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THREE ESSAYS ON SPLITS, INSIDERS, AND RETAIL TRADERS

Paul van Nes
vanx5230@mylaurier.ca

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by

Paul C. van Nes

DISSERTATION

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ABSTRACT

The first essay tests the split signaling hypothesis by examining the reaction of sophisticated investors to stock split announcements. Return-based tests of signaling used in earlier studies produce conflicting results and have been criticized as unreliable. I bypass this criticism by focusing on long-term post-split behavior of short sellers who are generally recognized as sophisticated investors. Upon controlling for alternative hypotheses and conventional short selling determinants, I show that short interest permanently declines in reaction to split announcements. Furthermore, consistent with signaling, the degree of the decline is positively related to signal strength and to the splitter's level of information asymmetry. Overall, the results are consistent with the view that firms use stock splits to relay positive value-relevant signals.

The second essay shows that the return predictability associated with retail trades cannot be attributed to insider trades, as hypothesized by Kaniel, Liu, Saar, and Titman (2012). Retail purchases predict future abnormal returns, and the effect is amplified around insider purchases. Similar results do not hold for insider sales. The results are consistent with retail investors trading on primarily positive price-relevant information that overlaps with insider information sets. The shared information appears to be of at least a medium-term nature. The results cannot be explained by retail traders selectively mimicking insider trades.

The third essay analyzes the role of retail traders in stock pricing around quarterly earnings announcements using a comprehensive and recent dataset. The data show that retail trading activity predicts future earnings surprises, earnings announcement returns, and medium-term post-earnings announcement returns. These results are not driven by insider trades prior to earnings announcements. Most results are neither driven by individuals trading on the last day prior to

earnings announcements, potentially including a group of hackers. Overall, the results are consistent with retail investors trading on price-relevant information.

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STATEMENT OF ORIGINALITY AND DECLARATION OF CO-AUTHORSHIP

I hereby declare that this thesis incorporates material that is the result of joint research, as follows: Chapter 1 incorporates unpublished material co-authored with Andriy Shkilko, M. Fabricio Perez, and Ning Tang. I certify that, with the above qualification, this thesis, and the research to which it refers, is the product of my own work.

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1. STOCK SPLIT SIGNALLING: EVIDENCE FROM SHORT INTEREST

1.1. Introduction

The literature on stock split motives mainly focuses on two hypotheses: (i) signaling and (ii) catering. The signaling hypothesis suggests that companies use splits to send positive value-relevant signals to the market (Grinblatt, Masulis, and Titman, 1984; Brennan and Copeland, 1988; Ikenberry, Rankine, and Stice, 1996). Alternatively, the catering hypothesis posits that firms split to (a) attract small investors (Baker and Gallagher, 1980), (b) reward liquidity providers (Angel, 1997), and (c) time investors' preferences for low-priced stocks (Baker, Greenwood, and Wurgler, 2009). The literature is, however, still far from an agreement as to whether the two abovementioned hypotheses sufficiently explain cross-asset pervasiveness and historical persistence of stock splits. For instance, the discussion in Weld, Michaely, Thaler, and Benartzi (2009) casts doubt on both signaling and catering explanations and argues that splitting to lower prices is merely a societal norm.

Studies that find support for the signaling hypothesis base their conclusions on the positive return reaction to split announcements (Grinblatt et al., 1984; McNichols and Dravid, 1990). They posit that the announcement return reflects positive changes in the opinion of the marginal investor; hence, splits must relay positive signals about firms' prospects. The criticism of this conclusion is implied by a growing body of research on inefficient information processing by some investors. In particular, Busse and Green (2002), Barber and Odean (2008), and Hou, Peng, and Xiong (2009) argue that the market may overreact to information in a corporate announcement if the announcement attracts an unusual level of investor attention to the stock. Thus, if the positive split announcement return represents a temporary overreaction to a sudden increase in a firm's visibility, conclusions based on positive announcement returns may be innately spurious.

Sidestepping announcement returns, a group of signaling studies focuses on long-term post-split performance. The shift to long-term measures is quite sensible, considering the market's ability to correct overreactions over time. If splits indeed relay positive signals, these studies expect to find evidence of improved long-term post-split performance. Notable in this group are papers by Lakonishok and Lev (1987), Asquith, Healy, and Palepu (1989), and Byun and Rozeff (2003) who find no evidence of increases in earnings and no evidence of positive long-term returns after stock splits, thus undermining the signaling hypothesis. In the meantime, studies that use alternative techniques to measure long-term post-split returns (Ikenberry et al., 1996; Desai and Jain, 1997; Ikenberry and Ramnath, 2002) find evidence of positive abnormal returns, consistent with signaling. In a way, the split signaling debate is at an impasse due to a disagreement as to the proper way to measure long-term returns.

We innovate by testing the split signaling hypothesis from a new angle that does not rely solely on short-term or long-term return measurement. Instead, we focus on the post-split behavior of sophisticated investors, as represented by short sellers. Our focus on short selling is prompted by the literature that argues that short sellers possess superior investment skills (Dechow, Hutton, Meulbroek, and Sloan, 2001; Boehmer, Jones, and Zhang, 2008; Engelberg, Reed, and Ringgenberg, 2012; Rapach, Ringgenberg, and Zhou, 2016; Akbas, Boehmer, Erturk, and Sorescu, 2017).

We examine 5,979 splits that occur during a 31-year period from January 1988 through December 2018. Throughout the entire sample period, our results are consistent with the hypothesis that stock splits are interpreted by sophisticated investors as positive signals. Our most conservative estimation shows a post-split drop in short interest of about 10%. Furthermore, the decline in short interest is larger when the split signal is stronger. Specifically, short interest declines more in reaction to (i) large splits and (ii) splits that bring stock prices to a level that is lower than that achieved by the previous split – two characteristics that the literature recognizes as amplifiers of positive signals (McNichols and Dravid, 1990; Conroy and Harris, 1999). In addition, split signals have a more prominent effect when they are sent by firms with higher information asymmetries.

We test the results for robustness to the catering to small investors hypothesis of Baker and Gallagher (1980). This hypothesis states that splits are meant to attract individual investors into stock ownership by making shares nominally more affordable. Schultz (2000), Easley, O'Hara, and Saar (2001), and Dyl and Elliott (2006) provide evidence consistent with post-split increases in shareholdings of individual investors. In our setting, an increase in individual investor activity may exacerbate idiosyncratic volatility (Brandt, Brav, Graham, and Kumar, 2009) and thus increase short selling costs (Duan, Hu, and McLean, 2010). As a result, short sellers may tend to avoid recent splitters, and the decline in short interest may be driven by their aversion to idiosyncratic volatility rather than by signalling. The data refute this possibility; controlling for the post-split changes in idiosyncratic volatility does not alter the results. The results are also robust to controlling for conventional short interest determinants such as institutional holdings, returns, liquidity, and total volatility.

In addition, we conduct a set of robustness tests that eliminate confounding events that may (i) affect the strength and clarity of the split signal or (ii) affect short interest independently of the split signal. Since short selling is often used by index arbitrageurs, we test the results for dependence on inclusions to and exclusions from the S&P 500 index. Our findings are also robust to dropping a set of splits that are accompanied by changes in dividends and therefore may capture dividend rather than split signals (Nayak and Prabhala, 2001).

Finally, we ask whether short sellers are successful at anticipating post-split long-term returns. If they are not successful in this setting, their short positions may be reduced for other reasons and could not be interpreted as evidence in favor of signaling. We show that the splits associated with the largest declines in short interest are followed by long term positive returns, measured with the calendar time abnormal return (CTAR) framework of Byun and Rozeff (2003). Meanwhile, the remaining splits are followed by non-positive returns. This result holds for both strong and weak split signals. Thus, although our evidence suggests that short sellers react to the known split signal amplifiers, their understanding of the signal seems to be enhanced by the ability to analyze information beyond that revealed in the split announcement. This result is consistent with findings by Engelberg et al. (2012) who suggest that short sellers' trading performance is related to their ability to process publicly available information better than an average investor.

Our contribution to the literature is threefold. First, our main contribution is in showing that sophisticated investors infer positive signals from split announcements. As such, our approach avoids relying solely on return measurements that have produced conflicting results in earlier studies. Second, we show that short sellers' reaction to split announcements is more consistent with signaling after the financial crisis, possibly due to weakening of alternative split motives. Thus, our results are consistent with existence of a time-variant set of split motives. We propose that the lack of consistent evidence on split signaling in earlier studies may be due to the fact that earlier samples contain a number of splits that were initiated for non-signaling reasons. Finally, we find some evidence that the decline in short interest may be based on information that is beyond that derived from the conventional amplifiers of split signals. Our results are thus consistent with research that argues that short sellers possess superior ability to process value-relevant information.

The remainder of the paper is organized as follows. Section 1.2 discusses the literature on stock splits and short seller informedness. Section 1.3 describes the data and sample. Section 1.4 contains the main empirical results. Section 1.5 concludes.

1.2. Background

The finance literature identifies two most likely motives for stock splits: (i) signaling, and (ii) catering to investors and/or intermediaries. The literature, however, is far from a consensus as to whether the two motives are able to explain cross-asset pervasiveness and historical persistence of stock splits (Rozeff, 1998; Weld et al., 2009). Although the motives behind split decisions are somewhat unclear, there is abundant evidence of nontrivial costs associated with stock splits. Among these are direct administrative/legal cost, the costs of getting a split approved by the shareholders, the additional per-share listing and maintenance fees levied by some stock exchanges, and the per-share franchise taxes levied by the states of incorporation. Together, these costs may often add up to millions of dollars. Also notable are higher trading costs and lower liquidity in the post-split months (Conroy, Harris, and Benet, 1990; and Kadapakkam, Krishnamurthy, and Tse, 2005) that may have an adverse effect on the cost of capital.[1] Thus, with the obvious costs and unclear benefits, stock splits remain one of the less understood corporate decisions.

Surveyed CFOs cite the expansion of the shareholder base as the main split motive (e.g., Baker and Gallagher, 1980). The per-share price reduction that results from a split makes a stock more affordable to small investors, attracting them to the realm of stock ownership. Schultz (2000), Easley et al. (2001), and Dyl and Elliott (2006) find empirical evidence consistent with this proposition, showing that the numbers of small transactions, uninformed traders, and small shareholders increase post-split. In the meantime, Weld et al. (2009) examine a long time series of splits and argue that the clientele explanation is not sensible when one takes into account changes in individual nominal incomes. Additionally, attempts to attract small shareholders are unable to

[1] The direction of liquidity changes subsequent to stock splits is subject to debate. While Conroy et al. (1990), Easley et al. (2001), and Kadapakkam et al. (2005) argue that liquidity, as measured by bid-ask spreads, declines post-split, Lin, Singh, and Yu (2009) show that, if measured as incidence of no trading, liquidity increases post-split.

explain mutual fund splits (Rozeff, 1998). Mutual fund shares are infinitely divisible, and therefore splitting them is not necessary to make the fund affordable to small investors. Curiously, mutual funds regularly split their shares.

Muscarella and Vetsuypens (1996) and Angel (1997) propose a different angle to the catering hypothesis. They suggest that firms split to increase the tick-to-price ratio and hence the profits from providing liquidity in their stock. In addition, by increasing the number of shares required to transact a particular dollar amount, splits increase profits from intermediating institutional trades (Chemmanur, Hu, and Huang, 2015), as institutional trading fees are a function of the number of transacted shares. In exchange for higher profitability, market makers reward splitters with better liquidity and exert more effort to promote the firm's stock.[2] Angel suggests that the importance of the catering to intermediaries should diminish upon reduction in the minimum tick size. Whereas a 2-for-1 split has the potential to double market making revenues under any tick size regime; the amount of revenue being doubled is much smaller under decimals than it is under the eighths or the sixteenths.

Signaling is another widely studied split motive. Return reaction to stock splits is usually positive, which Grinblatt et al. (1984) interpret as evidence that splits are positive signals. In a theory model by Brennan and Copeland (1988), undervalued firms credibly signal their higher quality by splitting the stock. McNichols and Dravid (1990) suggest that firms signal private information with their choice of a split factor, with larger splits being interpreted as more positive signals. Conroy and Harris (1999) show that seasoned splitters, and especially seasoned splitters that split to prices that are lower than those achieved by their previous split, send stronger signals.

[2] Schultz (2000) reports evidence consistent with such promotional activity.

According to Lakonishok and Lev (1987) and Weld et al. (2009), stock splits are merely mechanical adjustments toward a commonly accepted price benchmark. Such adjustments, although costly, are in line with Akerlof's (2007) proposition that a number of economic decisions are driven by societal norms. Weld et al. (2009) argue that, in North America, a nominal stock price of around $40 per share is one such norm. They conclude that the norms explanation is the only split motive that is able to explain splits persistence over time. Bae, Bhattacharya, Kang, and Rhee (2019) provide further evidence that nominal price anchors are a global phenomenon. Our results pose a challenge to norms as the only explanation for stock splits, as the decline in short interest in reaction to split announcements is not easily explained in the norms framework.

We are not the first to study short interest in relation to stock splits. Kadiyala and Vetsuypens (2002) do not find significant changes in short interest around splits in a relatively small sample of 296 splitters during a 4-year period in the mid-1990s. With a larger sample, we are able to show significant and meaningful declines in short interest.

1.3. Data and sample

Our sample includes all NYSE and NASDAQ ordinary common shares (CRSP exchange codes 1 and 3 and share codes 10 and 11) from January 1988 through December 2018. For each security, we identify splits as distributions with the CRSP event code 5523. We exclude reverse splits and stock dividends that we identify as distribution events with split factors less than 0.25.

We obtain monthly short interest data from the NYSE (January 1988 through December 2007), NASDAQ (June 1988 through July 2007),[3] and Compustat (January 1973 through December 2018). Short interest is normalized by the number of outstanding shares and winsorized

[3] Short interest data are missing for all NASDAQ stocks in February and July of 1990. Similarly to earlier studies, we use linear extrapolation to estimate short interest for these two months.

at the 1% level. When there are discrepancies between the data from the exchanges and the adjusted records from Compustat, we keep the exchange data because the source is more direct. Whereas the data from the exchanges are comprehensive, the short interest data from Compustat cover virtually no NASDAQ stocks prior to July 2003. After 2003, almost all NASDAQ stocks are included. For the NYSE stocks, Compustat short interest coverage gradually increases from about 13% of splitting companies in 1973 to 57% in 1988 and 99% in 2006. To avoid a potential data selection bias in the missing observations before 1988, we restrict the analysis to the period 1988-2018 where joint coverage is near 100% throughout the sample period. Nevertheless, our results are robust to including the data from 1973.

In Table 1.1, for every sample year, we report the following statistics: (i) the number of splits; (ii) the percent of splits that are large (a split factor of two or greater); (iii) the percent of seasoned splitters (firms that have split at least once prior to the current split); (iv) the average stock price; and (v) the percent short interest, $SI_{i,t}$. An average of 193 firms split their stock every year during our sample period. Notably, the early sample years are considerably different from the later years, with the number of splits declining in 2001, concurrently with decimalization and the economic recession. Consistent with the findings of Minnick and Raman (2014), the number of splits declines even further in 2008, during the global financial crisis, and does not rebound to the pre-crisis levels even a decade later, at the end of the sample period. These structural breaks motivate sub-period analyses of 1988-2000, 2001-2007, and 2008-2018 for the main empirical results.

Statistics in Table 1.1 also show that the shares of large splits and seasoned splitters are both increasing over time where the shares of seasoned splitters increase gradually across the subperiods. Large splits, on the other hand, are increasingly common except for a drop in 2001, resulting in comparable averages for the first two subperiods and a meaningful increase from 54% to 72% in the last 2008-2018 period. These sample features motivate subperiod analysis for subsequent tests that rely on stratifying the sample by these two characteristics. We also note that stock prices rise exponentially across the sample period, with increasing growth rates as the number of splits declines.

Furthermore, the statistics show that short interest, *SI*, increases steadily for most of the sample period, starting at 0.60% in 1988, reaching a peak of 5.90% in 2008 and then stabilizing around 4.60%. This pattern is not surprising, as institutional trading, which is the primary driver of short selling (Boehmer et al., 2008), intensifies in the years leading up to the financial crisis (French, 2008), with some institutions (i.e., hedge funds) notoriously relying on short sales. Although not surprising, the strong time trend in short interest presents a methodological challenge. With the unconditional annual growth in short interest being 6.87% (Table 1.1), and our event-windows capturing close to two years of data, the tests need to control for this strong time trend.

To adjust for the time trend, we identify, for each splitter, a matched firm that does not split during the event window and has a set of split-relevant characteristics that closely resemble those of the splitter. Since short interest in all firms is affected by the upward trend, differencing *SI*s for the splitters and matched non-splitters should eliminate the trend. In addition, the matched-firm approach should reduce possible endogeneity in the short sellers' decision to reduce positions and the firm's decision to split. For instance, stocks that are becoming more liquid are more likely to split and such stocks may simultaneously become less attractive to short (Diether, Lee, and Werner, 2009). We thus address both time trends and endogeneity concerns by evaluating the difference-in-difference between splitters and stocks which closely resemble them in terms of changes in liquidity and other split and short interest determinants.

To find suitable matches, we use the two-step propensity score methodology of Heckman, Ichimura, and Todd (1997). In the first step, we model the binary split decision as a function of observable split and short interest determinants, and in the second step, we use the predicted split probabilities to find matched non-splitters that have similar split propensities. The details of the propensity score matching are discussed in the Appendix. Having paired splitters with their non-splitting matches, we proceed to inquire whether there is evidence of changes in short interest around split announcements.

1.4. Short interest around stock split announcements

1.4.1. Univariate analysis

To gain initial insight into short seller behavior around stock splits, we begin with a conventional event study methodology. For every splitter, we compute an abnormal short interest statistic, $ASI_{i,t}$, during a 21-month event window centered on the split announcement month. To compute *ASI*, short interest in the event window months is compared to the average short interest computed during the control period that spans months *t*-20 through *t*-11:

$$ASI_{i,t} = SI_{i,t} - 10^{-1} \sum_{-11}^{-20} SI_{i,t}, \qquad (1.1)$$

where $SI_{i,t}$ is the short interest ratio of firm *i* in month *t*. Next, we compute our main variable of interest, $\Delta_{ij}SI_t$, as the difference between abnormal short interest of the splitting firm *i* and its matching non-splitting firm *j*. To estimate the pure announcement effect, we limit the analysis to the splits, for which the announcement and the split event are separated by at least one month.[4] The results are organized in a [-10; +10]-month event window centered on the month of the split announcement. Month 0 represents the first post-announcement short interest record.

Figure 1.1 reports the average $\Delta_{ij}SI_t$ around split announcements. By construction, the difference in short interest between the splitters and their matches is rather stable in the pre-split period. After the splits, short interest decreases substantially; by 0.12% after one month. This

[4] We relax this restriction in the regression models that follow, as these models focus on long-term split effects.

decline stabilizes around -0.30% after several months, representing a relative drop of over 10% when compared to the mean short interest of 2.57% in the month before the split. This is a substantial decline, especially given the fact that not all short sellers are bearish about the specific stock they short and are thus unlikely to reduce their positions when positive firm-specific information becomes available. For example, an investor may buy inverse ETFs to express macro views or to hedge long positions, and the ETF may subsequently short the securities comprising its benchmark (Cheng and Madhavan, 2009; Huang, O'Hara, and Zhong, 2021). As another example, investors may simply be risk averse and buy put options to hedge their downside risk, possibly resulting in the option market maker shorting the underlying stock for hedging purposes (Battalio and Schultz, 2011). The preliminary results in Figure 1.1 are thus consistent with split signaling, as stock splits result in a significant and permanent decline in short interest.

1.4.2. Multivariate analysis

Although the decline in short interest is consistent with signaling, it may be alternatively attributed to, or partly influenced by, split-induced changes in widely known short interest determinants. Although our matching procedure controls for the pre-split changes in these determinants, it does not control for the post-split changes. To provide a background, in Table 1.2, we report pre- and post-split changes in short interest and the following short interest determinants: returns, volatility, illiquidity, and institutional ownership. All variables are computed as differences between abnormal levels of the respective variables for splitters and matched non-splitters.

The results presented in Table 1.2 confirm our previous assertion, as the relative levels of all covariates are much smaller in absolute value in the pre-split period compared to the post-split period. The only exception is institutional ownership, but the economic significance of a 0.45% difference is marginal given that average institutional ownership is over 50%. Economically, the most significant pre-split difference is in abnormal returns, which implies that the splitters have a somewhat higher abnormal return than their matches. However, the difference is not statistically significant and post-split abnormal returns are roughly 4 times as large. Corroborating Conroy et al. (1990) and Kadapakkam et al. (2005), post-split illiquidity increases by about 14%, and post-split volatility increases by about 16% consistent with Koski (1998).

Since the post-split changes in abnormal returns, volatility, illiquidity, and institutional ownership may affect short interest in the post-split period and therefore may affect the univariate results reported in Figure 1.1, we proceed to the multivariate tests. In Table 1.3, we report the estimated coefficients from the following difference-in-differences model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 SPLITTER_i \times POST_t + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t}, \tag{1.2}$$

where $SI_{i,t}$ is the number of shares in short positions of firm *i* in month *t* divided by the number of shares outstanding; $POST_{i,t}$ is the indicator variable equal to 0 in the pre-split months and equal to 1 in the split and the post-split months; and $SPLITTER_i$ is the indicator variable equal to 1 for the splitters and 0 for the matched non-splitters. The $\boldsymbol{x_{i,t}}$ vector of control variables includes (i) $AR_{i,t}$ – abnormal return; (ii) $VOLAT_{i,t}$ – volatility estimated as the average daily (high price – low price)/low price; (iii) $ILLIQUID_{i,t}$ – illiquidity estimated as the average daily bid-ask spread as in Corwin and Schultz (2012); and (iv) $INST_{i,t}$ – institutional ownership computed as the number of shares in institutional holdings reported via 13-F forms and scaled by the number of shares outstanding. We further adjust the model for firm and year fixed effects, so we do not include the *SPLITTER* variable by itself in model (1.2) and we do not estimate the intercept.

In specification [1] of Table 1.3, we begin with a baseline specification that includes *POST* and *SPLITTER*×*POST*. The effects of a split on the splitters compared to the matched firms is captured by *SPLITTER*×*POST*. Short interest declines by 0.21% after a stock split. The magnitude of this estimate is comparable to the univariate case discussed in Figure 1.1. In turn, *POST* captures the time trend for the matched non-splitters around the splits in their matches.

In specification [2], we expand the model to include the short interest determinants discussed above. Controlling for these does not change the main result – short interest declines after the splits – further corroborating the signaling hypothesis. The remaining coefficients indicate that higher volatility and institutional ownership correspond to higher levels of short interest. The opposite relation holds for returns and illiquidity, which are negatively related to short interest. These results corroborate the findings of Diether et al. (2009) with respect to volatility and expectations of Kadiyala and Vetsuypens (2002) with respect to illiquidity. They are also consistent with the prior literature that reports greater short interest in stocks held by institutional investors.

Baker and Gallagher (1980) report that managers commonly cite attracting small investors as the main motive for stocks splits. Schultz (2000), Easley et al. (2001) and Dyl and Elliott (2006) find that small investor activity and ownership significantly increase after splits. We must, therefore, consider the possibility that the post-split decline in short interest is driven by changes in small investor activity. On the one hand, greater individual ownership may result in more mispricing and more profitable arbitrage opportunities for short sellers. On the other hand, retail investors increase stock's idiosyncratic volatility (Brandt et al., 2009), which in turn increases short sellers' costs and may lead to reductions in their positions (Duan et al., 2010). Thus, it is important to test if the decline in short interest is affected by changes in the level of retail investor activity after the splits. We use idiosyncratic volatility as in Brandt et al. (2009) as a proxy for retail investor activity.

Specifications [3] and [4] include idiosyncratic volatility $IVOLAT_{i,t}$, estimated as in Brandt, et al. (2009). Its correlation coefficient with $VOLAT_{i,t}$ is 0.60, raising the possibility of collinearity. To address this concern, we exclude $VOLAT_{i,t}$, in specification [3] and include both in specification

[4]. The estimated coefficient on idiosyncratic volatility is positive and significant in specification [3], but insignificant in [4]. Most importantly, the decline in short interest continues to be observed when idiosyncratic volatility is controlled for.

As a robustness check, we follow Bertrand, Duflo, and Mullainathan (2004) in collapsing the pre-split and post-split time periods to address potential correlation in the standard errors of our estimates. Specifically, we include only two observations in the [t-10, t+10] window for each splitter and each matched non-splitter; one averaged over the pre-announcement months and the other averaged over the post-announcement months. The coefficients of interest, i.e., those on *SPLITTER×POST*, reported in Panel B of Table 1.3 are similar to the main analysis and remain statistically significant.

In summary, the multivariate results indicate that post-split decreases in short interest are not driven solely by the changes in conventional short interest determinants, corroborating the signaling explanation. The most conservative estimation points to a relative drop in short interest of nearly 10% (0.172 relative to 1.77, the mean short interest of splitters in specification [3] of Table 1.3 in the month before the split).

1.4.3. Additional tests of signaling

Our tests so far imply that short seller behavior is consistent with receiving a positive signal from stock splits. In this section, we look for additional confirmation of the signaling explanation by examining short interest in the signaling framework proposed by earlier studies. In particular, we rely on findings of McNichols and Dravid (1990) and Conroy and Harris (1999), who show that larger splits and splits by seasoned splitters (especially to prices that are lower than those achieved through the previous split) serve as stronger signals.

In addition, we test the generally accepted premise of the signaling theory that reactions to signals should be stronger for firms with greater information asymmetry (Desai and Jain, 1997); for such firms the utility of the signal is greater. The literature proposes a number of information asymmetry proxies, from which we choose: (i) the dispersion of analyst opinion, (ii) the firm's share of R&D expenditures, and (iii) the firm's percentage of intangible assets.

To test these expectations in our setting, we estimate the following equation:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 POST_t \times \delta_{i,t} + \beta_3 SPLITTER_i \times POST_t +$$
$$\beta_4 SPLITTER_i \times POST_t \times \delta_{i,t} + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t} \tag{1.3}$$

where $SI_{i,t}$ is defined as previously, and δ_i is the indicator variable that takes the value of 1 if a split or a splitting firm has a specific characteristic. We consider the following characteristics: (i) *large split* (similarly to McNichols and Dravid (1990) and Byun and Rozeff (2003), we define large splits as 2:1 and larger); (ii) firm is a *seasoned splitter* (similarly to Conroy and Harris (1999), firms that have split their shares in the past are considered seasoned splitters); (iii) splitting to a *lower price* as compared to that achieved through the previous split; (iv) *high dispersion* of analyst opinion (firms in the three upper deciles by dispersion); (v and vi) *high R&D* expenditures as a share of all expenditures and *high intangible* assets as a share of total assets (firms in the three upper deciles by, respectively, R&D and intangibles). Control variables are defined as in equation (1.2), and the difference-in-differences regression structure is preserved.

The estimated coefficients from various equation (1.3) specifications are reported in Table 1.4. In the signaling context, we are mainly interested in the coefficient of the interaction term *POST×SPLITTER×δ,* which captures the post-split differences between the splitters and their matches for various characteristics discussed above. For instance, the negative coefficient on *POST×SPLITTER×δ*, where *δ* is equal to 1 for large splits, indicates that short interest declines by

0.26% more in response to large splits relative to small splits. The largest incremental short interest decline of 0.51% is observed for high R&D firms, one of our proxies of information asymmetry.

Larger splits, splits by seasoned splitters, and splits to lower than previous prices have stronger negative effects on short interest. When it comes to firm characteristics, all information asymmetry proxies also have expected signs. Specifically, firms with high dispersion in analysts' opinion, firms with high R&D expenses, and firms with high levels of intangible assets – all proxies for high information asymmetry – send stronger signals by splitting their shares. Overall, these results are consistent with the signaling hypothesis.

1.4.4. Alternative explanations, sub-period results, and confounding events

In this sub-section, we split the sample period into three sub-periods to examine whether the evidence of signalling changes over time. In addition, we carry out a series of robustness checks to determine if our main result is affected by two potentially confounding events: inclusions in and exclusions from the S&P 500 index and dividend changes.

We noted earlier that the number of splits permanently decreases after decimalization and again after the financial crisis, and that average short interest appears to have reached an equilibrium after rising from the start of the sample until the crisis. It is therefore useful to ask if our results hold throughout the sample period. We might expect that the evidence of signaling intensifies after decimalization as the incidence of splits that are motivated by catering to intermediaries should reduce.

Using 2001 and 2008 as cut-offs, we estimate the models (1.2) and (1.3) and report the coefficients of interest in Table 1.5. We find that for 1988-2000, all the results still hold except for stocks with a high analyst dispersion. Interestingly, the post-split decline in short interest after the financial crisis is over four times as large as in the earlier periods, consistent with the possibility that the number of stocks splits driven by non-signalling motives has declined. The statistical significance is lost for many of the tests of split signaling strength in the latter two subperiods. However, the sub-period analysis shows that the full sample results for large splits and seasoned splitters are not driven by time trends in their relative occurrence.

We note that the coefficients on illiquidity become insignificant after decimalization. This result might be attributable to the fact that the accuracy of our illiquidity measure drops after decimalization, as noted by Corwin and Schultz (2012). In unreported results, we find that when we estimate illiquidity by the CRSP spread (in cents), the coefficient remains negative and significant in the 1988-2000 and 2001-2007 subperiods. It does lose statistical significance in the last period 2008-2018, but the magnitude is slightly greater in absolute value than either of the first two subperiods. With the CRSP spread as illiquidity measure, the estimated post-split decline in short interest of splitters is slightly larger.

Short positions are often used by index arbitrageurs for hedging; if an index funds trades below its intrinsic value, arbitrageurs can buy the index and short its constituents for a riskless profit. In addition, short sales constraints may weaken after index inclusion as index funds lend out holdings as well as institutions that start including the stock in various benchmarks. Thus, if a split is accompanied by an index inclusion (exclusion), we expect an increase (decrease) in short positions of these market participants. Panel A of Table 1.6 shows that about 2% of splits are accompanied by an inclusion into the S&P 500 index during the 21-month event window that surrounds split announcements. On the other hand, exclusions rarely occur during the event window, we only observe one exclusion.

Nayak and Prabhala (2001) caution that the magnitude of split signals may be misestimated because split announcements often occur simultaneously with the announcements of dividend changes. In our sample, 2,092 split announcements (about 53% of all splits) are adjacent to dividend changes. To check if our results are driven by split signals instead of dividend signals, we re-estimate the signaling model for the sub-sample of splits that are not accompanied by dividend changes.

In Table 1.6, we re-estimate equations (1.2) and (1.3) excluding the above-mentioned confounding events, with the latter equation estimated for large splits. The results support our previous conclusions. Short interest declines for the splitters relative to matches, and exclusively for large splits. Eliminating splits accompanied by dividend changes or S&P in- and exclusions results in a smaller coefficient on *POST×SPLITTER* than that reported in Table 1.3, consistent with the fact that some signals might be attributable to dividend announcements. In contrast, the large split results become even stronger in magnitude when we eliminate confounding events, consistent with the split ratio being more important as a signal when there is no concurrent change in the stock's dividend. Overall, the results consistent with the signaling hypothesis are robust to controlling for confounding events.

1.4.5. Post-split returns

Ikenberry and Ramnath (2002) use the buy-and-hold returns, BHARs, and find strong evidence that post-split long-term returns are positive. Using an arguably more robust methodology of calendar time abnormal returns, CTARs, Byun and Rozeff (2003) find that splits are usually not followed by long-term positive abnormal returns, thus undermining the signaling hypothesis. Our tests suggest that sophisticated investors behave consistently with signaling, as they reduce short positions in anticipation of positive post-split returns. If such positive returns do not ensue, our argument of short sellers' superior ability to analyze value-relevant information is untenable, at least in relation to stock splits.

Since Byun and Rozeff's CTAR approach produces more conservative return estimates compared to those from the BHAR approach, we estimate CTARs to test short sellers' long-term return forecasting ability. In month *t*, $CTAR_t$ is the average abnormal return for all sample firms that have effected a split within the prior 12, 24, or 36 months. As in Nain and Yao (2013), CTARs are measured as the estimated alpha from the 4-factor model:[5]

$$R_{i,t} - R_{f,t} = \alpha_i + \beta_i\left(R_{m,t} - R_{f,t}\right) + s_i SMB_t + h_i HML_t + m_i MOM_t + \varepsilon_{i,t}, \qquad (1.4)$$

where $R_{i,t}$ is firm i's monthly return, including dividends, in month *t*; $R_{f,t}$ is one-month Treasury bill return; and $R_{m,t}$ is the return on the CRSP value-weighted portfolio of all NYSE and NASDAQ stocks. The SMB and HML factors are defined as in Fama and French (1993) and the momentum factor is defined as in Carhart (1997). The models are estimated over the entire sample period 1988-2018.

As Byun and Rozeff, we estimate CTARs separately for large and small splits. Additionally, within split size groups, we estimate CTARs for all splitters in the sample and then, separately, (i) for the splits followed by the largest declines in short interest in the five months following the split, $\Delta_{ij}SI_{large}$, and (ii) for the splits followed by the smallest changes in short interest, $\Delta_{ij}SI_{small}$. We choose five months because Figure 1.1 shows that most of the post-split short interest decline happens within this period. The largest (smallest) changes in short interest are defined as those below (above) the 30th (70th) percentile of all relative changes.

The results reported in Table 1.7 are consistent with the notion that short sellers have superior ability to analyze corporate signals. First, the data show that, in the cross-section, splits are followed by abnormal long-term returns. Second, and more importantly, when we focus on splits

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[5] We use the 4-factor model, because the splitters are characterized by sizeable pre-split returns, and thus price momentum may relate to subsequent returns. Of particular concern is short sellers potentially decreasing their positions for splits with larger pre-split returns because of the associated stronger momentum effects.

that are accompanied by significant declines in short interest (columns titled $\Delta_{ij}SI_{large}$), we find that short sellers are able to identify splits that represent true positive signals. In the meantime, for the group of splits followed by the smallest changes in short interest, post-split CTARs are statistically insignificant. This result obtains for large and small splits and for the 12-, 24- or 36-month portfolio inclusion windows. Overall, these results are consistent with signaling and with short sellers' superior ability to interpret the signals.

1.5. Conclusions

In this study, we test the split signaling hypothesis of Brennan and Copeland (1988) from an angle that avoids relying solely on return measurement. Specifically, we focus on the post-split actions of a group of sophisticated investors, short sellers, and ask whether their behavior is consistent with receiving a positive value-relevant signal from a stock split.

Our findings suggest that splits are interpreted by short sellers as positive signals, as short interest usually decreases by about 10% in reaction to split announcements. The decline in short interest is larger when the split signal is stronger as represented by (i) larger split factors, (ii) seasoned splitters, and (iii) lower post-split prices. In addition, split signals have a more prominent effect when they are sent by firms with greater information asymmetries.

We study 5,979 splits during a 31-year period from January 1988 through December 2018. Our results corroborate the signaling hypothesis throughout the entire sample period. The results are unchanged in a series of robustness checks that account for split-related changes in individual investor activity and the conventional short interest determinants such as abnormal returns, institutional holdings, liquidity, and volatility. In addition, the results are robust to eliminating a set of confounding events that may (i) affect the strength and clarity of the split signal or (ii) affect short interest levels. Sub-period analyses show that the short interest decline is apparent throughout the sample period, with a stronger decline after the financial crisis of 2008. Given that the number of splits concurrently declined, the results suggest that it is mostly splits driven by non-signaling motives that have disappeared.

We also ask whether short sellers are successful at predicting long-term returns that follow stock splits. We show that split announcements that are associated with the largest declines in short interest are followed by positive calendar time abnormal returns, CTARs. Meanwhile, splits that do not lead to a significant decline in short interest are followed by zero abnormal returns. Thus, although the evidence suggests that short sellers react to the known split signal amplifiers, their understanding of split signals seems to be enhanced by the ability to analyze information beyond that revealed in the split announcement.

1.6. Appendix: Propensity Score Matching

To identify the composition of the split decision model, we refer to the existing literature for guidance. We use a combination of the binary models of Lakonishok and Lev (1987), Nayak and Prabhala (2001), and Baker et al. (2009). In particular, we model split decisions as a function of (i) the price ratio – the relation of the firm's pre-split price to the average market price, (ii) the pre-split price runup, (iii) the pre-split average monthly change in stock return volatility, (iv) the pre-split average monthly change in stock illiquidity, (v) the pre-split average monthly changes in institutional ownership, (vi) firm size, (vii, viii) dummy variables to distinguish between the tick size regimes (eighths, sixteenths, and decimals), and (ix) a post-financial crisis dummy variable. In additional specifications, we include the level of short interest and the pre-split average monthly changes in short interest.

We offer the following reasoning for this model structure. As argued by Lakonishok and Lev (1987) and Weld et al. (2009), firms with high price ratios are likely to lower their prices by splitting. Such decisions are often conditional on the relatively rapid price runup and company size, with larger companies often opting for higher nominal prices as suggested by Nayak and Prabhala (2001). More recently, Baker et al. (2009) introduce volatility as a split determinant, arguing that firms with high volatility should be reluctant to force their prices down. To allow firms' split decision criteria to change with tick size and after the recent financial crisis, we also add indicator variables that distinguish between three tick size regimes (eighths, sixteenths, and decimals) and for before and after the financial crisis.

Since short interest is our main variable of focus, we require that the splitters and their matches have similar conditions for establishing short interest positions. Hence, in addition to the split determinants, we control for the conventional short interest determinants: institutional holdings and liquidity. Institutional ownership is often used as a proxy for short selling constraints

(Asquith, Pathak, and Ritter, 2005), whereas liquidity is deemed to be an important short selling determinant, although the direction of the relation is not yet settled in the literature. Whereas Kadiyala and Vetsuypens (2002) suggest that, in the long run, short sellers tend to avoid illiquid stocks due to higher risk of unwinding positions in such stocks, Diether et al. (2009) view short sellers as short-term liquidity providers who seek out illiquid stocks.

In summary, as the first step of the propensity score matching procedure, we estimate the following logistic regression:

$$\begin{aligned} \Pr\,(split_{i,t} = 1) = \quad & \alpha + \beta_1\, SI_{i,t\text{-}1} + \beta_2 \Delta SI_{i,[t\text{-}\kappa;\, t\text{-}1]} + \beta_3\, PRATIO_{i,t\text{-}1} + \beta_4\, AR_{i,[t\text{-}10;\, t\text{-}1]} + \\ & \beta_5 \Delta VOLAT_{i,[t\text{-}10;\, t\text{-}1]} + \beta_6 \Delta ILLIQ_{i,[t\text{-}10;\, t\text{-}1]} + \beta_7 \Delta INST_{,[t\text{-}10;\, t\text{-}1]} + \\ & \beta_8\, NYSED_{i,t\text{-}1} + \beta_9\, SIXTNTHS_t + \beta_{10}\, DECIMALS_t + \\ & \beta_{11}\, POSTCRISIS_t + \varepsilon_{i,t}, \end{aligned} \qquad (1.A1)$$

where the binary dependent variable $split_{i,t}$ is equal to 1 if firm i announces a split in month t and is equal to 0 otherwise; $PRATIO_{i,t\text{-}1}$ is firm i's price ratio computed as its price in the pre-split announcement month divided by the average price for all sample stocks other than i; $AR_{i,[t\text{-}10;t\text{-}1]}$ is the buy-and-hold abnormal return that represents the price runup; $\Delta VOLAT_{i,[t\text{-}10;t\text{-}1]}$ is the monthly change in stock i's daily volatility estimated as the average daily (high price – low price)/low price; $\Delta ILLIQ_{i,[t\text{-}10;t\text{-}1]}$ is the mean monthly change in stock i's illiquidity estimated as the average daily bid-ask spread as in Corwin and Schultz (2012); $\Delta INST_{,[t\text{-}10;\, t\text{-}1]}$ is the mean monthly change in institutional ownership of stock i, where institutional ownership is computed as the number of shares in institutional holdings reported via Form 13-F, scaled by the number of shares outstanding; $NYSED_{i,t\text{-}1}$ is the NYSE market capitalization decile that firm i belongs to; $SIXTNTHS_t$ and $DECIMALS_t$ are indicator variables that identify minimum tick size regimes, with the eighths being the base regime; and $POSTCRISIS_t$ is an indicator variable that equals one from the year 2008 onward. The monthly buy-and-hold abnormal return is estimated as in Ikenberry and Ramnath

(2002), using the Fama and French 3 factor model as the benchmark return. Daily stock volatility is estimated as the average daily (high price – low price)/low price. Illiquidity is estimated as the average daily bid-ask spread as in Corwin and Schultz (2012).

Specification [1] of Table 1.A1 reports the marginal effects obtained from the estimated coefficients. Although the economic interpretation of split determinants is not the main focus of this study, we briefly discuss them to highlight similarities with prior research. Split likelihood increases in the price ratio, price runup, liquidity (decreases in illiquidity), and institutional ownership, while it declines upon the switch to sixteenths and, further, upon the switch to decimals and after the financial crisis of 2008. Changes in volatility are only marginally relevant for firms' split decisions. Finally, large firms are less likely to split, consistent with the cross-sectional pattern, in which the median stock price for large firms is $41.88 whereas it is just $7.85 for small firms. Overall, the estimated effects corroborate prior research and our expectations.

In order to ensure that the environment, in which short sellers operate, is as similar as possible between the splitters and their matches, we also want to match on the short interest level and pre-split changes. In specifications [2] and [3] of Table 1.A1, we add the level of short interest and the average monthly changes in short interest in, respectively, five and ten pre-split months as explanatory variables in model (1.1). The estimated effects on the level of short interest and the change in short interest are insignificant. However, to improve our matching procedure in terms of short interest, which is our main variable of interest, we opt to use specification [3] as the propensity score model to match splitting firms to matching non-splitting firms.

Having estimated the propensity to split for each firm in each sample month, we continue to the second step of Heckman et al. (1997) procedure that involves finding suitable matches for the splitting firms. We match (without replacement) every firm *i* that announces a split in month *t* with a non-splitting firm *j*, whose estimated propensity to split in month *t*-1 is the closest to that of firm *i*'s. To be eligible for matching, firm *j* must not split during the 10 months before or during the 10 months after the split by firm *i*. Having paired splitters with their non-splitting matches, we proceed to inquire whether there is evidence of changes in short interest after the split announcements.

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Table 1.1. Sample statistics

The table contains sample statistics for a 31-year sample period that spans January 1988 through December 2018. We report (i) the number of splits; (ii) percentage of large splits; (iii) percentage of splits by seasoned splitters; (iv) short interest, *SI*; and (v) stock price. The sample includes all NYSE and NASDAQ common shares (CRSP exchange codes 1 and 3 and share codes 10 and 11). Splits are defined as distributions with CRSP event code 5523 excluding reverse splits, stock dividends, splits occurring within 1 year of the last eligible split, and splits with missing announcement dates. Large splits are defined as those with CRSP split factor of at least 1 (i.e., 2:1 and larger splits). Seasoned splitters are defined as firms that have split at least once prior to the current split. Short interest, *SI*, is the number of shares in short positions scaled by the number of shares outstanding.

Year	*# splits*	*% large splits*	*% seasoned*	*Stock price*	*SI, %*
1988	208	36.54	58.65	15.72	0.60
1989	290	44.14	59.66	15.63	0.54
1990	173	57.23	57.80	13.89	0.74
1991	238	48.32	64.71	15.13	0.82
1992	378	47.09	59.26	16.90	0.85
1993	416	49.28	53.61	19.15	0.98
1994	300	54.33	49.33	18.58	1.12
1995	407	54.05	47.42	21.07	1.20
1996	451	55.43	50.11	22.95	1.21
1997	456	54.82	57.02	25.26	1.40
1998	382	60.99	58.64	28.71	1.57
1999	343	68.51	47.23	29.19	1.47
2000	278	74.82	46.40	28.84	1.52
2001	158	41.77	66.46	28.44	1.85
2002	168	49.40	60.71	30.55	2.34
2003	208	43.75	59.13	32.67	2.52
2004	238	56.72	61.34	39.97	2.81
2005	251	58.96	64.54	41.13	3.26
2006	185	56.22	62.16	44.51	4.01
2007	117	68.38	65.81	51.36	5.00
2008	33	78.79	72.73	48.43	5.90
2009	13	69.23	69.23	40.02	3.97
2010	43	46.51	74.42	50.76	4.28
2011	52	76.92	61.54	54.52	4.34
2012	39	82.05	69.23	59.05	4.43
2013	50	70.00	76.00	76.90	4.16
2014	46	82.61	58.70	87.76	4.52
2015	18	100.00	50.00	92.51	4.70
2016	10	60.00	90.00	94.04	4.79
2017	19	57.89	73.68	117.39	4.82
2018	11	63.64	90.91	141.94	4.71
Mean	193	60.04	61.98	45.26	2.79
Mean 1988-2000	333	54.17	53.51	20.85	1.08
Mean 2001-2007	190	53.46	62.75	38.37	3.11
Mean 2008-2018	30	71.60	71.49	78.48	4.60
Total	5,988				
% annual growth				7.36	6.87

Table 1.2. Short interest and covariates around split announcements

The table reports differences between splitting firms and their matches for the following covariates: (i) $\Delta_{ij}SI$ computed as the mean difference in abnormal short interest, $ASI_{i,t} = SI_{i,t} - 10^{-1}\sum_{-11}^{-20} SI_{i,t}$, where $SI_{i,t}$ is the number of firm i's shares in short positions in month t scaled by the number of shares outstanding; (ii) $\Delta_{ij}AR$ is computed as the difference in abnormal returns estimated with the Fama and French three-factor model; (iii) $\Delta_{ij}VOLAT$ computed as the difference in abnormal volatility, $VOLAT_{i,t} - 10^{-1}\sum_{-11}^{-20} VOLAT_{i,t}$, where $VOLAT_{i,t}$ is estimated as (high price – low price)/low price; (iv) Δ_{ij} $ILLIQ$ computed as the difference in abnormal bid-ask spreads, $ILLIQ_{i,t} - 10^{-1}\sum_{-11}^{-20} ILLIQ_{i,t}$, where $ILLIQ_{i,t}$ is estimated as in Corwin and Schultz (2012), with negative spreads set to zero; and (v) $\Delta_{ij}INST$ computed as the difference in abnormal institutional ownership, $INST_{i,t} - 10^{-1}\sum_{-11}^{-20} INST_{i,t}$. The results are computed in the [*t*-10; *t*+10]-month window. The pre-announcement period is identified as *pre*, and the post-announcement period as *post*. *t*-statistics are in parentheses.

	$\Delta_{ij}SI$	$\Delta_{ij}AR$	$\Delta_{ij}VOLAT$	$\Delta_{ij}ILLIQ$	$\Delta_{ij}INST$
pre	-0.01%	0.14%	-0.05%*	-0.01%	-0.45%**
	(-0.13)	(1.38)	(-1.97)	(-0.79)	(-2.25)
post	-0.26%**	0.56%***	0.51%***	0.14%***	-0.37%
	(-2.44)	(6.04)	(14.26)	(8.37)	(-1.25)

*** p<0.01, ** p<0.05, * p<0.1

Table 1.3. Post-split changes in short interest: multivariate framework

The table contains coefficients from a set of panel regressions of short interest in a [t-10, t+10] window around split announcements. We estimate the following model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 SPLITTER_i \times POST_t + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t},$$

where $SI_{i,t}$ is the number of firm i's shares in short positions in month t scaled by the number of shares outstanding. $SPLITTER_i$ is the indicator variable equal to 1 for splitters and 0 for matched non-splitters. $POST_t$ is the indicator variable equal to 0 in the pre-event months and equal to 1 in the event and post-event months. The $\boldsymbol{x_{i,t}}$ vector of control variables includes (i) $AR_{i,t,}$ is the abnormal return estimated with the Fama and French three-factor model; (ii) $VOLAT_{i,t}$ is volatility estimated as the average daily (high price – low price)/low price; (iii) $ILLIQ_{i,t}$ is illiquidity estimated as the average daily bid-ask spread as in Corwin and Schultz (2012), with negative spreads set to zero; and (iv) $INST_{i,t}$ is institutional ownership, computed as the number of shares in institutional holdings reported via 13-F forms and scaled by the number of shares outstanding. Specification [3] and [4] include idiosyncratic volatility $IVOLAT_{i,t}$, estimated as in Brandt, Brav, Graham and Kumar (2009). Panel A reports the pooled regressions results, where we have one observation for every month *t* for every splitter and matched non-splitter in the [t-10, t+10] window. Panel B reports the averaged regression results, containing only two observations in the [t-10, t+10] window for each splitter and each matched non-splitter: one averaged over the pre-announcement months and the other averaged over the post-announcement months. The models are adjusted for firm and year fixed effects, and autocorrelation and heteroskedasticity using the Newey-West estimator. *t*-statistics are in parentheses.

Panel A: Pooled	[1]	[2]	[3]	[4]
POST	0.402***	0.244***	0.246***	0.244***
	(18.15)	(11.39)	(11.49)	(11.39)
SPLITTER×POST	-0.207***	-0.217***	-0.203***	-0.217***
	(-7.93)	(-8.61)	(-8.07)	(-8.61)
AR		-0.003***	-0.003***	-0.003***
		(-7.25)	(-8.43)	(-7.19)
VOLAT		0.057***		0.056***
		(12.11)		(9.29)
ILLIQ		-0.031***	0.006	-0.031***
		(-2.77)	(0.58)	(-2.76)
INST		0.076***	0.076***	0.076***
		(74.10)	(74.28)	(74.07)
IVOLAT			0.003***	0.000
			(8.75)	(0.17)
# obs.	165,732	165,732	165,732	165,732
R^2	0.027	0.092	0.091	0.092

Panel B: Averaged

POST	0.558***	0.061	0.061	0.060
	(14.57)	(1.47)	(1.48)	(1.46)
SPLITTER×POST	-0.193***	-0.195***	-0.172***	-0.196***
	(-3.60)	(-3.71)	(-3.31)	(-3.71)
AR		-0.049***	-0.052***	-0.049***
		(-10.42)	(-11.12)	(-10.34)
VOLAT		0.120***		0.117***
		(4.01)		(2.81)
ILLIQ		-0.202***	-0.114*	-0.201***
		(-2.65)	(-1.65)	(-2.64)
INST		0.092***	0.093***	0.092***
		(29.45)	(29.73)	(29.34)
IVOLAT			0.787***	0.041
			(2.89)	(0.11)
# obs.	16,840	16,414	16,414	16,380
R^2	0.040	0.157	0.156	0.157

*** p<0.01, ** p<0.05, * p<0.1

Table 1.4. Testing the signaling hypothesis

The table contains coefficient estimates from a set of panel regressions of short interest between the pre-announcement and post-announcement periods. We estimate the following model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 POST_t \times \delta_{i,t} + \beta_3 SPLITTER_i \times POST_t + \beta_4 SPLITTER_i \times POST_t \times \delta_{i,t} + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t},$$

where $SI_{i,t}$ is the number of firm *i*'s shares in short positions in month *t* scaled by the number of shares outstanding, where $t \in [-10, +10]$. $SPLITTER_i$ is the indicator variable equal to 1 for splitters and 0 for matched non-splitters. $POST_t$ is the indicator variable equal to 0 in the pre-event months and equal to 1 in the event and post-event months. $\delta_{i,t}$ is the indicator variable corresponding to a split characteristic or a splitting firm characteristic and has the same value for splitting firms and their matches. We consider the following characteristics: (i) *split size* (similarly to Byun and Rozeff (2003), 2:1 and larger splits are considered large); (ii) firm's *splitting experience* (similarly to Conroy and Harris (1999), firms that have split prior to the current split are considered seasoned splitters); (iii) splitting to a *lower price* that that achieved through a previous split; (iv) *dispersion of analyst opinion* (similarly to Hou, Peng and Xiong (2009), dispersion is measured as the standard deviation of the earnings forecast of the current fiscal year, scaled by the absolute value of the mean forecast, averaged over the previous year; firms in the 3 highest dispersion deciles are considered to have a high dispersion); (v and vi) *R&D expenses* and *the ratio of intangible assets to total assets* (firms in the 3 highest R&D and relative intangibles deciles are considered to have high R&D or intangibles). The $\boldsymbol{x_{i,t}}$ vector of control variables (estimated but not tabulated) includes: (i) $AR_{i,t}$, is the monthly abnormal return estimated with the Fama and French 3 factor model; (ii) $VOLAT_{i,t}$ is the monthly volatility estimated as the average daily (high price – low price)/low price; (iii) $ILLIQ_{i,t}$ is the monthly illiquidity estimated as the average daily bid-ask spread as in Corwin and Schultz (2012), with negative spreads set to zero; (iv) $INST_{i,t}$ is the institutional ownership computed as the number of shares in institutional holdings reported via 13-F forms and scaled by the number of shares outstanding. The models are adjusted for firm and year fixed effects, and autocorrelation and heteroskedasticity using the Newey-West estimator. *t*-statistics are in parentheses.

	large split	*seasoned splitter*	*seasoned to lower price*	*high dispersion*	*high R&D*	*high intangibles*
POST	0.183***	0.228***	0.234***	0.256***	0.236***	0.224***
	(6.37)	(7.41)	(9.95)	(11.44)	(10.87)	(9.90)
POST×δ	0.112***	0.034	0.053	-0.099*	0.129**	0.127***
	(3.20)	(0.93)	(1.35)	(-1.75)	(2.17)	(2.84)
POST×	-0.063*	-0.050	-0.184***	-0.190***	-0.165***	-0.127***
SPLITTER	(-1.70)	(-1.23)	(-6.38)	(-7.11)	(-6.36)	(-4.63)
POST×	-0.264***	-0.261***	-0.109**	-0.235***	-0.505***	-0.449***
SPLITTER×δ	(-5.32)	(-5.11)	(-1.96)	(-2.98)	(-6.00)	(-7.17)
CONTROLS	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.092	0.093	0.093	0.092	0.092	0.092

*** p<0.01, ** p<0.05, * p<0.1

Table 1.5 Sub-period analysis

Panel A contains the coefficients from a set of panel regressions of short interest of splitters and their matches in a [t-10, t+10] window around split announcements. We estimate the following model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 SPLITTER_i \times POST_t + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t},$$

where $SI_{i,t}$ is the number of firm *i*'s shares in short positions in month *t* scaled by the number of shares outstanding. $SPLITTER_i$ is the indicator variable equal to 1 for splitters and 0 for matched non-splitters. $POST_t$ is the indicator variable equal to 0 in the pre-event months and equal to 1 in the event and post-event months. The $\boldsymbol{x_{i,t}}$ vector of control variables $AR_{i,t}$, $VOLAT_{i,t}$, $ILLIQ_{i,t}$, and $INST_{i,t}$ are defined as in Table 1.4. Panel B reports the β_4 coefficients in the model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 POST_t \times \delta_{i,t} + \beta_3 SPLITTER_i \times POST_t + \beta_4 SPLITTER_i \times POST_t \times \delta_{i,t} + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t},$$

where $\delta_{i,t}$ is the indicator variable corresponding to a split characteristic or a splitting firm characteristic and has the same value for splitting firms and their matches. The characteristics are defined as in Table 1.4 and the $\boldsymbol{x_{i,t}}$ vector of control variables includes $AR_{i,t}$, $VOLAT_{i,t}$, $ILLIQ_{i,t}$, and $INST_{i,t}$. The models are adjusted for firm and year fixed effects, and autocorrelation and heteroskedasticity using the Newey-West estimator. *t*-statistics are in parentheses.

Panel A: Pooled			
	1988-2000	2001-2007	2008-2018
POST	0.229***	0.177***	0.259*
	(9.13)	(3.86)	(1.95)
SPLITTER×POST	-0.160***	-0.151***	-0.671***
	(-5.41)	(-2.79)	(-4.35)
AR	-0.002***	-0.002**	0.002
	(-4.74)	(-2.54)	(0.92)
VOLAT	0.051***	0.105***	0.044*
	(9.74)	(8.20)	(1.72)
ILLIQ	-0.073***	-0.005	-0.028
	(-6.16)	(-0.15)	(-0.31)
INST	0.053***	0.163***	0.133***
	(45.60)	(64.25)	(17.76)
# obs.	101,724	44,184	5,796
R^2	0.065	0.202	0.117
Panel B: Split or splitting firm characteristics			
large split	-0.204***	-0.314***	-0.465
	(-3.62)	(-2.98)	(-1.45)
seasoned splitter	-0.312***	-0.158	-0.612*
	(-5.44)	(-1.41)	(-1.84)
seasoned to lower price	-0.305***	0.260**	0.020
	(-4.84)	(2.17)	(0.06)
high dispersion	0.198**	-0.163	-0.808***
	(1.98)	(-1.15)	(-2.58)
high R&D	-0.566***	-0.095	-0.632*
	(-5.41)	(-0.58)	(-1.80)
high intangibles	-0.459***	-0.489***	0.869***
	(-5.67)	(-4.21)	(2.89)

*** p<0.01, ** p<0.05, * p<0.1

Table 1.6. Addressing confounding events

The table contains the coefficients from a set of panel regressions of short interest of splitters and their matches in a [t-10, t+10] window around split announcements. We estimate the following model:

$$SI_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 POST_t \times LS_{i,t} + \beta_3 SPLITTER_i \times POST_t + \beta_4 SPLITTER_i \times POST_t \times LS_{i,t} + \boldsymbol{x_{i,t}\gamma} + \varepsilon_{i,t},$$

where $SI_{i,t}$ is the number of firm *i*'s shares in short positions in month *t* scaled by the number of shares outstanding. $SPLITTER_i$ is the indicator variable equal to 1 for splitters and 0 for matched non-splitters. $POST_t$ is the indicator variable equal to 0 in the pre-event months and equal to 1 in the event and post-event months. LS_i is the indicator variable equal to 1 for 2:1 and larger splits and is equal to 0 otherwise. The $\boldsymbol{x_{i,t}}$ vector of control variables includes (i) $AR_{i,t,}$ is the monthly abnormal return estimated with the Fama and French 3 factor model; (ii) $VOLAT_{i,t}$ is the monthly volatility estimated as the average daily (high price – low price)/low price; (iii) $ILLIQ_{i,t}$ is the monthly illiquidity estimated as the average daily bid-ask spread as in Corwin and Schultz (2012), with negative spreads set to zero; (iv) $INST_{i,t}$ is the institutional ownership computed as the number of shares in institutional holdings reported via 13-F forms and scaled by the number of shares outstanding. In this table, we control for the following confounding effects: (i) inclusions and exclusions from the S&P 500 index, and (ii) dividend changes. Panel A contains the number of splits that are accompanied by events of each kind and the total number of confounding events. Panel B contains the results for the sample without confounding effects. The models are tested and adjusted for firm and year fixed effects and heteroskedasticity using Newey-West estimator. *t*-statistics are in parentheses.

Panel A: The number of confounding events

S&P500 inclusions & exclusions	*dividend changes*	*total confounding events*
96 & 1	2,092	2,155

Panel B: Elimination of confounding events

	[1]	[2]
POST	0.213***	0.112**
	(5.87)	(2.28)
POST×LS		0.188***
		(3.10)
POST×SPLITTER	-0.157***	0.053
	(-3.67)	(0.83)
POST×SPLITTER×LS		-0.386***
		(-4.51)
CONTROLS	Yes	Yes
# obs.	75,222	75,222
R^2	0.098	0.098

*** p<0.01, ** p<0.05, * p<0.1

Table 1.7. Calendar-time abnormal returns (CTARs)

The table contains calendar-time abnormal returns computed similarly to Nain and Yao (2013). We report the CTARS as the alphas estimated from a four-factor model over the sample period 1988-2018:

$$R_{p,t} - R_{f,t} = \alpha_i + \beta_i\left(R_{m,t} - R_{f,t}\right) + s_i SMB_t + h_i HML_t + m_i MOM_t + \varepsilon_{i,t},$$

where $R_{p,t}$ is the monthly return on the portfolio that includes all firms that have split within the prior 12, 24, or 36 months (corresponding to panels A, B, and C, respectively), including dividends, in month t, $R_{f,t}$ is the one-month Treasury bill return, and $R_{m,t}$ is the return on the CRSP value- or equal-weighted portfolio of all NYSE, AMEX, and NASDAQ stocks. The size and book-to-market factors are defined as in Fama and French (1993), and the momentum factor is defined as in Carhart (1997). As Byun and Rozeff (2003), we estimate CTARs separately for large splits (2:1 and larger) and small splits. Additionally, within split size groups, we estimate CTARs for all splitters in the sample and then separately (i) for splits followed by the largest declines in short interest in the 5 months following the split, $\Delta_{ij}SI_{large}$, and (ii) for splits followed by the smallest changes in short interest in the 5 months following the split, $\Delta_{ij}SI_{small}$. Largest declines in short interest are defined as those below the 30th percentile of all changes. Smallest changes are defined as those above the 70th percentile.

Panel A: 12-month CTAR

	large split			*small split*		
	all	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$	*all*	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$
EW	0.69**	1.48***	-0.22	0.45	0.94**	-0.18
	(2.11)	(3.85)	(-0.48)	(1.40)	(2.47)	(-0.48)
VW	0.97***	1.81***	0.06	0.77**	1.29***	0.23
	(2.87)	(4.55)	(0.13)	(2.29)	(3.25)	(0.57)

Panel B: 24-month CTAR

	large split			*small split*		
	all	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$	*all*	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$
EW	0.59**	1.36***	0.02	0.33	0.96***	-0.01
	(2.01)	(4.28)	(0.05)	(1.10)	(2.73)	(-0.04)
VW	0.84***	1.68***	0.31	0.61**	1.28***	0.31
	(2.79)	(5.06)	(0.82)	(1.99)	(3.51)	(0.86)

Panel C: 36-month CTAR

	large split			*small split*		
	all	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$	*all*	$\Delta_{ij}SI_{large}$	$\Delta_{ij}SI_{small}$
EW	0.53**	1.21***	0.02	0.35	0.81**	-0.22
	(1.99)	(3.91)	(0.06)	(1.22)	(2.36)	(-0.67)
VW	0.78***	1.53***	0.30	0.61**	1.10***	0.09
	(2.81)	(4.73)	(0.85)	(2.06)	(3.11)	(0.27)

*** p<0.01, ** p<0.05, * p<0.1

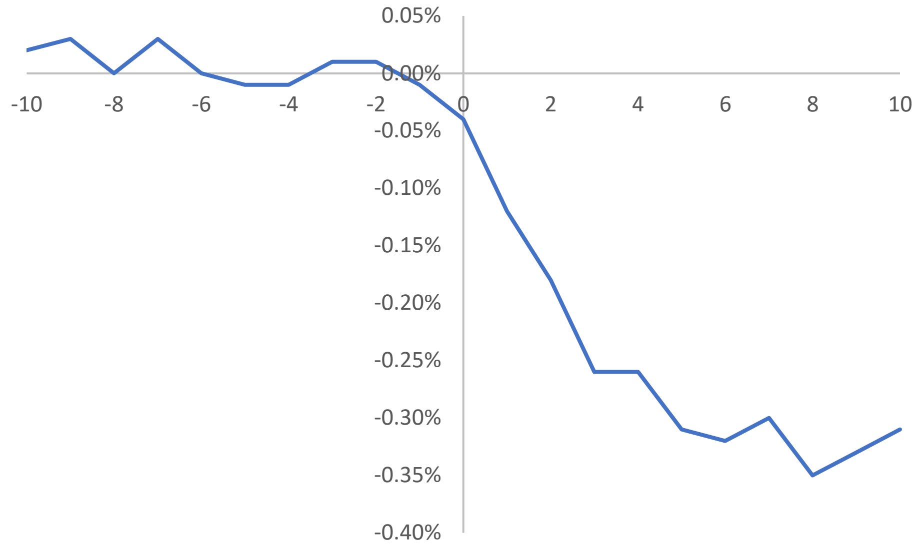

Figure 1.1 Abnormal short interest around split announcements
We estimate the propensity to split as the predicted value from the following panel logistic regression:

$$Pr\ (split_{i,t} = 1) = \alpha + \beta_1 SI_{i,t-1} + \beta_2 \Delta SI_{i,[t-10;\ t-1]} + \beta_3 PRATIO_{i,t-1} + \beta_4 AR_{i,[t-10;\ t-1]} + \beta_5 \Delta VOLAT_{i,[t-10;\ t-1]} + \beta_6 \Delta ILLIQ_{i,[t-10;\ t-1]} + \beta_7 \Delta INST_{,[t-10;\ t-1]} + \beta_8 NYSED_{i,t-1} + \beta_9 SIXTNTHS_t + \beta_{10} DECIMALS_t + \beta_{11} POSTCRISIS_t + \varepsilon_{i,t},$$

where the binary dependent variable $split_{i,t}$ equals 1 if firm i announces a split in month t and equals 0 otherwise; $SI_{i,t-1}$ is the short interest in shares divided by the total number of shares outstanding; $\Delta SI_{i,[t-10;\ t-1]}$ is the average rate of monthly change in short interest during the 10-month pre-split announcement period (all changes are computed as month-to-month continuous growth rates that are then averaged across the 10 pre-split months); $PRATIO_{i,t-1}$ is firm i's price in the pre-split announcement month divided by the average price for all sample stocks excluding firm i as in Lakonishok and Lev (1987); $AR_{i,[t-10;\ t-1]}$ is the mean monthly pre-split BHAR estimated as in Ikenberry and Ramnath (2002) with the Fama and French 3 factor model as benchmark return; $\Delta VOLAT_{i,[t-10;\ t-1]}$ is the change in stock i's volatility, where the monthly volatility is estimated as the average daily (high price – low price)/low price. $\Delta ILLIQ_{i,[t-10;t-1]}$ is the change in stock i's illiquidity computed as the average daily bid-ask spread as in Corwin and Schultz (2012); $\Delta INST_{i,[t-10;\ t-1]}$ is the change in stock i's institutional ownership computed as the number of shares in institutional holdings reported via Form 13-F, scaled by the number of shares outstanding; $NYSED_{i,t-1}$ is the NYSE market capitalization decile of firm i; $SIXTNTHS_t$ and $DECIMALS_t$ are indicator variables that identify minimum tick size regimes. $POSTCRISIS_t$ is an indicator variable that equals 1 from the year 2008 onwards. Having obtained predicted values for the propensity to split, $\widehat{Pr}(split\ =\ 1)_{i,t}$, we match (without replacement) every firm i that announces a split in month t with a non-splitting firm j whose propensity to split in month t-1 is the closest to that of firm i's. To be eligible for matching, firm j cannot have split in the 10 months preceding or the 10 months following the split by firm i. Upon matching, we compute (for splitters and matched non-splitters, in each event-window month) the abnormal short interest metric as $ASI_{i,t} = SI_{i,t} - 10^{-1}\sum_{-11}^{-20} SI_{i,t}$, where $SI_{i,t}$ is the number of i's shares in short positions in month t scaled by the number of shares outstanding. Finally, for each splitter/non-splitter pair, we compute the relative short interest, $\Delta_{ij}SI_t$, as the difference between abnormal short interest statistics for every month in the event window. The mean differences are plotted in the figure. Time 0 represents the first post-split announcement collection of short interest.

Table 1.A1. Split decision determinants and propensity score estimation

The table examines split determinants and reports the model used for estimation of the propensity to split. For the analysis of split determinants, we estimate the following logistic regression:

$$Pr\,(split_{i,t} = 1) = \alpha + \beta_1 SI_{i,t-1} + \beta_2 \Delta SI_{i,[t-\kappa;\,t-1]} + \beta_3 PRATIO_{i,t-1} + \beta_4 AR_{i,[t-10;\,t-1]} + \beta_5 \Delta VOLAT_{i,[t-10;\,t-1]} + \beta_6 \Delta ILLIQ_{i,[t-10;\,t-1]} + \beta_7 \Delta INST_{,[t-10;\,t-1]} + \beta_8 NYSED_{i,t-1} + \beta_9 SIXTNTHS_t + \beta_{10} DECIMALS_t + \beta_{11} POSTCRISIS_t + \varepsilon_{i,t},$$

where the binary dependent variable $split_{i,t}$ equals 1 if firm *i* announces a split in month *t* and equals 0 otherwise; $SI_{i,t-1}$ is the short interest; $\Delta SI_{i,[t-\kappa;\,t-1]}$, with $\{\kappa = 10 \text{ or } \kappa = 5\}$ is the average rate of monthly change in short interest during the 10-month (or 5-month) pre-split announcement period; $PRATIO_{i,t-1}$ is firm *i*'s price in the pre-split announcement month divided by the average price for all sample stocks excluding firm *i*; $AR_{i,[t-10;\,t-1]}$ is the mean monthly pre-split BHAR; $\Delta VOLAT_{i,[t-10;\,t-1]}$ is the monthly pre-split change in stock *i*'s volatility, where the monthly volatility is estimated as the average daily (high price – low price)/low price. $\Delta ILLIQ_{i,[t-10;t-1]}$ is the mean monthly pre-split change in stock *i*'s illiquidity computed as the average daily bid-ask spread as in Corwin and Schultz (2012), with negative spreads set equal to zero; $\Delta INST_{i,[t-10;\,t-1]}$, the mean monthly pre-split change in institutional ownership of stock *i*. Institutional ownership is computed as the number of shares in institutional holdings reported via Form 13-F, scaled by the number of shares outstanding; $NYSED_{i,t-1}$ is the NYSE market capitalization decile of firm *i*; $SIXTNTHS_t$ and $DECIMALS_t$ are indicator variables that identify minimum tick size regimes. $POSTCRISIS_t$ is an indicator variable equal to 1 from the year 2008 onward. Reported coefficients represent marginal effects, and *t*-statistics are in parentheses.

	[1]	[2]	[3]
SI		0.009	0.034
		(0.09)	(0.37)
$\Delta SI_{[t-5;\,t-1]}$		0.007	
		(0.69)	
$\Delta SI_{[t-10;\,t-1]}$			-0.022
			(-1.15)
PRATIO	0.153***	0.153***	0.153***
	(45.40)	(43.77)	(43.74)
$AR_{i,[t-10;\,t-1]}$	2.557***	2.425***	2.440***
	(27.32)	(25.41)	(25.34)
$\Delta VOLAT_{i,[t-10;t-1]}$	0.217	0.284*	0.312**
	(1.44)	(1.79)	(1.96)
$\Delta ILLIQ_{i,[t-10;t-1]}$	-0.430***	-0.456***	-0.467***
	(-8.19)	(-8.35)	(-8.54)
$\Delta INST_{i,[t-10;t-1]}$	2.064***	2.058***	2.059***
	(24.20)	(23.47)	(23.20)
NYSED	-0.021***	-0.021***	-0.021***
	(-13.58)	(-13.58)	(-13.70)
SIXTNTHS	-0.062***	-0.063***	-0.064***
	(-6.94)	(-7.10)	(-7.18)
DECIMALS	-0.068***	-0.066***	-0.067***
	(-8.08)	(-7.52)	(-7.54)
POSTCRISIS	-0.354***	-0.352***	-0.356***
	(-26.10)	(-26.48)	(-26.59)
# obs.	1,158,330	1,094,155	1,093,343
pseudo-R^2	0.135	0.139	0.139

*** p<0.01, ** p<0.05, * p<0.1

2. INSIDER AND RETAIL TRADING

2.1 Introduction

Retail investors are traditionally seen as noise traders, with no shortage of studies documenting systematic biases in their trading behavior (e.g., Odean, 1998; Barber and Odean, 2001; Seasholes and Zhu, 2010). More recently however, studies utilizing comprehensive datasets show that retail stock purchases (sales) are followed by abnormal positive (negative) returns, suggesting that retail investors are informed (e.g., Kaniel, Liu, Saar, and Titman, 2012; Kelley and Tetlock, 2013; Boehmer, Jones, Zhang, and Zhang, 2021). In the meantime, a large body of literature shows that despite legal and corporate restrictions, insider traders are informed at both short and long horizons (e.g., Jaffe, 1974; Lakonishok and Lee, 2001; Ben-David, Birru, and Rossi, 2019). What has not gathered a lot of attention, however, is how the trading patterns of these groups are related to each other. Such patterns are useful to study for at least two reasons.

First, since corporate insiders are in fact retail traders, Kaniel et al. (2012) raise the question of whether return predictability associated with retail trades can be attributed to insider trades. If this is true, as Chung (2020) argues, much of the literature on retail investors needs to be reinterpreted in the light of the literature on insiders. Using recent and comprehensive U.S. data., this paper shows that this is not the case. When retail traders purchase more shares than they sell, future returns are positive irrespective of whether corporate insiders traded simultaneously. Relatedly, there is evidence consistent with retail investors learning from publicly disclosed insider trades (Sias and Whidbee, 2010; Stotz, 2012; Boehmer, Sang, and Zhang, 2021), raising the question of how much of the return predictability remains after stripping out these trades. The data show that retail purchases similarly predict future returns during weeks when no recent insider trade has occurred.

A second reason to study the joint trading patterns of insider and retail traders is to better understand their sources of information. Do they trade on distinct and unrelated information, or is there some overlap? In addition to retail traders mimicking insiders, do insiders strategically trade on retail trading activity?

One possibility is that insiders trade in the opposite direction of retail investors to exploit temporary price pressures induced by intense retail trading (Mansi, Peng, Qi, and Shi, 2021). It is well established that insider trades tend to be contrarian (e.g., Lakonishok and Lee, 2001; Piotroski and Roulstone, 2005) however even after controlling for past returns insiders may earn abnormal returns by trading contrary to retail traders specifically. This is because the reversal of retail-induced price movements is stronger than that of simple price changes (Barber and Odean, 2008; Da, Engelberg, and Gao, 2011). Insiders may further buy their stock not just for personal profits but also to signal to the market that the stock is undervalued (Babenko, Tserlukevich, and Vedrashko, 2012). This arguably makes more sense after retail traders are selling the stock, to convince them to stop selling or start (re)purchasing the stock.

This paper sheds new light on the relative importance of these channels by analyzing the returns following instances where insiders and retail traders either trade in the same or the opposite direction. When the dominant channel is insider signaling, positive returns would be expected when insiders buy following retail sales. When insiders exploit retail-induced temporary price pressures, returns should be large when the groups trade in opposite directions, especially after retail purchases (Mansi et al., 2021). On the other hand, when insiders share information sets with retail traders, returns should be large when insiders and retail investors trade in the same direction. In this case, we might expect stronger results for informed trades. It is well known that insider sales contain considerably less price-relevant information relative to insider purchases, making it harder to separate noise from information. In addition, retail sales appear to contain less information relative to retail purchases (Kaniel, Saar, and Titman, 2008; Boehmer, Jones, Zhang, and Zhang, 2021), suggesting that the interactions should be stronger for purchases relative to sales. Consistent with the overlapping information sets channel, the data show that future returns are abnormally large after both insiders and retail investors purchase a security. No interaction is observed for sales, consistent with sales being less information-driven. It is also possible that insider exploitation of retail-induced upward price pressures negates the effects of overlapping information.

Perhaps the most obvious mechanism which would cause overlapping information between the two groups is retail investors mimicking insider trades, as suggested by Boehmer, Sang, and Zhang (2021). Alternatively, they may share private information, or they might be superior at processing public information. The critical feature of these channels that allows tests of their relative importance is that insider mimicking requires that retail investors must know about upcoming or executed insider trades, whereas this is largely irrelevant if they share other information.

Therefore, I test whether there is still an information overlap in settings where retail investors are unlikely to learn from insider trades. The first setting focuses on periods where insider trades were executed but not yet disclosed by the SEC. This may not be sufficient because some retail traders could discover insider trades before they are officially disclosed by the SEC, perhaps through word of mouth. I therefore also study retail trades occurring not only prior to SEC disclosure but also prior to the execution of insider trades. In these settings the data continue to suggest that there is some overlap in information between insiders and retail investors, suggesting that it is not primarily retail investors mimicking insiders that drive these results.

The expected positive interaction between insider and retail trades on returns for the overlapping information sets channel hinges on the notion that the two groups will *only* be more likely to trade in the same direction if an insider trade is informed. When an insider trade is not informed there could not be any information overlap, so there is no reason to believe that retail traders would be trading in the same direction. As a robustness check, therefore, I confirm that the interaction disappears for routine insider trades as classified by Cohen, Malloy, and Pomorksi (2012) and Ali and Hirshleifer (2017). The interaction between retail purchases and opportunistic purchases as classified by either algorithm is similarly strong to the overall universe of insider purchases. All interactions for insider sales remain insignificant, including interactions with opportunistic insider sales.

To gain a deeper understanding of the interaction between retail and insider purchases on future returns, it is worthwhile to examine the relationship at various horizons. Do the effects relate to short-term or long-term information? Do insiders who buy stocks contemporaneously with retail investors earn higher profits? If it is only a short-term phenomenon, this may not be the case given that they need to hand over any profits made on trades closed within 6 months (Cziraki and Gider, 2021). Market-adjusted returns keep increasing in magnitude up to at least a year after insider purchases accompanied by retail purchases, ending the year around 2.7%. In contrast, returns after insider purchases accompanied by retail sales appear to stabilize around 1% after 4 months. This analysis suggests that the positive overlapping information when both parties buy is of a longer-term nature, though it does not control for known stock return predictors.

Finally, portfolio return analyses corroborate the main results. A portfolio that mimics insider trades in the past week where retail traders agree (i.e., long (short) stocks bought by insiders on days with retail buying (selling)) generates at least 0.43% weekly excess returns, while the corresponding pure insider mimicking portfolio earns 0.33% and a similar pure retail mimicking portfolio generates 0.07% per week. The fourth portfolio that is examined mimics insider trades where retail investors disagree (i.e., long (short) stocks bought by insiders in the past week on days with retail selling (buying)), which earns at least 0.22% per week. All results are stronger on an equal-weighted basis relative to value-weighted, consistent with small stocks being less efficient and thus allowing for greater profits from informed trading.

To provide a more robust answer to the question of whether the retail-insider interaction relates to short- or long-term information, holding periods are extended from one week to 4, 12, and 52 weeks. Consistent with most of the information contained in insider (and retail) trades being impounded in prices within a few weeks, alphas for all portfolios decline as the horizon is extended (with alphas continuing to be measured on a weekly basis). No alpha is observed for the retail mimicking portfolio on a 12-week basis, and value-weighted alphas become insignificant for the portfolio that mimics insiders where retail traders disagree. With a 52-week holding period, only the portfolio that mimics insiders where retail traders agree retains significant alpha, albeit only on an equal-weighted basis. These results support the notion that the information overlap between insiders and retail investors is of at least a medium-term nature.

Overall, this paper contributes to the literature on retail trading and insider trading by showing that the return predictability associated with retail trades cannot be attributed to insiders being a subset of retail traders, nor retail traders mimicking insiders. It is still possible that it is driven by insiders trading stocks other than their own, which is plausible as they earn abnormal returns trading in same-industry stocks (Ben-David et al., 2019). These effects are not controlled for in this study due to a lack of data. I further document an interaction between insider and retail purchases that predicts particularly large positive returns. These results are most consistent with retail traders and insiders sharing positive price-relevant information that is of at least a medium-term nature. It could very well be that there are other interactions between insiders and retail traders, such as insiders exploiting temporary retail-induced price pressures or retail traders selectively mimicking high-value insider trades. However, these other channels cannot by themselves explain why returns are especially large after insiders and retail investors purchase around the same time.

The next section contains a literature review and discusses alternative explanations for the main findings. The data are discussed in section 2.3, and the methods and main empirical results in section 2.4. The final section concludes.

2.2 Literature Review

This paper is not the first to study interactions between retail investors and insiders. Sias and Whidbee (2010) use U.S. data from a brokerage firm from 1991 to 1996 to show that retail investors are net buyers (sellers) in the 60 (20) trading days surrounding insider purchases (sales). Of particular interest is their finding that individual investors appear to start trading in the same direction as upcoming insider trades up to 10 days in advance, consistent with these groups sharing information beyond retail investors mimicking insiders. My study utilizes more recent and comprehensive data and applies more specific tests to answer this question.

Stotz (2012) uses German data from a stockbroker from 2008 and 2009 to show that retail traders copy insider trades up to 20 trading days after insider trades are publicly disclosed. Boehmer, Sang, and Zhang (2021) study retail mimicking of insider trades where retail trades are inferred using the algorithm of Boehmer, Jones, Zhang, and Zhang (2021) applied to U.S. TAQ data from 2010 through 2018. These papers are complementary to mine in that they focus on the patterns of retail trading following insider trading whereas I examine how interactions among these groups may explain the overall return predictability associated with retail trades, and what drives the overall positive interaction effect between insider and retail purchases. Boehmer, Sang, and Zhang (2021) also find such a positive interaction and state that it "suggests that retail investors learn from the opportunistic insider purchases". While this may be true, I find that the interaction is still there under circumstances where retail investors did not learn from insider trades, suggesting that these groups also share information through other means.

Chung (2020) also studies the interaction between retail and insider trades directly and argues that retail trading contains no incremental information about future stock returns for stocks with insider trading. His retail trading data originate from the NYSE which captures a much smaller share of overall retail trading, may be less representative of the typical retail trader, and may contain relatively more insiders than the sample studied in this paper. I identify retail trades using TAQ data and apply the algorithm of Boehmer, Jones, Zhang, and Zhang (2021), which is explained and contrasted to NYSE data in section 3. The more insider trades are contained in the retail data, the less likely it is to find incremental information in retail trades relative to insider trades. This could help explain why Chung (2020) does not find a statistically significant positive interaction.

Mansi et al. (2021) show that there are more opportunistic insider sales (purchases) when Google's Search Volume Index (SVI), a proxy for retail investor attention, is high (low). Building on the finding that a high SVI is positively related to transient increases in stock prices (Da, Engelberg, and Gao, 2011), they argue that insiders take advantage of temporary price pressures induced by retail investor attention. This line of reasoning seems plausible, especially given that Kelley and Tetlock (2017) find that retail shorts are particularly informed when other retail investors are buying. Given that shorting is expensive for individual traders and requires a certain sophistication, such trades may be particularly informed. Since corporate insiders are uniquely informed, it might not be surprising to find a similar exploitation of retail trading by insiders. However, this is not necessarily the case, since insider trades are monitored, restricted, and scrutinized, whereas retail short sellers typically remain anonymous and experience relatively few trading restrictions.

Mansi et al. (2021) do not, however, relate insider trading directly to retail trading activity, relying instead on indirect evidence. My data facilitate studying the direct link between insider and retail trades and it is not found that insider sales are more profitable during periods of net retail buying. However, if this channel is stronger for insider sales than for insider purchases, as in Mansi et al. (2021), this may help explain why the data does not reveal a negative interaction between retail sales and insider sales.

2.3 Data and sample

Retail trades are identified using TAQ data from 2010-2015 and applying the algorithm of Boehmer, Jones, Zhang, and Zhang (2021). This algorithm builds on the fact that many retail orders are executed internally by brokers or by wholesalers (market makers that combine order flow from multiple sources) who offer better prices than publicly posted bids and asks. For example, if the current best ask is $10.00 and a retail investor places a market order to buy, a broker can route this order to a wholesaler that executes the order at $9.999. These price improvements are usually smaller than $0.005, allowing retail purchases to be distinguished from sales based on the execution price. Most price-improved retail transactions take place off-exchange but are reported to a FINRA Trade Reporting Facility (TRF). Therefore, the algorithm works by classifying transactions reported to a FINRA TRF with prices ending in the interval (0,0.4) cents as retail sales and those in the interval (0.6,1) as retail purchases.

Following Boehmer, Jones, Zhang, and Zhang (2021), the sample period is restricted to 2010-2015 even though data on sub-penny price improvements are available from 2005. This is because up until 2010 there is an upward trend of the number of sub-penny trades, likely because the practice of offering price improvement for retail trades in sub pennies was increasingly adopted. The tick size pilot program came into effect in 2016, limiting price improvements for many stocks.

Not all market orders by retail traders are captured in the data. While many retail trades are executed by wholesalers who pay for retail order flow and offer better prices, resulting in sub-penny execution prices, some retail trades are executed on the exchanges directly (Battalio, Corwin, and Jennings, 2016). Moreover, if any retail trades are executed at prices that end in the [0.4, 0.6] interval, they cannot be distinguished from institutional trades which can also be executed at midpoint dark pools. Nevertheless, the total retail dollar volume constitutes 6.85% of total volume in the sample, a notably larger percentage than studies using other U.S. datasets. To the best of my knowledge, the most comprehensive U.S. retail dataset other than this is used by Kelley and Tetlock (2013), whose retail data comprise 2.3% of total dollar volume traded.

Besides capturing a relatively large chunk of retail trades, this dataset is also attractive relative to the NYSE data studied by Chung (2020) because it seems likely that insider trades constitute a smaller portion of retail trades. First, insider traders tend to be informed and so brokers and wholesalers are incentivized to avoid taking the other side. As retail investors need to report to brokers if they are insiders, brokers know which clients are insiders and could opt not to offer price improvement to these trades. Second, insider trades are often large and wholesalers have the right to reject large orders[6]. In lieu of being offered price improvement, insider orders may be more likely to be placed directly on the NYSE and flagged as retail trades. Third, many firms adopt

[6] https://www.sec.gov/Archives/edgar/data/316709/000031670904000037/exh10_262.txt, section II (a) (i).

special insider trading policies which may include restrictions on which broker to use[7] (which may execute trades differently from typical retail brokers). Fourth, the daily dollar volume of at least half of the insider trades is greater than the combined retail trading volume on the same stock, indicating that most insider trades are at least not fully identified as retail trades.

The sample is constructed by narrowing over 2 million individual insider trades down to about 625 thousand based on various standard filters described in Table 2.1. The sample is further reduced by aggregating multiple insider trades on the same stock and day to one observation, leaving just over 250 thousand observations. Stock returns and characteristics from CRSP, book-to-market values from Compustat, and quarterly earnings announcements are added from both Compustat and I/B/E/S.

The earnings announcements are used to identify opportunistic trades according to the algorithm of Ali and Hirshleifer (2017) described below. Observations with timestamps at or after 4 p.m. are assumed to correspond to after-market hours because almost all earnings during the sample period are announced outside trading hours (Gregoire and Martineau, 2021). Only I/B/E/S data contain timestamps, resulting in 18% of the merged announcements not having timestamps. To avoid a potential forward-looking bias, these announcements are assumed to have taken place before market hours. For the same reason, the earlier date of the two sources is used if the earnings dates from Compustat and I/B/E/S conflict (less than 1% of the data).

Opportunistic and routine trades are identified using the algorithms of Cohen et al. (2012) and Ali and Hirshleifer (2017). Cohen et al. (2012) classify opportunistic trades as those made by insiders that have made at least 1 trade in three consecutive prior years but are not categorized as routine. Routine trades are defined as those executed by insiders that have traded in the same

[7] https://corpgov.law.harvard.edu/2016/03/24/a-guide-to-rule-10b5-1-plans/

calendar month for three consecutive years before the evaluated trades. Insiders that have not traded in at least 3 consecutive prior years are not categorized.

Ali and Hirshleifer (2017) use the profitability of prior insider trades in the 21 trading days prior to quarterly earnings announcements (QEAs) to distinguish between opportunistic and routine trades. Despite many firms having blackout periods prior to QEAs, these trades comprise about 15% of total insider trades in my sample. Pre-QEA insider trades happen because many firms allow exceptions upon request, not all firms implement blackout periods, and sometimes trading restrictions are violated (Ali and Hirshleifer, 2017). Profitability is captured by the 5-day stock return centered around QEAs. At the start of each calendar year, the insiders in the most profitable quintile are deemed opportunistic, and those in the lower three quintiles are deemed routine traders. Insiders that have not traded in pre-QEA periods and those in the fourth quintile are not categorized.

Sample characteristics are summarized in Table 2.2. To minimize the impact of outliers, variables are winsorized at 1% and 99% where appropriate. Not surprisingly, there are no insider trades during most days in the sample, and on insider trading days there is typically just one insider trade. The average number of retail trades is 233 per day per stock, whereas the daily dollar volumes of insiders and retail investors are more comparable at about 1 and 3 million dollars, respectively. Consistent with previous literature, insider sales are more prevalent and substantially larger than purchases. Retail trading volume captures 6.85% (= (1.47+1.47)/42.91) of total dollar trading volume on average, and 3.81% (= (118.24 + 115.17)/6123) of total trades. The larger share of volume is consistent with Boehmer, Jones, Zhang, and Zhang (2021) in that the rise of algorithmic trading caused the average retail trade to become larger than other trades.

All types of trading volumes have much larger averages than medians, are highly skewed and leptokurtic. Therefore, for the main analysis, I simplify retail trading activity by defining a dummy variable *RetailBuy* which is equal to one if retail purchases are greater in dollar volume than retail sales and zero otherwise. Similarly, *InsiderBuy* and *InsiderSell* are dummy variables equal to one if insiders are net buying or selling. A richer measure of the directionality of retail trading is retail order imbalance (*ROIB*) computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume) where volumes are in number of shares traded. An equivalent insider order imbalance is not evaluated because there are relatively few days where insiders are both buying and selling. Collapsing insider trading activity into one variable also has the disadvantage of forcing insider purchases to have the exact opposite effect as insider sales. Using indicator variables to capture both retail and insider trading activity further has the advantage of easier interpretation relative to using order imbalances as the retail and insider order imbalance distributions are very different.

To further understand the data better, correlations between these dummy variables and various firm characteristics are shown in Table 2.3. If insiders and retail investors share information, they are probably more likely to trade in the same direction, especially for purchases as insider purchases tend to be informed whereas insider sales may not be. Indeed, the correlation between retail and insider purchases is significantly positive. However, it is small (0.01) in part because on most days there are no insider days – when evaluated only over the days where there are insider trades, the correlation becomes 0.05. A smaller and only weakly significant negative correlation is found between insider sales and retail purchases. The correlation between the insider trading dummy variables is -0.02 (instead of -1) because on most days there are no insider trades and both variables equal zero. Most correlations between firm characteristics and trading activity variables are significant and slightly larger in absolute value, motivating the inclusion of these variables into later regression analyses.

Comparing insider purchases and sales to retail trading activity, Table 2.4 further shows that retail investors and insiders tend to trade in the same direction: 54.6% of all insider trades are in the same direction as the average retail trade on the same day. When both insiders and retail traders buy, stock returns in the following week are about 0.28% (= 1.17% - 0.91%) higher compared to when insiders buy and retail traders sell. Analogous returns in the following year are substantially larger at 2.33% (2.91% - 0.58%). For sales, there does not appear to be an analogous relation, and average returns are close to zero.

The table also contains statistics on opportunistic and routine insider trades. Providing out-of-sample evidence of the findings of Cohen et al. (2012) and Ali and Hirshleifer (2017), returns are higher (lower) for opportunistic insider purchases (sales) with either classification algorithm. The Cohen et al. (2012) algorithm appears to be slightly better able to capture outperforming purchases whereas Ali and Hirshleifer's (2017) classification is relatively good at capturing outperforming opportunistic sales. Panel B reports next-week returns by retail order imbalance quintiles. There is a largely monotonic relationship across the quintiles, where the outperformance associated with retail trades is concentrated for the largest quintile corresponding to retail purchases. Notably, the difference between the 4^{th} and 5^{th} quintiles is 0.25% for insider purchases, more than double the 0.10% difference for all observations.

2.4. Empirical results

This section contains the empirical results and briefly discusses their implications. Section 2.4.1 presents the results on whether the return predictability associated with retail trades can be attributed to insider trades, section 2.4.2 presents the results on interactions between insider and retail trading using weekly returns, and section 2.4.3 covers returns to portfolios constructed based on publicly available information, including longer-term return predictability up to a year after portfolio formation.

2.4.1 Can the return predictability associated with retail trades be attributed to insider trades?

If the insider subset of retail traders is responsible for the documented retail trade return predictability, there should be no predictive power for weeks during which no insiders traded. To test this, I follow Boehmer, Jones, Zhang, and Zhang (2021) and predict future returns by estimating Fama-MacBeth regressions. In the first stage, weekly market-adjusted stock returns with overlapping daily frequencies are regressed on a dummy variable that is equal to one when retail traders were net buying in the past week and control variables:

$$Ret_{i,w} = \alpha + \beta_1 RetailBuy_{i,w-1} + \beta_2 Controls_{i,w} + \varepsilon_{i,w}, \quad (2.1)$$

where i refers to stock and w to the week (= 5 consecutive trading days), and the control variables are past returns at various horizons (1 week, 1 month, 6 months) and the firm characteristics defined in Table 2.3: turnover, volatility, size, and book-to-market ratio. At the second stage, statistical inference is conducted using the time series of coefficients. To account for serial correlation in the coefficients induced by using overlapping daily frequency data for weekly returns and retail trading variables, standard errors are adjusted using Newey-West (1987) with five lags.

The first specification in Table 2.5 is intended to see if the data corroborates the evidence that retail trades predict future returns. The sample is unconstrained and only $RetailBuy_{i,w-1}$ is included as an independent variable. The results show that weekly market-adjusted returns are a highly significant 141 bps higher when retail traders were net purchasing last week relative to when they are not. To get a sense of what impact the insider subset of retail traders might have on this result, the analysis is repeated for the sample where no insiders traded in the previous week. These results are reported in the second specification and show that retail investors retain almost the same predictive power during non-insider trading weeks.

Excluding observations where insiders traded during the prior week is sufficient to exclude potential effects of insiders being a subset of retail investors, but it might not be if one wants to answer the question of whether retail traders *mimicking* insiders may drive the results. There could be a delay between when insiders trade and when retail traders copy those trades. Most insider trades are disclosed to the SEC and become public within 2 business days as mandated by the SEC, and it’s not obvious how quickly retail traders would find out and copy them. To be on the conservative side, the third specification removes observations where at least one insider traded in the previous four weeks. Retail purchases continue to predict next week’s returns with only a negligibly changed coefficient. The fourth specification adds control variables to the third (most conservative) specification, which does not reduce the predictive power associated with retail purchases. In summary, retail purchases predict future returns, and this is not driven by insiders being a subset of retail investors nor by the latter group copying insiders.

2.4.2 Interactions among Retail and Insider Traders

This section focuses on the question of whether there is any overlapping information between retail investors and insiders. To start getting an answer to this question, model (2.1) is modified to include insider trading dummy variables $InsiderBuy_{i,w-1}$ and $InsiderSell_{i,w-1}$ that equal one during weeks when insiders were net buying and selling, respectively (both being zero when there are no insider trades or when insider purchases exactly equal insider sales). In addition, interactions between $RetailBuy_{i,w-1}$ and the insider trading variables are included.

The results are in Table 2.6, where the first specification confirms that both insider trading dummy variables predict future weekly returns. As expected, insider purchases predict returns more strongly than insider sales and retail purchases. The second specification adds $RetailBuy_{i,w-1}$ and its estimated coefficient is not far off from what is observed in Table 2.5, while it also does not materially impact the coefficients on either of the insider trading dummy variables. The third specification adds the two interactions and uncovers a meaningful positive interaction between insider and retail purchases. A weaker marginally negative interaction is found between insider sales and retail purchases, but the effect disappears when adding control variables in the final specification.

The positive interaction between insider and retail purchases suggests these groups share positive information, but it's not yet clear if this could be explained by retail traders mimicking insider trades. Three tests are conducted in Table 2.7 to answer this question. The first removes any observations where insider trades could have been copied by retail investors after trades were disclosed to the SEC. More precisely, observations are excluded where at least one of the insider trades in week w-1 was disclosed to the SEC within the same week. This filter removes just over half of the observations that contain insider trades as the median time to disclosure is 2 trading days resulting from the 2-day SEC disclosure requirement. The results are in the first specification and show that all coefficients remain of similar magnitudes and statistical significance.

It is, however, in principle possible for retail investors to discover insider trades before SEC disclosure (through say word of mouth). As a second test, therefore, the second specification evaluates retail trading activity one week *before* insider trades are evaluated. Consistent with the return predictability associated with retail trades being concentrated in the first week after such trades (Boehmer, Jones, Zhang, and Zhang), the coefficient on retail purchases decreases as the time between these trades and the return increases. The interaction between insider and retail purchases similarly decreases (although less) and remains significant at the 99% confidence level.

One could argue that these results might still be driven by retail investors copying insider trades because of positive serial correlation in insider purchases. For instance, if insiders started buying in week *w*-2 and continue to buy into week *w*-1, then if retail traders copied the first insider purchase this might still result in a positive coefficient on the interaction. To address this concern, the third specification takes the second specification and further removes all observations where any insider trade was made in weeks [*w*-5,*w*-2]. The interaction effect remains strong. In conclusion, insider mimicking by retail traders does not drive the positive interaction effect between insider and retail purchases.

If the results are driven by overlapping positive information, they might be stronger for subsets of insider trades that are informed, and certainly, the positive interaction should disappear for subsets that are not informed. Using the algorithms of Cohen et al. (2012) and Ali and Hirshleifer (2017), Table 2.8 reports returns to routine and opportunistic insider trades as well as their interactions with retail purchases. The first and third specifications present the non-interacted results and provide out-of-sample evidence of the two algorithms. Opportunistic purchases (sales) are followed by positive (negative) returns. Routine purchases are followed by meaningfully smaller, marginally significant returns and routine sales are not significantly related to future returns. The second and fourth specifications add the interaction terms and confirm that the only significant interaction is between opportunistic insider purchases and retail purchases.

2.4.3 Longer-term and Portfolio Results

So far, all tests have been performed on weekly returns. This section first examines longer-term returns by plotting market-adjusted returns up to a year after 8 conditions based on retail and insider trading activity. The left chart in Figure 2.1 shows that average cumulative market-adjusted returns to retail purchases remain relatively flat until 5 months after, when returns reverse and slightly drop below zero. This pattern is consistent with the reversal of returns in Barrot, Kaniel, and Sraer (2016). In contrast, returns to insider purchases, and especially those that coincided with retail purchases, continue to outperform the market up to a year after initiation. Market-adjusted returns after instances with both insider and retail purchases steadily rise from 0.8% one week after initiation to 2.7%. In contrast, the returns to insider purchases that coincided with net retail sales stabilize at 1% around 4 months, with negligible net changes in the following 8 months. This suggests that the shared information between retail investors and insider is of a long-term nature, whereas the typical retail trade is not based on long-term information. The right chart focuses on retail and insider sales, and these trades continue to not have much of a relationship to future returns.

While interesting and indicative of market dynamics, the results in Figure 2.1 do not control for known stock return predictors. To address this, portfolios are formed based on insider and retail trades with different holding periods, and abnormal returns are constructed using both the CAPM and the Fama and French (1993) 3-factor model augmented with Carhart's (1997) momentum factor. More specifically, Table 2.9 reports both equal- and value-weighted weekly alphas to four portfolios. The first portfolio copies retail investors and is long (short) stocks that have been net bought (sold) by retail investors in the previous week. All alphas are positive and significant when the holding period is 1 week, consistent with the results in Table 2.5. Most of the alpha derives from small stocks as value-weighted alphas are almost halved in magnitude. All alphas become insignificant when the holding period is extended to 4 weeks or beyond, consistent with Figure 2.1.

The second portfolio similarly focuses on only one set of participants by copying insider trades made in the prior week. All alphas are positive and significant up to 4 weeks, whereas value-weighted alphas start to lose some significance for 12-week holding periods. Despite the continuation of positive returns up to a year after initiation, alphas become largely insignificant when the holding period is extended to 52 weeks.

The third portfolio mimics insider trades in the past week where retail traders agree (i.e., long (short) stocks bought by insiders on days with retail buying (selling)). All alphas are larger than the individual alphas on retail and insider mimicking portfolios combined, corroborating the positive interaction found in earlier tests. Value-weighted alphas retain statistical significance at 12 weeks but lose it at 52 weeks. Equal-weighted alphas retain significance even at 52 weeks.

The fourth portfolio mimics insider trades where retail investors disagree (i.e., long (short) stocks bought by insiders in the past week on days with retail selling (buying)) and earns substantially smaller abnormal returns relative to the pure insider mimicking portfolio. Its alphas start to lose some significance at 4-week holding periods, value-weighted alphas are insignificant at 12 weeks, and all alphas are insignificant at 52 weeks. Taken together, these results suggest that the nature of the information that is shared between insiders and retail investors is of at least a medium-term nature.

2.5. Conclusions

This paper contributes to the continuing debate on the informational role of retail investors in financial markets. Using the algorithm of Boehmer, Jones, Zhang, and Zhang (2021) applied to U.S. TAQ data to identify retail trades, this paper shows that net buying by retail investors predicts next week's stock returns even when no insider traded in the same week or the 3 weeks before that. This means that the documented return predictability associated with retail trades cannot be attributed to insiders potentially being a subset of retail investors nor to retail investors copying insider trades. The change in return predictability between the overall sample and the sample that excludes observations with insider trades in the previous 4 weeks is negligible (0.141% higher returns vs. 0.142%, respectively). Insiders trading same-industry stocks other than their own (as in Ben-David et al., 2019) could still play a role as data limitations do not allow this study to control for these.

In addition, there exists an interaction between insider and retail purchases that results in positive abnormal stock returns up to at least 12 weeks. These results are most consistent with retail traders and insiders sharing positive price-relevant information. No interactions are observed for insider sales nor routine insider purchases, consistent with these insider trades not containing much information. Retail traders copying high-value insider trades as in Boehmer, Sang, and Zhang (2021) and insiders exploiting temporary retail-induced price pressures as in Mansi et al. (2021) do not explain the results.

2.6. References

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Table 2.1. Sample Selection

This table describes the sample selection procedure from the full sample of insider trades available through Thomson Reuters (TR) for the years 2010 to 2015, to the final sample that is restricted to insider trades that are (i) executed as open market purchases or sales; (ii) sufficiently reliable (Thomson Reuters' cleansing indicators R, H, L, C, and Y, as in e.g. Alldredge and Cicero (2015)); (iii) successfully merged with CRSP; (iv) involve common ordinary shares (share codes 10 and 11); and (v-vii) sufficiently reliable and complete (transaction prices do not exceed more than 10% outside the CRSP-reported high-low range; the daily volume of insider trades does not exceed the total CRSP-reported volume; and the CRSP data contains prices, returns, and volumes); The sample is further constructed by (viii) aggregating insider trades to the stock-day level; (ix) adding CRSP data from 2009 and 2016 (to be able to compute lagged and future variables); (x) filtering to 2010-2015, stock-years in which there is at least 1 retail trade identified on an insider trade day, and stock-years where the stock price remained above $1.

# stocks	# obs	Sample selection criteria
9,512	2,224,336	Full TR sample 2010-2015
8,048	774,893	Keep only open market purchases and sales
7,623	760,466	Keep only cleansing indicators R, H, L, C, and Y
5,973	704,547	Merge with CRSP
4,818	643,952	Keep only share codes 10 and 11
4,804	640,053	Keep only transaction prices within [0.9*daily CRSP low,1.1*daily CRSP high]
4,752	627,641	Keep only aggregate insider trades ≤ CRSP reported daily volume
4,711	625,159	Keep only non-missing CRSP price/return/volume data
4,711	255,651	Aggregate individual insider trades to stock-day observations
4,667	7,195,952	Add CRSP data from 2009-2016 for all stocks retained & merge with retail TAQ data
4,476	4,459,334	Keep only 2010-2015, stock-years in which there is at least 1 retail trade identified on an insider trade day, and stock-years where the stock price remained above $1

Table 2.2. Sample Characteristics

This table shows the number of observations, mean, and the 25th, 50th, and 75th percentiles of a selection of variables used in the remainder of the paper. The frequency is daily. Retail purchases and sales are identified as in Boehmer, Jones, Zhang, and Zhang (2021): Transactions reported to the FINRA TRF with prices ending in the interval (0,0.4) cents are identified as retail sales, and those in the interval (0.6,1) as retail purchases. *ROIB* is retail order imbalance computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume) where volumes are in dollars. *TURNOVER* is the mean number of shares traded in a month, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %. *VOLAT* is the volatility estimated as the standard deviation of daily returns per month. *BM* is the book-to-market ratio, computed at the end of the previous calendar year.

	# obs	*mean*	*p25*	*median*	*p75*
Retail Purchases (#, daily)	4,398,791	118.24	5	24	86
Retail Sales (#, daily)	4,398,791	115.17	6	26	89
Retail Purchases ($M, daily)	4,398,791	1.47	0.01	0.10	0.53
Retail Sales ($M, daily)	4,398,791	1.47	0.02	0.11	0.55
ROIB	4,185,447	-0.03	-0.28	-0.02	0.21
Insider Purchases (#, daily)	52,529	2.06	1	1	2
Insider Sales (#, daily)	194,728	2.55	1	1	2
Insider Purchases ($M, daily)	52,529	0.56	0.01	0.03	0.12
Insider Sales ($M, daily)	194,728	1.27	0.08	0.26	0.86
Volume (#trades)	4,398,791	6,123	319	1,596	5,801
Volume ($M)	4,398,791	42.91	0.57	4.37	25.61
TURNOVER	4,446,519	17.49	6.27	12.53	22.07
VOLAT	4,444,767	2.38	1.39	2.01	2.95
BM	4,361,653	0.65	0.29	0.54	0.89

Table 2.3. Correlations.

This table shows correlations for the main sample used in subsequent tests. $RetailBuy_{i,t}$ is an indicator variable that equals one when retail buy dollar volume is greater than retail sell dollar volume on day *t* for security *i*, and zero otherwise; $InsiderBuy_{i,t}$ is an indicator variable equal to one when insiders were net buying on day *t* and zero otherwise; $InsiderSell_{i,t}$ is an indicator variable equal to one when insiders were net selling on day *t* and zero otherwise. $VOLAT_{i,m-1}$ is the volatility estimated as the standard deviation of daily returns over the previous month. $TURN_{i,m-1}$ is the turnover computed as the mean number of shares traded in the previous month, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %. $BM_{i,y-1}$ is the book-to-market ratio, computed at the end of the previous calendar year. $SIZE_{i,y-1}$ is defined as the log of the market capitalization of the previous calendar year.

	RetailBuy	*InsiderBuy*	*InsiderSell*	*TURN*	*VOLAT*	*BM*
$RetailBuy_{i,t}$	1.00					
$InsiderBuy_{i,t}$	0.01***	1.00				
$InsiderSell_{i,t}$	-0.00*	-0.02***	1.00			
$TURN_{i,m-1}$	0.06***	-0.03***	0.03***	1.00		
$VOLAT_{i,m-1}$	-0.00	0.02***	-0.03***	0.34***	1.00	
$BM_{i,y-1}$	-0.06***	0.03***	-0.06***	-0.18***	0.03***	1.00
$SIZE_{i,y-1}$	0.07***	-0.05***	0.08***	0.26***	-0.35***	-0.30***

*** p<0.01, ** p<0.05, * p<0.1

Table 2.4. Insider and Retail Trade Characteristics

This table shows the number of insider trades and corresponding average market-adjusted stock returns for various segments. Panel A reports the number of insider purchases and sales along with subsequent weekly and annual market-adjusted returns. These statistics are shown for (i) all insider trades, (ii and iii) trades where retail order imbalance is positive or negative, (iv – vii) opportunistic and routine trades, using the definitions of opportunistic and routine trades of Cohen, Malloy, and Pomorski (2012, henceforth CMP) and Ali and Hirshleifer (2017, henceforth AH). CMP define routine trades as those executed by insiders that have traded in the same calendar month for three consecutive years before the evaluated trades. Opportunistic trades are those made by insiders that have made at least 1 trade in three consecutive prior years but are not categorized as routine. Insiders that have not traded in at least 3 consecutive prior years are not categorized. AH use the profitability of prior insider trades in the 21 trading days prior to quarterly earnings announcements (QEAs) to distinguish between opportunistic and routine trades. Profitability is captured by the 5-day stock return centered around QEAs. At the start of each calendar year, the insiders in the most profitable quintile are deemed opportunistic, and the lower three quintiles are deemed routine traders. Insiders that have not traded in pre-QEA periods and those in the fourth quintile are not categorized. Panel B reports subsequent weekly returns by retail order imbalance quintiles, for (i) all observations, (ii) days with insider purchases only, and (iii) days with insider sales only. Retail order imbalance (OIB) is defined as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume) where volumes are in dollars.

Panel A: Number of insider trades & short and long term returns

	Insider # trades		*[1,5] returns*		*[1,252] returns*	
	purchases	*Sales*	*purchases*	*sales*	*Purchases*	*Sales*
All	47,283	191,162	1.05%	-0.21%	1.86%	-0.48%
Retail buy (OIB > 0)	24,204	85,098	1.17%	-0.19%	2.91%	-0.53%
Retail sell (OIB < 0)	23,079	106,064	0.91%	-0.23%	0.58%	-0.43%
Opportunistic (CMP)	8,513	48,021	1.27%	-0.26%	3.23%	-1.50%
Routine (CMP)	6,291	30,489	0.10%	-0.05%	0.18%	0.07%
Opportunistic (AH)	3,108	16,707	1.13%	-0.43%	3.18%	-2.51%
Routine (AH)	9,323	49,956	0.42%	0.02%	1.13%	-0.67%

Panel B: [1,5] returns by retail order imbalance quintiles

	Retail OIB Quintile				
	1 (low)	*2*	*3*	*4*	*5 (high)*
All	-0.04%	-0.04%	-0.04%	0.01%	0.11%
Insider Buy	1.36%	1.44%	1.46%	1.47%	1.72%
Insider Sell	-0.27%	-0.19%	-0.19%	-0.24%	-0.13%

Table 2.5. Return Predictability

This table reports estimation results on whether retail trades retain return predictability after controlling for trading by corporate insiders. Fama-MacBeth regressions are estimated where the dependent variable is weekly market-adjusted returns. The independent variables include *RetailBuy(w-1),* a lagged weekly indicator variable that equals one when retail buy dollar volume is greater than retail sell dollar volume; *Ret(w-1), Ret(m-1),* and *Ret(m-7,m-2),* lagged weekly, monthly, and previous 6-month returns, respectively; *TURNOVER(m-1),* lagged monthly turnover computed as the mean number of shares traded in month m-1, divided by the number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %; *VOLAT(m-1)* is the lagged monthly volatility estimated as the standard deviation of daily returns per month; *SIZE(y-1)* is the log market cap at the end of the previous calendar year; *BM(y-1)* is the log of the book-to-market ratio, computed at the end of the previous calendar year. The first specification forms the base; the second specification limits the sample to when there were no insider trades in week *w-1*; the third specification limits the sample to when there were no insider trades between weeks *w-4* and *w-1*; and the final specification adds other control variables to the sample limited to no insider trades between weeks *w-4* and *w-1*. Standard errors are adjusted using Newey-West with 5 lags and t-statistics are in parentheses.

	base	*No insiders w-1*	*No Insiders w-4 to w-1*	*No Insiders w-4 to w-1*
RetailBuy(w-1)	0.141***	0.140***	0.142***	0.151***
	(12.15)	(11.28)	(8.60)	(9.97)
Ret(w-1)				-0.058***
				(-12.96)
Ret(m-1)				0.032***
				(7.85)
Ret(m-7,m-2)				0.002
				(1.37)
TURNOVER(m-1)				-0.003***
				(-3.30)
VOLAT(m-1)				0.023
				(1.08)
BM(y-1)				-0.035
				(-0.97)
SIZE(y-1)				-0.013
				(-1.09)
Constant	-0.064	-0.071	-0.082	-0.114
	(-0.50)	(-0.57)	(-0.61)	(-0.89)
Observations	4,378,235	3,636,845	1,943,459	1,792,249
R-squared	0.004	0.004	0.004	0.068

*** p<0.01, ** p<0.05, * p<0.1

Table 2.6. Interactions Retail and Insider Trades.
This table reports estimation results on whether retail purchases retain predictive power when insiders trade. Fama-MacBeth regressions are estimated where the dependent variable is weekly market-adjusted returns. The independent variables include *RetailBuy(w-1)*, a lagged weekly indicator variable that equals one when retail buy dollar volume is greater than retail sell dollar volume; *InsiderBuy(w-1)*, an indicator variable equal to one when insiders were net buying in the past week and zero otherwise; *InsiderSell(w-1)*, an indicator variable equal to one when insiders were net selling in the past week and zero otherwise; interaction terms *InsiderBuy(w-1)×RetailBuy(w-1)*, and *InsiderSell(w-1)×RetailBuy(w-1);* and various control variables: *Ret(w-1)*, *Ret(m-1)*, and *Ret(m-7,m-2)* are lagged weekly, monthly, and previous 6-month returns, respectively; *TURNOVER(m-1)* is lagged monthly turnover computed as the mean number of shares traded in month m-1, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %; *VOLAT(m-1)* is the lagged monthly volatility estimated as the standard deviation of daily returns per month; *SIZE(y-1)* is the log market cap at the end of the previous calendar year; *BM(y-1)* is the log of the book-to-market ratio, computed at the end of the previous calendar year. Standard errors are adjusted using Newey-West with 5 lags and t-statistics are in parentheses.

	(1)	(2)	(3)	(4)
RetailBuy(w-1)		0.134***	0.132***	0.134***
		(11.61)	(11.21)	(12.50)
InsiderBuy(w-1)	0.582***	0.581***	0.484***	0.494***
	(16.25)	(16.09)	(11.62)	(14.00)
InsiderSell(w-1)	-0.145***	-0.145***	-0.130***	-0.101***
	(-5.74)	(-5.74)	(-3.93)	(-5.03)
InsiderBuy(w-1)×RetailBuy(w-1)			0.194***	0.149***
			(3.91)	(3.55)
InsiderSell(w-1)×RetailBuy(w-1)			-0.035*	-0.031
			(-1.70)	(-1.61)
Controls	NO	NO	NO	YES
Constant	-0.004	-0.067	-0.070	-0.081
	(-0.03)	(-0.53)	(-0.58)	(-0.61)
Observations	4,378,235	4,378,235	4,378,235	3,871,675
R-squared	0.010	0.011	0.011	0.063

*** p<0.01, ** p<0.05, * p<0.1

Table 2.7. Interaction due to mimicking?

This table reports estimation results on whether retail mimicking of insider trades can explain the interaction between insider and retail trades. Fama-MacBeth regressions are estimated where the dependent variable is weekly returns for the sample where there was at least one insider trade in the prior week. The independent variables include *InsiderBuy(w-1)*, an indicator variable equal to one when insiders were net buying in the past week and zero otherwise; *RetailBuy(w-x)*, a lagged weekly indicator variable that equals one when retail buy dollar volume is greater than retail sell dollar volume; and the control variables are defined as in Table 2.5. The first specification excludes observations where insider trades were disclosed to the SEC within the prior week; the second specification measures *RetailBuy* one week prior to the week where *InsiderBuy* is measured; the third specification further removes observations where insider trades were preceded by at least one other insider trade in the past 20 trading days. Standard errors are adjusted using Newey-West with 5 lags and t-statistics are in parentheses.

	No disclosed insider trades in w-1	*Unconstrained*	*No insider trades in [w-5,w-2]*
RetailBuy(w-1)	0.133***		
	(12.17)		
RetailBuy(w-2)		0.084***	0.085***
		(7.59)	(4.61)
InsiderBuy(w-1)	0.642***	0.481***	0.330***
	(14.17)	(12.39)	(14.00)
InsiderSell(w-1)	-0.143***	-0.127***	-0.254***
	(-6.35)	(-5.04)	(-13.12)
InsiderBuy(w-1)×RetailBuy(w-1)	0.171***		
	(3.49)		
InsiderSell(w-1)×RetailBuy(w-1)	-0.041		
	(-1.43)		
InsiderBuy(w-1)×RetailBuy(w-2)		0.145***	0.169***
		(3.39)	(3.48)
InsiderSell(w-1)×RetailBuy(w-2)		-0.027	-0.021
		(-1.10)	(-0.81)
Controls	YES	YES	YES
Constant	-0.078	-0.068	-0.059
	(-0.46)	(-0.49)	(-0.26)
Observations	3,590,616	3,851,327	1,592,157
R-squared	0.063	0.063	0.078

*** p<0.01, ** p<0.05, * p<0.1

Table 2.8. Opportunistic Trades

This table reports estimation results on whether the interaction effect is observed for routine and opportunistic insider trades as classified by Cohen, Malloy, and Pomorski (2012, henceforth CMP) and Ali and Hirshleifer (2017, henceforth AH). Fama-MacBeth regressions are estimated where the dependent variable is weekly market-adjusted returns. Other independent variables are defined in Table 5. The first (last) two specifications use the opportunistic and routine definitions of CMP (AH). Standard errors are adjusted using Newey-West with 5 lags and t-statistics are in parentheses.

	CMP	*CMP*	*AH*	*AH*
RetailBuy(w-1)		0.131***		0.128***
		(12.49)		(12.21)
OpportunisticInsiderBuy(w-1)	0.701***	0.626***	0.683***	0.613***
	(16.62)	(13.87)	(13.08)	(10.82)
OpportunisticInsiderSell(w-1)	-0.159***	-0.161***	-0.231***	-0.231***
	(-6.45)	(-6.19)	(-6.74)	(-6.71)
RoutineInsiderBuy(w-1)	0.090*	0.083	0.154*	0.125*
	(1.79)	(1.60)	(1.95)	(1.72)
RoutineInsiderSell(w-1)	-0.033	-0.025	0.001	0.004
	(-1.58)	(-1.27)	(0.08)	(-0.21)
OpportunisticInsiderBuy(w-1)×RetailBuy(w-1)		0.151***		0.155***
		(3.48)		(3.02)
OpportunisticInsiderSell(w-1)×RetailBuy(w-1)		-0.005		-0.001
		(0.11)		(-0.04)
RoutineInsiderBuy(w-1)×RetailBuy(w-1)		0.015		0.057
		(0.35)		(1.01)
RoutineInsiderSell(w-1)×RetailBuy(w-1)		-0.017		-0.005
		(-0.49)		(-0.20)
Controls	YES	YES	YES	YES
Constant	0.001	-0.058	-0.015	-0.074
	(0.00)	(-0.49)	(-0.12)	(-0.70)
Observations	3,871,675	3,871,675	3,871,675	3,871,675
R-squared	0.063	0.064	0.063	0.064

*** p<0.01, ** p<0.05, * p<0.1

Table 2.9. Portfolio Returns.

This table reports weekly excess returns (alpha) on long-short zero net investment portfolios based on publicly observable information regarding insider and retail trades. The *Retail* portfolio is long (short) stocks that have been net bought (sold) by retail traders in the previous week. The *Insider* portfolio is long (short) stocks that have been net bought (sold) by insider traders in the previous week. The *Ins-Retail Agree* portfolio is long (short) stocks that have been net bought (sold) by insiders while retail traders were net buying (selling) in the previous week. The *Ins-Retail Disagree* portfolio is long (short) stocks that have been net bought (sold) by insiders while retail traders were net selling (buying) in the previous week. Insider trades that were not disclosed to the SEC by the end of the previous week are excluded from all insider portfolios. The returns on all portfolios are adjusted for the 4 Carhart (1997) factors *Mkt, SMB, HML,* and *MOM*. Equal-weighted (EW) and value-weighted (VW) alphas are reported separately. Panel A reports alphas to portfolios with holding periods of 1 week. Panels *B, C,* and *D* report results for holding periods of 4, 12, and 52 weeks, respectively. *t*-statistics are in parentheses.

Panel A: 1 week holding period	*Retail*	*Insider*	*Ins-Retail Agree*	*Ins-Retail Disagree*
EW CAPM Alpha	0.120***	0.685***	0.885***	0.485***
	(2.62)	(6.35)	(8.23)	(4.47)
EW 4-factor Alpha	0.110***	0.764***	0.946***	0.582***
	(2.55)	(7.25)	(8.98)	(5.52)
VW CAPM Alpha	0.067**	0.327***	0.433***	0.221**
	(2.26)	(3.13)	(4.12)	(2.14)
VW 4-factor Alpha	0.072**	0.405***	0.512***	0.298**
	(2.21)	(3.47)	(4.38)	(2.55)
Panel B: 4 week holding period				
EW CAPM Alpha	0.043	0.349***	0.485***	0.212***
	(1.08)	(5.20)	(7.17)	(3.20)
EW 4-factor Alpha	0.041	0.377***	0.509***	0.244***
	(0.96)	(5.91)	(7.54)	(3.89)
VW CAPM Alpha	0.023	0.174**	0.253***	0.096*
	(0.78)	(2.56)	(3.46)	(1.85)
VW 4-factor Alpha	0.023	0.208***	0.290***	0.125**
	(0.79)	(2.84)	(3.78)	(2.14)
Panel C: 12 week holding period				
EW CAPM Alpha	0.035	0.194***	0.250***	0.137**
	(0.96)	(3.56)	(4.62)	(2.49)
EW 4-factor Alpha	0.031	0.211***	0.261***	0.160***
	(0.90)	(4.00)	(4.96)	(3.03)
VW CAPM Alpha	0.019	0.102*	0.127**	0.076
	(0.80)	(1.91)	(2.38)	(1.44)
VW 4-factor Alpha	0.018	0.117**	0.147**	0.087
	(0.69)	(2.01)	(2.52)	(1.50)

Panel D: 52 week holding period				
EW CAPM Alpha	0.005	0.057	0.077**	0.037
	(0.16)	(1.46)	(2.13)	(0.79)
EW 4-factor Alpha	-0.002	0.065*	0.086**	0.044
	(-0.07)	(1.73)	(2.45)	(1.02)
VW CAPM Alpha	-0.010	0.025	0.031	0.020
	(-0.51)	(0.68)	(0.87)	(0.48)
VW 4-factor Alpha	-0.004	0.031	0.038	0.023
	(-0.18)	(0.74)	(0.98)	(0.49)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

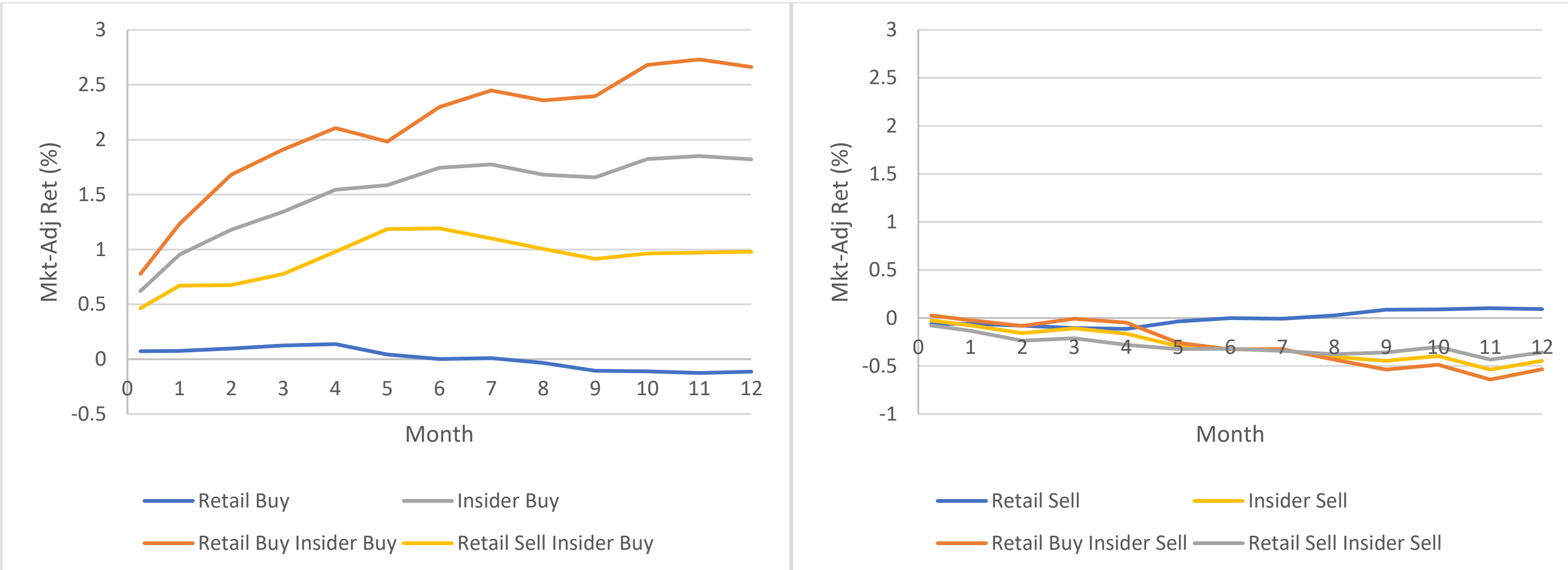

Figure 2.1. Market-adjusted returns to insider and retail trading over time.
The figure shows average cumulative market-adjusted stock returns under eight conditions based on retail and insider trading activity. The chart on the left shows future market-adjusted returns to stocks that (i) were net bought by retail traders in the previous week, (ii) were net bought by insiders in the previous week, (iii) were net bought by both retail traders and insiders in the previous week, (iv) were net bought (sold) by insiders (retail traders) in the previous week. The chart on the right shows future market-adjusted returns to stocks that (v) were net sold by retail traders in the previous week, (vi) were net sold by insiders in the previous week, (vii) were net bought (sold) by retail traders (insiders) in the previous week, and (viii) were net sold by both retail traders and insiders in the previous week.

Chapter 3. RETAIL TRADING AROUND EARNINGS ANNOUNCEMENTS

3.1 Introduction

What is the role of retail traders in stock pricing? Are they noise traders as in Black (1986), or are they as a group able to predict stock returns? The literature has not yet settled on an answer as previous studies provide mixed empirical results. On the one hand, there is ample evidence of individual investors suffering from behavioral biases such as the disposition effect, overconfidence, and the familiarity bias (e.g., Odean, 1998; Barber and Odean, 2001; Seasholes and Zhu, 2010). Moreover, some studies find a negative association between overall retail trading and future returns (Odean, 1999; Barber, Odean, and Zhu, 2009). On the other hand, recent studies using comprehensive datasets tend to find that aggregated retail trades can predict returns (Kaniel, Liu, Saar, and Titman, 2012; Kelley and Tetlock, 2013; Fong, Gallagher, and Lee, 2014; Barrot, Kaniel, and Sraer, 2016; Boehmer, Jones, Zhang, and Zhang, 2021).

This study extends the prior literature on retail investor stock trading by using a recent and comprehensive U.S. dataset and focusing on earnings announcements. I focus on earnings announcements because informed individuals may be especially active around times when earnings information is released to the market. Moreover, individuals may trade more aggressively on such information than institutions before earnings releases if institutions are reluctant to do so due to reduced liquidity or out of fear of litigation. Consistent with retail traders being informed, I document that their pre-announcement trading predicts earnings surprises, announcement returns, and medium-term post-announcement returns.

Can these results be explained in light of the evidence that retail traders trade sub-optimally and that institutions tend to be informed? One possibility is that while many retail traders are uninformed, there is a subset of individuals that is informed. I estimate the extent to which my results are driven by two such groups of individuals: corporate insiders and a group of hackers that had access to earnings figures several hours before public disclosures.

When Kaniel et al. (2012) find that retail trades predict earnings announcements, they note that it remains an open question whether these results can be attributed to corporate insiders that constitute a part of their individual trader dataset. There are good reasons to believe the answer is affirmative. It is well known that insiders tend to be informed (Jaffe, 1974, Lakonishok and Lee, 2001; Ben-David, Birru, and Rossi, 2019). While insiders constitute a small fraction of all individual traders and the typical dollar profits to insider trades are modest, they trade in relatively large amounts (Cziraki and Gider, 2020). Moreover, Lee, Lemmon, Li, and Sequeira (2014) find that insiders continue to trade on private information through pre-approved trades even as they face trade restrictions in the days leading up to earnings announcements. Furthermore, Ali and Hirshleifer (2017) find that trades executed by insiders, who have historically profited from pre-announcement trades, better predict subsequent abnormal returns than other insider trades. As such, the contribution of pre-announcement insider trades to the informedness of retail traders could be sizable.

Providing more suggestive evidence of the importance of this channel, Chung (2020) finds that in a general context that is not limited to earnings announcements at least half of the predictive power of retail trades for future returns can be attributed to insider trading. However, in the context of earnings announcements, this does not appear to be the case; my results are similarly strong for the sample that excludes earnings announcements where insiders traded before the announcement. My sample may also have relatively fewer insiders among retail traders compared to Kaniel et al. (2012) and Chung (2020) as argued in section 3.2.1.

Turning to the aforementioned group of hackers, there are also reasons to believe they may significantly impact the aggregate predictive power of retail trades. By hacking several major newswire services, they gained early access to earnings that had been uploaded to these services before being released publicly. The hackers traded on this valuable information and did so quite aggressively, making more than 100 million dollars in profits. They potentially had early access to nearly 10,000 quarterly earnings reports (about 12% of my sample), with at least several hundred known cases of illegal trading according to publicly available court documents. To determine the extent to which the predictive power of retail trades is driven by these hackers, I first exclude the last day before earnings announcements from the analysis. This ensures that the sample contains few, if any, hacker trades because earnings figures were uploaded onto the newswires in the late morning or early afternoon to be released after markets closed. The evidence regarding earnings surprises becomes considerably weaker.

However, if the hackers are primarily driving the results, the evidence should also be weaker when the analysis is restricted to earnings news where hackers were unsuccessful or inactive. Akey, Gregoire, and Martineau (2021) find that the hackers focused on large stocks, whereas my evidence extends to small stocks. Taken together, these findings suggest that much of retail trades predictive power for earnings surprises is driven by individuals, who trade opportunistically the day before the announcement, but that not all of them possess hacked press releases. It remains an open question whether these individuals constitute a large diverse group or a small subset of retail traders.

In addition to studying the role of insiders and hackers in informed retail trading around earnings announcements, the 2010-2015 sample examined in this paper is more recent and arguably more representative of retail traders compared to earlier studies. The sample is constructed as in Boehmer et al. (2021) using public TAQ data, and its construction relies on the practice of brokers to offer marginally better prices to retail orders through internalization or to route them to market makers who offer price improvement. Boehmer et al. (2021) estimate that this approach identifies about one-half of all self-directed retail trades.

In contrast, Kaniel et al. (2012) use U.S. data from the NYSE, which may not be as representative because retail brokers have incentives to route retail orders away from the NYSE as other venues offer to pay brokers for execution (Battalio and Loughran, 2008). Indeed, Barber, Odean, and Zhu (2009) show that most discount retail brokers route fewer than 1% of orders to the NYSE during the 2000-2003 sample period in Kaniel et al. (2012).

Further to the advantages of my sample, Kelley and Tetlock (2013) show that retail market orders can predict earnings surprises, whereas limit orders have negligible predictive power. Their data originate from one market maker during 2003-2007. My data only include marketable orders, but are not limited to one market maker, capturing about three times the retail volume in terms of percentage shares of total volume.

Boehmer et al. (2021) also study earnings announcements but focus on retail trades executed the week prior to the announcements. They find that the predictive power of retail order imbalances nearly doubles on earnings announcement weeks, but the increase is not statistically significant. I use their algorithm to construct nearly the same sample but expand the analysis to focus on pre-announcement trading and its relation to earnings surprises, earnings announcement returns, and post-earnings returns. My results are stronger because the predictive power of retail trades is concentrated in the final day prior to earnings announcements, whereas this activity is not captured in most of the weekly regressions performed by Boehmer et al. (2021).

Michels (2021) uses Robinhood data to infer that returns to both positive and negative earnings surprises are higher when more retail traders hold the stock the day after earnings relative to the day before. In contrast, Friedman and Zeng (2021) combine Robinhood and TAQ data and utilize exogenous Robinhood outages to infer that increased retail trading activity is associated with increased price responsiveness to earnings surprises for both negative and positive surprises. These papers focus on contemporaneous and post-earnings retail trading activity whereas my paper focuses on pre-announcement retail trades.

More generally (not specific to earnings news), Welch (2022) finds, using Robinhood data from 2018 through 2020, that a representative portfolio of the average Robinhood user generates positive alpha. On the other hand, Barber, Huang, Odean, and Schwarz (2022) find that *attention-driven* retail trading through Robinhood strongly negatively predicts future returns. In addition, Barber et al. (2022) argue that retail investor trading activity is changing due to gamification and simplicity through fintech applications such as Robinhood. These studies highlight the continuing debate on the information content of retail traders, to which this paper contributes. However, they have limited impact on my results because the popularity of these applications was limited during the 2010-2015 period I study (e.g., the Robinhood app launched in March 2015[8]).

The remainder of the paper is organized as follows. Section 2 describes the data, sample, and preliminary results. Section 3 contains main empirical results. Section 4 concludes.

[8] https://blog.robinhood.com/news/2015/3/11/start-investing-stop-paying

3.2. Data, retail trading imbalance measures, and sample

3.2.1 Data

The sample includes all ordinary common shares from January 2010 to December 2015. Retail trading is identified using TAQ data and applying Boehmer et al.'s (2021) algorithm, which exploits the fact that retail orders usually receive price improvement in fractional pennies. This means that trades are executed at a price that is better than quoted – for example, if the best quoted ask price for a security is $35.00, and a retail client places a market order to buy, a broker can choose to internalize this order at $34.999 offering one tenth of a penny price improvement. Such price improvement can also occur when brokers route orders to wholesalers, which are market makers that combine order flow from multiple sources.

Typically, price improvements in sub-pennies are smaller than half a cent, meaning that the price at which transactions are executed provides a way to distinguish retail sales from retail buys. Most retail transactions that are price-improved do not take place on exchanges and instead are reported to a FINRA Trade Reporting Facility (TRF). The procedure to identify retail trades is to take the transactions reported to TRFs and flag transactions with prices ending in the interval (0, 0.4) cents as retail sales and those in the interval (0.6, 1) as retail purchases.

While data on sub-penny price improvements are available from 2005, there is an upward trend in the number of sub-penny trades, likely because the practice of offering price improvement for retail trades in sub-pennies was increasingly adopted. The trend continues until 2010 and then stabilizes. In 2016, the tick size pilot program came into effect, affecting the ability to offer price improvement for many stocks. Therefore, I chose to use the same 2010-2015 sample period as in Boehmer et al. (2021).

It is conceivable that insider trades constitute a smaller portion of retail trades in my data compared to the NYSE data of Kaniel et al. (2012) or Chung (2020). The wholesalers have incentives to avoid taking the other side of insider trades because these trades are usually informed. Meanwhile, brokers know which of their clients are insiders because of the requirement to self-identify. From the legal standpoint, agreements between brokers and wholesalers give the latter the right to reject large orders,[9] and insider trades are typically much larger than the average retail trade. Rejected orders might instead be placed on an exchange such as the NYSE and captured as retail trades in studies based on the NYSE data.

Further as to why insider trades may be appearing less in my data, many firms have special policies on insider trading plans. For instance, some companies require employees to use a "captive" broker for insider plans,[10] which may execute trades differently than typical retail brokers. Finally, the data also show that at least 50% of insider trades are larger than retail trading volume on the same day for the same stock, suggesting that a large proportion of insider trades is not captured as retail trades.

Stock returns and characteristics are taken from CRSP, book-to-market values are computed using data from Compustat, and the data on insider trades and insider characteristics come from Thomson Reuters. Quarterly earnings announcements are taken from both Compustat and I/B/E/S where in less than 1% of the data the dates are in conflict. If this happens, the earlier date of the two is used to avoid any potential look-ahead bias. Day 0 is defined as the trading day on which announcements are made or the first trading day after if they occur after trading hours. Although I/B/E/S timestamps are systematically delayed (Bradley, Lee, Clarke, and Ornthanalai, 2014), the observations at 4 p.m. are assumed to correspond to after-market hours because almost all earnings are announced outside regular trading hours[11.] For some analyses, earnings announcements are

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[9] https://www.sec.gov/Archives/edgar/data/316709/000031670904000037/exh10_262.txt, section II (a) (i).
[10] https://corpgov.law.harvard.edu/2016/03/24/a-guide-to-rule-10b5-1-plans/
[11] Gregoire and Martineau (2021) report that after adjusting timestamps using Ravenpack, over 99% of earnings announcements of S&P 1500 firms from 2011 to 2015 take place outside of market hours.

separated into positive and nonpositive earnings surprises, where positive surprises are those where the actual earnings were higher than the median analyst estimate provided by I/B/E/S.

For earnings announcements in Compustat that could not be matched to I/B/E/S announcements, comprising 18% of the sample, no timestamps are available. For these observations, I assume that the earnings were announced before markets close to be conservative and prevent misclassifying some post-announcement retail trading as having occurred pre-event. The main results continue to hold when excluding these announcements. Whereas these data issues introduce some noise to 1-day announcement returns, the impact is smaller on returns evaluated over longer horizons used in this paper (ranging from 2 to 60 days).

3.2.2 Sample

The sample starts with 5,877 stocks from the CRSP universe with share codes 10 and 11 and is merged with I/B/E/S, Compustat, and TAQ. Non-missing retail trade data are required on the day of the announcements and the calendar year's stock price minimum must be at least \$1.00 (this means that if the stock price has dropped below \$1 in say 2011 but remained above it in other years, only the 2011 data for this stock are dropped). The final sample consists of 4,919 stocks with 80,685 earnings announcements. The sample selection procedure is summarized in Table 3.1. To gain a better understanding of the data, the sample characteristics are described in Table 3.2. Results are shown for all trading days (Panel A) and for the earnings announcement days (Panel B). On days with earnings announcements, there is generally higher overall trading volume and higher retail volume, by a factor of nearly 3, with the factor being somewhat greater for retail volume compared to the overall volume. Retail order imbalance (as used by for instance Boehmer et al., 2021) is defined for a stock i on day t as

$$ROIB_{i,t} = \frac{Retail\ buy\ volume_{i,t} - Retail\ sell\ volume_{i,t}}{Retail\ buy\ volume_{i,t} + Retail\ sell\ volume_{i,t}}, \tag{3.1}$$

where volumes are expressed in shares.

Barber and Odean (2008) argue that retail investors are net buyers of attention-grabbing stocks. If earnings announcements only draw attention to the stocks, we may expect the same phenomenon. However, in my sample, retail order imbalance is negative on announcement days. Unsurprisingly, returns are more volatile on news days, while the average return is very similar, and the median return is the same at 0%.

When there are no retail trades identified on a trading day for a particular security, the number of retail trades is set to zero. In these instances, retail order imbalance has a division-by-zero problem resulting in missing values and a lower number of observations. Also note that volatility, turnover, size, book-to-market ratios, and stock prices are only reported for the full sample because the reported statistics are very similar for the two samples by construction.

For a preview of the results and to gain a better sense of the data, Table 3.3 contains correlations for the primary variables used in the remainder of the paper, for the sample restricted to earnings announcement days. The sign of an earnings surprise is related to various variables that are known prior to the announcements. In particular, cumulative retail order imbalances in the 10 and 60 days prior to earnings announcements are positively correlated with the sign of earnings surprises, suggesting that (at least some) retail traders are informed. However, both these imbalances and the sign of earnings surprises are correlated to volatility, turnover, market capitalization, and the book-to-market ratio. It is thus important to control for these variables in the earnings surprise tests.

Cumulative retail order imbalances are also positively correlated to returns on event days but not as strongly as to the earnings surprises. Interestingly, the signs of the correlations of returns with the four control variables run opposite to the signs of the correlations of these measures with the direction of the earnings surprise and the cumulative retail order imbalances. This shows that predicting earnings surprises does not necessarily translate to predicting returns, so it is worthwhile to determine whether any predictive power of retail trading on earnings surprises translates to stock returns. Therefore, in the next section, I separately test the predictive power of cumulative retail order imbalances on earnings surprises and on returns.

3.3. Empirical results

3.3.1 Graphical results

Before turning to formal regression analyses, I plot returns and retail order imbalances around earning announcements in Figure 3.1 and discuss the underlying intuition. Both charts comprise a 21-day event window period centered on day 0. The graph on the left shows the difference between cumulative market-adjusted returns to stocks experiencing positive earnings surprises and nonpositive surprises. These differences are plotted for stocks separated into three market capitalization groups, defined as in Kaniel et al. (2012): large (top three deciles), medium (next three deciles), and small (bottom four deciles). Daily market-adjusted returns are defined as daily stock returns minus the average equal-weighted daily return on the stocks in the sample.

For each size group, average returns to stocks that are about to experience positive earnings surprises are slightly increasing relative to those that will experience nonpositive surprises. This is consistent with superior forecasting relative to the available analyst forecasts or some leakage of earnings information. Post-earnings announcement drifts appear to be primarily if not only observed for small stocks, consistent with Martineau (2021) and those stocks being less informationally efficient.

The graph on the right is similarly constructed as the one on the left but with cumulative retail order imbalances instead of returns. For each size group, the difference in these imbalances increases in the days prior to earnings news. This means that retail investors tend to buy more stocks that are about to experience positive earnings surprises relative to those that are about to experience nonpositive surprises, suggesting that some retail traders are informed. The trend is strongest for small stocks, consistent with the notion that superior earnings forecasting is easier for these stocks for retail investors. Informed retail trading activity appears to be concentrated on the final day prior to earnings news which might be attributable to the aforementioned hackers, who only had early access for several hours (and are primarily recorded at day -1 because most earnings were released after market hours).

After earnings are released, cumulative imbalances stabilize for small stocks while retail traders tend to temporarily trade contrarian for the roughly 5 days after earnings surprises for large and medium stocks. In contrast, using a richer but smaller dataset over nearly the same period (2010-2014), Luo, Ravina, and Viceira (2020) find that retail trading is contrarian after large earnings surprises for small stocks. The reconciliation of these findings is left to future research utilizing more detailed transaction data.

Overall, these graphical results tell us that prior to earnings announcements, the overall patterns are not much different for the different size groups. Some interesting differing patterns emerge after announcements, but the focus of this paper is on pre-announcement retail trading. Therefore, I do not separately analyze different size stocks in the remaining regression analyses.

3.3.2 Regression Analyses

The results discussed above seem to indicate that retail order flow predicts earnings surprises. However, the correlation analysis discussed in section 2.2 reveals the need to control for various measures. To formalize the analysis, I follow Kelley and Tetlock (2013) and estimate the following logistic regression model:

Let $p(x) = \mathrm{P}(Positive\ Earnings\ Surprise[x,y]_i |\ Covariates)$. Estimate

$$p(x) = \frac{1}{1+e^{\alpha + \beta_1 ROIB_{i,0} + \beta_2 Controls_i + \varepsilon_i}} \tag{3.2}$$

where $Positive\ Earnings\ Surprise[x,y]_i$ equals 1 if there is a positive earnings surprise between days x and y, and 0 if the surprise is nonpositive. This structure tests whether retail flow predicts an upcoming earnings surprise between x and y days into the future. To be consistent with

Kelley and Tetlock (2013), I use [1,5] and [6,20] days as values for x and y. As a robustness check, I also use [2,5] that excludes retail trading on the last day prior to the earnings announcements to account for the fact that a sizeable group of hackers was actively trading on this day during the sample period.

An earnings surprise is defined to be positive when the reported earnings are higher than the I/B/E/S provided median analyst estimate of the earnings. $ROIB_{i,0}$ is the retail order imbalance as defined earlier at event time 0. Control variables are the same as in Kelley and Tetlock (2013), augmented with volatility and turnover measures as in Boehmer et al. (2021). Specifically, these are the stock returns at time 0; lagged returns from time -5 to -1 (in trading days) and from time -26 to -6; volatility estimated as the standard deviation of daily returns over the previous month; turnover computed as the mean daily number of shares traded in the previous month, divided by the total number of shares outstanding, and multiplied by 21 (the average number of trading days per month); size defined as the log of the market capitalization at the end of the previous calendar year, and the book-to-market ratio computed at the end of the previous calendar year.

The results are in Table 3.4. The main coefficient β_1 is positive and significant for earnings surprises in the 5 trading days ahead, implying that aggregate retail trading activity predict earnings surprises. Economically, when evaluated at the means of the independent variables, if retail order imbalance increases by one standard deviation, there is a 0.87% $\left(= \frac{e^{0.34+0.046*0.078}}{1+e^{0.34+046*0.078}} - \frac{e^{0.34}}{1+e^{0.34}}\right)$ increase in the likelihood of a positive earnings surprise if the announcement is due in 1 to 5 days. When the horizon is changed to [2,5] days in the second specification, the predictability largely disappears and the marginal effect of a one-standard deviation reduces to 0.30%. The predictive power is thus concentrated in the last day, perhaps because of the aforementioned hackers.

Retail order imbalance also fails to predict earnings surprises in the [6,20] event time period. In contrast, Kelley and Tetlock (2013) find positive and significant order imbalance coefficients for both the [1,5] and [6,20] time windows, while their magnitude and significance are also considerably lower for the latter. These findings are consistent with Figure 3.1, where the differential retail order imbalance measures start to rise around 5 days prior to earnings announcements, and primarily so on the last day.

Despite restrictions on insider trading prior to earnings announcements, 10% of the observations are preceded by insider trades in the two weeks prior to the earnings announcements. There is thus a sizeable number of insiders who manage to trade during these periods, presumably through pre-approved transactions that predict earnings (Lee, Lemmon, Li, and Sequeira, 2014). To determine how much of the retail order evidence might be driven by insiders, Table 3.5 Panel A shows the results for the sample excluding announcements preceded by insider trades. The evidence regarding earnings surprises in the next 5 days weakens somewhat while the predictability disappears in the [2,5] period and remains absent in the [6,20] period.

To delve deeper into the question of how much of the predictive power on day -1 is related to hackers, Panels B and C show the results for samples that were unlikely targets for hackers. Akey et al. (2021) argue that hackers are more likely to target stocks that are liquid, easy to short, and where they are more certain about the signal of the hacked earnings. They further argue that stocks with low institutional ownership are less liquid and/or have more short-selling constraints, and signals are less precise when analyst coverage is low, thus making them less attractive targets. They go on to show that there is no evidence of hacked press releases impacting prices for these subsamples. Consistent with hackers being largely responsible for the predictability of earnings surprises in the [1,5] period, retail order imbalances fail to predict surprises for stocks with low institutional ownership. However, retail order flow continues to predict earnings surprises for the low analyst coverage sample for [1,5] days, and there is even evidence of predictability for [6,20] days for this sample. The enhanced predictability is consistent with the analysts' earnings estimates being less accurate and thus more easily improved upon by retail traders.

Given that retail traders appear to predict earnings surprises, and that positive earnings surprises are on average accompanied by positive returns (Figure 3.1), it seems likely that retail imbalances can predict earnings announcement returns. However, it does not immediately follow because they could be forecasting earnings superior to the median known earnings forecast but not relative to the implied market consensus as measured by returns. In addition, various variables are correlated to the sign of earnings surprises and earnings announcement returns in the opposite direction, even though positive earnings are typically accompanied by positive returns.

The return predictability analysis is formalized with the aim to determine whether retail order flow predicts returns, and if it does, whether it extends beyond the information contained in earnings surprises. Market-adjusted returns over the [0,1] trading period are regressed on cumulative retail order imbalances, pre-announcement cumulative abnormal returns, and earnings surprise quintiles. Cumulative imbalances and returns are computed over horizons [-10,-1] and [-60,-1] to be consistent with Kaniel et al. (2012).

To correct for possibly contemporaneously correlated errors for earnings announcements that are clustered in time, I employ the Fuller-Battese (1974) methodology as in Kaniel et al. (2012) and the results are found in Table 3.6. Panel A shows the results for the full sample, where the first specification shows that cumulative retail order imbalances over the 10 pre-announcement days predict announcement returns after controlling for pre-announcement returns. The second specification shows that this predictability is robust to controlling for earnings surprises, confirming the findings of Kaniel et al. (2012). A larger reduction in return predictability is observed for the [-60,-1] event window, but the result remains significant.

Kaniel et al. (2012) ponder whether their results are driven by corporate insiders or by a larger group of retail traders. To determine if these results are driven by insiders, in Table 3.7 panel A I exclude earnings announcements with insider trades in the 10 trading days prior to the announcement. As in Table 3.4, this excludes just over 10% of the events while the results are similar (if anything, stronger) for this subsample. I conclude that the predictive power of retail order imbalances for future earnings announcement returns is not driven by insider trading. To account for the hackers, I again drop the last day prior to the announcements and show the results in Panel B. Contrary to estimating earnings surprises, the evidence remains strong for returns. Taken together, the results do not suggest that the earnings surprise predictability is only driven by hacked earnings announcements. Future research could provide a more definitive answer by excluding the stocks that have been hacked.

Finally, I follow Kaniel et al. (2012) in evaluating the predictive power of pre-announcement retail trades of post-earnings announcement returns. Table 3.8 is similar to Table 3.6 with the independent variable evaluated over the 60 trading days after earnings announcements (i.e., approximately up to the next quarterly earnings announcement). The results show that predictive power of retail trading extends beyond the immediate announcement return, with similar statistical significance as for announcement returns. Economically, the coefficients are two to three times as large, while the return horizon is thirty times larger. This suggests that while the return predictability of retail order flow extends beyond earnings announcement returns, it is concentrated around them. Given that there is no strong post-earnings announcement drift in this sample, it is not surprising that the inclusion of earnings surprise quintiles in the regressions has a smaller effect on the other coefficients.

In a similar spirit to the previous analyses, Table 3.9 show that the results are robust to controlling for insider trades and excluding the last day prior to earnings announcements. The statistical significance becomes weaker for the latter subsample with 60-day returns while the magnitudes of the relevant coefficients only reduce by about 5%.

3.4. Conclusion

Using a comprehensive U.S. dataset covering 2010-2015, I find that retail traders tend to trade in the same direction as future earnings surprises. This predictive power is not driven by insider trades; however, it is driven in part by individuals who trade on the last day prior to announcements, potentially including a group of hackers who gained access to earnings news before the public.

After controlling for lagged returns and the strength of the earnings surprise, pre-announcement retail trades predict both earnings announcement returns and the 3-month post-earnings returns. These results are not driven by insiders nor individuals trading on the last day prior to earnings announcements. Overall, the data are consistent with retail traders encountering unpriced valuable information and trading on it.

3.5 References

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Table 3.1. Sample Selection

This table describes the sample selection procedure from the full sample of CRSP stocks with share codes 10 and 11 to the sample that is restricted to earnings announcements (EAs) that (i) are matched to the CRSP data, (ii) have non-missing retail trade volume data, and (iii) have corresponding stock prices that stay above $1, evaluated by calendar year.

#Stocks	#EAs	
5,877	-	CRSP universe restricted to share codes 10 and 11
5,169	91,688	Require at least 1 identified earnings announcement
5,100	86,061	Merge with Retail TAQ data
4,919	80,685	Stock price > $1 for the entire calendar year

Table 3.2. Sample Characteristics

This table shows the number of observations, mean, standard deviation, and the 10th, 25th, 50th, 75th, and 90th percentiles of a selection of variables used in the remainder of the paper, for the sample of earnings announcements (panel A) and the full sample (panel B). The frequency is daily unless otherwise specified. Retail purchases and sales are identified as in Boehmer, Jones, Zhang, and Zhang (2021): Transactions reported to a FINRA TRF with prices ending in the interval (0,0.4) cents are identified as retail sales and those in the interval (0.6,1) as retail purchases. $RET_{i,0}$ is the gross stock return of stock i. $ROIB_{i,0}$ is retail order imbalance computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume) where volumes are in number of shares traded. $VOLAT_{i,m-1}$, the volatility estimated as the standard deviation of daily returns over the previous month; $TURN_{i,m-1}$ is the turnover computed as the mean number of shares traded in the previous month, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %. $SIZE_{i,y-1}$ is defined as the log of the market capitalization of the previous calendar year. $BM_{i,y-1}$ is the book-to-market ratio, computed at the end of the previous calendar year. All continuous variables are winsorized at 1% and 99%.

	#obs	mean	sd	p25	median	p75
Panel A: Full sample						
Retail Purchases (#)	5,092,177	109	414	4	21	77
Retail Sales (#)	5,092,177	106	357	5	22	80
Retail Purchases ($M)	5,092,177	1.31	11.13	0.01	0.08	0.45
Retail Sales ($M)	5,092,177	1.31	10.72	0.01	0.09	0.47
Volume (#)	5,092,177	5627	13625	236	1339	5132
Volume ($M)	5,092,177	38.59	181.89	0.40	3.36	21.40
$RET_{i,0}$ (%)	5,182,088	0.07	3.06	-1.14	0.00	1.19
$Retail\ OIB_{i,0}$	4,801,943	-0.04	0.46	-0.29	-0.03	0.21
$VOLAT_{i,m-1}$	5,166,627	2.45	1.52	1.42	2.07	3.04
$TURN_{i,m-1}$	5,168,154	16.83	17.85	5.52	11.83	21.33
$SIZE_{i,y-1}$	5,165,916	13.30	1.98	11.87	13.24	14.63
$BM_{i,y-1}$	4,686,149	0.65	0.57	0.28	0.54	0.88
$Stock\ Price$	5,182,088	29.44	63.08	7.72	17.40	36.17
Panel B: Earnings announcement days only						
Retail Purchases (#)	80,685	292	1035	10	57	217
Retail Sales (#)	80,685	278	843	11	60	222
Retail Purchases ($M)	80,685	3.77	23.41	0.03	0.25	1.45
Retail Sales ($M)	80,685	3.73	22.89	0.03	0.27	1.51
Volume (#)	80,685	13163	29083	443	3203	13319
Volume ($M)	80,685	99.05	407.39	0.83	9.19	63.23
$RET_{i,0}$ (%)	81,671	0.08	7.51	-2.94	0.00	2.99
$ROIB_{i,0}$	77,293	-0.03	0.39	-0.21	-0.02	0.15

Table 3.3. Correlations.

This table shows correlations computed using the sample restricted to eligible earnings announcement days. $ROIB_{i,0}$ is retail order imbalance computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume) where volumes are in number of shares traded. $ROIB_{i,[x,y]}$ are cumulative retail order imbalances from days x to y. $PES_{i,0}$ is an indicator variable that is equal to one when there is a positive earnings surprise of stock i on day 0, and zero when there is a nonpositive earnings surprise. An earnings surprise is positive when the earnings are higher than the I/B/E/S median analyst estimate, and nonpositive otherwise. $RET_{i,0}$ is the gross stock return of stock i on day 0. $VOLAT_{i,m-1}$ is the volatility estimated as the standard deviation of daily returns over the previous month. $TURN_{i,m-1}$ is the turnover computed as the mean number of shares traded in the previous month, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %. $SIZE_{i,y-1}$ is defined as the log of the market capitalization of the previous calendar year. $BM_{i,y-1}$ is the book-to-market ratio, computed at the end of the previous calendar year.

	ROIB	*ROIB [-60,-1]*	*ROIB [-10,-1]*	*PES*	*RET*	*VOLAT*	*TURN*	*SIZE*
$ROIB_{i,0}$	1.00							
$ROIB_{i,[-10,-1]}$	0.11***	1.00						
$ROIB_{i,[-60,-1]}$	0.09***	0.77***	1.00					
$PES_{i,0}$	-0.00	0.01***	0.02***	1.00				
$RET_{i,0}$	0.01*	0.01*	0.01*	0.27***	1.00			
$VOLAT_{i,m-1}$	-0.03***	0.02***	0.02***	0.06***	-0.01**	1.00		
$TURN_{i,m-1}$	0.00	0.04***	0.02***	0.03***	-0.03***	0.34***	1.00	
$SIZE_{i,y-1}$	0.02***	0.03***	0.01*	0.12***	-0.01	-0.39***	0.22***	1.00
$BM_{i,y-1}$	0.03***	-0.04***	-0.03***	-0.03***	0.01**	0.03***	-0.13***	-0.27***

Table 3.4. Predicting Earnings Surprises

This table reports results of daily logistic regressions estimating the sign of earnings surprises. The dependent variable is the log-odds of $PES[x,y]_i$, which is equal to one when there is a positive earnings surprise between days x and y, and zero when there is a nonpositive earnings surprise. An earnings surprise is positive when the earnings are higher than the I/B/E/S median analyst estimate, and nonpositive otherwise. The independent variable is retail order imbalance $ROIB_{i,0}$. It is defined as (retail buy dollar volume – retail sell dollar volume)/(retail buy dollar volume + retail sell dollar volume), and is evaluated at day 0. The control variables include the same-day return $RET_{i,0}$; lagged returns $RET_{i,[-5,-1]}$ and $RET_{i,[-26,-6]}$; $VOLAT_{i,m-1}$, the volatility estimated as the standard deviation of daily returns over the previous month; turnover $TURN_{i,m-1}$ computed as the mean number of shares traded in the previous month, divided by the total number of shares outstanding, multiplied by the average number of trading days per month (21), expressed in %; $SIZE_{i,y-1}$ defined as the natural logarithm of the market capitalization of the previous calendar year; and $BM_{i,y-1}$, the book-to-market ratio computed at the end of the previous calendar year. At least 50 observations are required for each daily regression. Average coefficients and standard errors adjusted for Newey and West (1987) with 5 lags are reported.

	$PES[1,5]_i$	$PES[2,5]_i$	$PES[6,20]_i$
$ROIB_{i,0}$	0.078***	0.027*	0.003
	(0.015)	(0.015)	(0.009)
$RET_{i,0}$	0.038***	0.028***	0.013***
	(0.003)	(0.003)	(0.002)
$RET_{i,[-5,-1]}$	0.023***	0.024***	0.014***
	(0.002)	(0.002)	(0.002)
$RET_{i,[-26,-6]}$	0.016***	0.015***	0.011***
	(0.001)	(0.001)	(0.001)
$VOLAT_{i,m-1}$	-0.017***	-0.036***	-0.052***
	(0.007)	(0.008)	(0.004)
$TURN_{i,m-1}$	0.003***	0.003***	0.005***
	(0.000)	(0.000)	(0.000)
$SIZE_{i,y-1}$	0.157***	0.147***	0.170***
	(0.015)	(0.015)	(0.013)
$BM_{i,y-1}$	-0.125***	-0.127***	-0.111***
	(0.012)	(0.013)	(0.008)
Observations	270,717	207,992	820,834
Average Pseudo R-squared	0.086	0.082	0.065

*** p<0.01, ** p<0.05, * p<0.1

Table 3.5. Predicting Earnings Surprises in Subsamples

This table reports results of daily logistic regressions estimating the sign of earnings surprises. The dependent variable is $PES[x, y]_i$, which is equal to one when there is a positive earnings surprise between days x and y, and zero when there is a nonpositive earnings surprise. An earnings surprise is positive when the earnings are higher than the I/B/E/S median analyst estimate, and nonpositive otherwise. The independent variable is retail order imbalance $ROIB_{i,0}$. It is defined as (retail buy dollar volume – retail sell dollar volume)/(retail buy dollar volume + retail sell dollar volume), and is evaluated at day 0. The control variables are defined as in Table 3.4. At least 50 observations are required for each daily regression. Average coefficients and standard errors adjusted for Newey and West (1987) with 5 lags are reported. Panel A presents the results for the sample that excludes observations with insider trades in the 10 trading days prior to earnings announcements; panel B limits the sample to stocks with below-median institutional ownership; and panel C limits the sample to stocks with below-median number of analysts.

Panel A: No Insiders	$PES[1,5]_i$	$PES[2,5]_i$	$PES[6,20]_i$
$ROIB_{i,0}$	0.055***	0.001	-0.015
	(0.017)	(0.017)	(0.009)
Controls	YES	YES	YES
Observations	249,506	199,148	746,047
Average Pseudo R-squared	0.085	0.081	0.064
Panel B: Low Institutional Ownership			
$ROIB_{i,0}$	0.031	0.009	0.009
	(0.022)	(0.023)	(0.016)
Controls	YES	YES	YES
Observations	94,028	75,038	281,904
Average Pseudo R-squared	0.071	0.068	0.059
Panel C: Low Number of Analysts			
$ROIB_{i,0}$	0.044**	0.036	0.040**
	(0.021)	(0.023)	(0.016)
Controls	YES	YES	YES
Observations	90,713	72,214	269,954
Average Pseudo R-squared	0.032	0.030	0.028

*** p<0.01, ** p<0.05, * p<0.1

Table 3.6. Predicting Earnings Announcement Returns

This table reports regression results relating abnormal returns on and after earnings announcements to pre-announcement retail trading activity. The dependent variable is $CAR[0,1]_i$, which is the cumulative abnormal stock return over the [0,1] trading period where time 0 is the trading day of the earnings announcement. Abnormal returns are computed using the sample's equal-weighted index as the benchmark. Earnings surprises are put into quintiles by quarter and dummy variables are defined correspondingly, with $ES1$ containing the most negative surprises. Earnings surprises are defined as the actual earnings minus the I/B/E/S provided median analyst estimate of the earnings. $ROIB_{i,[x,y]}$ are cumulative retail order imbalances from x to y where daily order imbalances are computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume), and volumes are expressed in the number of shares traded in stock i. Fuller-Battese (1974) corrected t-statistics are in parentheses.

	$[-10,-1]$	$[-10,-1]$	$[-60,-1]$	$[-60,-1]$
$ROIB_{i,[x,y]}$	0.064***	0.055***	0.090***	0.063**
	(3.39)	(3.01)	(3.11)	(2.28)
$CAR_{i,[x,y]}$	-0.025***	-0.041***	-0.005***	-0.012***
	(-6.42)	(-11.02)	(-3.24)	(-7.78)
$ES1$		-4.728***		-4.722***
		(-46.53)		(-46.39)
$ES2$		-2.256***		-2.258***
		(-24.67)		(-24.67)
$ES4$		1.649***		1.643***
		(17.34)		(17.26)
$ES5$		3.054***		3.044***
		(30.17)		(30.03)
Intercept	0.042	0.484***	0.043	0.509***
	(0.83)	(6.83)	(0.83)	(7.12)
Observations	79,314	79,314	79,314	79,314

Table 3.7. Robustness Checks on Predicting Earnings Announcement Returns

This table reports regression results relating abnormal returns on and after earnings announcements to pre-announcement retail trading activity. The dependent variable is $CAR[0,1]_i$, which is the cumulative abnormal stock return over the [0,1] trading period where time 0 is the trading day of the earnings announcement. Earnings surprises are put into quintiles by quarter and dummy variables are defined correspondingly, with $ES1$ containing the most negative surprises. Earnings surprises are defined as the actual earnings minus the I/B/E/S provided median analyst estimate of the earnings. $ROIB_{i,[x,y]}$ are cumulative retail order imbalances from x to y where daily order imbalances are computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume), and volumes are expressed in the number of shares traded in stock i. Panel A shows the results for the sample that excludes observations with insider trades in the 10 trading days prior to earnings announcements. Panel B excludes the last trading day before earnings announcements in the independent variables, i.e., for $[x, y]$ equal to $[-10, -2]$ and $[-60, -2]$. Fuller-Battese (1974) corrected *t*-statistics are in parentheses.

Panel A: No Insiders	$[-10, -1]$	$[-10, -1]$	$[-60, -1]$	$[-60, -1]$
$ROIB_{i,[x,y]}$	0.069***	0.061***	0.096***	0.074**
	(3.51)	(3.26)	(3.18)	(2.55)
$CAR_{i,[x,y]}$	-0.023***	-0.039***	-0.006***	-0.012***
	(-5.44)	(-9.83)	(-3.39)	(-7.63)
$ES1$		-4.725***		-4.722***
		(-44.44)		(-44.35)
$ES2$		-2.230***		-2.232***
		(-22.99)		(-22.99)
$ES4$		1.618***		1.613***
		(15.98)		(15.91)
$ES5$		3.057***		3.047***
		(28.73)		(28.60)
Intercept	0.006	0.458***	0.010	0.484***
	(0.12)	(6.16)	(0.18)	(6.46)
Observations	71,105	71,105	71,105	71,105
Panel B: No Day -1	$[-10, -2]$	$[-10, -2]$	$[-60, -2]$	$[-60, -2]$
$ROIB_{i,[x,y]}$	0.053***	0.050***	0.064*	0.051*
	(2.64)	(2.61)	(1.95)	(1.65)
$CAR_{i,[x,y]}$	-0.025***	-0.041***	-0.005***	-0.012***
	(-6.41)	(-11.02)	(-3.28)	(-7.81)
$ES1$		-4.728***		-4.722***
		(-46.52)		(-46.39)
$ES2$		-2.256***		-2.258***
		(-24.67)		(-24.67)
$ES4$		1.650***		1.644***
		(17.35)		(17.28)
$ES5$		3.056***		3.046***
		(30.19)		(30.05)
Intercept	0.509***	0.484***	0.045	0.509***
	(7.13)	(6.83)	(0.87)	(7.13)
Observations	79,314	79,314	79,314	79,314

*** p<0.01, ** p<0.05, * p<0.1

Table 3.8. Predicting Post-Earnings Announcement Returns

This table reports regression results relating abnormal returns after earnings announcements to pre-announcement retail trading activity. The dependent variable is $CAR[2,61]_i$, which is the cumulative abnormal stock return over the [2,61] trading period where time 0 is the trading day of the earnings announcement. Abnormal returns are computed using the sample's equal-weighted index as the benchmark. Earnings surprises are put into quintiles by quarter and dummy variables are defined correspondingly, with $ES1$ containing the most negative surprises. Earnings surprises are defined as the actual earnings minus the I/B/E/S provided median analyst estimate of the earnings. $ROIB_{i,[x,y]}$ are cumulative retail order imbalances from x to y where daily order imbalances are computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume), where volumes are expressed in the number of shares traded in stock i. Fuller-Battese (1974) corrected t-statistics are in parentheses.

	$[-10,-1]$	$[-10,-1]$	$[-60,-1]$	$[-60,-1]$
$ROIB_{i,[x,y]}$	0.164***	0.160***	0.145**	0.141**
	(3.91)	(3.82)	(2.26)	(2.20)
$CAR_{i,[x,y]}$	-0.068***	-0.069***	-0.050***	-0.050***
	(-7.91)	(-8.02)	(-14.26)	(-14.31)
$ES1$		-0.103		-0.276
		(-0.43)		(-1.16)
$ES2$		-1.019***		-1.076***
		(-4.77)		(-5.06)
$ES4$		-0.284		-0.300
		(-1.28)		(-1.36)
$ES5$		0.912***		0.806***
		(3.82)		(3.40)
Intercept	-0.182	-0.08	0.01	0.174
	(-1.34)	(-0.45)	(0.08)	(0.99)
Observations	79,314	79,314	79,314	79,314

*** p<0.01, ** p<0.05, * p<0.1

Table 3.9. Robustness Checks on Predicting Post-Earnings Announcement Returns

This table reports regression results relating abnormal returns after earnings announcements to pre-announcement retail trading activity. The dependent variable is $CAR[2,61]_i$, which is the cumulative abnormal stock return over the [2,61] trading period where time 0 is the trading day of the earnings announcement. Earnings surprises are put into quintiles by quarter and dummy variables are defined correspondingly, with $ES1$ containing the most negative surprises. Earnings surprises are defined as the actual earnings minus the I/B/E/S provided median analyst estimate of the earnings. $ROIB_{i,[x,y]}$ are cumulative retail order imbalances from x to y where daily order imbalances are computed as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume), where volumes are expressed in the number of shares traded in stock i. Panel A excludes observations with insider trades in the 10 trading days prior to earnings announcements. Panel B excludes the last trading day before earnings announcements in the independent variables, i.e., for $[x, y]$ equal to $[-10, -2]$ and $[-60, -2]$. Fuller-Battese (1974) corrected *t*-statistics are in parentheses.

Panel A: No Insiders	$[-10,-1]$	$[-10,-1]$	$[-60,-1]$	$[-60,-1]$
$ROIB_{i,[x,y]}$	0.188***	0.184***	0.178***	0.174***
	(4.30)	(4.23)	(2.66)	(2.60)
$CAR_{i,[x,y]}$	-0.061***	-0.062***	-0.049***	-0.050***
	(-6.60)	(-6.77)	(-13.43)	(-13.55)
$ES1$		-0.249		-0.415*
		(-0.99)		(-1.66)
$ES2$		-1.049***		-1.106***
		(-4.62)		(-4.89)
$ES4$		-0.288		-0.302
		(-1.21)		(-1.28)
$ES5$		0.952***		0.859***
		(3.78)		(3.43)
Intercept	-0.079	0.44	-0.073	0.275
	(-0.54)	(0.23)	(-0.50)	(1.47)
Observations	71,105	71,105	71,105	71,105
Panel B: No Day -1	$[-10,-2]$	$[-10,-2]$	$[-60,-2]$	$[-60,-2]$
$ROIB_{i,[x,y]}$	0.164***	0.161***	0.137*	0.136*
	(3.70)	(3.64)	(1.91)	(1.88)
$CAR_{i,[x,y]}$	-0.068***	-0.069***	-0.050***	-0.050***
	(-7.91)	(-8.02)	(-14.28)	(-14.32)
$ES1$		-0.103		-0.276
		(-0.43)		(-1.16)
$ES2$		-1.018***		-1.075***
		(-4.77)		(-5.05)
$ES4$		-0.282		-0.297
		(-1.27)		(-1.34)
$ES5$		0.917***		0.810***
		(3.84)		(3.42)
Intercept	-0.176	-0.075	0.015	0.177
	(-1.30)	(-0.42)	(0.12)	(1.01)
Observations	79,314	79,314	79,314	79,314

*** p<0.01, ** p<0.05, * p<0.1

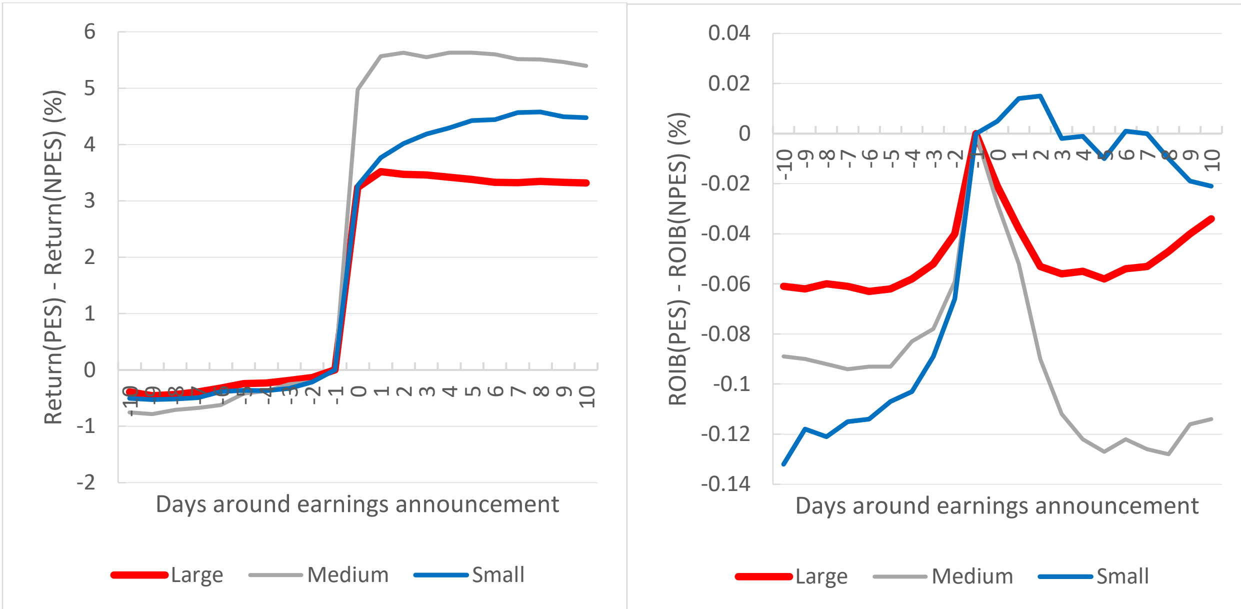

Figure 3.1. Cumulative market-adjusted returns and retail order imbalances around earnings announcements

Market-adjusted returns are defined as stock returns minus the average equal-weighted return on the stocks in the sample. Retail order imbalances (ROIB) are defined as (retail buy volume – retail sell volume)/(retail buy volume + retail sell volume), where volumes are expressed in the number of shares traded. Earnings announcements are separated into positive and nonpositive earnings surprises (PES and NPES), where positive surprises are those where the actual earnings were higher than the median analyst estimate provided by I/B/E/S. Stocks are separated into small, medium and large stocks; large firms are defined as firms in the top three deciles of market capitalization as of the end of the previous year; medium firms are those in deciles 5 to 7; small firms are those in the bottom 4 deciles. The figures encompass the 21-day event window period centered on day 0, defined as the trading day on which announcements have been made or the first subsequent trading day, if announcements were made outside of trading hours. The left figure shows the differences between average cumulative market-adjusted returns to stocks with positive and nonpositive earnings surprises, separated by market capitalization group; The right figure shows the differences between average cumulative retail order imbalances of stocks with positive and nonpositive earnings surprises, separated by market capitalization group.

Table 3.A1. Panel A results of Table 3.5 (Predicting Earnings Announcement Returns) excluding earnings announcements in Compustat that could not be matched to I/B/E/S.

	$[-10,-1]$	$[-10,-1]$	$[-60,-1]$	$[-60,-1]$
$ROIB_{i,[x,y]}$	0.058***	0.050**	0.087***	0.054*
	(2.67)	(2.43)	(2.84)	(1.87)
$CAR_{i,[x,y]}$	-0.013***	-0.034***	-0.004**	-0.013***
	(-2.98)	(-7.97)	(-2.06)	(-7.16)
$ES1$		-4.925***		-4.927***
		(-41.79)		(-41.77)
$ES2$		-2.425***		-2.427***
		(-23.19)		(-23.21)
$ES4$		1.491***		1.490***
		(13.80)		(13.79)
$ES5$		2.870***		2.863***
		(24.52)		(24.45)
Intercept	-0.023	0.661***	-0.020	0.696***
	(-0.41)	(7.17)	(-0.35)	(7.50)
Observations	66,940	66,940	66,940	66,940

*** p<0.01, ** p<0.05, * p<0.1

Table 3.A2. Panel A results of Table 3.6 (Predicting Post-Earnings Announcement Returns) excluding earnings announcements in Compustat that could not be matched to I/B/E/S.

	$[-10,-1]$	$[-10,-1]$	$[-60,-1]$	$[-60,-1]$
$ROIB_{i,[x,y]}$	0.141***	0.136***	0.149**	0.143*
	(2.96)	(2.85)	(2.03)	(1.94)
$CAR_{i,[x,y]}$	-0.075***	-0.076***	-0.046***	-0.046***
	(-7.87)	(-7.91)	(-11.58)	(-11.55)
$ES1$		0.514*		0.216
		(1.89)		(0.80)
$ES2$		-0.532**		-0.618***
		(-2.23)		(-2.60)
$ES4$		0.216		0.157
		(0.87)		(0.64)
$ES5$		1.498***		1.265***
		(5.54)		(4.74)
Intercept	0.128	0.047	0.022	0.132
	(1.09)	(0.21)	(0.10)	(0.64)
Observations	66,940	66,940	66,940	66,940

*** p<0.01, ** p<0.05, * p<0.1