Retail Short Selling and Stock Prices

ERIC K. KELLEY and PAUL C. TETLOCK*

May 2016

ABSTRACT

Using proprietary data on millions of trades by retail investors, we provide the first large-scale evidence that retail short selling predicts negative stock returns. A portfolio that mimics weekly retail shorting earns an annualized risk-adjusted return of 9%. The predictive ability of retail short selling persists after controlling for known predictors of returns, including institutional short selling. In contrast to institutional shorting, retail shorting best predicts returns in small stocks and those that are heavily bought by other retail investors. Our findings are consistent with retail short sellers having unique insights into the retail investor community and small firms' fundamentals.

* University of Tennessee and Columbia University. The authors thank the following people for their helpful comments: Brad Barber, Kent Daniel, Robin Greenwood, Cam Harvey, Charles Jones, Andy Puckett, Jay Ritter, Sorin Sorescu, Ingrid Werner, and Wei Xiong; seminar participants at Alabama, Arizona State, Columbia, Drexel, Georgia State, HEC Paris, the NY Fed, Temple, Tennessee, Texas Tech, UC San Diego, UT Austin, Washington U., and Wharton; and participants at the Alberta Frontiers in Finance, Miami Behavioral Finance and WFA meetings. The authors also thank Arizona, Columbia, and Tennessee for research support and Dow Jones and RavenPack for news data and analysis, respectively, and Dallin Alldredge for providing the corporate insider trading data. All results and interpretations are the authors' and are not endorsed by the news or retail order data providers. Please send correspondence to paul.tetlock@columbia.edu.

Researchers, regulators, and the financial press have long held short sellers under a microscope. There is now mounting empirical evidence that these important market participants are informed in the sense that they can predict stock returns (e.g., Cohen, Diether, and Malloy (2007); Boehmer, Jones, and Zhang (2008; hereafter BJZ); and Diether, Lee, and Werner (2009; hereafter DLW)). But not all short sellers are alike in their information, abilities, and constraints. Analyzing this heterogeneity can deliver insights into the nature of short sellers' information and their role in stock markets. BJZ provide broad evidence of heterogeneity in their study of short selling in NYSE stocks. They find that institutional short sellers correctly predict stock returns, while other short sellers such as retail traders do not. This latter finding appears to be consistent with the long-standing view that retail traders are poorly informed (e.g., Barber and Odean (2000)).

However, recent empirical studies challenge the stereotype that retail investors are uninformed (Surowiecki (2004), Kaniel, Saar, and Titman (2008), and Kelley and Tetlock (2013)). As BJZ note, retail short sellers in particular have some significant advantages over their institutional counterparts. Because potential retail short sellers vastly outnumber institutions, retail shorting could convey unique information distilled from diverse sources. Through their jobs and social networks, retail short sellers may naturally access firm-specific or industry-wide information that is unavailable to institutions. Moreover, as members of the retail investor community, retail short sellers could learn which stocks attract unsophisticated retail investors, a potentially informative measure of investor sentiment. As managers of their own money, retail short sellers do not suffer from principal-agent problems that plague professional arbitrageurs, who must devise investment strategies that account for clients' inflows and redemptions of capital (Shleifer and Vishny (1997), Berk and Green (2004), and Lamont and Stein (2004)). Finally, retail short sellers typically cannot use the proceeds from their trades, so their shorting is unlikely to arise from liquidity needs. Rather, the costly nature of short selling, especially for retail investors facing relatively higher stock lending fees, suggests that only those most confident in their information will trade (Diamond and Verrecchia (1987)).

In this paper, we provide the most extensive evidence to date on retail short selling. Our analysis of seven million trades originating from retail clients of dozens of discount brokerage firms reveals the first large-scale evidence that retail shorting predicts negative stock returns. A portfolio that mimics weekly retail shorting earns a risk-adjusted return of 0.68% in trading days 2 through 20 after shorting occurs, which is an annualized return of 9.08%. Most of the predictive power of retail shorting persists even after controlling for buying, selling, and short selling by institutions and buying and selling from other retail traders, as well as trading by corporate insiders. Our empirical results are consistent with the hypothesis that retail short sellers possess and act on unique information beyond that held by other investors.

On the surface, our main result contradicts BJZ's finding for retail shorts. However, these authors' short-sale data come from the New York Stock Exchange (NYSE), a venue to which retail brokers route orders usually as a last resort, which precludes the authors from making strong claims about the informativeness of these trades.[1] Our large and broad sample, in contrast, enables us to identify novel patterns in return predictability from retail shorting and show that our results are not attributable to selection bias. The only other study of retail short selling is Gamble and Xu (2013), which finds that overall retail shorting does not predict returns. However, this evidence is confined to orders from a single retail broker from 1991 to 1996 and contains fewer than two short sales per stock per year.

While our main contribution highlights that retail short sellers, like institutional short sellers, correctly anticipate stock returns, we also identify three important ways in which these types of traders differ. First, we demonstrate that retail short sales are much better predictors of negative returns in small stocks than in large stocks. In contrast, institutional short sales are similarly informative in large and small stocks, as shown in DLW.[2] Retail investors' knowledge

[1] BJZ show that fewer than 2% short sale orders at the NYSE come from retail investors. Battalio and Loughran (2007) point out that the NYSE receives retail orders only if a retail broker cannot profitably internalize them or route them to market centers that pay for the receipt of order flow.
[2] This finding for institutional short sellers could be specific to the 2005 to 2007 period in which RegSHO data are available. In a study of short sales routed to the NYSE from 2000 to 2004, BJZ find that institutional short sales are somewhat better predictors of negative returns in small stocks.

of small firms could reflect serendipitous information arrival, whereas institutional investors choose to investigate relatively large and liquid firms in which they may take sizable positions.

Second, we provide evidence suggesting that retail short sellers uniquely benefit from their knowledge of the retail investor community. Retail shorting is most predictive of returns within the subset of stocks that other retail investors have bought most heavily. We find no evidence of a similar result within the subset of stocks institutions have bought heavily, as measured with trades from (mainly) mutual funds in the Ancerno database. Together, these findings suggest that retail short sellers identify and exploit excessively bullish *retail* investor sentiment. In sharp contrast, the extent to which institutional short selling predicts returns does not depend on past retail buying, but it does depend on past buying from other institutions, which is consistent with Arif, Ben-Rephael, and Lee (2015). Thus, institutional short sellers appear to understand the forces driving institutional buying activity, whereas retail short sellers know more about retail buying behavior.

Third, we provide evidence that retail and institutional short sellers' each trade on unique firm-specific information. Specifically, we test whether each group of short sellers can predict how markets respond to news events, including earnings announcements, in the week following shorting activity. While both types of shorting are much stronger predictors of returns in periods with news events as compared to returns in nonnews periods, retail shorting is an especially strong predictor of returns arising around earnings announcements, whereas institutional shorting is an excellent predictor of news generated by stock analysts. These findings shed light on the nature and uniqueness of both groups of short sellers' information.

Beyond its contribution to the literature on short selling, our study also contributes to research on retail investors in general. The retail investors who short sell stocks could be quite different from other retail investors, such as those studied by Barber and Odean (2000), and in some ways resembles institutional investors. Retail investors who short sell stocks could be more sophisticated than typical retail traders, most of whom do not have margin accounts that enable short sales (Gamble and Xu (2013)). Our evidence shows that some retail investors are informed, bolstering evidence in recent studies by Kaniel, Liu, Saar, and Titman (2012) and Kelley and

Tetlock (2013), and highlights the importance of recognizing heterogeneity within investor subgroups such as retail traders that many researchers treat as homogenous.

I. Data

Our sample, drawn from the proprietary dataset of Kelley and Tetlock (2013), is particularly well-suited for the study of short selling by retail investors. This dataset covers an estimated one-third of self-directed retail buying and selling in U.S. stocks from February 26, 2003 through December 31, 2007. This dataset includes over 225 million orders, amounting to $2.60 trillion, executed by two related over-the-counter market centers. One market center primarily deals in NYSE and Amex securities, while the other primarily deals in NASDAQ securities. Orders originate from retail clients of dozens of different brokers. SEC Rule 11Ac1-6 (now Rule 606 under Regulation National Market Systems) reports reveal that most large retail brokers, including four of the top five online brokerages in 2005, route significant order flow to these market centers during our sample period.

The order data include codes identifying retail orders and differentiating short sales from long sales. The sample includes nearly seven million executed retail short sale orders, representing $144 billion in dollar volume.[3] Short sales account for 5.54% (9.66%) of the dollar volume of all executed orders (executed sell orders). The average trade size for short sales is $20,870, which is larger than the average size of all trades in the sample ($11,566) as well as average trade sizes in the retail trading samples of Barber and Odean (2000) and Kaniel, Saar, and Titman (2008). Analyzing the Barber and Odean (2000) discount broker data from 1991 to 1996, Gamble and Xu (2013) report that 13% of all investors—and 24% of those with margin accounts—conduct short sales. They also document that short sellers trade four times as often as long-only investors, and short sellers' stock holdings are more than twice as large. These differences underscore the importance of studying short sellers separately.

[3] Of these executed orders, $103 billion are marketable orders and $41 billion are nonmarketable limit orders at the time of order placement. The data also contain over four million orders that do not execute. In our analysis, we aggregate all executed short sale orders across order types. Separate analyses of executed marketable orders, executed nonmarketable orders, and all nonmarketable orders yield quantitatively similar results.

We commence our empirical analysis with all common stocks listed on the NYSE, AMEX, or NASDAQ exchanges from February 26, 2003 to December 31, 2007. To minimize market microstructure biases associated with highly illiquid stocks, we exclude stocks with closing prices less than one dollar in the prior quarter. We also require nonzero retail shorting in the prior quarter to eliminate stocks that retail investors are unable to short. Because of this retail shorting filter, the final sample spans June 4, 2003 through December 31, 2007 and contains an average of 3,376 stocks per day.

Throughout the paper, we aggregate retail short-selling activity across five-day windows and use weekly variables as the basis for our analysis as in BJZ.[4] Our main variable is *RtlShort*, defined as shares shorted by retail investors scaled by total CRSP share volume. We primarily analyze shorting scaled by total share volume, again following BJZ, but we also consider scaling by retail share volume (*RtlShortFrac*) as in Kelley and Tetlock (2013) and by shares outstanding (*RtlShortShrout*). We measure other aspects of retail trading using the variables *RtlTrade*, which is retail trading scaled by total volume, and *RtlBuy*, which is shares bought minus long positions sold (imbalance) scaled by volume. Table I provides definitions for all variables used in this study.

We also compare shorting by retail investors to shorting by institutional traders. Our proxy for institutional shorting is based on short selling data reported by all stock exchanges pursuant to Regulation SHO (RegSHO) from January 3, 2005 to July 6, 2007, about half our sample period. These data include all executed short sales and are used by a number of other studies such as DLW. We define the variable *AllShort* as total shares shorted over a five-day window scaled by total CRSP share volume, analogous to the *RtlShort* definition. We define an institutional shorting proxy, *InstShort*, as *AllShort* minus *RtlShort*. Because our retail trading dataset is not exhaustive, *InstShort* still contains some retail transactions, making both *RtlShort* and *InstShort* imperfect measures.

[4] The weekly horizon is short enough to precisely capture a shock to short selling but also long enough for the variable to be nonzero for a large fraction of observations. Measuring retail shorting over a weekly horizon results in about half of the observations being nonzero. We also consider a daily version of the retail shorting variable and report in Section III.A below and the Internet Appendix that our main results are similar with this definition.

[Insert Table I here.]

Table II, Panel A provides statistics for the daily cross-sectional distribution, averaged across all days in the sample. The row for *RtlShort* shows that retail shorting is a small percentage (0.13%, on average) of overall trading. This result arises for three reasons: 1) shorts are a small percentage of retail trades (5.5% in our data); 2) retail trading is a small percentage of all trading (3% to 12% estimated from retail broker disclosures); and 3) our sample represents a fraction of retail trading (1/3 estimated from SEC Rule 606 reports). Thus, if retail trading is 7% of total trading, our retail shorting data would account for 5.5% x 7% x 1/3 = 0.13% of total trading, consistent with the average value of *RtlShort*. In a typical week, roughly half of the stocks in the final sample have retail shorting activity, while the other half do not.

[Insert Table II here.]

Table II, Panel B reports average daily cross-sectional correlations among our main variables. When computing these correlations and estimating regressions, we apply log transformations to variables with high skewness to minimize the influence of outliers.[5] Notably, the three retail shorting variables have average pairwise correlations exceeding 0.8. Our main retail shorting measure (Ln(*RtlShort*)) has positive correlations of 0.37 with Ln(*Turnover*) and 0.24 with Ln(*ShortInt*), two known predictors of the cross section of stock returns. The next biggest correlation is between retail shorting and *Beta* (0.19), implying that adjusting for market risk is important in evaluating return predictability from retail shorting. Retail short sellers tend to act as contrarians; the correlations with weekly, monthly, and yearly returns (*Ret*[-4,0], *Ret*[-25,-5], and *Ret*[-251,-26], respectively) are positive, and the correlation with book-to-market (Ln(*BM*)) is negative, though most of these correlations are weaker than 0.1.

Retail short selling has a modest positive correlation of 0.15 with Ln(*AllShort*). A common component in shorting remains after subtracting retail shorting from the RegSHO variable as the correlation between Ln(*RtlShort*) and Ln(*InstShort*) is 0.12. However, the very high (0.99) correlation between Ln(*AllShort*) and Ln(*InstShort*) reflects the fact retail shorting is

[5] To transform a variable that sometimes equals zero, we add a constant *c* to the variable before taking the natural log. Each day we set *c* to be the 10th percentile of the raw variable conditional on the raw variable exceeding zero.

a very small fraction of total shorting, as noted in prior studies such as BJZ. Therefore one can reasonably interpret evidence that total short selling predicts returns (e.g., Senchack and Starks (1993); Cohen, Diether, and Malloy (2007); and DLW) as evidence that nonretail—i.e., "institutional"—short sellers are informed. Finally, retail shorting has only a weak correlation of 0.03 with institutional shorting inferred from the change in short interest, ΔLn(*ShortInt*).

II. Portfolios that Mimic Retail Short Selling

We first analyze whether retail short selling predicts stock returns. Under the information hypothesis, retail short sellers trade on genuine information about stocks' fundamental values that is not fully incorporated in stock market prices. As such, retail shorting will predict negative returns. Alternatively, the sentiment hypothesis is that retail shorts act on pessimistic investor sentiment, and shorting activity will predict positive returns. If sentiment-driven underpricing is short-lived, this positive relationship should arise soon after shorting activity. However, if sentiment is persistent, retail shorting could be followed initially by negative returns prior to an eventual reversion to fundamentals. Thus, we analyze return predictability over both short (one week to one month) and long (up to one year) horizons in our main tests. We also provide direct evidence on the persistence of retail shorting and the persistence of returns around the occurrence of retail shorting.

Our initial analysis features calendar-time portfolios whose returns represent the performance of stocks with different degrees of retail shorting. We construct portfolios based on retail short selling by sorting stocks into five "quintiles" each day based on weekly *RtlShort*. Quintile 1 actually comprises stocks with no weekly retail shorting, which represents roughly half of the stocks in the sample. We assign equal numbers of stocks with positive retail shorting to quintiles 2 through 5, with quintile 2 containing stocks with the lowest positive shorting and quintile 5 containing stocks with the most shorting.

The daily calendar-time return of each quintile portfolio is a weighted average of individual stocks' returns, where day t weights are based on stocks' gross returns on day $t - 1$. Asparouhova, Bessembinder, and Kalcheva (2010) show that the expected return of this gross-return weighted (GRW) portfolio is the same as that of an equal-weighted portfolio, except that it corrects for the bid-ask bounce bias described by Blume and Stambaugh (1983).

Following BJZ, we rebalance portfolios daily according to stocks' values of weekly shorting. A portfolio with a one-day horizon rebalances up to 100% of the portfolio each day, depending on whether stocks' values of weekly shorting have changed sufficiently to affect their quintile rankings. Our analysis focuses on portfolios with horizons beyond one day, which represent combinations of portfolios formed on adjacent days following the method of Jegadeesh and Titman (1993). The return on calendar day t of quintile portfolio $q \in \{1, 2, 3, 4, 5\}$ with an $[x,y]$-day horizon is the equal-weighted average of the returns on day t of the quintile q portfolios formed on days $t - x$ through $t - y$. In this method, no more than $1/(y - x + 1)$ of the portfolio is rebalanced on each day. For example, no more than 1/19 of a quintile 5 portfolio with a [2,20]-day horizon is rebalanced each day to ensure that the stocks in the portfolio are those with the highest values of weekly retail shorting between 2 and 20 days ago.

We compute the excess return on a long-short spread portfolio as the return of the top minus the return of the bottom quintile portfolio. Each quintile portfolio's excess return is its daily return minus the risk-free rate at the end of the prior day. Each portfolio's alpha is the intercept from a time-series regression of its daily excess returns on the three Fama and French (1993) daily return factors, which are based on the market, firm size, and book-to-market ratio.

Panel A of Table III reports the average daily GRW returns of five portfolios sorted by retail shorting scaled by volume (*RtlShort*) at horizons up to one quarter after portfolio formation. The spread portfolio return in the last row equals the return of heavily shorted stocks (quintile 5) minus the return of stocks with no shorting (quintile 0). The left side of Panel A shows portfolios' three-factor alphas, while the right side shows portfolios' excess returns. Panel B displays the three-factor loadings of the five retail shorting portfolios and the spread portfolio, along with the average number of firms in these portfolios at the time of portfolio formation.

[Insert Table III here.]

The main result in Table III is that retail shorting predicts negative returns at horizons ranging from daily to annual, consistent with the information hypothesis. The three-factor alpha on the spread portfolio indicates that risk-adjusted returns are significantly negative in each of the first three months (days [2,20], [21,40], and [41,60]) after portfolio formation. Daily (annualized) alphas of the spread portfolios are -0.036%, -0.031%, -0.019% (-9.1%, -7.9%, -4.7%) in the first, second, and third months, respectively. The annualized alphas in days [2,20] decline monotonically from 2.9% to -6.2% from the bottom to the top retail shorting quintile. Thus, both the high-shorting and no-shorting groups contribute to the spread portfolio alpha, but most of the alpha comes from the low returns of stocks with high levels of retail shorting. This result ostensibly differs from BJZ and Boehmer, Huszar, and Jordan's (2010) findings of relatively stronger return predictability in stocks with light shorting. Rather, it more closely resembles DLW's finding of return predictability in both lightly and heavily shorted stocks. In our data, the strongest predictability occurs on day 1, when the annualized spread alpha is an impressive -16.9% = 252 * (-0.067%). However, because microstructure biases could affect returns on day 1, we exclude this day in our main tests, resulting in conservative estimates of predictability.

Properly adjusting for risk is important when analyzing the performance of the retail shorting portfolios. Panel B shows that market risk increases significantly across the retail shorting portfolios. Highly shorted stocks have market betas (*MKT*) of 1.068, as compared to betas of 0.794 for stocks with no shorting—a substantial difference of 0.274. Size factor loadings (*SMB*) also increase significantly with retail shorting, with highly shorted stocks having 0.335 higher exposures to the small stock factor than stocks without shorting. The value factor loadings (*HML*) are similar for all retail shorting portfolios.

Exposure to market risk decreases the difference in raw returns between extreme shorting portfolios relative to the difference in risk-adjusted returns. The reason is that retail short sellers tend to short stocks with high market betas and the realized return of the market factor was highly positive during our sample period.[6] The right side of Panel A shows that the raw excess returns of retail shorting portfolios are less striking than the alphas, though they are still economically meaningful. The annualized day-[2,20] predictability in raw excess returns is 252 * -0.023% = -5.9%, as compared to the corresponding alpha of -9.1%.

We repeat our calendar-time analysis using portfolios with value weights instead of gross-return weights, which mimic equal weights. Table IA.I of the Internet Appendix shows that the three-factor alphas for the value-weighted spread portfolio are negative at -3.0% annualized but insignificantly different from zero. The difference between the value- and equal-weighted results implies that retail short sellers are better able to pick stocks among small stocks.[7] Indeed, when we partition the sample into NYSE market equity quintiles, spread portfolio alphas are largest in the bottom quintile and statistically significant in all but the largest quintile of stocks, which effectively determine value-weighted returns. We report these results in Table IA.II of the Internet Appendix.[8] We further explore the interaction between retail short selling and firm size in the multivariate regressions in Section III.

[Insert Figure 1 here.]

Figure 1 summarizes the cumulative risk-adjusted returns of the retail shorting portfolios with gross-return weights before and after portfolio formation. In the month before formation, the typical stock in the high retail shorting portfolios experiences positive abnormal returns exceeding 3%, suggesting that retail short sellers look for shorting opportunities among stocks with high recent returns. In the three months after formation, stocks with high retail shorting underperform those with low retail shorting by 1.8%. The post-formation trajectories of the portfolios suggest that this underperformance decays over time but does not reverse.

[6] This tendency to short high beta stocks is not unique to retail investors. Table II Panel B reveals a similar positive correlation between *InstShort* and *Beta* of 0.279, consistent with the positive relation between total short interest and beta reported by Asquith, Pathak, and Ritter (2005).
[7] BJZ find that retail short selling does not predict returns in value-weighted portfolios and cross-sectional regressions that give equal weight to each stock. The latter finding differs from our results. Our findings for value- and equal-weighted portfolios sorted on retail shorting resemble those from Asquith, Pathak, and Ritter's (2005) analysis of portfolios sorted by short interest, which is a significant predictor of only equal-weighted returns.
[8] Within size quintiles, switching from gross return weights to value weights has a negligible impact on the results.

The results in Table III and Figure 1 are inconsistent with the hypothesis that retail shorting is a proxy for temporarily pessimistic sentiment, which should predict positive risk-adjusted returns. The evidence is also inconsistent with the more subtle hypothesis in which retail shorting is a proxy for persistent sentiment that has a long-lived impact on prices but eventually reverses: the negative return of the spread portfolio persists beyond three months to days [61,252] in which the annualized alpha is -5.5%. A one-year horizon is long relative to the horizons of short sellers: BJZ estimate the typical short seller's horizon to be 37 trading days, and Gamble and Xu (2013) report similar estimates of retail short sellers' horizons. Furthermore, Table II, Panel B shows that retail shorting is positively correlated with past returns at the weekly, monthly, and annual horizons, indicating that the typical retail short seller acts contrary to past returns.[9] Moreover, the average autocorrelation of retail shorting is only 0.36 (0.18) at the weekly (quarterly) frequency, casting further doubt on the persistent sentiment theory.

III. The Informational Content of Retail Short Sales

The portfolio tests in Table III demonstrate that retail short selling predicts stock returns. For the five portfolios in Table III, the relation between average risk-adjusted return and average log retail shorting is almost exactly linear (R^2 = 0.99), implying that a linear regression specification is reasonable. In our main analysis, we estimate cross-sectional return regressions in the spirit of Fama and MacBeth (1973) to control simultaneously for numerous predictors of stock returns.

A. Is Retail Short Sellers' Information Unique?

A key question is whether retail short sellers identify and trade on information that could not be gleaned from other investors' actions or publicly observable signals. The portfolio tests in Table III are based on univariate sorts that disregard other predictors of stock returns and could

[9] To further illustrate the contrarian nature of retail short selling, we compute calendar-time returns as in Table III for the three months prior to the portfolio formation week. The annualized three-factor alpha for the spread portfolio in days [-64,-5] is 21.4%, which is statistically different from zero at the 1% level.

therefore reflect omitted variable bias. Several variables that could predict returns are correlated with retail shorting. Potential confounds include the trades of other investors with related information and firm characteristics that are related to expected stock returns. For example, unobserved variation in institutional shorting could explain the relation between retail shorting and future returns observed in the portfolio tests.

To test this possibility, we estimate multivariate regressions of future returns on retail shorting, institutional shorting, and myriad control variables. We focus on returns at the monthly horizon to match retail short sellers' likely horizons. The dependent variable is cumulative abnormal returns (*CAR*[2,20]) with the benchmark based on each firm's Fama and French (1993) three-factor loadings (*MKT*, *SMB*, and *HML* betas) measured with daily data from the prior year. In Table IA.IV of the Internet Appendix, we report similar results using specifications in which the dependent variable is raw compound returns (*Ret*[2,20]), and independent variables include *Beta*, log firm size (Ln(*Size*)), and log ratio of book equity to market equity (Ln(*BM*)) as in Fama and French (1992). Consistent with the linear relation in the portfolio tests, the main independent variable is Ln(*RtlShort*) with a weekly shorting window from day -4 to day 0 ending one day before the start of the return window.

The regression specifications include control variables that predict stock returns according to prior research. The first set of control variables is based on public information, such as firm characteristics and past returns. The firm characteristics are logarithms of prior-week turnover (Ln(*Turnover*)) and prior-month idiosyncratic volatility (Ln(*IdioVol*)), similar to Gervais, Kaniel, and Mingelgrin (2001) and Ang et al. (2006), respectively. The past return variables are prior one-week (*Ret*[-4,0]), one-month (*Ret*[-25,-5]), and one-year stock returns (*Ret*[-251,-26]) as in Gutierrez and Kelley (2008) and Jegadeesh and Titman (1993). These variables could be important because retail shorting tends to be contrarian.

The second set of control variables represents trading by other investors. We measure other short sellers' positions using the most recently reported level of short interest decomposed into its prior level (Ln(Lag(*ShortInt*))) and its most recent change (ΔLn(*ShortInt*)), following Figlewski (1981) and Senchack and Starks (1993). We compute these variables from the most recent values of short interest reported by trading exchanges, which report twice per month during our sample period, and scale them by shares outstanding. We interpret these short interest variables as crude proxies for institutional shorting because all regression specifications already include retail shorting and the vast majority of short interest comes from institutions. Because short interest is reported twice per month and represents the stock not the flow of shorting activity, we employ the more comparable institutional shorting proxy from RegSHO short sale data, which covers about half of our time period, in the next subsection. We also measure net buying by retail investors (*RtlBuy*) in our database as buys minus long sales, scaled by volume to be consistent with the scaling of retail shorting.

We standardize all independent variables each day to facilitate comparison of coefficients. We conduct cross-sectional regressions on each day and draw inferences from the time series of coefficient estimates. As in our calendar-time portfolio analysis, we weight observations by gross stock returns, which is similar to using equal weights. The point estimate of each coefficient is the time-series average of daily regression coefficients; and the standard error comes from the Newey-West (1987) formula with 19 lags to match the horizon of the return observations.

[Insert Table IV here.]

Table IV reports estimates from several regression specifications. The first column shows the univariate impact of Ln(*RtlShort*). The standardized coefficient on log retail shorting is -0.27%. We compare this coefficient to the spread portfolio return in Table III. Based on the distribution reported in Table II.A, the change in Ln(*RtlShort*) shorting from the top to the bottom quintile portfolio is equivalent to 2.73 ((-5.08 − -9.15) / 1.49) standard deviations. Multiplying the standardized coefficient in Table IV by 2.73 standard deviations yields -0.74% (-0.271% * 2.73) as an estimate of the change in abnormal return predicted by retail shorting, which is very close to the cumulative days-[2,20] return of -0.68% (-0.036% per day * 19 days) in the portfolio tests. Both magnitudes are consistent with an annualized risk-adjusted returns between 9% and 10%. The *t*-statistic of -7.39 on the retail shorting coefficient indicates that we can easily reject the hypothesis that retail shorting does not predict returns at the 1% level.

The regression shown in the second column of Table IV adds control variables for public information. If retail short selling does not predict returns in this specification, the interpretation would be retail traders are simply shorting based on useful public information—a sign of sophistication, but not a sign of unique information. The magnitude of the retail shorting coefficient in this specification is slightly lower at -0.234%, though it remains strongly statistically significant. The slightly lower coefficient magnitude suggests that retail short sellers trade on public information, but this is not their primary source of information. The modest negative coefficients on turnover and past weekly returns are two sources of public information exploited by retail short sellers. Because retail shorting increases with turnover and weekly returns, controlling for these variables decreases the predictive coefficient on retail shorting. Controlling for one-year price momentum partially offsets this decrease because momentum predicts positive returns and short sellers are contrarian.

The regressions in the third and fourth columns add control variables for the activities of other traders. The third regression examines whether retail shorting conveys information beyond that in other short sellers' positions, as measured by short interest and its change. The highly significant coefficient of -0.185% on Ln(*RtlShort*) suggests that the vast majority of retail short sellers' information is orthogonal to that in publicly observable short interest. Still, there is some overlap in information judging by the reduction in the coefficient from -0.234% to -0.185%. Both measures of shorting are highly significant predictors of negative returns. The measure most comparable to retail short sales is the change in log short interest, which has a standardized coefficient of -0.105% that is somewhat lower than that of retail shorting.[10]

The fourth regression controls for *RtlBuy*, which reflects the buying and selling of long positions by retail investors. We can evaluate whether our main result on retail short selling is just another manifestation of the finding that net retail buying predicts positive stock returns (e.g., Kaniel, Saar, and Titman (2008) and Kelley and Tetlock (2013)). In column four, the coefficient on log retail shorting remains robust at -0.236% and highly statistically significant.

[10] The coefficient on the level of total short interest is quite large at -0.47%, but one cannot make direct comparisons without data on the level of retail short interest.

Comparing columns two and four, one sees that controlling for net retail buying has a negligible impact on the coefficient for retail shorting. The reason is that the correlation between net retail buying and log retail shorting is just 0.052, as shown in Table II.B. Consistent with prior studies, we find that net retail buying (*RtlBuy*) has a strong positive coefficient in predicting monthly returns. The natural interpretation is that retail short sellers possess information that is distinct from that of other retail traders. Short sellers must know enough about the investing process to be able to open a margin account, submit the necessary paperwork to gain permission to short stocks, and execute a short sale—all signs of sophistication. Moreover, unlike traders with long positions, short sellers must be sufficiently confident in their beliefs to be willing to forego interest on collateral and incur risks of unbounded losses.[11]

Next we use interaction variables to examine whether retail short sellers specialize in particular stocks, such as small or large stocks. In the last regression in Table IV, we include the indicator variable *SizeQuint* and its interaction with retail shorting (Ln(*RtlShort*) * *SizeQuint*) as a regressor. We set the variable *SizeQuint* to -2, 1, 0, 1, or 2 according to the firm's size quintile within the NYSE size distribution, so that *SizeQuint* = 2 for the largest firms.[12] The interaction coefficient between retail shorting and size is positive (0.031%) and marginally significant at the 5% level. The main effects of retail shorting and size (*SizeQuint*) are also significantly negative. The main effect of retail shorting represents the predictive ability of retail shorting for firms in NYSE size quintile 3—that is, conditional on *SizeQuint* = 0. The estimates of this main coefficient and the size interaction coefficient in column one show that the predictive ability of retail shorting ranges from just -0.085% (-0.146 + 2*0.031) in the top size quintile up to -0.207% (-0.146 – 2*0.031) in the bottom size quintile.

The negative size interaction coefficient has a natural interpretation. As argued in Kaniel, Liu, Saar, and Titman (2012), retail investors could serendipitously observe valuable information in their day-to-day activities. In contrast, institutions choose to expend resources gathering

[11] We show in Table IA.V of the Internet Appendix that long sales do not predict future returns.
[12] In this and subsequent specifications with interactions between shorting and size, we standardize all shorting variables within each size quintile on each day.

information only when the benefit exceeds its cost, implying that information acquisition is more common in large stocks in which traders can take large positions. Thus, retail short sellers who are simply endowed with private information or expertise face less competition from large institutional traders in small stocks, allowing them to attain higher returns. As other proxies for competition in information gathering, we consider the number of analysts providing earnings forecasts (*Analysts*) and the number of firm-specific news stories in the prior quarter (*MediaCvg*). In addition, we consider the *RtlTrade*, the fraction of trading volume attributable to retail traders, as an inverse proxy for competition for information from other traders. All three proxies exhibit correlations with *Size* exceeding 0.7, as shown in Table II, Panel B. In Table IA.III of the Internet Appendix, we show that the estimated interaction coefficients between retail shorting and these variables are qualitatively similar to the estimated size interaction coefficient.

Tables IA.IV and IA.V of the Internet Appendix shows that the findings from Table IV are robust in five additional ways. First, the results are qualitatively similar when we use raw returns instead of abnormal returns as the dependent variable. The magnitudes of the retail shorting coefficients are slightly smaller in these specifications because the characteristic-based model does a slightly better job of explaining returns than the corresponding factor model. Second, using alternative scaling of retail shorting—by shares outstanding or retail trading volume instead of total volume—has little impact on the results. Third, controlling for retail long sales scaled by volume (*RtlSell*) has almost no impact on the results. Fourth, our results are similar for stocks listed on the NYSE and NASDAQ exchanges. Fifth, a daily version of the variable *RtlShort* is also negatively related to future returns.

B. Contrasting Retail and Institutional Short Sales

Having established that retail shorting predicts returns, we now investigate the relation between retail and institutional short sellers' information. Here we use our institutional shorting measure (*InstShort*), which is based on RegSHO short sales and is constructed to be directly comparable to our main retail shorting measure (*RtlShort*). Because the *InstShort* measure is only available from January 3, 2005 to July 6, 2007, about half our sample period, we confine this analysis to the period in which RegSHO data are available, resulting in a moderate loss of power. To isolate the impact of changing the sample period, we estimate the regression from the last column in Table IV in two subsamples: the pre-RegSHO period and the RegSHO period. The first two columns in Table V reveal that the coefficients are quite similar across the two sample periods, indicating that return predictability is stable during the full sample. In particular, the coefficients on retail shorting, retail shorting interacted with size, short interest, and change in short interest are practically indistinguishable in the two periods.

[Insert Table V here.]

The third regression in Table V includes weekly institutional short sales (*InstShort*). This regression provides a test of whether institutional shorting subsumes the explanatory power of retail shorting. If so, the interpretation would be that retail short sellers, while predictive of returns, are not uniquely informed about stocks and thus play no special role in informing market prices. Although retail shorting predicts returns after controlling for short interest and its change, we do not yet know the impact of controlling for weekly institutional short sales.

The third regression in Table V directly addresses this critique by including *InstShort* and its interaction with *SizeQuint* as independent variables. Including these variables reduces the main coefficient on retail shorting by 12%, from -0.148 to -0.131, and increases the interaction coefficient between retail shorting and size by 26%, from 0.040 to 0.050. Both retail shorting coefficients in column two are economically large terms and statistically significant at the 1% level. This result is consistent with the low (0.12) average cross-sectional correlation between *RtlShort* and *InstShort* in Table II.B. These findings show that retail short sellers primarily trade on independent information.

The third regression also confirms prior findings that institutional short sales predict negative returns. The estimated coefficient on *InstShort* is significant at -0.090%. The difference between the direct effects of *InstShort* and *RtlShort* is not statistically significant. Interestingly, Table V Model 3 shows that the coefficient on the interaction between institutional shorting and firm size (*InstShort* * *SizeQuint*) is marginally significant and negative at -0.041%. Thus, retail and institutional shorting exhibit interactions with firm size that have opposite signs. These interaction coefficients are significantly different with a *t*-statistic of 3.34. The point estimates indicate that retail shorting is the better predictor of returns for firms in NYSE size quintiles 1, 2, and 3, whereas institutional shorting is the better predictor for firms in size quintiles 4 and 5. Most firms in quintiles 4 and 5 are members of either the S&P 500 Index or the Russell 1000 Index (or both). Whereas institutions expend considerable resources actively researching large companies, most retail investors do not have such budgets. Retail investors, on the other hand, could be endowed with diverse information that in aggregate informs smaller firms' prices. Even so, the point estimates of predictability from both retail and institutional shorting are negative in all size quintiles.

To better assess the economic and statistical differences between retail and institutional shorting, we consider firms in the smallest and largest NYSE size quintiles. Based on the direct and interaction coefficients, for firms in *SizeQuint* = -2 (smallest), the coefficients on retail and institutional shorting are -0.231 and -0.008, respectively. The *t*-statistic of the difference in coefficients is -3.14. In contrast, for the largest NYSE size quintile (*SizeQuint* = +2), the retail and institutional shorting coefficients are -0.031 and -0.172; and the difference is significant with a *t*-statistic of 2.09.

C. The Nature of Retail Shorts' Information

The evidence in the prior two subsections is consistent with the hypothesis that retail short sellers possess unique information about stocks' true values. That is, even after controlling for the information in publicly observable variables and other investors' trades, including short sales, retail short selling remains a robust predictor of risk-adjusted stock returns. We now consider the nature of this unique information. On one hand, retail short sellers could use their superior understanding of firm values to exploit uninformed decisions of other traders. They could trade against unduly optimistic investor sentiment and gain from subsequent negative stock returns. On the other hand, retail short sellers could be privy to firm-specific information before prices fully incorporate it. We now refine our analysis to explore these nonexclusive possibilities.

C.1. Interactions with Other Traders

We consider two groups of traders with which retail short sellers interact: other retail traders and institutions. Using small trade buying imbalance as a proxy for net retail buying, Barber, Odean, and Zhu (2009) link persistent retail buying with negative subsequent stock returns. Hvidkjaer (2008) offers a similar interpretation in his study of small trade imbalance. Likewise, Coval and Stafford (2007) and Lou (2012) show that flow-driven net purchases by mutual funds are negatively related to future returns. We directly measure net retail buying from our proprietary dataset (*RtlBuy*). Note that this variable excludes short sales. We measure institutional buying using buy orders minus sell orders scaled by total volume in the Ancerno database, which includes orders mainly from mutual funds and some orders from pension funds. We focus on how these two net buying variables interact with retail short selling, though we also interact each with institutional short selling to contrast the informational roles of different groups of short sellers. We do not focus on the direct effects of the net buying measures because Table IV shows that controlling for retail imbalance does not materially reduce the retail shorting coefficient and Arif, Ben-Rephael, and Lee (2015) show that institutional net buying actually predicts negative abnormal returns.

For our tests, we create two variables (*RtlBuyQuint* and *InstBuyQuint*) to represent the quintiles of net retail buying (*RtlBuy*) and net institutional buying. We initially align the timing of retail and institutional net buying with that of the weekly shorting variables by measuring net buying over days [-4,0]. On each day, we assign each stock a value of -2, -1, 0, 1, or 2 for *RtlBuyQuint* and *InstBuyQuint* according to its quintile rankings of net retail and institutional buying, respectively. The first regression specification in Table VI spans the full sample and includes interactions between these quintile variables and retail shorting, as well as the direct effects of the quintile variables. The second specification restricts the sample to the RegSHO period and includes interactions with the institutional shorting variable as well. Specifications also include independent variables from the models in Table V. Each shorting interaction coefficient measures how a group of short sellers' ability to predict returns depends on the level of net buying by other traders.

[Insert Table VI here.]

The estimates in the first two columns of Table VI show that retail and institutional short sales interact quite differently with net buying by retail and institutional investors. In the second specification, the interaction between retail shorting and *RtlBuyQuint* is -0.060%, as compared to the institutional shorting interaction with *RtlBuyQuint* of just -0.012%. The former interaction is highly statistically significant, while the latter interaction is within one standard error of zero. The difference in the coefficients is marginally statistically significant with a *t*-statistic of -1.94. By combining the interaction coefficient with the direct effect of retail shorting, we estimate that the standardized predictive coefficient of retail shorting ranges from -0.247% in the top quintile of net retail buying to just -0.006% in the bottom quintile. Thus, retail shorting is a very strong predictor of returns in stocks that are heavily purchased by other retail investors.[13] The similarity between the first two specifications shows that these coefficients are relatively stable throughout the sample. A natural interpretation is that retail short sellers have insights into the motives behind other retail investors' buying activity—for example, whether buying is based on genuine information or unjustified optimism. Such insights could come from encounters with others in the retail investor community. The weak *InstShort* * *RtlBuyQuint* interaction in the second model could reflect the fact that institutions have difficulty distinguishing whether retail buying is driven by information or sentiment. Alternatively, institutions could decide not to trade against retail sentiment because they prefer to hold stocks that attract retail flows (Solomon, Soltes, and Sosyura (2014)).

The positive and significant interaction between *RtlShort* and *InstBuyQuint* shows that retail shorting is actually a worse predictor of returns in stocks that are heavily bought by institutions. This result could arise from adverse selection in executed retail short sales, as

[13] The size interaction coefficients in the third regression reinforce the earlier interpretation that retail (institutional) short selling is more informative in small (large) stocks.

informed institutional buyers could pick off some limit orders as described in Linnainmaa (2010). However, institutional shorting is a better predictor of returns in stocks that are heavily bought by other institutions, as shown by the negative interaction between *InstShort* and *InstBuyQuint*. Although this interaction is only marginally statistically significant at the 5% level (*p*-value = 0.057), its economic magnitude is substantial. The predictive coefficient of *InstShort* ranges from -0.126% in the top quintile of institutional buying to -0.031% in the bottom quintile. This evidence suggests that much of institutional short sellers' informational advantage comes from their ability to interpret buying by other institutions. For example, they could be able to discern whether buying is based on novel information about a stock or just inflows to mutual funds used to augment funds' existing stock positions, as suggested by Arif, Ben-Rephael, and Lee (2015).[14] Retail short sellers do not seem to possess the same advantage; the difference between the two interaction coefficients is significantly negative (*t* = -2.48).

We next consider the timing of the net buying variables. The third and fourth specifications incorporate net buying over the prior month, i.e., days [-25,-5] as opposed to contemporaneous buying in days [-4,0]. The coefficients in these models are quite similar to those in the previous specifications. Notably, the coefficient on *RtlShort* is negative and highly significant and its interaction with *RtlBuy*[-25,5] is negative and significant as well. The predictive coefficient on *RtlShort* ranges from -0.244% in the top quintile of retail buying to approximately zero in the bottom quintile of retail buying. There is no significant pattern across institutional buying quintiles. The predictive coefficient on *InsShort* ranges from -0.131% to -0.031% across *InstBuy*[-25,-5] quintiles, but it does not vary with retail buying. While neither group of short sellers may be able to directly observe specific buying activity, the results in these two models suggest retail (institutional) short sellers are able to observe and trade profitably against the price effects of retail (institutional) buying pressure that has built over the recent past.

C.2. Shorting Around News Events

[14] The negative coefficient on *InstBuyQuint* is also consistent with a result in Arif, Ben-Rephael, and Lee (2015).

The preceding results suggest that retail short sellers exploit the uninformed buying activity of other retail traders. Even if short sellers have information about security demand that helps them interpret stock price movements, they might lack information about firms' fundamental values. We now test the hypothesis that retail short sellers possess private signals and trade before prices fully incorporate these signals. Specifically, we analyze whether retail short sellers' can anticipate news events, including earnings announcements, in the week following shorting activity. Again for comparison, we also estimate institutional short sellers' abilities to predict news.

We consider several categories of material information that retail short sellers could possess using a proprietary classification algorithm from RavenPack.[15] We retain stories that RavenPack deems relevant for only one or two U.S. stocks and eliminate stories about stock price movements or order imbalances. We separately analyze the six most common categories of stories:

- Earnings – earnings results, management guidance, or analyst estimates;
- Analyst – analysts' revisions in buy/hold/sell ratings or price targets;
- Revenue – revenue results, management guidance, or analyst estimates;
- Merger – merger or acquisition rumors, approvals, completions, or failures;
- Executive – compensation and turnover; and
- Insider – reported purchases and sales.

In the main text, we focus on news stories relating to earnings and analysts, as well as firms' earnings announcements, because of their significant implications for firm value. In the Internet Appendix, we present additional results for the other news categories. We separately consider earnings announcements based on the earlier of the Compustat and I/B/E/S announcement date.

Our tests focus directly on the market's response to new information, as measured by stock returns during intervals in which different types of news events occur. We assess predictability based on stocks' abnormal returns in days [2,5] after shorting occurs and condition

[15] RavenPack identifies highly specific news categories. We group several of their categories into six broad groups.

on whether news occurs in this same day-[2,5] window. The shorter one-week time frame in these tests improves the alignment of news with market reactions to news.

On each day, we estimate the following regression to predict each stock's abnormal returns (*CAR[2,5]*) based on information available that day and whether news occurs in days [2,5]:

$$CAR[2,5] = b_0 + b_1 RtlShort[-4,0] + b_2 RtlShort[-4,0] * News_J[2,5] + b_3 News_J[2,5] + controls + e. \quad (1)$$

We use separate regressions for each type of news (*J*) that could occur in days [2,5]. The variable $News_J[2,5]$ equals 1 if there is a type-*J* news story in days [2,5] and 0 otherwise. In addition to the types of news listed above (*J* = *Earn*, *Analyst*, *Rev*, *Merger*, *Executive*, or *Insider*), we estimate the model using all stories RavenPack deems relevant for a given firm (*J* = *All*). We also estimate specifications in which we treat a firm's earnings announcements as news. We augment the model above with a dummy variable, *Earn*[2,5], that equals one if a firm has an earnings announcement in days [2,5] and the dummy's interaction with retail shorting.

All regression specifications include institutional short selling (*InstShort*) variables that are analogous to the retail shorting variables. The set of control variables is identical to those in the second column of Table V, which includes interactions between size and shorting variables. The size interactions are necessary to distinguish the impact of news coverage from that of firm size. As before, cumulative abnormal returns (C*AR*[2,5]) are based on the three-factor model, and all independent variables are standardized. This methodology is similar to that used by Boehmer, Jones, and Zhang (2012) to analyze short sellers' ability to predict returns around earnings surprises and analyst updates.

[Insert Table VII here.]

The first regression in Table VII displays evidence that short selling can predict returns accompanying news in days [2,5] after short selling occurs. The key finding is that retail shorting is a powerful predictor of returns in weeks with relevant news, as defined by RavenPack. The main coefficient on Ln(*RtlShort*) is -0.028%, and its interaction coefficient with $News_{All}[2,5]$ is -0.025%. Thus, return predictability from retail shorting increases by 87% (0.025 / 0.028) in weeks with news. The direct coefficient on *InstShort* is just -0.004%, and its interaction coefficient with *News*[2,5] is -0.028%. We infer that institutional shorting weakly predicts returns in weeks without news but robustly predicts returns in weeks with news.

The second regression in Table VII shows that shorting predicts returns accompanying earnings announcements in days [2,5]. The main coefficient on Ln(*RtlShort*) is -0.029%; and its interaction with *Earn*[2,5] is -0.220%. Both coefficients are statistically significant at the 1% level. They imply that the predictive power of retail shorting is 770% (0.220 / 0.029) higher in weeks with earnings announcements.

The point estimates of the institutional shorting coefficient and its interaction coefficient with *Earn* are also negative. The magnitude of the Ln(*InstShort*) x *Earn*[2,5] coefficient is large relative to the direct effect of Ln(*InstShort*), indicating that institutional shorts might predict returns on earnings announcement days, though the standard error is too large to justify a strong statement. We cannot reject the hypothesis that the institutional interaction is equal to the analogous retail shorting interaction coefficient, as the *t*-statistic for the difference is less than 1.0. The first two regressions suggest that the ability to forecast public news events and earnings announcements partly explains why retail and institutional short sellers predict negative returns.

The last three regressions in Table VII show which types of events contribute most to the predictive power of retail and institutional short selling. We create a dummy variable $News_{All}$*_NoEarn*[2,5] that equals one if at least one news story occurs in days [2,5] but no earnings announcements occur in this period. Analogously, we define the dummy variables $News_{Earn}$*_NoEarn*[2,5] and $News_{Analyst}$*_NoEarn*[2,5] to represent earnings and analyst news stories, respectively, that do not coincide with earnings announcements. The former stories include those referencing management guidance, while the latter cover analyst updates and forecasts as reported by the financial press. We include these three dummy variables and their interactions with the shorting variables in the last three regressions.

The third regression in Table VII reveals a notable difference between retail and institutional shorting. Most of the predictability from retail shorting occurs during weeks with earnings announcements, as shown by the highly significant Ln(*RtlShort*) x *Earn*[2,5] coefficient of -0.219%. Retail shorts have no special ability to predict returns in weeks with news stories that do not accompany earnings announcements, as shown by the small and insignificant Ln(*RtlShort*) x $News_{All}$*_NoEarn*[2,5] coefficient. In contrast, institutional shorting is a much more powerful predictor of returns during weeks with news stories that do not accompany earnings announcements, as shown by the highly significant Ln(*InstShort*) x $News_{All}$*_NoEarn*[2,5] coefficient of -0.031%. However, the *t*-statistic for the difference between the retail and institutional shorting interactions with $News_{All}$*_NoEarn*[2,5] is only 1.48.

In the fourth regression in Table VII, the Ln(*RtlShort*) x *Earn*[2,5] coefficient remains negative and significant. The retail shorting interaction coefficients with $News_{Earn}$*_NoEarn*[2,5] is not statistically significant and the point estimate is slightly positive. This evidence confirms the earlier interpretation that retail shorting is highly informative about future earnings announcements but not about other earnings-related news, such as management guidance. All institutional shorting coefficients are statistically insignificant in this specification. The near-zero point estimate of the Ln(*InstShort*) x $News_{Earn}$*_NoEarn*[2,5] coefficient suggests that institutional short sellers are unable to forecast the market impact of earnings news stories.

The last regression in Table VII brings additional clarity. The most notable results are that retail short sellers can predict the market impact of analyst-related news stories and earnings announcements. The latter effect is more than twice as large as the former. Institutional short sellers derive most of their predictive power from analyst-related news stories. The Ln(*InstShort*) x $News_{Analyst}$*_NoEarn*[2,5] coefficient is more than twice as large as the analogous retail shorting interaction (though the *t*-statistic of the difference is only 1.37) and far larger than the other institutional shorting coefficients. One interpretation is that institutional short sellers have strong connections to sell-side analysts and receive advance warning of analyst downgrades, consistent with studies of tipping (Irvine, Lipson, and Puckett (2007) and Christophe, Ferri, and Hsieh (2010)). Alternatively, analysts could respond to institutional short sellers' information with a lag. Some retail short sellers could also have indirect connections with analysts, such as knowing privileged clients, though direct connections are unlikely for clients of discount brokers. In summary, the ability to predict the market impact of earnings announcements (analyst-related news) is the most important source of retail (institutional) short sellers' information.

In Table IA.VI of the Internet Appendix, we consider whether short sellers predict revenue-, merger-, executive, or insider ownership-related news. We employ regression specifications that are analogous to those in columns four and five of Table VII. After removing the effects of earnings announcements, retail shorting does not predict returns accompanying these types of news above and beyond typical predictability. There is some evidence that the predictability from institutional shorting is different in weeks with revenue- or merger-related news, though the magnitudes of the interaction coefficients are not large.

C.3. The Distribution of Private Information

Here we explore different versions of the theory that retail short sellers are informed. One possibility is that retail shorts' information is highly concentrated in the hands of a few corporate insiders or leaked to a small group of investors within their personal networks. Since our dataset contains individual short sale orders but no information identifying specific traders, we conduct two indirect tests. The first is based on Cohen, Malloy, and Pomorski (2012), who show that nonroutine trades by corporate insiders predict monthly stock returns. That is, after excluding trades that are likely scheduled and thus unrelated to privileged information, they find evidence that insiders trade opportunistically. If the retail short sellers driving our main results are high-level executives or act on the same information that these insiders possess, controlling for opportunistic selling as in Cohen, Malloy, and Pomorski (2012) should diminish our main results. To this end, we create a dummy variable (*InsideSale*) that is equal to one for all stock-weeks in our dataset in which there is one or more opportunistic insider sales using their definition.

[Insert Table VIII here.]

The first column in Table VIII reports the coefficients from a regression of monthly abnormal returns on retail shorting and *InsideSale*, along with all other variables in the specification in column three of Table IV. Consistent with the findings of Cohen, Malloy, and

Pomorski (2012), the predictive coefficient on *InsideSale* is negative (-0.27%) and statistically significant. However, the inclusion of *InsideSale* in the specification results in an immaterial reduction in the key retail shorting coefficient, from -0.185% in Table IV to -0.184% here. We also find that the correlation between *InsideSale* and Ln(*RtlShort*) is just 0.022. These results indicate that informed retail shorting is only weakly related to insider selling and that retail shorting conveys information beyond insider trading.

Our second test examines whether short sales of different dollar amounts are informed. Because our shorting variables are dollar-weighted, the main results could be driven by a small number of extremely large short sales, conducted by a few wealthy and sophisticated individuals with connections to firm insiders or other information networks. Recall that retail short sales are on average larger than other retail trades in our database and the retail shorts in the NYSE data studied by BJZ, who find no significant relation between retail shorting and future returns. A finding that only very large short sales are informed could reconcile our results with theirs. On the other hand, if information is dispersed across a wide range of traders, we expect to find that even small short sales could predict returns.

We therefore decompose our main retail shorting variable, *RtlShort*, into three components based on the dollar amounts of short sales. Our method accounts for differences in typical trade sizes across stocks. For each stock-day, we compute the 25th and 75th percentiles (P25 and P75) of the distribution of all retail short sales from the prior quarter, defined as days [-67,-5]. Then we compute *RtlShort* in days [-4,0] separately using either small (short size ≤ P25), medium (P25 < short size < P75), or large (short size ≥ P75) short sales, labeling these variables *RtlShortSmall*, *RtlShortMedium*, and *RtlShortLarge*, respectively. The mean (median) sizes of small, medium, and large short sales are $5,808 ($3,925), $18,851 ($15,000), and $35,697 ($31,100), respectively.

The second column in Table VIII presents coefficient estimates for a predictive regression in which we replace *RtlShort* with its small, medium, and large short sale components. Otherwise, the specification is identical to that shown in column three of Table IV. The coefficients on small, medium, and large short sales are -0.073%, -0.109%, and -0.094%, respectively. All three coefficients are statistically significant at the 1% level. The coefficients are statistically indistinguishable and economically similar. These findings demonstrate that broad categories of retail short sales, including small retail shorts, predict returns.[16] In contrast, prior research on institutional short sellers finds that small short sales are not informed and that such orders actually predict positive returns in some specifications (BJZ).

The third and fourth columns in Table VIII augment the regression specifications in the first two columns with interactions between firm size and the retail shorting variables. In all cases, the size interactions are positive, consistent with the results in Tables IV and V. The statistical significance is weak in the regression with the three retail shorting variables because it is difficult to precisely estimate coefficients on three positively correlated interaction terms. With that caveat, the inference that retail shorting is a stronger predictor of returns in small firms is robust.

IV. Alternative Explanations

Retail shorting could predict negative returns for reasons other than retail short sellers' information. In this section, we consider three alternative hypotheses. First, retail brokers could route only well-informed short sales to the market centers in our data and opt to trade against uninformed short sales with their own capital—i.e., internalize them. Second, rather than being informed about fundamentals, retail short sellers could be providing liquidity to buyers who demand immediate execution. Kaniel, Saar, and Titman (2008) and Kelley and Tetlock (2013) argue that an analogous liquidity provision mechanism partially explains their findings of a positive relation between retail net buying and future returns. Third, retail shorting could be a proxy for investor attention. Miller (1977) demonstrates that attention, when combined with difference of opinion and short-sale constraints, can cause overpricing. Consequently, retail shorting could predict negative returns even if retail investors are uninformed.

[16] An alternative interpretation of this finding is that large retail short sellers split their orders.

A. The Internalization Hypothesis

Even though we observe roughly 1/3 of all retail orders in the United States, our sample might not be representative of all retail short sales if retail brokers selectively internalize uninformed order flow. We test this possibility by creating variables similar to those used in Kelley and Tetlock (2013) that measure the extent of internalization by brokers that route to the market centers in our data. We observe which orders come from a large group of brokers that internalizes according to SEC Rule 605 and 606 disclosures. For each stock-month, we create an internalization ratio as the value of orders internalized as per Rule 605 disclosures to orders routed to our market centers. In these tests, we compute retail shorting using only orders from brokers with internalization data. Because these brokers account for only 39% of orders in our sample, the tests below are less powerful than our main tests.

We create a variable, *IntQuant*, to summarize variation in internalization ratios across stocks. We set *IntQuant* to zero if no brokerage internalizes any orders in the stock and equal to 1, 2, 3, or 4 based on a ranking of stocks with positive internalization ratios into quartiles in the preceding month. We construct interactions between log retail shorting and *IntQuant* (Ln(*RtlShort*)***IntQuant*) and size (Ln(*RtlShort*)***SizeQuint*). We include the latter interaction because internalization ratios vary with size. The internalization hypothesis predicts that the direct effect of Ln(*RtlShort*) will be insignificant and the coefficient on Ln(*RtlShort*)***IntQuant* will be negative because stocks in which brokers internalize the most order flow fully account for the negative return predictability of retail shorting.

[Insert Table IX here.]

The first regression in Table IX reports the coefficient estimates for the internalization variables and all control variables. For comparison purposes, the second regression in Table IX shows the last regression from Table IV, which spans the full sample and includes the same variables except for the internalization variables. The key result is that the main coefficient on Ln(*RtlShort*) remains statistically and economically significant (-0.129% vs. -0.146% initially), a rejection of the internalization hypothesis. This significance occurs despite the fact that this regression includes only 39% of short sales. The partial sample decreases the precision of the retail shorting variable, causing attenuation bias and an increase in the standard error of the coefficient.

The other notable aspect of the regression is that the magnitudes of internalization coefficients are economically small and statistically insignificant, implying that selection bias in order routing has little influence on our main findings. Most importantly, the point estimate of the interaction coefficient (Ln(*RtlShort*)**IntQuant*) is slightly positive, suggesting that selective internalization slightly weakens our main result. That is, if we could observe all orders from the retail brokers, the coefficient on retail shorting would be slightly larger in magnitude.

B. The Liquidity Hypothesis

Some of our findings could be consistent with the idea that retail shorts provide liquidity to impatient buyers. In particular, positive returns tend to precede high retail shorting, and high retail shorting predicts negative returns. Such a pattern could arise because retail short sellers capitalize on temporary price pressure induced by impatient buyers. Here we analyze whether liquidity provision could explain our main finding that retail shorting predicts negative returns.

Two arguments cast doubt on the liquidity hypothesis. First, retail short sellers are not natural liquidity providers. Short sellers incur costs from foregone interest on collateral and risks from recall of shares lent and unlimited potential liability, whereas sellers of long positions do not. Second, the evidence in Table VII shows that retail shorting predicts the revelation of information, indicating that the liquidity hypothesis is at best an incomplete explanation.

We now test an additional prediction of the liquidity hypothesis. Motivated by the notion that liquidity provision strategies benefit from temporary price reversals, we analyze the extent to which including prior return controls affects return predictability from retail shorting. As a benchmark, we consider the regression in the second column of Table IX. We then evaluate the effect of excluding prior return controls (*Ret*[-4,0], *Ret*[-25,-5], and *Ret*[-251,-26]), shown in the third column. The liquidity hypothesis predicts the inclusion of past returns should weaken the retail shorting coefficient.

Table IX shows that including control variables for past returns in these regressions has no impact on the main retail shorting coefficient within rounding error. The coefficient is -0.146 without return controls (column 3) and -0.146 with return controls (column 2).[17] Thus, most of the return predictability from retail short selling does not seem to come from liquidity provision strategies based on price reversals.

C. The Attention Hypothesis

In Miller (1977), differences in opinion combine with short-sale constraints to generate overpricing. The intuition is that shorting constraints sideline some investors with the lowest valuations, while investors with relatively high valuations still can buy and so exert a disproportionate impact on the equilibrium price. By increasing the number of prospective buyers and sellers, an increase in investor attention exacerbates overpricing because some sellers face short-sale constraints. Thus, if it proxies for attention-based overpricing, retail shorting could predict negative returns even if retail shorts are uninformed. Here we test the main prediction from this attention hypothesis: attention-based overpricing is greater under more severe shorting constraints.

A key shorting constraint is the cost to a short seller of borrowing stock, i.e., the equity lending fee. Because data on equity lending fees are not widely available, we instead rely on two proxies for shorting constraints. The first is *NoOption*, a dummy variable set to one if a stock has zero option trading volume, according to Option Metrics data, during the prior quarter. As argued in Diamond and Verrecchia (1987), by allowing additional ways to establish a short position, the introduction of options reduces the equilibrium cost of short selling, relaxing shorting constraints. The second proxy for shorting constraints is based on the number of fails-to-deliver shares reported by trading exchanges. Data on fails are available for all but the first ten months of our sample period. The variable *HighFails* is a dummy set to one if fails-to-deliver

[17] In contrast, Kelley and Tetlock (2013) show that including prior return controls does affect the relation between retail buy-sell imbalance and future returns when nonmarketable limit orders are used to construct the imbalance measure.

exceeds 0.1% of shares outstanding on any day of the prior week. Evans et al. (2009) show that options market makers usually choose to fail to deliver stocks to buyers when lending fees are high. Thus, we consider stocks with high fails-to-deliver to be short-sale constrained. We find that 48% of stocks are constrained by the *NoOption* criterion, while 15% are constrained by the *HighFails* criterion, indicating the latter criterion is far more restrictive.

The fourth and fifth regressions in Table IX include the shorting constraint variables and their interactions with log retail shorting as independent variables. In both regressions, the key coefficient on log retail shorting (Ln(*RtlShort*)) remains negative. In fact, this coefficient is actually slightly larger with the inclusion of the shorting constraint variables with the interpretation that retail shorting is slightly more predictive of returns in stocks without short sale constraints. In addition, neither of the interaction coefficients between log retail shorting and shorting constraints is statistically significant at even the 10% level. This evidence contradicts the attention hypothesis. More generally, it shows that shorting constraints do not play a major role in explaining why retail shorting predicts negative returns.

V. Concluding Discussion

Using a broad and representative sample of retail trading, we demonstrate that retail short selling is a strong predictor of negative stock returns, even after controlling for other traders' behavior and known predictors of returns. This predictability does not contradict the weak form of the efficient market hypothesis because retail shorting is nonpublic information. Our evidence is most consistent with the theory that retail short sellers possess and act on unique information about stocks' fundamental values. Prices gradually incorporate this information in the next few months.

Our evidence suggests that retail and institutional short sellers differ in how they access, process, and trade on information. Our interpretation is that myriad retail short sellers serendipitously encounter diverse information about fellow retail investors and firms' fundamentals through geographical proximity, social networks, and employment relationships. Such information presents especially valuable trading opportunities in stocks with limited competition from institutions. In contrast, institutional short sellers invest heavily in stock research and understand the forces driving institutional order flows. Our evidence of actual short sales by retail investors complements the growing literature showing that certain individuals possess information about future stock returns, earnings surprises, and consumer products (Chen, De, Hu, and Hwang (2014), Adebambo and Bliss (2015), and Huang (2015)) by showing that retail short sellers actually trade on their information.

Differences in retail and institutional short sellers' constraints could also contribute to the patterns that we observe. To attract funds from clients, professional asset managers can engage in window dressing in which they increase their holdings of stocks favored by retail clients (e.g., Lakonishok et al. (1991), Sias and Starks (1997), and Solomon, Soltes, and Sosyura (2014)), even though such stocks could be overpriced (Frazzini and Lamont (2008) and Fang, Peress, and Zheng (2014)). This incentive from fund flows could deter well-informed institutions from short selling overpriced stocks, as argued in Lamont and Stein (2004).

Retail short sellers also could benefit from a lack of competition from other retail traders with poor access to short selling. Retail brokerage customers must open margin accounts to be able to short stocks, and many brokerages do not permit retail customers with margin accounts to short large subsets of stocks, such as newly public firms. These entry restrictions could contribute to the persistence of return predictability from retail shorting insofar as they exclude informed retail traders from shorting. On the other hand, some entry restrictions could selectively deter sentiment-driven short selling, helping explain why retail shorting is able to predict negative returns. Our empirical evidence indicates that retail shorting is similarly informative in stocks with and without short-sale constraints, as measured by either stocks that lack options or exhibit high fails-to-deliver. It is therefore possible that these two constraints discourage sentiment-driven shorting and information-driven shorting roughly in proportion.

Future empirical studies should test such hypotheses based on heterogeneity in investor sophistication within groups of retail investors as well as within groups of institutions. Indeed, placed in the context of prior research, our findings suggest that within-group heterogeneity could be just as important as accounting for differences between retail and institutional investors.

Bibliography

Adebambo, Biljana N., and Barbara A. Bliss, 2015, The value of crowdsourcing: evidence from earnings forecasts, Working paper, University of San Diego.

Ang, Andrew, Robert J. Hodrick, Yuhang Xing, and Xiaoyan Zhang, 2006, The cross-section of volatility and expected returns, *Journal of Finance* 61, 259−299.

Arif, Salman, Azi Ben-Rephael, and Charles M.C. Lee, 2015, Do short sellers profit from mutual funds? Evidence from daily trades, Working paper, University of Indiana.

Asparouhova, Elena, Hendrik Bessembinder, and Ivalina Kalcheva, 2010, Liquidity biases in asset pricing tests, *Journal of Financial Economics* 96, 215−237.

Asquith, Paul, Parag A. Pathak, and Jay R. Ritter, 2005, Short interest, institutional ownership, and stock returns, *Journal of Financial Economics* 78, 243−276.

Barber, Brad M., and Terrance Odean, 2000, Trading is hazardous to your wealth: the common stock investment performance of individual investors, *Journal of Finance* 55, 773−806.

Barber, Barber M., Terrance Odean, and Ning Zhu, 2009, Do retail trades move markets? *Review of Financial Studies* 22, 151−186.

Battalio, Robert, and Tim Loughran, 2007, Does payment for order flow to your broker help or hurt you? Working paper, University of Notre Dame.

Berk, Jonathan B., and Richard C. Green, 2004, Mutual fund flows and performance in rational markets, *Journal of Political Economy* 112, 1269−1295.

Blume, Marshall E., and Robert F. Stambaugh, 1983, Biases in computed returns: an application to the size effect, *Journal of Financial Economics* 12, 387−404.

Boehmer, Ekkehart, Zsuzsa R. Huszar, and Bradford D. Jordan, 2010, The good news in short interest, *Journal of Financial Economics* 96, 80−97.

Boehmer, Ekkehart, Charles M. Jones, and Xiaoyan Zhang, 2008, Which shorts are informed? *Journal of Finance* 63, 491−527.

Boehmer, Ekkehart, Charles M. Jones, and Xiaoyan Zhang, 2012, What do short sellers know? Working paper, EDHEC Business School.

Chen, Hailiang, Prabuddha De, Yu (Jeffrey) Hu, and Byoung-Hyoun Hwang, 2014, Wisdom of crowds: the value of stock opinions transmitted through social media, *Review of Financial Studies* 27 1367−1403.

Christophe, Stephen E., Michael G. Ferri, and Jim Hsieh, 2010, Informed trading before analyst downgrades: evidence from short sellers, *Journal of Financial Economics* 95 85−106.

Cohen, Lauren, Karl B. Diether, and Christopher J. Malloy, 2007, Supply and demand shifts in the shorting market, *Journal of Finance* 62, 2061−2096.

Cohen, Lauren, Christopher J. Malloy, and Lukasz Pomorski, 2012, Decoding inside information, *Journal of Finance* 67, 1009−1043.

Coval, Joshua, and Erik Stafford, 2007, Asset fire sales (and purchases) in equity markets, *Review of Financial Studies* 86, 479−512.

Diamond, Douglas W., and Robert E. Verrecchia, 1987, Constraints on short-selling and asset price adjustment to private information, *Journal of Financial Economics* 18, 277−311.

Diether, Karl B., Kuan-Hui Lee, and Ingrid M. Werner, 2009, Short-sale strategies and return predictability, *Review of Financial Studies* 22, 575−607.

Evans, Richard B., Christopher C. Geczy, David K. Musto, and Adam V. Reed, 2009, Failure is an option: impediments to short selling and option prices, *Review of Financial Studies* 22, 1955−1980.

Fama, Eugene F., and Kenneth R. French, 1992, The cross-section of expected stock returns, *Journal of Finance* 47, 427−465.

Fama, Eugene F., and Kenneth R. French, 1993, Common risk factors in the returns on stocks and bonds, *Journal of Financial Economics* 33, 3−56.

Fama, Eugene F., and James D. MacBeth, 1973, Risk, return, and equilibrium: empirical tests. *Journal of Political Economy* 81, 607−636.

Fang, Lily H., Joel Peress, and Lu Zheng, 2014, Does media coverage of stocks affect mutual funds' trading and performance? *Review of Financial Studies* 27, 3441−3466.

Figlewski, Stephen, 1981, The informational effect of restrictions on short sales: some experimental evidence, *Journal of Financial and Quantitative Analysis* 16, 463−476.

Frazzini, Andrea, and Owen A. Lamont, 2008, Dumb money: mutual fund flows and the cross-section of stock returns, *Journal of Financial Economics* 88, 299−322.

Gamble, Keith Jacks, and Wei Xu, 2013, Informed retail investors: evidence from retail short sales, Working paper, DePaul University.

Gervais, Simon, Ron Kaniel, and Dan H. Mingelgrin, 2001, The high-volume return premium, *Journal of Finance* 61, 877−919.

Gutierrez, Roberto C., and Eric K. Kelley, 2008, The long-lasting momentum in weekly returns, *Journal of Finance* 61, 415−447.

Huang, Jiekun, 2015, The customer knows best: The investment value of consumer opinions, Working paper, University of Illinois, Urbana-Champaign.

Hvidkjaer, Soeren, 2008, Small trades and the cross-section of stock returns, *Review of Financial Studies* 21, 1123-1151.

Irvine, Paul, Marc Lipson, and Andy Puckett, 2007, Tipping, *Review of Financial Studies* 20, 741−768.

Jegadeesh, Narasimhan, and Sheridan Titman, 1993, Returns to buying winners and selling losers: implications for stock market efficiency, *Journal of Finance* 48, 65−91.

Kaniel, Ron, Shuming Liu, Gideon Saar, and Sheridan Titman, 2012, Individual investor trading and return patterns around earnings announcements, *Journal of Finance* 67, 639-680.

Kaniel, Ron, Gideon Saar, and Sheridan Titman, 2008, Individual investor sentiment and stock returns, *Journal of Finance* 63, 273−310.

Kelley, Eric K., and Paul C. Tetlock, 2013, How wise are crowds? Insights from retail orders and stock returns, *Journal of Finance* 68, 1229−1265.

Lakonishok, Josef, Andrei Shleifer, Richard Thaler, and Robert Vishny, 1991, Window dressing by pension fund managers, *American Economic Review Papers and Proceedings* 81, 227−231.

Lamont, Owen A., and Jeremy C. Stein, 2004, Aggregate short interest and market valuations, *American Economic Review* 94, 29–32.

Linnainmaa, Juhani T., 2010, Do limit orders alter inferences about investor performance and behavior? *Journal of Finance* 65, 1473–1506.

Lou, Dong, 2012, A flow-based explanation for return predictability, *Review of Financial Studies* 25, 3457–3489.

Miller, Edward M., 1977, Risk, uncertainty, and divergence of opinion, *Journal of Finance* 32, 1151–1168.

Newey, Whitney K., and Kenneth D. West, 1987, A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix, *Econometrica* 55, 703–708.

Senchack, A. J., Jr., and Laura T. Starks, 1993, Short-sale restrictions and market reaction to short-interest announcements, *Journal of Financial and Quantitative Analysis* 28, 177–194.

Shleifer, Andrei, and Robert Vishny, 1997, Limits of arbitrage, *Journal of Finance* 52, 35–55.

Sias, Richard W., and Laura T. Starks, 1997, Institutions and individuals at the turn-of-the-year, *Journal of Finance* 52, 1543–1562.

Solomon, David H., Eugene Soltes, and Denis Sosyura, 2014, Winners in the spotlight: media coverage of fund holdings as a driver of flows, *Journal of Financial Economics* 113, 53–72.

Surowiecki, James, 2004, *The Wisdom of Crowds* (Doubleday).

Table I
Variable Definitions

This table defines variables used in this study. Panel A provides definitions for the main retail shorting and trading variables. Panels B defines control variables. Henceforth, the abbreviations Ln(*x*), Lag(*x*), and Δ*x* denote the natural logarithm of *x*, previous value of *x*, and change in *x*, respectively.

Panel A: Retail trading variables

Variable	Definition
RtlShort	Retail shares shorted / total CRSP share volume
RtlShortShrout	Retail shares shorted / shares outstanding
RtlShortFrac	Retail shares shorted / (retail shares bought + retail shares sold)
RtlTrade	(Retail shares bought + retail shares sold) / total volume
RtlBuy	(Shares bought – long positions sold) / total CRSP share volume
RtlSell	Long positions sold / total CRSP share volume

Panel B: Control variables

Variable	Definition
Size	Market value of equity from CRSP as of prior quarter end
BM	(Compustat book equity) / CRSP market equity as of prior December
Beta	Market beta based on a daily regression of excess returns on excess market returns estimated over the prior year
Ret[x,y]	Stock return over days $t + x$ through $t + y$
Analysts	Number of analysts with I/B/E/S annual earnings forecasts in prior month
MediaCvg	Number of firm-specific articles from Dow Jones Newswires in prior quarter
IdioVol	Standard deviation of residuals from a daily Fama and French (1993) three-factor model estimated in the prior calendar month
Turnover	(Weekly CRSP share volume) / shares outstanding
ShortInt	(Most recently reported short interest from Compstat) / shares outstanding
AllShort	Total weekly short selling from Regulation SHO / total CRSP share volume
InstShort	*AllShort* less *RtlShort*
InstBuy	(Ancerno shares bought– Ancerno shares sold) / total CRSP share volume
InsideSale	Dummy set to one if the stock has one or more opportunistic insider sale during the week as defined by Cohen, Malloy, and Pomorski (2012)
NoOption	Dummy set to one if the stock has zero reported or unreported options trading in OptionMetrics during the prior quarter
HighFails	Dummy set to one if exchanges report fails-to-deliver exceeding 0.10% of shares outstanding on any day during the prior week

Table II
Cross-sectional Summary Statistics

This table presents time-series averages of daily cross-sectional summary statistics. All variables and notational conventions are as defined in Table I. Panel A contains daily means, numbers of observations (N), standard deviations (Std Dev), and percentiles (Pctl). Panel B contains average daily cross-sectional correlation coefficients.

Panel A: Average Statistics Across Days

Variable	Mean	N	Std Dev	Pctl 5	Pctl 25	Pctl 50	Pctl 75	Pctl 95
RtlShort (%)	0.127	3376	0.321	0.000	0.000	0.007	0.099	0.638
Ln(*RtlShort*)	-7.930	3376	1.489	-9.146	-9.146	-8.754	-6.863	-5.057
RtlShortShrOut (bp)	0.767	3376	2.245	0.000	0.000	0.023	0.424	3.947
RtlShortFrac (%)	3.169	3360	7.079	0.000	0.000	0.243	3.274	15.120
RtlTrade (%)	9.604	3376	15.037	0.520	1.618	4.006	10.889	38.242
RtlBuy (%)	-0.260	3376	7.379	-7.831	-0.930	-0.020	0.763	6.426
AllShort (%)	26.219	1875	12.203	5.122	17.963	26.038	34.353	46.271
Ln(*AllShort*)	-1.040	1875	0.366	-1.799	-1.227	-0.984	-0.784	-0.552
InstShort (%)	26.045	1875	12.226	4.882	17.817	25.880	34.191	46.093
Ln(*InstShort*)	-1.048	1875	0.372	-1.817	-1.234	-0.991	-0.790	-0.557
ShortInt (%)	4.833	3328	5.537	0.115	1.340	3.274	6.238	15.042
Beta	1.119	3376	0.600	0.175	0.721	1.093	1.489	2.156
Size ($Billion)	4.068	3376	17.049	0.044	0.207	0.603	1.989	15.782
BM	0.503	3185	0.736	0.073	0.263	0.439	0.662	1.250
Ret[-4,0]	0.004	3376	0.065	-0.080	-0.024	0.001	0.027	0.093
Ret[-25,-5]	0.018	3372	0.136	-0.161	-0.047	0.009	0.069	0.216
Ret[-251,26]	0.296	3372	0.693	-0.376	-0.051	0.158	0.448	1.393
IdioVol (%)	2.279	3376	1.841	0.718	1.228	1.831	2.772	5.191
Turnover (%)	4.451	3376	4.790	0.453	1.591	2.983	5.450	13.571
Analysts	6.163	3376	6.494	0.000	1.000	4.172	8.993	19.691
NewsStories	53.230	3376	86.293	9.233	20.495	32.854	53.712	152.198

Table II: continued

Panel B: Average Correlations Across Days

	Ln(*RtlShort*)	Ln(*RtlShortShrOut*)	Ln(*RtlShortFrac*)	Ln(*RtlTrade*)	*RtlBuy*
Ln(*RtlShort*)	1.000	0.941	0.868	0.078	0.061
Ln(*RtlShortShrOut*)	0.941	1.000	0.825	0.058	0.071
Ln(*RtlShortFrac*)	0.868	0.825	1.000	-0.253	0.052
Ln(*RtlTrade*)	0.078	0.058	-0.253	1.000	-0.040
RtlBuy	0.061	0.071	0.052	-0.040	1.000
Ln(*AllShort*)	0.152	0.166	0.197	-0.242	0.048
Ln(*InstShort*)	0.116	0.137	0.171	-0.253	0.045
Ln(*ShortInt*)	0.234	0.332	0.258	-0.271	0.066
ΔLn(*ShortInt*)	0.033	0.042	0.008	0.035	0.007
Beta	0.186	0.245	0.194	-0.168	0.042
Ln(*Size*)	0.115	0.114	0.365	-0.696	0.049
Ln(*BM*)	-0.088	-0.112	-0.117	0.073	-0.037
Ret[-4,0]	0.081	0.098	0.049	0.018	0.028
Ret[-25,-5]	0.075	0.093	0.033	0.039	-0.030
Ret[-251,26]	0.130	0.173	0.062	0.097	0.000
Ln(*IdioVol*)	0.099	0.155	-0.081	0.510	0.002
Ln(*Turnover*)	0.363	0.579	0.360	-0.168	0.098
Ln(*Analysts*)	0.137	0.160	0.318	-0.430	0.036
Ln(*NewsStories*)	0.118	0.156	0.323	-0.533	0.041

Table II: continued

Panel B: Average Correlations Across Days

	Ln(*AllShort*)	Ln(*InstShort*)	Ln(*ShortInt*)	ΔLn(*ShortInt*)	*Beta*	Ln(*Size*)	Ln(*BM*)
Ln(*RtlShort*)	0.152	0.116	0.234	0.033	0.186	0.115	-0.088
Ln(*RtlShortShrOut*)	0.166	0.137	0.332	0.042	0.245	0.114	-0.112
Ln(*RtlShortFrac*)	0.197	0.171	0.258	0.008	0.194	0.365	-0.117
Ln(*RtlTrade*)	-0.242	-0.253	-0.271	0.035	-0.168	-0.696	0.073
RtlBuy	0.048	0.045	0.066	0.007	0.042	0.049	-0.037
Ln(*AllShort*)	1.000	0.994	0.395	0.031	0.279	0.230	-0.068
Ln(*InstShort*)	0.994	1.000	0.394	0.029	0.279	0.236	-0.068
Ln(*ShortInt*)	0.395	0.394	1.000	0.072	0.462	0.226	-0.185
ΔLn(*ShortInt*)	0.031	0.029	0.072	1.000	-0.035	-0.037	0.018
Beta	0.279	0.279	0.462	-0.035	1.000	0.201	-0.116
Ln(*Size*)	0.230	0.236	0.226	-0.037	0.201	1.000	-0.190
Ln(*BM*)	-0.068	-0.068	-0.185	0.018	-0.116	-0.190	1.000
Ret[-4,0]	0.080	0.077	-0.027	-0.008	-0.014	-0.015	0.014
Ret[-25,-5]	0.017	0.015	-0.044	0.088	-0.039	-0.034	0.031
Ret[-251,26]	-0.047	-0.049	-0.004	0.053	-0.012	0.000	0.055
Ln(*IdioVol*)	-0.152	-0.156	-0.023	0.059	0.052	-0.546	-0.024
Ln(*Turnover*)	0.162	0.160	0.566	0.043	0.357	0.186	-0.151
Ln(*Analysts*)	0.079	0.082	0.172	-0.024	0.120	0.712	-0.144
Ln(*NewsStories*)	0.151	0.155	0.179	-0.042	0.123	0.736	-0.194

Table II: continued

Panel B: Average Correlations Across Days

	Ret[-4,0]	*Ret*[-25,-5]	*Ret*[-251,26]	Ln(*IdioVol*)	Ln(*Turnover*)	Ln(*Analysts*)	Ln(*NewsStories*)
Ln(*RtlShort*)	0.081	0.075	0.130	0.099	0.363	0.137	0.118
Ln(*RtlShortShrOut*)	0.098	0.093	0.173	0.155	0.579	0.160	0.156
Ln(*RtlShortFrac*)	0.049	0.033	0.062	-0.081	0.360	0.318	0.323
Ln(*RtlTrade*)	0.018	0.039	0.097	0.510	-0.168	-0.430	-0.533
RtlBuy	0.028	-0.030	0.000	0.002	0.098	0.036	0.041
Ln(*AllShort*)	0.080	0.017	-0.047	-0.152	0.162	0.079	0.151
Ln(*InstShort*)	0.077	0.015	-0.049	-0.156	0.160	0.082	0.155
Ln(*ShortInt*)	-0.027	-0.044	-0.004	-0.023	0.566	0.172	0.179
ΔLn(*ShortInt*)	-0.008	0.088	0.053	0.059	0.043	-0.024	-0.042
Beta	-0.014	-0.039	-0.012	0.052	0.357	0.120	0.123
Ln(*Size*)	-0.015	-0.034	0.000	-0.546	0.186	0.712	0.736
Ln(*BM*)	0.014	0.031	0.055	-0.024	-0.151	-0.144	-0.194
Ret[-4,0]	1.000	-0.008	0.008	0.010	0.081	-0.010	-0.008
Ret[-25,-5]	-0.008	1.000	0.011	0.101	0.078	-0.013	-0.022
Ret[-251,26]	0.008	0.011	1.000	0.112	0.173	0.006	-0.079
Ln(*IdioVol*)	0.010	0.101	0.112	1.000	0.180	-0.241	-0.314
Ln(*Turnover*)	0.081	0.078	0.173	0.180	1.000	0.250	0.278
Ln(*Analysts*)	-0.010	-0.013	0.006	-0.241	0.250	1.000	0.573
Ln(*NewsStories*)	-0.008	-0.022	-0.079	-0.314	0.278	0.573	1.000

Table III
Calendar-time Returns of Retail Shorting Portfolios

This table presents calendar-time returns for portfolios based on weekly retail short selling (*RtlShort*). Each day, we sort firms into five portfolios based on retail short selling over the prior week. Quintile 1 contains stocks with zero shorting, and Quintiles 2 through 5 represent a quartile sort of the remaining stocks. We evaluate the returns to these portfolios in calendar time during days [*x*,*y*] after formation. To mitigate the Blume and Stambaugh (1983) bias, we weight firms within each cohort portfolio on calendar day t by their gross returns on day $t-1$. When evaluating portfolios' returns at horizons exceeding one day, we use the procedure developed in Jegadeesh and Titman (1993). Specifically, the calendar day t return of each portfolio with horizon [*x*,*y*] days after formation is the average of day t returns of portfolios formed by sorting on shorting on days $t-x$ through $t-y$. Panel A presents daily Fama and French (1993) three-factor alphas and average raw returns at various horizons. Panel B presents three-factor loadings for the [2,20] horizon, where *b*(*rmrf*), *b*(*smb*), and *b*(*hml*) denote the loadings on the market, size, and value factors. Newey and West (1987) *t*-statistics based on five lags appear in parentheses.

Panel A: Three-factor Alphas and Returns on Days [x,y]

	Three-factor Alpha					Excess Return				
Shorting Quintile	[1,1]	[2,20]	[21,40]	[41,60]	[61,252]	[1,1]	[2,20]	[21,40]	[41,60]	[61,252]
1	0.018	0.011	0.009	0.004	0.005	0.056	0.049	0.043	0.042	0.036
2	0.005	0.003	0.003	0.002	-0.002	0.050	0.046	0.044	0.047	0.034
3	-0.002	-0.003	-0.006	-0.002	-0.005	0.046	0.044	0.037	0.045	0.031
4	-0.020	-0.012	-0.012	-0.012	-0.011	0.030	0.037	0.033	0.037	0.025
5	-0.049	-0.025	-0.023	-0.015	-0.017	0.003	0.026	0.022	0.033	0.018
5 – 1 spread	-0.067	-0.036	-0.031	-0.019	-0.022	-0.053	-0.023	-0.021	-0.009	-0.018
t-stat	(-8.01)	(-4.92)	(-4.04)	(-2.38)	(-3.28)	(-3.94)	(-1.78)	(-1.60)	(-0.72)	(-1.57)

Table III: continued

Panel B: Factor Loadings

Shorting Quintile	*b*(*rmrf*)	*b*(*smb*)	*b*(*hml*)	Firms per Day
1	0.794	0.653	0.118	1612
2	1.015	0.489	0.033	441
3	1.078	0.671	0.009	441
4	1.125	0.853	0.004	441
5	1.068	0.988	0.112	441
5 – 1 spread	0.274	0.335	-0.006	
t-stat	(16.73)	(12.27)	(-0.18)	

Table IV
Cross-sectional Regressions of Returns on Retail Shorting and Control Variables

This table presents results from daily Fama-MacBeth (1973) regressions of stocks' returns from days $t + 2$ through $t + 20$ on retail shorting (*RtlShort*) and control variables measured as of day t. The variable *SizeQuint* equals -2, -1, 0, 1, or 2 based on the NYSE quintile rank of the firm's market equity in the prior June. Other independent variables are as defined in Table I, and all are standardized each day t. The dependent variable is a stock's Fama and French (1993) three-factor cumulative abnormal return, measured in percent, with factor loadings based on daily data from the prior year. Regressions apply observation weights equal to stocks' lagged gross returns. The table reports average regression coefficients. Newey-West (1987) t-statistics with 19 lags appear in parentheses.

	Model 1	Model 2	Model 3	Model 4	Model 5
Ln(*RtlShort*)	-0.271	-0.234	-0.185	-0.236	-0.146
	(-7.39)	(-8.90)	(-7.48)	(-9.06)	(-5.58)
Ln(*RtlShort*) x *SizeQuint*					0.031
					(1.93)
RtlBuy				0.097	
				(4.58)	
Ln(*Lag_ShortInt*)			-0.474		-0.498
			(-5.41)		(-5.53)
ΔLn(*Lag_ShortInt*)			-0.105		-0.108
			(-3.08)		(-3.16)
Ret[-4,0]		-0.068	-0.115	-0.069	-0.123
		(-1.53)	(-2.60)	(-1.55)	(-2.72)
Ret[-25,-5]		0.026	-0.005	0.029	-0.002
		(0.42)	(-0.07)	(0.48)	(-0.03)
Ret[-251,26]		0.283	0.176	0.285	0.176
		(3.85)	(2.01)	(3.87)	(2.00)
Ln(*IdioVol*)		-0.136	-0.242	-0.135	-0.322
		(-1.06)	(-2.00)	(-1.05)	(-2.87)
Ln(*Turnover*)		-0.110	0.190	-0.118	0.237
		(-2.26)	(2.58)	(-2.42)	(3.10)
SizeQuint					-0.120
					(-4.35)
Intercept	-0.061	-0.055	-0.113	-0.055	-0.209
	(-0.64)	(-0.58)	(-1.15)	(-0.58)	(-2.35)
R^2	0.002	0.022	0.027	0.023	0.029
Avg. n	3359	3359	3278	3359	3278

Table V
Cross-sectional Regressions of Returns on Retail and Institutional Shorting

This table presents results from daily Fama-MacBeth (1973) regressions of stocks' returns from days $t + 2$ through $t + 20$ on retail shorting (*RtlShort*), institutional shorting (*InstShort*), and control variables measured as of day t. The independent variables are as defined in Table I, and all are standardized each day t. The dependent variable is a stock's Fama and French (1993) three-factor cumulative abnormal return, measured in percent, with factor loadings based on daily data from the prior year. Model 1 is based on the pre-RegSHO period (June 4, 2003 to December 31, 2004), while Models 2 and 3 use only the RegSHO period (January 3, 2005 to July 6, 2007). Regressions apply observation weights equal to stocks' lagged gross returns. The table reports average regression coefficients. Newey-West (1987) t-statistics with 19 lags appear in parentheses.

	Model 1	Model 2	Model 3
Ln(*RtlShort*)	-0.117	-0.148	-0.131
	(-2.48)	(-4.46)	(-4.02)
Ln(*RtlShort*) x *SizeQuint*	0.054	0.040	0.050
	(2.14)	(1.97)	(2.61)
Ln(*InstShort*)			-0.090
			(-3.21)
Ln(*InstShort*) x *SizeQuint*			-0.041
			(-1.89)
Ln(*Lag_ShortInt*)	-0.675	-0.529	-0.515
	(-6.67)	(-5.76)	(-6.11)
ΔLn(*ShortInt*)	-0.080	-0.131	-0.126
	(-1.42)	(-2.84)	(-2.74)
Ret[-4,0]	-0.154	-0.095	-0.088
	(-1.68)	(-1.84)	(-1.68)
Ret[-25,-5]	-0.202	0.061	0.065
	(-1.64)	(0.79)	(0.85)
Ret[-251,26]	-0.061	0.163	0.161
	(-0.37)	(1.93)	(1.92)
Ln(*IdioVol*)	-0.365	-0.193	-0.191
	(-1.41)	(-1.93)	(-1.95)
Ln(*Turnover*)	0.215	0.315	0.310
	(1.80)	(3.83)	(3.89)
SizeQuint	-0.126	-0.126	-0.124
	(-3.24)	(-3.64)	(-3.56)
Intercept	-0.211	-0.057	-0.056
	(-1.07)	(-0.79)	(-0.77)
R^2	0.035	0.022	0.024
Avg. n	2977	3413	3412
Sample period	Pre-RegSHO	RegSHO	RegSHO

Table VI
Cross-sectional Regressions of Returns on Retail and Institutional Shorting Interacted with Retail and Institutional Net Buying

This table presents results from daily Fama-MacBeth (1973) regressions of stocks' returns from days t + 2 through t + 20 on retail and institutional shorting variables interacted with measures of other retail and institutional traders' buy-sell imbalances and control variables measured as of day t. The independent variables are as defined in Table I, and all are standardized each day t. The dependent variable is a stock's Fama and French (1993) three-factor cumulative abnormal return, measured in percent, with factor loadings based on daily data from the prior year. The imbalance measures in Models 1 and 2 are computed using days t – 4 through t, while those in Models 3 and 4 are computed using days t – 25 through t – 5. Regressions apply observation weights equal to stocks' lagged gross returns. Models include the control variables in Table IV Model 5. The table reports average regression coefficients. Newey-West (1987) t-statistics with 19 lags appear in parentheses.

	Model 1	Model 2	Model 3	Model 4
Ln(*RtlShort*)	-0.141	-0.126	-0.139	-0.122
	(-5.62)	(-4.04)	(-5.52)	(-3.90)
Ln(*RtlShort*) x *SizeQuint*	0.032	0.050	0.030	0.049
	(2.02)	(2.54)	(1.88)	(2.48)
Ln(*RtlShort*) x *RtlBuyQuint*	-0.064	-0.060	-0.068	-0.061
	(-5.32)	(-3.93)	(-3.70)	(-2.70)
Ln(*RtlShort*) x *InstBuyQuint*	0.013	0.026	-0.009	0.005
	(1.44)	(2.31)	(-0.80)	(0.33)
Ln(*InstShort*)		-0.079		-0.081
		(-2.86)		(-2.97)
Ln(*InstShort*) x *SizeQuint*		-0.038		-0.040
		(-1.70)		(-1.82)
Ln(*InstShort*) x *RtlBuyQuint*		-0.012		0.017
		(-0.81)		(0.98)
Ln(*InstShort*) x *InstBuyQuint*		-0.024		-0.025
		(-1.90)		(-1.87)
SizeQuint	-0.126	-0.129	-0.125	-0.130
	(-4.58)	(-3.70)	(-4.53)	(-3.76)
RtlBuyQuint	0.081	0.050	0.027	0.011
	(5.00)	(2.72)	(1.54)	(0.48)
InstBuyQuint	-0.069	-0.073	-0.068	-0.071
	(-6.09)	(-5.63)	(-4.68)	(-3.73)
R^2	0.031	0.026	0.031	0.026
Avg. n	3276	3410	3265	3399
Sample period	Full	RegSHO	Full	RegSHO
Controls	Yes	Yes	Yes	Yes
Imbalance days	[-4,0]	[-4,0]	[-25,-5]	[-25,-5]

Table VII

Cross-Sectional Regressions of News and Non-news Period Returns on Retail Shorting

This table presents results from daily Fama-MacBeth (1973) regressions of Fama and French (1993) three-factor abnormal returns from days $t + 2$ through $t + 5$, measured in percent, on retail and institutional shorting as of day t and interactions between these variables and various news dummy variables. The variable $News_J[2,5]$ equals 1 if there is a type-J news story during days $t + 2$ through $t + 5$ and 0 otherwise. The types of news, as determined by RavenPack, include all relevant firm-specific stories, earnings stories, and analyst stories. The variable *Earn*[2,5] equals 1 if there is an earnings announcement during days $t + 2$ through $t + 5$ and 0 otherwise. The variable $News_J\_NoEarn[2,5]$ equals 1 if the type-J news story is not accompanied by an earnings announcement during days $t + 2$ through $t + 5$ and 0 otherwise. All models are estimated during the RegSHO sample period and include the control variables in Table IV Model 5. Independent variables are standardized each day t. Regressions weight observations by lagged gross returns. The table reports average regression coefficients. Newey-West (1987) t-statistics with four lags appear in parentheses.

	Model 1	Model 2	Model 3	Model 4	Model 5
Ln(*RtlShort*)	-0.028	-0.029	-0.029	-0.027	-0.023
	(-3.29)	(-3.58)	(-3.40)	(-3.27)	(-2.95)
Ln(*RtlShort*) x *SizeQuint*	0.013	0.010	0.009	0.010	0.011
	(2.35)	(1.93)	(1.74)	(2.01)	(2.22)
Ln(*RtlShort*) x $News_{All}[2,5]$	-0.025				
	(-2.16)				
Ln(*RtlShort*) x *Earn*[2,5]		-0.220	-0.219	-0.222	-0.224
		(-2.70)	(-2.68)	(-2.72)	(-2.74)
Ln(*RtlShort*) x $News_{All}\_NoEarn[2,5]$			-0.006		
			(-0.55)		
Ln(*RtlShort*) x $News_{Earnings}\_NoEarn[2,5]$				0.046	
				(0.60)	
Ln(*RtlShort*) x $News_{Analyst}\_NoEarn[2,5]$					-0.099
					(-2.14)
Ln(*InstShort*)	-0.004	-0.011	-0.001	-0.011	-0.003
	(-0.49)	(-1.46)	(-0.08)	(-1.36)	(-0.34)
Ln(*InstShort*) x *SizeQuint*	-0.006	-0.008	-0.003	-0.008	-0.004
	(-0.90)	(-1.30)	(-0.55)	(-1.30)	(-0.71)
Ln(*InstShort*) x $News_{All}[2,5]$	-0.028				
	(-2.45)				
Ln(*InstShort*) x *Earn*[2,5]		-0.112	-0.117	-0.113	-0.117
		(-1.38)	(-1.44)	(-1.39)	(-1.43)
Ln(*InstShort*) x $News_{All}\_NoEarn[2,5]$			-0.031		
			(-2.84)		
Ln(*InstShort*) x $News_{Earnings}\_NoEarn[2,5]$				-0.003	
				(-0.05)	
Ln(*InstShort*) x $News_{Analyst}\_NoEarn[2,5]$					-0.206
					(-4.04)
R^2	0.025	0.027	0.028	0.030	0.029
Avg. n	3425	3425	3425	3425	3425
Controls	Yes	Yes	Yes	Yes	Yes

Table VIII
Cross-sectional Regressions of Returns on Insider Trading and Retail Shorting of Different Order Sizes

This table presents results from daily Fama-MacBeth (1973) regressions of stocks' returns from days $t + 2$ through $t + 20$ on retail shorting (*RtlShort*) and control variables measured as of day t. The variable *InsideSale* equals one if during days-[-4,0] there is an opportunistic insider sale as in Cohen, Malloy, and Pomorski (2012) and zero otherwise. The variables *RtlShortLarge*, *RtlShortMedium*, and *RtlShortSmall* are separate weekly retail short selling measures based on orders of varying sizes according to each stock's 25^{th} and 75^{th} order size percentiles computed over the prior quarter. The independent variables are as defined in Table I, and all are standardized each day t. The dependent variable in a stock's Fama and French (1993) three-factor cumulative abnormal return, measured in percent, with factor loadings based on daily data from the prior year. Models 1 and 3 (2 and 4) include control variables in Table IV Model 3 (Model 5). Regressions apply observation weights equal to stocks' lagged gross returns. The table reports average regression coefficients. Newey-West (1987) t-statistics with 19 lags appear in parentheses.

	Model 1	Model 2	Model 3	Model 4
Ln(*RtlShort*)		-0.184		-0.145
		(-7.46)		(-5.56)
Ln(*RtlShort*) x *SizeQuint*				0.031
				(1.94)
Ln(*RtlShortLarge*)	-0.094		-0.078	
	(-4.82)		(-4.41)	
Ln(*RtlShortLarge*) x *SizeQuint*			0.020	
			(1.59)	
Ln(*RtlShortMedium*)	-0.109		-0.081	
	(-4.26)		(-3.56)	
Ln(*RtlShortMedium*) x *SizeQuint*			0.013	
			(0.89)	
Ln(*RtlShortSmall*)	-0.073		-0.053	
	(-4.11)		(-3.17)	
Ln(*RtlShortSmall*) x *SizeQuint*			0.020	
			(1.84)	
InsideSale		-0.256		-0.246
		(-2.15)		(-2.07)
R^2	0.028	0.028	0.030	0.029
Avg. *n*	3278	3278	3278	3278
Controls	Yes	Yes	Yes	Yes

Table IX
Robustness Regressions

This table presents results from daily Fama-MacBeth (1973) regressions of stocks' returns from days $t + 2$ through $t + 20$ on retail shorting (*RtlShort*) and control variables measured as of day t. Model 1 repeats Table IV Model 5 as a benchmark. Model 2 includes an interaction between retail shorting and a proxy for routing brokers' internalization activity, *IntQuant*, as defined in Section IV.A. Model 3 excludes controls for past returns. Models 4 and 5 include interactions between retail shorting and proxies for short-selling constraints, as measured by *HighFails* or *NoOption*. The *HighFails* dummy equals one if fails-to-deliver shares exceed 0.1% of shares outstanding in the prior week. The *NoOption* dummy equals one if a stock has no traded options in the prior quarter. Other variables are as defined in Table I, and all are standardized each day t. The dependent variable is a stock's Fama and French (1993) three-factor cumulative abnormal return, measured in percent, with factor loadings based on daily data from the prior year. Models include control variables in Table IV Model 5. Regressions apply observation weights equal to stocks' lagged gross returns. The table reports average regression coefficients. Newey-West (1987) t-statistics with 19 lags appear in parentheses.

	Model 1	Model 2	Model 3	Model 4	Model 5
Ln(*RtlShort*)	-0.146	-0.129	-0.146	-0.148	-0.163
	(-5.58)	(-2.98)	(-5.44)	(-5.75)	(-5.10)
Ln(*RtlShort*) x *SizeQuint*	0.031	0.034	0.038	0.029	0.044
	(1.93)	(2.03)	(2.39)	(1.60)	(2.53)
Ln(*RtlShort*) x *HighFails*				-0.042	
				(-0.55)	
Ln(*RtlShort*) x *NoOption*					0.055
					(1.11)
Ln(*RtlShort*) x *IntQuant*		0.004			
		(0.18)			
SizeQuint	-0.120	-0.100	-0.098	-0.112	-0.165
	(-4.35)	(-2.90)	(-3.51)	(-3.73)	(-5.89)
HighFails				-0.167	
				(-1.36)	
NoOption					-0.289
					(-3.10)
IntQuant		-0.032			
		(-0.84)			
R^2	0.029	0.031	0.019	0.029	0.030
Avg. *n*	3278	3223	3278	3402	3278
Return controls	Yes	Yes	No	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes

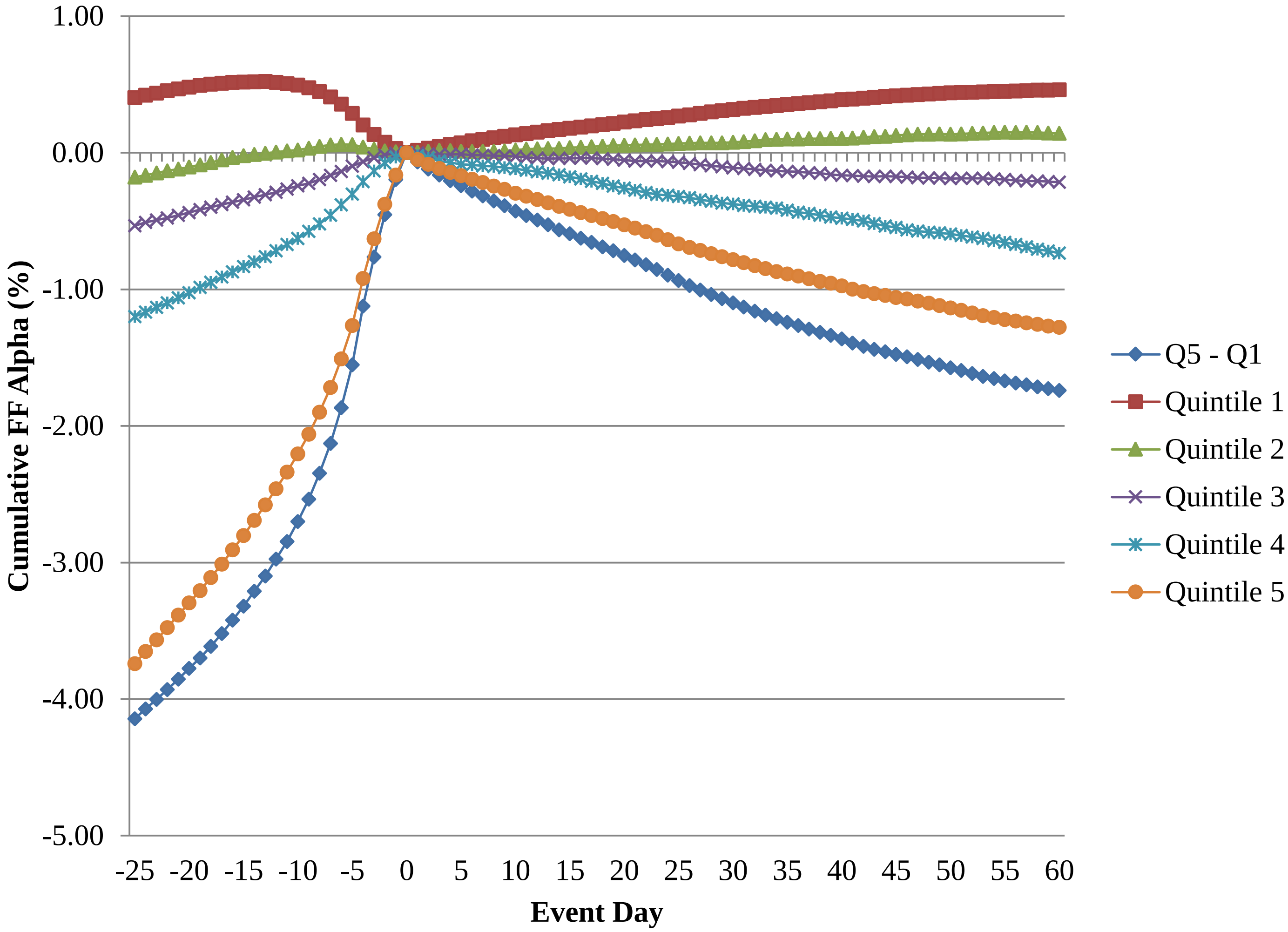

Figure 1. Alphas of portfolios based on retail shorting.
Each day *t*, we sort firms into quintiles based on weekly retail short selling scaled by total volume (*RtlShort*). Quintile 1 comprises stocks with zero retail shorting. Stocks with positive retail shorting are evenly distributed across quintiles 2 through 5, with quintile 2 containing stocks with the lowest positive shorting and quintile 5 containing stocks with the most shorting. Stocks' portfolio weights are based on their prior-day gross returns to mitigate the Blume and Stambaugh (1983) bias. For each event day from $t - 25$ to $t + 60$, we compute Fama-French (1993) three-factor (FF3) alphas for each shorting quintile and a spread portfolio that is short stocks in shorting quintile 5 and long stocks in shorting quintile 1 (Q5 – Q1). We plot cumulative alphas for each shorting quintile portfolio and for the spread portfolio.