that when sophisticated investors pay attention to the earnings surprise, they will trade on this surprise gradually in the week following the announcement. This, in turn, will raise both ERCs and short-term PEAD. So far, this makes sense to me.

Now, the paper argues that sophisticated investors' willingness to pay attention to the surprise is determined by liquidity, since they can earn greater profits from private information in a more liquid market. When retail trade falls due to a brokerage outage or otherwise, sophisticated investors can observe this and infer that liquidity has dried up, and so they are less willing to pay attention to the earnings surprise. This drop in attention, in turn, leads them to trade less on the surprise, leading to lower ERCs and short-term PEAD.

The issue I have with this is that trading on an earnings surprise is incredibly simple and requires almost no attention. All one needs to do is calculate the surprise – it is not necessary to process the financial statements, listen to the conference call, etc. to trade on PEAD. Importantly, it likely takes much more attention to recognize that the amount of retail trade, or market liquidity more generally, has changed. As such, any sophisticated trader who is paying close enough attention to the market for a firm's stock to recognize that there has been a change in liquidity (or has directly observed a retail brokerage outage) will almost surely also be aware of the earnings surprise.

Thank you for your in-depth explanation. As noted above, we have pivoted away from the dynamic liquidity interpretation of our results.

Empirical issues with the dynamic liquidity provision story

The evidence presented in Table 8 appears to be inconsistent with the dynamic liquidity provision story. This table shows that "retail order imbalance is not generally associated with returns during the earnings announcement window." But, an essential feature of noise trade in Kyle models like Banerjee and Breon-Drish (2020) is that noise trade moves price because other investors do not know whether this trade is informed or not (we can see this in their equation 8). As such, it is surprising to me that the paper claims these findings use these results to the dynamic liquidity provision theory over the susceptible investor theory.

Although we have pivoted away from the dynamic liquidity provision story, this is an important point for models of trade where price is set by market makers who cannot differentiate between order flow from informed non-retail traders and uninformed retail traders. Clearly, in our setting, the wholesalers offering price improvement are doing so on the basis of distinguishing retail order flow from other order flow. This breaks the association between retail/noise net flows and returns. To address this point in the paper, we have added footnote 21 on page 25, reproduced below:

> "In many theoretical microstructure studies, building on Kyle (1985), market makers set stock prices based on expectations of fundamental value conditional on aggregate order flow. In such a setting, price is responsive to noise trade because the market maker cannot distinguish noise from informed trade. If they could, then price would be independent of noise trade. In our setting, and inherent in the Boehmer et al. (2021) identification of retail trade, market makers (i.e., wholesalers paying for order flow) *can* distinguish retail trade from other order flows. As such, in our setting retail-as-noise should yield effectively no association between net order flow (i.e., Retail OIB) and returns."

Furthermore, Table 6 shows that retail trade has no impact on returns starting from day 2 and going to any period after 5 days, which also appears inconsistent with the dynamic liquidity provision theory. This theory implies that, if earnings predict returns from day 2 to 5, then they should also predict returns from day 2-22, 2-45, and 2-nextQ. The reason is that days 2 through 5 are included in each of these subsequent windows, and there is no reason under this theory that the impact sophisticated investors have on price with their trade would reverse after those 5 days. Yet, the coefficients on SUE*Log(Retail) and SUE*Outage in columns 3-5 of Tables 6 and 7 are insignificant. The authors do not separately present results on how retail trade impacts the association between earnings and returns from days 6 onwards. But, this suggests that there is a mild reversal of the day 2-5 returns that occurs from day 6 onwards, which, in turn, suggests that retail trade is associated with an initial overreaction to news. This is not consistent with the returns during days 2-5 reflecting sophisticated investors rationally impounding the earnings surprise into price.

Before addressing your point in detail, it is worthwhile for us to comment on how the PEAD results changed in our revision. In the initial submission, we found significant PEAD and retail trade associations with PEAD over the [2,5] window. In the revised version, in Table 6, we find PEAD over each window, and significant associations between retail trade and PEAD in the [2,22], [2,45], and [2,next EA] windows. In Table 8, which focuses on outages in the shorter subsample, we continue to find evidence of PEAD, though there is no association between outages (i.e., retail frictions) and PEAD. We would like to take a moment to explain some changes that we believe may drive the differences in PEAD results relative to the initial submission.

Most importantly, in revising the paper, we changed our sample. We excluded firms with multiple IBES earnings announcement time observations per quarter and dropped those with multiple ticker-permno-date observations in TAQ-CRSP links. Excluding firm-quarters with incomplete control variables and returns data further reduced the sample from 79,000 to 65,000 unique earnings announcements. Additionally, we updated our methodology for calculating abnormal returns. We use abnormal returns, defined as daily returns adjusted for returns on matched size and book-to-market portfolios taken from Ken French's data library, and based on the NYSE breakpoints he provides.

Returning to our PEAD results, in the revision, in Table 6, we find a significantly positive association between retail trade and PEAD in the [2,22], [2,45], and [2,next EA] windows. Still, we find a substantial decrease in the PEAD coefficients over the 2nd month following the EA, i.e., from column 3 to column 4. We discuss this starting on page 17, and present results for non-overlapping post-EA return windows in Table 7. In line with your observation, we find a reversal in the [22,45] window, both for overall PEAD (from the coefficient on SUE), and for the retail trade-PEAD association (the interaction between SUE and Log(Retail)). On page 18, we write that,

> "These patterns, to our knowledge, are novel to the PEAD literature, and deserve further investigation. However, our focus remains on the effects of retail trade, and these patterns may be affected by important selection effects. Rather than investing here in interpreting the time series pattern, we proceed to tests using brokerage outages as shocks to retail trade, as these are plausibly less susceptible to selection effects noted above."

Causality claims

The paper's primary empirical contribution is to obtain better identified variation in retail trade than the existing literature, and it makes strong causal claims. Identification in the paper comes from retail brokerage outages using the proxy developed by Barber et al. (2022 JF) and Eaton et al. (2022 JFE). Eaton et al. point out that brokerage outages may not be exogenous because heightened market activity might lead to outages. For instance, they find that many outages occurred in March 2020. This appears to a be a threat to identification: the distraction created by heightened market activity could lower attention to firm-specific information events, thereby slowing the impounding of earnings information into prices to beyond day 5.

I appreciate that the paper controls for market-wide volatility and removes economy-wide outages, but this remains less compelling than the approaches Eaton et al. (2022 JFE) take to address this concern. Specifically, they "contrast the effects of outages on stocks in the top quintile of predicted retail trading relative to stocks in the bottom four quintiles.

The difference-in-differences type approach is designed to mitigate concerns that outages are related to market-wide news. We also conduct several robustness tests, including pseudo-outage analysis, parallel trend analysis, and analysis that omits stocks with elevated activity on WallStreetBets around the outage." At the very least, a difference-in-difference analysis that contrasts how outages changes the ERCs of firms with high vs. low retail activity would be necessary to assuage this identification concern.

As discussed above, we have added analysis jointly exploiting outages and expected retail trade, presented in Section 4.5. After orthogonalizing retail trade to trailing returns in Table 9.A, since Table 4 shows that trailing returns significantly predict upcoming earnings surprises, we predict retail trade in Table 9.B. Then, we interact our variables (and interactions) of interest in the outage tests with an indicator for high (top-quintile) expected retail trade in Table 9.C. We find that the effect of outages on ERCs is more negative for announcements expected to have high retail trade. This, while consistent with our evidence in Table 8 using only outages, provides additional evidence that can help assuage the identification concern.

We did not conduct parallel trends analysis. While we are analyzing the difference in outage effects for high versus low retail trade, our diff-in-diff-type test does not have inherent pre/post time periods. We cannot look at heterogeneous trends in EA reactions in the days prior to the EA, because there is no EA news in those days. Looking at differences in returns to previous quarters' EAs seems too temporally remote (i.e., too long before the outage) to take seriously as a sample in which to examine parallel pre-trends. Additionally, we did not collect WallStreetBets mentions, as these provided a small fraction of the explanatory power in Eaton et al. (2022) and are more closely related to Robinhood trade than trade from retail investors/households more generally. This is discussed in detail starting on page 21 and in footnote 18 on page 21.

In Appendix Table A.11, we show that our Table 8 outage-ERC results are robust to entropy balancing and using the log of the number of complaints instead of an indicator for top-quintile complaints, and are not generated from randomized pseudo-outages. Additional robustness checks are provided in Table A.12.

Other comments

1) The paper devotes a considerable amount of space discussing another explanation, the susceptible investor theory (even though it ultimately rules it out in Section 6). This theory is first articulated in Section 5.5 and it was unclear. The idea appears to be that (1) retail investors interpret earnings correctly, which is why they lead to higher ERCs and (2) at the same time retail investors introduce noise by trading on false signals, which is why they lead to higher PEAD. These statements were hard for me to make sense of.

First, for retail to raise ERC's, they not only have to interpret earnings correctly, but they also must interpret them more accurately than other market participants, and this seems somewhat hard to believe. Second, if retail trade leads to higher ERCs, it must be because the amount of information is greater than the amount of noise they inject into price. That is, if they primarily trade on information, they should raise the ERC and lower PEAD, and vice versa if they primarily trade on noise. It is hard to see how they could simultaneously do both. After all, unless there is a systematic overreaction to the news, if ERC's are higher, PEAD must mechanically be lower. The reason is that, in the long term, earnings are fully and accurately impounded into price. So, if there is a greater response to earnings on the earnings date, there must be a lower response after that date. This is related to my earlier comment that there seems to be an unexplored short- term overreaction to news in the paper.

Thank you for pointing this out. We have pivoted away from the susceptible investor narrative.

2) The dynamic liquidity theory makes another prediction that the paper might consider. When sophisticated investors trade less on earnings surprises, these surprises ultimately still make their way into prices in the future. Thus, we should see that there is more PEAD in

the long term following retail outages.

As we have pivoted away from the dynamic liquidity theory narrative, we do not discuss this additional prediction in the paper. As shown in Table 8, we do not find that outages are associated with significant differences in subsequent PEAD, although Table 9.C, column 5 shows modestly higher PEAD in the [2,next EA] window for outages with high expected retail trade. We discuss this in the paper on page 24.