DO INDIVIDUAL INVESTORS CAUSE POST-EARNINGS ANNOUNCEMENT DRIFT? DIRECT EVIDENCE FROM PERSONAL TRADES

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DO INDIVIDUAL INVESTORS CAUSE POST-EARNINGS ANNOUNCEMENT DRIFT? DIRECT EVIDENCE FROM PERSONAL TRADES

This study tests whether naïve trading by individual investors, or some class of individual investors, causes post-earnings announcement drift (PEAD). Inconsistent with the individual trading hypothesis, individual investor trading fails to subsume any of the power of extreme earnings surprises to predict future abnormal returns. Moreover, individuals are significant net buyers after *both* negative and positive extreme earnings surprises, consistent with an attention effect, but not with their trades causing PEAD. Finally, we find no indication that trading by individuals explains the concentration of drift at subsequent earnings announcement dates.

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I. INTRODUCTION

Post-earnings announcement drift (PEAD) is the tendency for stocks to earn positive average abnormal returns in the three quarters subsequent to extreme positive earnings surprises and, more strongly, to earn negative average abnormal returns in the three quarters subsequent to extreme negative earnings surprises. This phenomenon is widely regarded as a puzzle from the perspective of the efficient markets hypothesis.

Bernard and Thomas (1990) suggest that investor naïveté about the time series properties of earnings may drive drift.[1] Several studies suggest that institutional investors are more sophisticated traders than are individual investors (Hand 1990; Lee et al. 1991; Walther 1997; Grinblatt and Keloharju 2000; Balsam et al. 2002; Bonner et al. 2003; Asthana et al. 2004; De Franco et al. 2006; Mikhail et al. 2007), and some suggest that PEAD may result from the trading activity of individuals. We call this proposition *the individual trading hypothesis*.

Bartov et al. (2000) provide a degree of support for this argument. They report that under some specifications, PEAD is strongest in firms with low institutional shareholdings (and thus, high individual shareholdings), but that the results are mixed. Because of this, the authors point out that their results do not provide strong evidence about whether individual investors cause PEAD. More recent evidence from changes in institutional ownership is mixed as to whether institutions are sophisticated arbitrageurs. Although Burch and Swaminathan (2003) document

[1] There is debate as to the nature of bias that might induce naïve investors to trade in a way that generates drift. Alternatives to the type of naïveté proposed by Bernard and Thomas (1990) are proposed in subsequent research (Ball and Bartov 1996; Jacob et al. 2000). Barberis et al. (1998) and Daniel et al. (1998) provide formal models in which PEAD can arise as an underreaction to earnings, owing to psychological biases such as overconfidence and conservatism. There is debate in the empirical literature about whether PEAD reflects a rational risk premium, a simple tendency for investors to underreact to earnings news, or a more complex intertemporal pattern of short-term underreaction and long-term overreaction to earnings (e.g., Lakonishok et al. 1994; Bernard et al. 1997; Dechow and Sloan 1997; Lee and Swaminathan 2000; Daniel and Titman 2006).

that institutional investors buy after both positive and negative earnings surprises, Ke and Ramalingegowda (2005) report that some types of 'transient' institutions arbitrage drift.[2]

Evidence from large versus small trades made after earnings announcements is also mixed as to whether naïve individual investors causes drift. Results in Bhattacharya (2001) and in Battalio and Mendenhall (2005) are consistent with individuals causing PEAD[3] but those in Shanthikumar (2004) are not.[4] However, as the authors of these studies recognize, trade size is not necessarily a good indicator of whether the trader is an individual or institution, nor whether the trader is sophisticated. To reduce the price impact of their trades, sophisticated investors split orders and make smaller trades when they disagree with the market price (Barclay and Warner 1993; Bernhard and Hughson 1997; Diether et al. 2007).[5] Furthermore, Campbell et al. (2005)

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[2] Some recent papers (e.g., Musto 1999; Griffin et al. 2003; Jackson 2003a, 2003b; Coval et al. 2005; Dasgupta et al. 2006; Kaniel et al. 2008) suggest that there is no simple dichotomy between naïve individuals and smart institutions. For example, there is evidence that some institutional investors engage in trades for 'window-dressing' purposes (Musto 1999), and some individual investors are able to achieve persistent high returns relative to standard benchmarks (Coval et al. 2005). Furthermore, institutions chase daily trends apparently without profiting thereby (Griffin et al. 2003). Jackson (2003a) provides evidence suggesting that institutions, rather than individuals, make non-fundamental based trades, and Jackson (2003b) reports that the net trades of brokerage clients in Australia positively forecast future returns. Dasgupta et al. (2006) provide evidence that stocks which were purchased by several institutional investors (perhaps owing to 'herding') over the preceding five quarters earn low returns, and that those that were sold earn high returns. With respect to PEAD itself, Burch and Swaminathan (2003) provide some evidence consistent with institutions in the aggregate driving poor returns after negative earnings surprises, and Ke and Ramalingegowda (2005) provide evidence that 'transient' institutions act as earnings contrarians, selling after positive surprises and buying after negative ones.

[3] Lee (1992) provides mixed evidence in this regard. He examines inferred-signed trades of investors for a sample of approximately 230 firms during 1988. He finds that small trades tend to be inferred-buys for more than two days after both favorable and unfavorable earnings surprises relative to analyst forecasts. Since individual investors tend to make smaller trades, Lee suggests that his findings are consistent with earnings announcements drawing the attention of individual investors to the stock. Bhattacharya (2001) provides evidence that the volume of small trades, but not large trades, is associated with the magnitude of random walk earnings surprises, suggesting that investors who make small trades (presumably less sophisticated investors) may cause PEAD. Battalio and Mendenhall (2005) provide evidence that large trades (made presumably by more sophisticated investors) respond more strongly to surprises relative to analyst forecasts, whereas small trades respond more strongly to surprises relative to a seasonal random walk model.

[4] Shanthikumar (2004) documents subtle patterns in the behavior of large and small trades made in relation to earnings surprises measured relative to analyst forecasts versus a seasonal random walk model, and concludes that there is some evidence of underreaction in *both* large and small trades.

[5] Barclay and Warner (1993) find that medium-sized trades affect price more than do large- or small-sized trades. Diether et al. (2007) find that small sell trades predict negative future returns, while medium and large sell trades do not. Both kinds of evidence suggest that investor sophistication is not monotonically increasing with trade size.

provide evidence that institutions tend to make both very large and very small trades, with individuals tending to make intermediate-sized trades.

In this paper, we offer direct tests of the individual trading hypothesis by examining actual individual investor trades following earnings announcements (rather than relying on trade size to proxy for trader identity). We examine all trades made by a random sample of individual investors through a major discount brokerage from 1991 through 1996. Under the individual trading hypothesis, trading by individual investors impedes a full price response after an earnings announcement, leading to underreaction and PEAD. We therefore examine whether individual trading after earnings announcements subsumes some of the ability of earnings to predict subsequent abnormal returns. We also test whether, as called for by the individual trading hypothesis, individual investors (as a group or in relevant sub-categories) trade as contrarians to earnings surprises. Finally, we examine individual trading in the days surrounding subsequent quarterly earnings announcements, to see whether these trades are consistent with individuals driving the concentration of drift at later earnings announcement dates.

Our paper differs from extant studies of PEAD because we examine the relation between trading behavior and subsequent returns, along with the relation between trading behavior and earnings surprises. Since PEAD is a returns and an earnings phenomenon, doing so allows us to speak more directly to the individual trading hypothesis. Our paper also differs from past studies because we examine *actual daily signed trades* made by individual investors after earnings surprises while past studies have used indirect methods.[6]

[6] These include examining the fraction of shares held by institutions, inferring institutional trades from quarterly changes in stockholdings, inferring the identity of investors (i.e., individual versus institutions) by trade size, inferring probabilistically the direction of the trade (i.e., buy versus sell) from microstructure data, and/or testing the properties of unsigned trading volume.

A key advantage of using data on actual daily signed trades is that it allows us to incorporate the daily timing of the individual investor trades as well as whether the trade is in the same direction as the earnings surprise (i.e., net buying after good news and net selling after bad news) or in opposition to it. Furthermore, our data provides us with two proxies for individual investor sophistication – capital invested and total trading activity. We use these proxies to test whether the least sophisticated investors – those with relatively little capital invested with the discount broker and/or those with relatively little trading experience – drive PEAD.

Since markets must clear, evidence from institutional trading is complementary with and potentially informative about individual trading. However, past evidence on institutional trading does not capture the information provided by our sample and method. Specifically, the CDA-Spectrum position data, used in institutional investor studies, is derived from quarterly SEC 13f filings. This sample is not ideal for testing the individual trading hypothesis for at least two reasons. First, individual positions are not just the inverse of institutional positions as inferred from 13f filings.[7] Second, quarterly net position changes are aggregates of trades made at different times throughout the quarter.

A benefit of our data is that it allows us to distinguish trades made one day before an earnings announcement from those made one day after – cases which have completely different implications for whether traders are driving drift. Empirically, PEAD has a sharp conditioning date (i.e., the earnings announcement date) and the effect is concentrated on particular days after the initial announcement of extreme earnings (i.e., around later earnings announcement dates). The use of daily data permits us to test whether investors purchase the day before the subsequent

[7] 13f data on institutions includes only institutions with greater than $100 million invested in equity securities, and even for these institutions, only positions of at least 10,000 shares or $200,000 need be disclosed. Thus, 13f data cannot resolve which other categories of investors – individuals, smaller institutions, or large institutions making smaller trades – help drive drift.

earnings announcement and reverse their position a few days thereafter, but with quarterly positions data, such behavior is invisible.

If individuals drive PEAD, then individual net sells after good earnings news (which generate underpricing) should predict high subsequent stock returns, and individual net buys after bad news (which generate overpricing) should predict low subsequent returns. Therefore, we examine the relation between earnings surprises, individual trading, and subsequent stock returns. Furthermore, if individual trading is a source of the relation between earnings surprises and subsequent returns, then the predictive power of individual trades should remain even after controlling for the earnings surprise. Most directly of all, if individual trading drives PEAD, then individual trading after earnings surprises should subsume part or all of the ability of the earnings surprise to predict subsequent returns. Thus, our paper differs from prior work in directly examining whether trading by individual investors subsumes the ability of earnings surprise to predict subsequent returns.

If individual investors are naïve with respect to earnings surprises, we expect to see significant net buying after negative earnings surprises and significant net selling after positive earnings surprises. Furthermore, given the evidence of stronger downward PEAD than upward PEAD, the tendency of naïve individuals to buy after negative surprises should be stronger than the tendency of naïve individuals to sell after positive surprises.

We also perform tests focusing on trades made during the days prior to the subsequent quarterly earnings announcement, where PEAD is strongest (Bernard and Thomas 1990). Given a positive (negative) earnings surprise, sophisticated investors should buy (sell) shares in the days prior to the next quarterly earnings announcement. For drift to exist despite arbitrage by sophisticated investors, naïve investors must be trading in the opposite direction, impeding the rapid adjustment of prices. Thus, if individual investors are naïve, they will sell (buy) just prior to the earnings announcement immediately following a(n) favorable (unfavorable) earnings announcement.

Results of our tests suggest that individual investors do *not* cause PEAD. We base this conclusion on three kinds of evidence. First, controlling for net trading by individuals does not reduce the ability of extreme earnings surprises to predict subsequent returns. Second, rather than trading in opposition to earnings surprises, individuals are significant net purchasers after both good and bad earnings news.[8] This pattern also holds for every investor class, suggesting that even the least sophisticated investors (i.e., those with low invested capital and low trading activity) are not driving drift. Third, when we measure the extent to which, conditional on an earnings surprise at a given date, individuals make abnormal trades in the days just prior to or just after the *subsequent* quarterly earnings announcement, our results are not consistent with the trading pattern (discussed earlier and in Section II) predicted by the hypothesis that individual investor trading causes the concentration of PEAD at subsequent earnings announcement dates.

The remainder of this paper is structured as follows. Section II explains in more detail how trading by individual investors could induce PEAD. Section III contains a description of the data, sample selection criteria, variable definitions, and descriptive statistics. In Section IV, we examine the relation between individual trading, earnings surprises, and subsequent stock returns. Section V provides evidence on individual investor trading following extreme earnings surprises, and section VI examines individual trading, conditional on an extreme earnings surprise, around the subsequent quarterly earnings announcement. Section VII concludes.

[8] Interestingly, we find that the greater the absolute value of the earnings surprise, the greater the quantity of shares purchased, but that the direction of trading is unrelated to the direction of the news. This is consistent with trading by individuals being influenced by a news attention effect (Barber and Odean 2008).

II. NAÏVE TRADING AND POST-EARNINGS ANNOUNCEMENT DRIFT

Testing whether individuals are the source of drift is useful only if we cannot exclude this possibility on *a priori* conceptual grounds. A possible argument against individuals driving drift is that institutions are big traders and therefore dominate price-setting. However, during the last year of our sample period, individuals held 48 percent of the market value of common stock (Securities Industry Fact Book 2002). Thus, there is reason to expect individuals (as well as institutions) to play a significant role in price-setting.

One could also argue that drift could not represent a market inefficiency because if naïve trading were to induce such a pattern of mispricing, smart arbitrageurs would find it profitable to trade to exploit it. This would tend to attenuate the pattern. However, a literature in behavioral finance and accounting argues that despite arbitrage by sophisticated investors, the behavior of imperfectly rational investors can induce mispricing (such as PEAD), and under some circumstances, mispricing can persist.[9] If naive investors are subject to common misperceptions, then in equilibrium, these misperceptions influence price by an amount that depends on the relative sizes and risk tolerances of different investor groups. (And since the individual trading hypothesis requires common misperceptions, if individual investors drive PEAD, we expect to see evidence of misperceptions within our sample of individual investor trades.) Evidence exists that, at least in some cases, the beliefs of unsophisticated investors influence security prices.[10]

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[9]See, for example, the models and surveys in DeLong et al. (1991), Kandel and Pearson (1995), Shleifer and Vishny (1997), Daniel et al. (1998), Fischer and Verrecchia (1999), Lee (2001), and Hirshleifer and Teoh (2003). These models indicate that irrational investors can influence price in the short run. Specifically, if irrational investors have non-negligible risk-bearing capacity, in general, they affect price. Moreover, whether rational investors will earn high profits at the expense of irrational investors, so that in the long run, the influence of irrational investors is eliminated, depends on the model specification.

[10] For example, confusion by investors over ticker symbols can cause short-run price reactions to news about unrelated firms (Rashes 2001). Moreover, during the Internet boom, relative mispricing between parent firms and sexy high-tech divisions existed (Lamont and Thaler 2003).

Furthermore, if sophisticated investors are risk averse, the degree to which they arbitrage mispricing will be limited. Finally, Lamont and Thaler (2003) discuss how limits to short-selling can prevent prices from adjusting to reflect the views of sophisticated investors.

Some authors suggest that PEAD represents a market inefficiency (e.g., Bernard and Thomas 1989, 1990; Bernard et al. 1997; Fama 1998; Mendenhall 2004), while others suggest that PEAD may reflect estimation issues such as a return benchmark not commensurate with risk (e.g., Ball 1992). Given existing theory and evidence, the hypothesis that PEAD is a market inefficiency resulting from individual investor trading deserves to be tested.

A possible limitation of using our sample to test the individual trading hypothesis is that it contains trades made by a random sample of investors at a single major discount brokerage. Whether this allows for an unbiased test depends on whether the investors at this brokerage are representative of individual investors as a whole. Several kinds of evidence suggest that the sample *is* broadly representative. First, early in the sample period, this brokerage had more than 1.25 million clients while the total number of individuals with direct share ownership of U.S. firms in the closest comparison year was 29.2 million (see "Share ownership 2000," NYSE, http://www.nyse.com/pdfs/shareho.pdf). Therefore, the brokerage represented approximately 4 percent of the population of individual shareholders. Second, we have no reason to expect that the individuals dealing with this brokerage are unusually naïve or sophisticated, relative to those dealing with other brokerages. Rather, the sample is a broad cross-section which includes a mixture of both traditional and online traders. Third, Ivkovich et al. (2005) document that patterns of stock sales by investors in our sample correspond well with general data on investor stock sales reported on income tax returns, and Ivkovich et al. (2007) provide evidence that the number of stocks and the total stock portfolio value held by households in our sample correspond well with data from the Federal Reserve Board's Survey of Consumer Finances on the stock holdings in the general U.S. population. Similarly, Barber and Odean (2000), Dhar and Kumar (2002), and Ivkovich et al. (forthcoming) provide other tests which support the representativeness (in relation to demographic characteristics) of the sample for U.S. investors in general.[11]

There are, however, some differences between customers of the discount brokerage that we examine and other individual investors. For example, customers of full-service brokerages are likely to receive more advice about which stocks to choose, which could steer them away from naïve trades. Furthermore, our sample is likely to contain relatively few extremely wealthy individual investors. Again, we expect such investors to be relatively sophisticated and to have the benefit of professional advice. These considerations suggest that our sample is comprised of the subset of individual investors who would be most likely to drive drift. However, for agency reasons, full-service brokerages might encourage investors to make naïve active trades to boost commissions (a practice known as 'churning'). Moreover, it is possible that overconfidence or agency problems on the part of these full-service brokerages might cause investors to make systematic errors in response to earnings announcements. Thus, we cannot rule out the possibility that drift is driven by some groups of individual investors that are not part of our sample. What we can be sure of is that the investors within our sample *do* make other systematic trading errors, and that there is ample power within our dataset to identify these systematic trading errors (see, for example, Odean (1999) and Barber and Odean (2000)).

Even taking as given that individual investors can affect prices, it could be argued that a sample that includes only a subset of individual investors may not include the 'marginal

[11] For example, Dhar and Kumar (2002) and Ivkovich et al. (forthcoming) verify that the portfolio size in our overall sample and for different investor age groups is very similar to that for comparable U.S. investors in general.

investors' who determine prices. However, in models of securities pricing that explicitly analyze how prices are determined in equilibrium through the market-clearing condition, prices reflect a weighted average of the beliefs of different traders, where the weights reflect the risk-bearing capacities of the different traders (Kandel and Pearson 1995; Hirshleifer and Teoh 2003). Thus, in these models, there is no single decisive group of 'marginal investors' – the beliefs of every investor influence price.[12] Furthermore, we believe that the conclusion to be drawn from the evidence that we present is independent of whether one adheres to a 'marginal investor' perspective or to the weighted average perspective. Under either perspective, if our sample is representative, then our tests tell us about the behavior of individual investors as a whole, and if our sample is unrepresentative, then our tests do not speak to whether some other group of individual investors drives drift.

Post-earnings-announcement drift is typically characterized as an underreaction to earnings news. Bernard and Thomas (1990) show that seasonal quarterly earnings changes are positively serially correlated. That is, after a positive (negative) earnings surprise, subsequent earnings surprises tend to be predictably positive (negative). Furthermore, abnormal stock returns subsequent to earnings surprises are predictable. While stock prices generally increase (decrease) upon the announcement of good (bad) earnings news, they do not seem to increase (decrease) enough. In consequence, returns are, on average, abnormally high (low) for the next three quarters (but at a decaying rate).

A group of investors that drives PEAD would trade in a way that opposes a full and rational stock price adjustment in response to earnings surprises. Thus, after favorable

[12] In general, in microeconomics, the price of a commodity depends on the demand curves of all traders, not just a 'marginal' subset. Similarly, models of security trading show that the demand curves of all investors play a role in price determination. Hirshleifer and Teoh (2003) analyze and discuss the "marginal investor" issue in an accounting context.

(unfavorable) earnings news, these individuals would, on average, sell (buy) stock. In other words, they could be contrarian with respect to current earnings news. This suggests a simple test of whether individual investors cause PEAD: test whether individuals, on average, buy after extreme negative earnings surprises and sell after extreme positive earnings surprises.[13]

Furthermore, if individual trading causes prices to underreact to earnings news (which manifests as PEAD), then net purchases by individuals must be related to subsequent abnormal stock returns. Thus, the individual trading hypothesis predicts that individual net selling generates underpricing, and should therefore predict high subsequent stock returns, and that individual net buying generates overpricing, and should therefore predict low subsequent stock returns.

Finally, if trading by individual investors is a source of the relation between extreme earnings surprises and subsequent returns, then in those cases where positive (negative) earnings surprises are followed by relatively little net individual investor selling (buying), there should be relatively little upward (downward) drift. Similarly, drift should be stronger in those cases where individuals trade in a more strongly contrarian fashion to the earnings surprise. Thus, individual net purchases should largely or even completely subsume the ability of the earnings surprise to predict subsequent returns. This implication offers a direct and powerful test of the individual trading hypothesis.

[13] Such a test assumes that the benchmark against which to measure buying or selling pressure on price is zero. For markets to clear, the average net trade by investors after an earnings surprise is zero. Thus, the deviation in a group's net purchases from zero is a measure of the degree to which trading by that group differs from trading by other groups. After an earnings surprise, net purchases by a group creates upward price pressure, and net sales creates downward price pressure. Of course, there are rational models with heterogeneous investors which can accommodate net purchases or sales by individual investors after earnings surprises. Thus, the more general premise of our basic trading tests is that any pressure toward mispricing exerted by individual investors is positively correlated with their net purchases. This rules out the possibility that when individuals are buying they are on average exerting downward price pressure (toward underpricing), and when they are selling, they are exerting upward price pressure (toward overpricing). The premise needed for our return prediction tests is even milder – if trading by individuals is driving drift, the price pressure they exert is manifested observably in their trades. We discuss this issue further in the concluding section.

Past empirical literature documents that after an extreme earnings surprise, drift is disproportionately concentrated in the days after each of the next two quarterly earnings announcements, and that there is a modest but significant reversal in the days following the fourth subsequent announcement (Bernard and Thomas 1989,1990). If naïve investors are driving drift, such a pattern would be apparent in trading by naïve investors around subsequent earnings announcements.

Specifically, conditional on an initial earnings surprise, naïve and sophisticated investors would differ in their assessments of fundamental value. For example, after a favorable announcement, sophisticated investors would believe that price is too low, and their purchases would drive the price higher. Naïve traders would believe that the price has moved up too much, and would therefore tend to sell. During the subsequent quarter, newly arriving public information may not suffice to resolve the gap between naïve and sophisticated expectations. If not, then before the next quarterly earnings announcement, sophisticated traders would buy and naïve traders would sell. Thus, selling by naïve traders would offset the pressure of rational arbitrage trading, preventing the price from adjusting upward sufficiently prior to the next earnings announcement. On average, subsequent earnings would be higher than expected by naïve traders, leading to an abnormally high average return on the earnings announcement date.

If PEAD represents a market inefficiency, sophisticated investors can exploit this pattern near subsequent earnings announcements by using a dynamic trading strategy. For example, after a positive earnings surprise, investors can earn high returns by buying shares a few days prior to the next quarterly earnings announcement and partly unwinding their positions in the days following the announcement. Such a strategy offers a favorable balance between risk and expected return.[14, 15] As discussed above, naïve investors incorrectly believe it is profitable to take the opposite sides of these trades. When there is a negative earnings surprise, sophisticated investors should do the reverse, selling just before the subsequent earnings announcement. In either case, sophisticated trading tends to accelerate the adjustment of prices.

If unopposed by the trades of naïve investors, such arbitrage would eliminate the concentration of PEAD at the subsequent earnings announcement dates. However, if naïve investors trade in the opposite direction (further delaying price adjustment), the concentration of drift remains. Thus, if individual investors are naïve, conditional on a favorable (unfavorable) earnings announcement, they will sell (buy) just prior to the next quarterly earnings announcement. These predictions have not hitherto been tested.

III. TRANSACTION DATA, SAMPLE SELECTION, VARIABLE DEFINITIONS, AND DESCRIPTIVE STATISTICS

Transaction Data

The data used in this study consists of trades made by a random sample of 78,000 households with 158,034 accounts at a large discount brokerage. The brokerage made 3,075,797 trades on behalf of these households between January 1991 and December 1996 inclusive. 1,969,747 of these trades involve common stock, while the remainder involve mutual fund shares, bonds, and other securities. The sample is, by construction, a random sample from the population of households with accounts at the brokerage, and is drawn independently of all other variables. Therefore, the sample accurately and without bias represents the full set of individual

[14] While the risk that is related to earnings announcements is greater at the time of the earnings announcements, the expected return is also greater around subsequent earnings announcements. Concentrating trades near the dates of earnings announcements reduces extraneous risk that is unrelated to these announcements.

[15] Even investors who do not trade actively to exploit drift can benefit in the quarters following a positive earnings surprise by advancing any planned purchases from a few days after to a few days before a subsequent earnings announcement, and by deferring any planned sales from a few days before to a few days after a subsequent earnings announcement.

investors at this brokerage firm. The full set is comprised of approximately 1.25 million households, so our sample represents a set of investors about 20 times as large as the sample itself.

We classify the households in our sample as *actively-trading investors* (6,000 households), *high-capital investors* (12,000 households), and *general investors* (60,000 households).[16] Any investor that conducts more than 48 trades in a year is classified as actively-trading, investors that are not classified as actively-trading and have more than $100,000 of invested wealth at any time are classified as high-capital investors, and all remaining investors are classified as general investors.

The high-capital and actively-trading investor classifications measure two aspects of investing experience – the amount of wealth invested and the frequency of trades. We use these two aspects of investing experience to proxy for individual investor sophistication. With respect to the amount of wealth invested, an investor who has a greater amount of wealth invested has a greater incentive to learn about stock trading.[17] Furthermore, greater invested wealth may be associated with past stock market success. With respect to the frequency of trades, investors may learn through experience about the time-series properties of earnings and about market price patterns. This suggests that more sophisticated individual investors may be better at avoiding errors in trading in response to earnings announcements, or may even be good at exploiting PEAD.

[16] Here, we follow the brokerage's classification scheme, but change the brokerage's label from affluent to high-capital investors to more accurately reflect the contents of this category.
[17] Consistent with this, Cready (1988) finds that wealthy institutional investors trade more quickly in response to earnings announcements, suggesting that the value of information increases with wealth.

Sample Selection and Variable Definitions

Our sample consists of all firm-quarters (with sufficient Compustat data) with at least one trade made by our sample of investors during the 25 days following an earnings announcement. From Compustat, we require primary earnings per share before extraordinary items (quarterly data item 19) at both quarter *t* and quarter *t-4*, price per share at the end of quarter *t* (item 14), and the corresponding split-adjustment factors (item 17). Additionally, we require that the earnings announcement date and the number of shares outstanding at the end of the quarter (item 61) be available. Using the Compustat data, we construct the standardized unexpected earnings (SUE) as the seasonal difference in split-adjusted earnings per share scaled by the split-adjusted end of quarter price (i.e., the price at the end of the quarter prior to the earnings announcement), similar to Bernard et al. (1997). We calculate SUE for all New York Stock Exchange (NYSE) firm-quarters with sufficient data available, and using this data, we define SUE 1 observations as the 10 percent of firm-quarters with the most negative random walk earnings surprise, SUE 10 observations as the 10 percent of firm-quarters with the most positive random walk earnings surprise, and SUE 5 and 6 observations as the 20 percent of firm-quarters with the smallest (in absolute value) random walk earnings surprise.

For each firm-quarter, we identify all trades of the firm's common stock made by our sample of investors during the following quarter. We measure their trading activity over various event windows, ranging in length from one day to a whole quarter. For example, we measure the trading activity on the earnings announcement day for quarter *q*, for firm *j*, by summing the number of common shares of firm *j* traded on the earnings announcement day by any investor in the dataset. Following Burch and Swaminathan (2003), we scale by the number of common shares outstanding for firm *j* at the end of quarter *q*. We repeat this procedure for subsamples of trades (i.e., for buys and sells) and for subsamples of investors (i.e., for high-capital investors, actively-trading investors, and general investors). We measure net purchases as the difference between the number of shares purchased and the number of shares sold in the event window, scaled by millions of shares outstanding at quarter-end.[18]

To address the possibility that individuals rationally trade differently from other investors in response to an earnings surprise, in our tests, we examine the deviation between individual investor trading of firms with extreme earnings surprises and individual investor trading of firms with little or no earnings surprise. That is, we use individual trades in the shares of firm-quarters in SUE 5 and 6 as the benchmark, and test how the trades of shares of firm-quarters in SUE 1 and of firm-quarters in SUE 10 differ from this benchmark.

Descriptive Statistics

Our final sample consists of 539,239 trades made in the 25 days following 51,627 earnings announcements. 54 percent of these trades are buys, with a mean number of shares purchased of 530, and 46 percent of these trades are sells, with a mean number of shares sold of 623.[19] Although 76.9 percent of the investors are classified as general investors, these investors make only 46 percent of the trades, and the 15.4 percent of the sample that is classified as high-capital investors make only 13 percent of the trades. The remaining 41 percent of the trades are made by the 7.7 percent of the sample that is classified as actively-trading investors. Further descriptive statistics are provided in Table 1.

[18] When the number of shares purchased exceeds the number of shares sold in the event window, net purchases are positive. When the number of shares sold exceeds the number of shares purchased in the event window, net purchases are negative.
[19] Table 1 descriptive statistics differ slightly from these because the Table 1 sample includes all trades of common stock made by investors in the dataset and is not restricted to trades of firms with available Compustat data.

Panel A of Table 1 describes the distribution of trade sizes for both buys and sells by year. It is interesting to note the large number of large trades in the database. For example, the trade size is greater than $5,000 for approximately half of the trades, and at least 500 shares are traded in more than 25 percent of the trades. Prior studies use either the dollar value of the transaction or the number of shares traded to classify trades as being initiated by individuals or institutions, and using their cutoffs, prior studies would incorrectly classify these large trades as either institutional trades or as indeterminate. The wide variation in the frequency of trading among individuals is also of interest.

Panel B of Table 1 shows that while the median individual trades 4 times per year for a total of approximately $21,000, the median actively-trading investor trades 22 times a year for approximately $158,000. It is also interesting to note how skewed the trading volume (measured in dollar value and in number of trades) is. For example, the mean trading volume, measured in dollars per year, is greater than the third quintile, indicating that there are a few very large trades. Finally, actively-trading investors trade, on average, more than 6 times as often and more than 10 times as much (in dollar value) as general investors, and more than 4 times as often and more than 5 times as much (in dollar value) as high-capital investors. Because the actively-trading investors are indeed highly active, these investors may be disproportionately important in generating empirically observed price patterns. On the other hand, these traders may be more sophisticated than other individual investors, suggesting that they are not the source of PEAD.

Put Table 1 about here.

IV. INDIVIDUAL INVESTOR TRADING AS A PREDICTOR OF POST-EARNINGS ANNOUNCEMENT DRIFT

As discussed in Section II, if trading by individual investors causes PEAD, then their net trading should negatively predict subsequent stock market returns. Furthermore, net trading by individuals should subsume part or all of the explanatory power of the earnings surprise for predicting subsequent abnormal stock returns. In this section, we examine the relation between individual investor trading and subsequent market-adjusted returns for the sample of investors as a whole and for the individual investor classes described previously.

Trading by Individual Investors and Subsequent Returns

Previous studies (e.g., Bernard and Thomas 1989, 1990) find that PEAD is strongest among firms with relatively extreme earnings surprises, suggesting that tests focusing on firms with extreme earnings surprises may be more powerful. Focusing on firms with extreme earnings surprises filters out the noise from firms with modest SUEs and little PEAD. Therefore, we restrict the sample for these analyses to those 4,405 firm-quarters in SUE deciles 1 and 10 with non-zero net buys in the 5 days following the earnings announcement.

Table 2 reports the results of regressions of abnormal returns (measured over the 3, 6, 9, and 12 months subsequent to the earnings announcement) on SUE (the decile rank of the earnings surprise), controlling for market-to-book ratio, size, and past momentum. To control for market-to-book, size, and momentum, we include as regressors in the first model the decile rank of the firm's market-to-book ratio (MTB), the decile rank of the firm's market value of equity (MVE), and the market-adjusted buy-and-hold returns over the 6 months prior to the earnings announcement date (momentum). The coefficients on SUE are highly significant in all returns windows, confirming that after controlling for size, market-to-book, and momentum, the PEAD effect was strong during our sample period. Thus, this time period is appropriate for testing whether individual investor trading drives PEAD.

Put Table 2 about here.

We next examine the effect of including the decile rank of net purchases (RANK NET PURCHASES) in the second regression model. Specifically, RANK NET PURCHASES is the decile rank based on the number of shares purchased minus the number of shares sold in days 1 through 5 relative to the earnings announcement date, scaled by the number of shares outstanding at the end of the fiscal quarter for which earnings is announced. Table 2 reveals that individual investor trading in the five days following extreme earnings announcements (RANK NET PURCHASES) is a significant negative predictor of future 3-, 6-, 9-, and 12-month stock returns, and that this effect is independent of the size, market-to-book, and momentum effects.

Table 2 also reveals that the PEAD effect is *not* subsumed by individual investor trading. That is, in every window, the coefficient on SUE remains highly significant ($p < .0001$) when RANK NET PURCHASES is included in the regression. One could argue that only a subset of individual investors drive PEAD. If so, including investors outside of this subset adds noise to our analyses and in this case, we may not expect RANK NET PURCHASES to subsume SUE completely. However, including RANK NET PURCHASES in the regression does not detract *at all* from the magnitude and significance of the SUE effect. Thus, the evidence strongly contradicts the individual trading hypothesis.

It is important to note that the failure of the individual trading hypothesis does not come from a lack of power. The test captures a strong and significant relation between SUE and future returns, and, after controlling for SUE, between RANK NET PURCHASES and future returns. Indeed, the coefficients on RANK NET PURCHASES from regressions that include SUE are larger and more significant than those from regressions (untabulated) that do not include SUE. Thus, individual investors have a special “skill” at picking losers, conditional on an extreme earnings surprise.[20]

In summary, these analyses reveal that individual investors trade foolishly in response to extreme earnings surprises in the sense that their trades in the 5 days following these surprises are negative predictors of returns over the next 3, 6, 9, and 12 months. This anti-arbitrage by individual investors suggests that they may be the driving force behind some kind of market inefficiency, perhaps losing money when the market misvaluation is corrected. However, this individual trading effect appears to be unrelated to PEAD. That is, there is no indication from the returns evidence that individual investors drive PEAD, nor is there any indication that PEAD underlies this individual investor trading effect.

Trading by Classes of Individual Investors and Future Returns

The results in the previous section rule out the hypothesis that the trades made by the individuals in our sample *in aggregate* are the source of drift. However, it remains possible that some unsophisticated class of individuals drives PEAD, and that this is being masked by sophisticated trades in the opposite direction made by another subset of individuals. Thus, to explore further whether a class of individuals drives PEAD, we report similar returns regressions in Table 3 for each of the investor classes. Most regressors other than RANK NET PURCHASES remain significant and for all investor classes, the coefficients on RANK NET PURCHASES remain negative, but are insignificant in some cases.

Put Table 3 about here.

[20] This is consistent with evidence, not conditioned on earnings surprises, that individual investor trades on average underperform (Odean 1999; Barber and Odean 2000).

The significance and magnitude of the coefficients on RANK NET PURCHASES increase from actively-trading investors to high-capital investors to general investors. The results suggest that actively-trading investors are less ‘skilled’ at picking losers following extreme earnings surprises, that ranked net purchases made by high-capital investors following extreme earnings surprises are stronger predictors of negative future returns, and that this effect is strongest for general investors. The key finding, however, is that for all classes of individual investors, adding RANK NET PURCHASES to the regression has virtually no effect on the coefficient estimate or significance of SUE. That is, SUE is not subsumed by the trading of any class of investors. This evidence strongly opposes the hypothesis that trading by any of these investor classes drives PEAD.

Trading by Individual Investors in Different Stock Categories and Subsequent Returns

As a further robustness check, we reconsider whether trading by individual investors can explain PEAD better in subsamples of stocks that are likely to be less efficiently priced, and whose prices are more likely to be influenced by individual investor trading. We therefore perform return prediction tests in subsamples of firms with no analyst following, with low stock prices, or with low market value of equity (i.e., small firms). Past research indicates that such firms have a poorer information environment, and, owing to higher transactions costs, are more difficult to arbitrage.[21]

Table 4 presents our findings on the prediction of 3-month-ahead returns. The main finding using these subsamples is the same as that in the full-sample tests. The first panel

[21] Similarly, Mashruwala et al. (2006) find that the accrual anomaly is found in low-price and low-volume stocks and suggest that transaction costs can impose a barrier that prevents investors from arbitraging mispricing. Similarly, Ng et al. (1998) find that PEAD is stronger for firms with higher transaction costs. Prior literature also finds that PEAD is more prevalent in small firms (Foster et al. 1984; Bernard and Thomas 1989, 1990) and since individual investors tend to be disproportionate holders of shares of small firms, several authors suggest that smaller firms are more likely to have a less sophisticated shareholder base (Lee et al. 1991; Potter 1992; Walther 1997).

considers firms with no analyst following. In a regression of returns on SUE, market-to-book, market value of equity, and momentum, we find that adding the variable RANK NET PURCHASES has essentially no effect on the coefficient on SUE. (The coefficient is 0.007 in both regressions and the *t*-statistic is 3.58 without RANK NET PURCHASES and is virtually identical at 3.65 when RANK NET PURCHASES is included in the model.) In other words, considering only those firms not followed by analysts, the trades of individual investors do not help to explain drift. Although not relevant for our main conclusion, we note that in contrast to the full sample findings, the coefficient on RANK NET PURCHASES is no longer significant. This may be due to lower power since there are fewer observations in this subsample, and firms with no analyst following are likely to have greater return volatility.

Put Table 4 about here.

The second panel considers firms with low stock prices. Adding the variable RANK NET PURCHASES has little effect on the coefficient on SUE, which increases slightly from 0.006 to 0.007. Once again, the trades of individual investors do not subsume the predictive power of SUE at all. In this subsample, as in the full sample, the coefficient on RANK NET PURCHASES is negative and significant, which indicates that individual investors lose money on their trades of firms with low stock prices.

The third panel considers small firms. Here, adding the variable RANK NET PURCHASES has essentially no effect on the coefficient on SUE, so in this subsample as well, the trades of individual investors do not help to explain drift. As in the full sample, the coefficient on RANK NET PURCHASES is negative and significant, indicating that individual investors lose money on their trades of small firms.

We also perform similar tests (untabulated) for the prediction of returns 6 and 9months ahead. The findings are similar. In any of our subsamples or returns prediction horizons, individual trading does not subsume the ability of SUE to predict returns.

V. INDIVIDUAL INVESTOR TRADING FOLLOWING EXTREME EARNINGS SURPRISES

Trading by Individual Investors Following Extreme Earnings Surprises

The individual trading hypothesis implies that individuals will buy after extremely bad earnings news (pushing the stock price up) and sell after extremely good earnings news (pushing the stock price down). As described previously, in the first set of tests that follow, we examine the trades made by individuals following extreme earnings surprises (SUE 1 firm-quarters and SUE 10 firm-quarters) and compare these to the trades made by individual investors following earnings announcements with little or no surprise (SUE 5 and 6).

Figure 1 reveals that in the 25 trading days following an extreme earnings surprise, cumulative abnormal net purchases made by individuals are greater on average for SUE 1 (bad news) firm-quarters than for SUE 10 (good news) firm-quarters.[22] However, two aspects of this evidence sharply contradict the proposition that individual investors cause PEAD. First, individuals are net purchasers after both good news and bad news. This confirms the findings in Lee (1992) for extreme earnings news, and suggests that the earnings attention effect documented by Lee (1992) is due, at least in part, to individual investors. The net buying by individuals in SUE 10 firm-quarters during the 16 days following an extreme earnings surprise is

[22] Cumulative net purchases is the sum of shares purchased minus the sum of shares sold beginning on the day following the earnings announcement and ending on day *t*, scaled by the number of shares outstanding at the end of the quarter for which earnings is announced. Cumulative abnormal net purchases is the difference between cumulative net purchases for SUE 1 (SUE 10) firm-quarters and cumulative net purchases for SUE 5 and 6 firm-quarters.

inconsistent with the hypothesis that individual investors trade against favorable earnings news, causing underreaction and subsequent drift. Second, the difference in cumulative net purchases between good news and bad news firm-quarters does not develop until 17 days (i.e., more than three weeks) after the earnings announcement, so differences in individual trading do not seem to explain any under- or overreaction in the days following the earnings announcement.

$\boxed{\text{Put Figure 1 about here.}}$

Table 5 reports abnormal trading behavior following the initial earnings announcement and provides statistics that confirm the pattern in Figure 1. Panel A (B) reports slope coefficients and *t*-statistics from separate regressions of buys, sells, and net purchases per million shares outstanding on an indicator variable set equal to 1 for extreme good news (SUE 10) (extreme bad news (SUE 1)) firms and to 0 for firms in SUE 5 and SUE 6 (i.e., for those with little or no earnings surprise).

$\boxed{\text{Put Table 5 about here.}}$

In the first 25 days following an extreme earnings surprise, there is statistically significant buying and selling in both good and bad news firm-quarters (relative to no news firm-quarters). However, the difference (untabulated) between the net purchases following good versus bad news is not significant in the first three weeks of trading following an earnings announcement. Overall, this evidence suggests that individuals are influenced by an earnings attention effect, but there is no indication that individuals systematically engage in the earnings-contrarian form of trading that would induce underreaction and cause PEAD.

Trading by Individual Investor Class

Next, we examine the trading behavior of those individual investor classes that are most likely to be either less or more sophisticated. As discussed previously, we posit that investors with more capital invested and investors who trade more actively may be more sophisticated in their processing of information. We therefore study trading by investor class to test whether general investors drive PEAD, and whether high-capital investors and actively-trading investors arbitrage PEAD.

Panel A (Panel B) of Table 6 tests whether net purchases are significantly different following extreme good news (bad news) earnings announcements measured relative to the non-news case for each of the three classes of investors (high-capital investors, actively-trading investors, and general investors).

Put Table 6 about here.

Comparing Panels A and B of Table 6, we find that general investors are net purchasers after bad earnings news, and rather weakly after good earnings news, which is fairly consistent with an earnings attention effect. Contrary to the individual trading hypothesis, there is no sign that the general investors sell after good news. Furthermore, we find no evidence that more actively-trading or high-capital individual investors exploit PEAD. (To do so, they would have to sell after good news and buy after bad news.) In fact, for high-capital investors, net purchases after good news are insignificant, and after bad news, are significantly *positive* during days 1 through 15. While trading by actively-trading investors is consistent with an earning attention effect (i.e., net buying in some windows after both good and bad news), since these investors are not net sellers after bad news, they do not seem to be taking advantage of PEAD.

Trading by Individual Investors in Different Stock Categories

As a further robustness check, we again examine individual trading in response to earnings announcements in the subsamples of stocks that are more likely to be inefficiently priced and to be influenced by individual investor trading. We therefore perform the trading tests in subsamples of firms with no analyst following, low stock prices, or low market value of equity (i.e., small firms).

In Table 7, Panel A considers trading in days +1 through +5 after good news announcements, and Panel B considers trading in response to bad news announcements. As before, we report trading relative to the amount of trading in SUE deciles 5 and 6 (no news), so this is a measure of the abnormal trading associated with extreme good or bad news. The panels provide results for firms with no analyst following, for low price firms, and for small firms.

Put Table 7 about here.

For each of these categories, net purchases are significant after bad news. This is potentially consistent with trading by individual investors hindering downward price adjustment. However, after good news there is no sign of net sales; the point estimates for all three categories of firms indicate positive net purchases, though the coefficients are insignificant. Thus, there is no sign of investor selling after good news and so no evidence suggesting that individual trading hinders upward price adjustment after good news.

However, two aspects of this evidence oppose the proposition that individual investors cause PEAD. First, individuals are net purchasers after both good news and bad news. This confirms the findings in Lee (1992) for extreme earnings news, and suggests that the earnings attention effect documented by Lee (1992) is due, at least in part, to individual investors. Second, the difference in cumulative net purchases between good news and bad news firm-quarters does not develop until 17 days (i.e., more than three weeks) after the earnings announcement, so differences in individual trading cannot explain any under- or overreaction in the days following the earnings announcement.

In summary, the evidence on individual investor trading does not support the hypothesis that individuals or any class thereof are systematically trading in a manner that would cause PEAD. Nor does it support the hypothesis that any of the classes of individuals that we examine is systematically trading in order to exploit PEAD.

VI. TRADING IN SHORT WINDOWS AROUND THE SUBSEQUENT EARNINGS ANNOUNCEMENT

The individual trading hypothesis predicts that individual investors are net sellers after initial positive earnings surprises, and more strongly, are net purchasers after initial negative earnings surprises. As discussed in Section II, if drift represents genuine mispricing, then sophisticated investors, who understand that the drift is particularly intense near the dates of subsequent earnings announcements, should also time their trades with respect to the *subsequent* announcements. Specifically, after a positive earnings surprise, they should avoid selling immediately before the next quarterly earnings announcement, and instead delay selling until after the announcement. Furthermore, sophisticated investors should accelerate any planned purchases so that they are made immediately before the next quarterly earnings announcement. If sophisticated arbitrageurs follow this strategy, then for markets to clear, unsophisticated investors who are driving the mispricing must display an opposite trading pattern. Thus, as a final test of whether individuals are driving PEAD, we also examine investor trades in the days surrounding the subsequent earnings announcement, conditional on an initial extreme earnings surprise. Because PEAD is strongest at the first subsequent earning announcement following a large surprise, we tabulate results relative to this earnings announcement (i.e., to Qtr +1). However, the results (untabulated) in quarters +2 through +4 are consistent with Qtr +1 results.

In Table 8, we report the Buys, Sells, and Net Purchases in four windows around the first earnings announcement following an extreme earnings surprise. Panel A (B) reports slope coefficients and *p*-values from separate regressions of Buys, Sells, and Net Purchases per million shares outstanding on an indicator variable set equal to 1 for SUE 10 (SUE 1) firm-quarters and to 0 for SUE 5 & 6 firm-quarters (i.e., for those firm-quarters with little or no earnings surprise).

Put Table 8 about here.

Panel A shows that after an extreme positive earnings surprise, the number of shares both purchased and sold are unusually high and strongly significant in the 10 days preceding and in the 25 days following the Qtr +1 earnings announcement, and Panel B shows that a similar pattern obtains after an extreme negative surprise. Thus, extreme earnings surprises trigger trading activity not only near the announcement, but in the days surrounding the subsequent quarterly earnings announcements. This is consistent with rational information-based trading or with an attention effect over a long horizon.

Turning to net purchases, conditional on good earnings news (see Panel A), net buying is significantly positive on days -1 to -10 relative to the earnings announcement in Qtr +1. This is not consistent with the hypothesis that individuals are naïvely driving the concentration of upside drift at later earnings announcement dates. Rather, the evidence is consistent with individual investors being sophisticated enough to accelerate buying to immediately before, rather than immediately after, the earnings announcement.

In summary, this evidence does not suggest that individuals are systematically trading in a way that would cause a concentration of drift around later quarterly earnings announcement dates. Nor does the evidence support the opposite hypothesis – that individual investors profit by systematically trading in a sophisticated fashion (i.e., by exploiting the drift at later earnings announcement dates).

Panel B of Table 8 is especially relevant for the individual trading hypothesis since drift is stronger after bad news than after good news. This panel reveals significant net buying in the 5 days subsequent to the Qtr +1 earnings announcement following bad news, and some indication of further buying in days 16 through 25. This suggests sophisticated behavior because by delaying net purchases to a few days after the announcement, individuals are able to avoid the concentration of downward drift on the Qtr +1 earnings announcement date.

Taking the evidence as a whole (conditioned on either good or bad news), there is no indication that individuals are systematically engaging in a form of trading that would be expected if individual investor errors were the source of the concentration of drift at later quarterly earnings announcement dates. If anything, there is a rather modest indication that individuals are acting as sophisticated arbitrageurs to exploit PEAD.[23,24]

VII. CONCLUSION

This paper examines the whether trading by individual investors drives post-earnings announcement drift, a proposition we call the individual trading hypothesis. At a descriptive level, several regularities are of interest. First, we document an earnings attention effect –

[23] The evidence after bad news is, however, broadly suggestive of some psychological stories. After initial bad news about earnings, individuals are net buyers. This net buying could reflect an attention effect coupled with a disposition effect, or a bias in self-attribution (i.e., an insistence on interpreting new information as supportive of the self and past judgments (see, for example, Langer and Roth 1975)).

[24] We also examined the trading behavior of the three categories of traders (high-capital, frequent traders, and general) around earnings announcements following extreme earnings surprises and find results consistent with those for investors as a whole.

extreme surprises trigger greater trading and greater net buying.[25] Second, the ability of individual trades to predict future returns (Odean 1999) extends to trades taken in response to extreme earnings surprises, and this effect is distinct from PEAD. Third, the amount of abnormal trading is greater after extreme negative earnings surprises than after extreme positive earnings surprises, suggesting that bad news is highly salient.

Turning to the main question of the paper, we find no evidence that trading by individual investors following extreme earnings surprises causes post-earnings announcement drift. Such trading would need to impede the efficient adjustment of market prices to earnings surprises. In other words, if individuals were causing PEAD, they would engage in earnings-contrarian trading – buying aggressively after extreme negative earnings news and selling after extreme positive earnings news. However, individuals are strongly significant net buyers in the first three weeks following both extreme positive and negative earnings surprises.

More importantly, we find direct evidence that, in our sample, individual investors in aggregate are *not* the source of PEAD. If trading by individuals was the source of PEAD, then their net purchases following an initial earnings announcement would subsume part or all of the ability of the earnings surprise to predict subsequent abnormal returns, but this is not the case. Although net buying by individuals in the five days following an extreme earnings surprise is a significant negative predictor of future abnormal returns, including ranked net purchases in a regression of abnormal returns on SUE does not at all weaken the ability of an extreme earnings surprise to predict returns. Nor does including the earnings surprise weaken the predictive power of individual trades. Thus, two distinct market inefficiencies seem to exist. The first is PEAD. The second is that the ranked net purchases made by individual investors in reaction to extreme

[25] Gervais et al. (2001) identify what they call the high-volume return premium, in which stocks with high volumes over the short term subsequently earn abnormally high average returns. Our findings suggest that individual investor buying after extreme earnings surprises may help to explain this effect.

earnings surprises are negative predictors of future abnormal returns. Further analysis by investor class suggests that none of our sub-categories of investors are driving PEAD.

Finally, because PEAD is especially strong at the next quarterly earnings announcement following extreme negative earnings surprises, smart arbitrageurs should time their purchases to be immediately after (rather than immediately before) subsequent earnings announcements, and time their selling to be immediately before (rather than immediately after) subsequent earnings announcements. The reverse pattern is predicted after positive earnings surprises. If individual investors are naïve with respect to the concentration of PEAD at subsequent earnings announcements and are thereby impeding price adjustment, we expect them to be making opposing trades. Thus, a further prediction of the individual trading hypothesis is that given a very negative earnings surprise, individuals will be net buyers immediately preceding the next earnings announcement, and will be net sellers immediately following the next earnings announcement. However, such patterns do not exist either for individuals in aggregate, or for investor classes based upon amount of capital invested or frequency of trading.

There are some limitations to the tests we perform. Although the number of observations in the sample is large, it includes only a random sample of individuals with accounts at a single major brokerage over a six-year period. Groups of individuals who do not use this brokerage may behave differently, and there may be further sub-categories of individuals who cannot be identified using the information in the dataset, who may behave differently.

Furthermore, as mentioned in footnote 14, a premise of our basic trading tests is that if individual investors drive drift, then their net purchases are positively correlated with the pressure they exert toward mispricing.[26] This assumption is intuitive. It will follow from the use

[26] This assumption is implicit in other tests of trading behavior and drift. For example, Bhattacharya (2001) and Battalio and Mendenhall (2005) perform tests on the relation between earnings announcements and trades or trading

as a benchmark of any rational setting in which different groups of investors behave sufficiently similarly. For example, in the Capital Asset Pricing Model (CAPM), all investors hold the market portfolio and a single earnings announcement has a negligible effect on the incentives for different investors to shift their holdings between the market portfolio and the riskfree asset. Thus, all investors have zero net purchases after earnings surprises. Relative to this benchmark, any observed net purchases create a pressure toward overpricing, and net sales create a pressure toward underpricing.

More generally, there are possible rational settings in which investors are more heterogeneous, so that earnings announcements trigger trading. If trading by an investor group is sufficiently naïve, so that members' trades systematically fluctuate widely from the trading implied by some rational benchmark model, then our premise that the net purchases of the group are positively correlated with pressure toward mispricing will still obtain.[27] However, a qualification to our conclusion is that there could be rational settings in which the premise of our basic trading tests our violated (i.e., where net buying is correlated with pressure toward underpricing). This situation would call into question the conclusions drawn from these tests.

In contrast, our return prediction tests do not rely heavily on the choice of a rational trading benchmark. Thus, if actual trades are informative about the pressure toward mispricing exerted by the investor group, in a regression of post-earnings abnormal returns on investor trades and on the earnings surprise, investor trades should subsume part of the drift.[28] The fact

volume measured relative to normal trading in a non-event window, rather than measured by adjusting trades for some rational benchmark (conditional on the value of the earnings surprise).

[27] Ideally, it would be desirable to calibrate a model of securities trading to provide a benchmark for rational individual investor trading after an earnings announcement. However, equilibrium in rational expectations models depends upon unobserved parameters, so it is not easy to calibrate these models to actual data.

[28] By coincidence, the irrational component of investor trades could exactly offset the rational component of the trades, so that individual investors drive drift, yet the net purchases of individuals always net to zero. However, this possibility is non-generic. Setting aside this set-of-measure-zero possibility, the trades that drive drift should be correlated (either positively or negatively) with the resulting drift.

that individual investor trades do not subsume any of the drift opposes the hypothesis that individuals drive drift.

In summary, we find no indication that trading by individual investors drives post-earnings announcement drift. As the discussion above indicates, there are limitations to our methodology. Given the surprising nature of our findings, future research using different datasets or methods to explore this issue further is warranted.

Our finding that individual investors do not seem to drive PEAD raises an obvious question: if individual investors don't cause PEAD, who does? One possibility, as argued by some authors, is that PEAD is an artifact of poor risk-adjustment of returns, but this is not the predominant viewpoint in the literature at this time. A second possibility is that individuals drive PEAD in a way that cannot be identified using our dataset. For example, some sub-category of individuals whose membership is unrelated to capital invested or trading experience may be naïve with respect to earnings announcements.

Finally, it is possible that some subset of institutional investors generates PEAD, as a result of agency incentives or cognitive biases. For example, Frazzini (2006) provides evidence suggesting that mutual funds that have lost money on a stock are subject to the disposition effect, and that this bias on the part of fund managers can explain PEAD. Frazzini's findings and ours can be viewed as providing independent reinforcement for each other using very different methods and databases.

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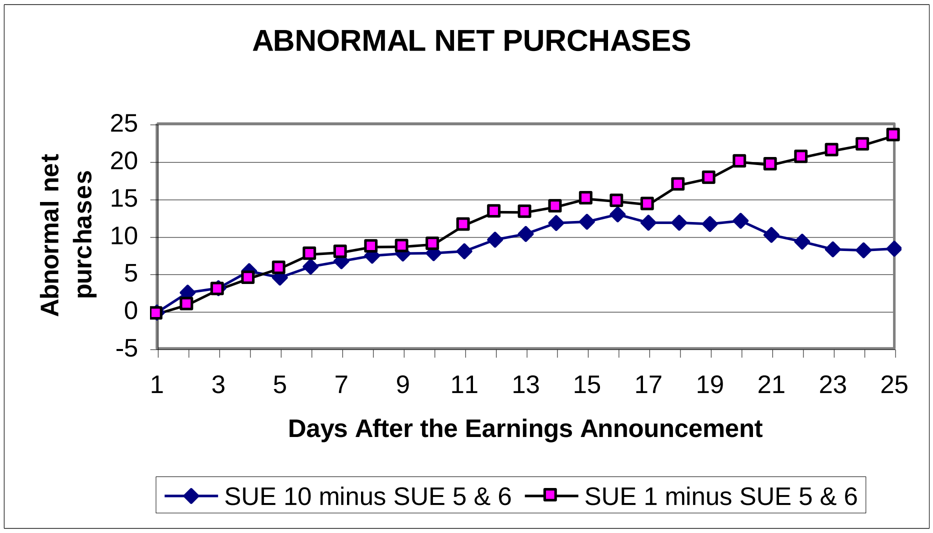
FIGURE 1
Cumulative Abnormal Net Purchases Relative to the Earnings Announcement Date



Notes:
SUE 1 observations are firm-quarters with the most negative random walk earnings surprise, SUE 10 observations are firm-quarters with the most positive random walk earnings surprise, and SUE 5 & 6 observations are firm-quarters with the smallest random walk earnings surprise (in absolute value).
Net purchases is measured as the number of shares purchased minus the number of shares sold, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 1
Descriptive Statistics

Panel A: Distribution of Trade Size in Dollars (Shares)

		$ Trade Size				
Year	Buy/Sell	Mean	Quintile 1	Median	Quintile 3	Trades
1991	Buy	$9,654 (653)	$2,213 (100)	$4,425 (200)	$9,325 (500)	197,277
1991	Sell	$12,399 (720)	$2,500 (100)	$5,200 (250)	$11,563 (700)	144,156
1992	Buy	$10,244 (645)	$2,388 (100)	$4,688 (200)	$9,625 (500)	186,819
1992	Sell	$12,957 (753)	$2,550 (100)	$5,400 (255)	$12,000 (800)	141,101
1993	Buy	$10,381 (700)	$2,500 (100)	$4,838 (200)	$10,000 (600)	181,008
1993	Sell	$12,385 (710)	$2,625 (100)	$5,450 (250)	$12,050 (700)	155,292
1994	Buy	$10,542 (695)	$2,475 (100)	$4,863 (250)	$10,250 (700)	152,609
1994	Sell	$12,508 (713)	$2,539 (100)	$5,500 (300)	$12,375 (702)	128,072
1995	Buy	$12,942 (689)	$2,788 (100)	$5,625 (225)	$12,125 (700)	178,391
1995	Sell	$14,961 (716)	$2,800 (100)	$6,125 (296)	$14,325 (800)	160,005
1996	Buy	$13,495 (700)	$2,900 (100)	$5,700 (200)	$12,750 (700)	186,003
1996	Sell	$16,554 (727)	$3,113 (100)	$6,768 (300)	$16,050 (800)	158,968

Panel B: Distribution of Dollar Volume (Number) of Trades per Investor-Year by Investor Class

Investor Type	Mean	Q1	Median	Q3	Number of Investor-years
All Investors	$125,503 (10.34)	$7,416 (1)	$21,256 (4)	$67,767 (10)	275,062
High-Capital Investors	$105,623 (8.60)	$13,984 (2)	$38,841 (5)	$101,975 (11)	52,482
Actively-Trading Investors	$577,042 (38.65)	$51,039 (8)	$158,276 (22)	$449,038 (46)	33,068
General Investors	$52,220 (6.15)	$5,563 (1)	$14,428 (3)	$37,700 (7)	189,512

Notes:
The sample consists of all trades of common shares made by investors from January 1991 to December 1996 inclusive. In panel A, the unit of measurement is a single trade by an investor. In panel B, the unit of measurement is a year of trading activity by a single investor.

TABLE 2
Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (MTB), Size (MVE), Momentum, and RANK NET PURCHASES for All Investors

N = 4,405

Returns Window	Intercept	SUE	MTB	MVE	Momentum	RANK NET PURCHASES	Adj_R^2
3 Months	0.025	0.005	-0.003	-0.006	0.009		1.17%
	(2.18)	(5.31)	(-1.69)	(-4.27)	(1.08)		
	0.040	0.006	-0.003	-0.006	0.008	-0.003	1.30%
	(3.14)	(5.41)	(-1.69)	(-4.41)	(0.87)	(-2.61)	
6 Months	0.039	0.009	-0.006	-0.009	0.027		1.38%
	(2.22)	(5.40)	(-2.68)	(-3.98)	(2.02)		
	0.064	0.009	-0.006	-0.009	0.024	-0.005	1.52%
	(3.21)	(5.50)	(-2.67)	(-4.14)	(1.08)	(-2.67)	
9 Months	0.045	0.010	-0.011	-0.008	0.051		1.61%
	(2.14)	(5.34)	(-4.06)	(-2.95)	(3.22)		
	0.068	0.010	-0.011	-0.008	0.048	-0.005	1.68%
	(2.86)	(5.42)	(-4.06)	(-3.07)	(3.04)	(-2.08)	
12 Months	0.096	0.013	-0.017	-0.011	0.080		1.68%
	(3.36)	(4.89)	(-4.41)	(-3.03)	(3.70)		
	0.131	0.013	-0.017	-0.011	0.076	-0.007	1.77%
	(4.04)	(4.98)	(-4.41)	(-3.16)	(3.51)	(-2.30)	

Notes:
Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3, 6, 9, or 12 months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period.
SUE includes those 4,405 firm-quarters in SUE deciles 1 and 10 with non-zero net buys in the 5 days following the earnings announcement.
MTB is the decile rank of the firm's market-to-book ratio.
MVE is the decile rank of the firm's market value of equity.
Momentum is market-adjusted buy and hold returns for the 6 months prior to the earnings announcement date.
RANK NET PURCHASES is calculated as the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.
Momentum is the market-adjusted buy and hold returns for the 6 months prior to the earnings announcement date.

TABLE 3
Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (MTB), Size (MVE), Momentum, and RANK NET PURCHASES by Investor Classes

High-Capital Investors
N = 1,914

Returns Window	Intercept	SUE	MTB	MVE	Momentum	RANK NET PURCHASES	Adj_R^2
3 Months	0.121	0.001	-0.005	-0.011	0.011		1.68%
	(6.31)	(0.98)	(-2.21)	(-4.77)	(0.82)		
	0.135	0.001	-0.005	-0.011	0.010	-0.003	1.74%
	(6.32)	(0.97)	(-2.19)	(-4.82)	(0.73)	(-1.46)	
6 Months	0.148	0.003	-0.009	-0.014	0.023		1.37%
	(4.96)	(1.41)	(-2.56)	(-3.83)	(1.08)		
	0.163	0.003	-0.009	-0.014	0.021	-0.003	1.38%
	(4.93)	(1.40)	(-2.55)	(-3.87)	(1.01)	(-1.05)	
9 Months	0.200	0.003	-0.015	-0.015	0.052		1.83%
	(5.59)	(1.03)	(-3.62)	(-3.61)	(2.06)		
	0.215	0.003	-0.015	-0.016	0.051	-0.003	1.81%
	(5.40)	(1.02)	(-3.61)	(-3.64)	(2.01)	(-0.84)	
12 Months	0.317	0.003	-0.013	-0.020	0.114		2.70%
	(6.50)	(0.82)	(-4.90)	(-3.44)	(3.30)		
	0.350	0.003	-0.028	-0.020	0.111	-0.007	2.75%
	(6.45)	(0.81)	(-4.88)	(-3.49)	(3.21)	(-1.37)	

Actively-Trading Investors
N = 3,261

Returns Window	Intercept	SUE	MTB	MVE	Momentum	RANK NET PURCHASES	Adj_R^2
3 Months	0.039	0.006	-0.003	-0.007	0.029		1.87%
	(2.82)	(4.68)	(-1.87)	(-4.47)	(2.88)		
	0.040	0.006	-0.003	-0.007	0.029	-0.000	1.84%
	(2.60)	(4.68)	(-1.87)	(-4.47)	(2.88)	(-0.07)	
6 Months	0.037	0.011	-0.008	-0.009	0.056		2.46%
	(1.74)	(5.89)	(-2.99)	(-3.52)	(3.67)		
	0.045	0.011	-0.008	-0.009	0.055	-0.002	2.46%
	(1.97)	(5.93)	(-3.00)	(-3.53)	(3.64)	(-0.93)	
9 Months	0.039	0.012	-0.013	-0.008	0.078		2.78%
	(1.60)	(5.97)	(-4.14)	(-2.70)	(4.42)		
	0.052	0.013	-0.013	-0.008	0.078	-0.003	2.79%
	(1.93)	(6.02)	(-4.14)	(-2.71)	(4.38)	(-1.16)	
12 Months	0.116	0.014	-0.020	-0.011	0.114		2.49%
	(3.34)	(4.72)	(-4.76)	(-2.67)	(4.55)		
	0.135	0.014	-0.021	-0.011	0.112	-0.005	2.51%
	(3.57)	(4.77)	(-4.77)	(-2.69)	(4.51)	(-1.27)	

General Investors
N = 3,293

Returns Window	Intercept	SUE	MTB	MVE	Momentum	RANK NET PURCHASES	Adj_R^2
3 Months	0.038	0.006	-0.003	-0.008	-0.002		1.34%
	(2.79)	(4.92)	(-1.48)	(-4.57)	(-0.21)		
	0.060	0.006	-0.003	-0.008	-0.004	-0.004	1.56%
	(3.84)	(5.01)	(-1.43)	(-4.81)	(-0.38)	(-2.89)	
6 Months	0.050	0.010	-0.008	-0.010	0.022		1.65%
	(2.40)	(5.23)	(-2.83)	(-3.74)	(1.48)		
	0.079	0.010	-0.008	-0.010	0.020	-0.006	1.81%
	(3.32)	(5.31)	(-2.79)	(-3.95)	(1.34)	(-2.52)	
9 Months	0.056	0.010	-0.013	-0.007	0.056		1.93%
	(2.32)	(4.89)	(-4.21)	(-2.46)	(3.23)		
	0.087	0.011	-0.013	-0.008	0.053	-0.006	2.06%
	(3.15)	(4.96)	(-4.17)	(-2.66)	(3.09)	(-2.31)	
12 Months	0.120	0.013	-0.020	-0.011	0.077		1.97%
	(3.63)	(4.49)	(-4.59)	(-2.76)	(3.23)		
	0.162	0.013	-0.019	-0.012	0.073	-0.009	2.10%
	(4.29)	(4.56)	(-4.55)	(-2.95)	(3.09)	(-2.30)	

Notes:
Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3, 6, 9, or 12 months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period.
SUE includes those 4,405 firm-quarters in SUE deciles 1 and 10 with non-zero net buys in the 5 days following the earnings announcement.
MTB is the decile rank of the firm's market-to-book ratio.
MVE is the decile rank of the firm's market value of equity.
Momentum is the market-adjusted buy and hold returns for the 6 months prior to the earnings announcement date.
RANK NET PURCHASES is calculated as the decile rank of the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 4
For High Transaction Cost / Low Information Firms
Regressions of Abnormal Returns on Ranks of Standardized Unexpected Earnings (SUE), Market-To-Book (MTB), Size (MVE), Momentum, and RANK NET PURCHASES for All Investors

3 Month Returns Window	Intercept	SUE	MTB	MVE	Momentum	RANK NET PURCHASES	Adj_R^2
Firms With No Analyst Following	0.031	0.007	-0.004	-0.010	-0.005		1.11%
N = 1,763	(1.54)	(3.58)	(-1.51)	(-2.92)	(-0.34)		
	0.046	0.007	-0.004	-0.010	-0.005	-0.003	1.16%
	(1.98)	(3.65)	(-1.48)	(-2.94)	(-0.41)	(-1.32)	
Low Stock Price Firms	0.017	0.006	-0.003	-0.005	-0.015		0.63%
N = 2,094	(0.91)	(3.66)	(-1.08)	(-1.47)	(-1.10)		
	0.043	0.007	-0.003	-0.005	-0.017	-0.006	0.88%
	(1.99)	(3.77)	(-1.10)	(-1.45)	(-1.28)	(-2.49)	
Small Firms							
N = 2,311	0.016	0.007	-0.001	-0.009	0.004		0.90%
	(0.85)	(4.15)	(-0.40)	(-2.31)	(0.35)		
	0.036	0.007	-0.001	-0.009	0.003	-0.004	1.03%
	(1.71)	(4.22)	(-0.39)	(-2.30)	(0.23)	(-2.05)	

Notes:
Abnormal returns are calculated as the buy and hold returns that begin on day +6 and end 3months later (where a month is defined as 21 trading days) minus the buy and hold value-weighted market returns for the same period. SUE includes those firm-quarters in SUE deciles 1 and 10 with non-zero net buys in the 5 days following the earnings announcement and no analysts forecasts of earnings in the month prior to the earnings announcement. MTB is the decile rank of the firm's market-to-book ratio. MVE is the decile rank of the firm's market value of equity. Momentum is market-adjusted buy and hold returns for the 6 months prior to the earnings announcement date.
RANK NET PURCHASES is calculated as the number of shares purchased minus the number of shares sold from day +1 to day +5 relative to the earnings announcement date, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 5
Abnormal Trading Following Extreme Earnings Surprises:
Regressions on SUE Indicators

Panel A: Good news firm-quarters (SUE 10) versus No news firm-quarters (SUE 5 & 6)

N=16,545 (firm-quarters)	**Buys**	**Sells**	**Net Purchases**
days +1 to +5	47.332	41.492	5.841
	(< 0.0001)	(< 0.0001)	(0.158)
days +6 to +15	56.346	44.633	11.713
	(< 0.0001)	(< 0.0001)	(0.002)
days +16 to +25	52.045	50.753	1.292
	(< 0.0001)	(< 0.0001)	(0.756)

Notes:
Cells contain coefficients and *p*-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 and set equal to 0 for firm-quarters in SUE 5 & 6.

Panel B: Bad news firm-quarters (SUE 1) versus No news firm-quarters (SUE 5 & 6)

N=11,888 (firm-quarters)	Buys	Sells	Net Purchases
days +1 to +5	46.232	26.862	19.370
	(< 0.0001)	(< 0.0001)	(< 0.0001)
days +6 to +15	73.644	65.548	8.095
	(< 0.0001)	(< 0.0001)	(0.162)
days +16 to +25	87.024	57.301	29.723
	(< 0.0001)	(< 0.0001)	(< 0.0001)

Notes:
Cells contain coefficients and *p*-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 1 and set equal to 0 for firm-quarters in SUE 5 & 6.

The columns measure the Buys, Sells and Net Purchases after an earnings announcement (quarter 0).
Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.
Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 6
Net Purchases by Individual Investor Classes
at the Initial Earnings Announcement

Panel A: Good news firm-quarters (SUE 10) versus No news firm-quarters (SUE 5 & 6)

	High-capital N = 5,334	Actively-trading N = 11,797	General N = 12,180
Net purchases days +1 to +5	3.539	1.700	4.550
	(0.167)	(0.696)	(0.168)
Net purchases days +6 to +15	-0.360	12.085	4.387
	(0.863)	(0.003)	(0.084)
Net purchases days +16 to +25	-0.743	5.711	-3.542
	(0.761)	(0.223)	(0.196)

Notes:
Cells contain coefficients and *p*-values, below in parentheses, from a regression of net purchases on an indicator variable set equal to 1 for firm-quarters in SUE 10 and set equal to 0 for firm-quarters in SUE 5 & 6, and a measure of market-wide trading.

Panel B: Bad news firm-quarters (SUE 1) versus No news firm-quarters (SUE 5 & 6)

	High-capital N = 6,993	Actively-trading N = 8,541	General N = 8,826
Net purchases days +1 to +5	14.178	2.766	17.514
	(0.000)	(0.488)	(0.000)
Net purchases days +6 to +15	15.811	4.701	-1.479
	(0.000)	(0.323)	(0.796)
Net purchases days +16 to +25	3.652	22.033	18.854
	(0.319)	(0.001)	(0.000)

Notes:
Cells contain coefficients and *p*-values, below in parentheses, from a regression of net purchases on an indicator variable equal to 1 for firm-quarters in SUE 1 and set equal to 0 for firm-quarters in SUE 5 & 6, and a measure of market-wide trading.
Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 7
For High Transaction Cost / Low Information Firms
Abnormal Trading Following Extreme Earnings Surprises:
Regressions on SUE Indicators

Panel A: Good news firm-quarters (SUE 10) versus No news firm-quarters (SUE 5 & 6)

Trading Days +1 to +5	Buys	Sells	Net Purchases
Firms With No Analyst Following	57.698	48.790	8.709
	(< 0.0001)	(< 0.0001)	(0.507)
Low Stock Price Firms	45.798	39.035	6.677
	(< 0.0001)	(< 0.0001)	(0.458)
Small Firms	49.757	37.700	12.016
	(< 0.0001)	(< 0.0001)	(0.215)

Notes: The sample sizes are as follow: for Firms with No Analysts Following, 4,823 observations; for Low Price Firms, 7,767 observations; for Small Firms, 7,190 observations
Cells contain coefficients and *p*-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 and set equal to 0 for firm-quarters in SUE 5 & 6.

Panel B: Bad news firm-quarters (SUE 1) versus No news firm-quarters (SUE 5 & 6)

Trading Days +1 to +5	Buys	Sells	Net Purchases
No Analyst Following	42.179	19.284	23.351
	(< 0.0001)	(< 0.0020)	(< 0.0250)
Low Stock Price Firms	37.402	14.731	24.401
	(< 0.0001)	(< 0.0001)	(< 0.0001)
Small Firms	38.818	12.562	28.002
	(< 0.0001)	(0.0290)	(< 0.0001)

Notes:
The sample sizes are as follow: for Firms with No Analyst Following, 2,558 observations; for Low Stock Price Firms, 4,704 observations; for Small Firms, 4,289 observations.
Cells contain coefficients and *p*-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 1 and set equal to 0 for firm-quarters in SUE 5 & 6.

The columns measure the Buys, Sells and Net Purchases after an earnings announcement (quarter 0).
Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.
Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.

TABLE 8
Abnormal Trading Around the First Earnings Announcement Following Extreme Earnings Surprises

Panel A: Good news firm-quarters (SUE 10) versus No news firm-quarters (SUE 5 & 6)

N=16,545 (firm-quarters)	**Buys**	**Sells**	**Net Purchases**
days –1 to –10	43.977	38.119	5.859
	(< 0.0001)	(< 0.0001)	(0.090)
days +1 to +5	39.578	41.286	-1.708
	(< 0.0001)	(< 0.0001)	(0.640)
days +6 to +15	56.121	50.196	5.925
	(< 0.0001)	(< 0.0001)	(0.140)
days +16 to +25	48.367	55.223	-6.855
	(< 0.0001)	(< 0.0001)	(0.116)

Notes:
Cells contain coefficients and *p*-values, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 10 and set equal to 0 for firm-quarters in SUE 5 & 6.

Panel B: Bad news firm-quarters (SUE 1) versus No news firm-quarters (SUE 5 & 6)

N=11,888 (firm-quarters)	**Buys**	**Sells**	**Net Purchases**
days –1 to –10	46.935	51.961	-5.026
	(< 0.0001)	(< 0.0001)	(0.246)
days +1 to +5	41.447	34.146	7.302
	(< 0.0001)	(< 0.0001)	(0.089)
days +6 to +15	62.826	56.370	6.455
	(< 0.0001)	(< 0.0001)	(0.305)
days +16 to +25	70.449	50.808	19.641
	(< 0.0001)	(< 0.0001)	(0.003)

Notes:
Cells contain coefficients and *t*-statistics, below in parentheses, from a regression of various measures of trading on an indicator variable set equal to 1 for firm-quarters in SUE 1 and set equal to 0 for firm-quarters in SUE 5 & 6.

Buys (Sells) are measured as the number of shares purchased (sold) in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.
Net purchases is measured as the number of shares purchased minus the number of shares sold in the window, scaled by millions of shares outstanding at the end of the fiscal quarter for which earnings is announced.