Speculating on Private Information: Evidence from Trades around Analyst Recommendations

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Abstract

We study how investors trade on short-lived private information. Our empirical identification rests on analysts' practice of "tipping" institutional clients before announcing their recommendations publicly. In line with theories of optimal trading on short-lived private information, institutions "buy the rumor and sell the fact," buying before analyst upgrades and then selling when upgrades are announced, generating short-term profits. Using a placebo test, we confirm that these institutional trading patterns are unique to instances where investors have a short-lived informational advantage. In contrast, individuals, who are unlikely to be tipped, do not buy before or sell when upgrades are announced. The results offer empirical support for existing theories and shed new light on how different types of investors trade on analyst recommendations.

JEL classification: G14, G18, G23, G24

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1. Introduction

In today's financial markets, active portfolio managers are continuously engaged in the pursuit of informational advantages. Indeed, the asset management industry is built around the business model of generating proprietary informational signals and then quickly exploiting these private signals either by trading or by disseminating them to clients. In this competitive and fast-paced environment, even a short-lived informational advantage may constitute a substantial edge. But what is the optimal way to trade on such short-lived pieces of private information? Wall Street adage suggests that professional investors often follow a "buy the rumor and sell the fact" strategy. That is, if an investor possesses a private, short-lived positive informational signal on an asset, she will first buy the asset and then sell it when the signal is revealed publically. This trading strategy appears to be popular among professional money managers, and yet the academic empirical literature provides no evidence documenting this behavior.[1] This paper is the first to fill this gap.

Our empirical analysis is guided by two theoretical papers that describe how investors possessing a short-lived informational advantage are expected to trade. Hirshleifer, Subrahmanyam, and Titman (1994) offer a rational expectations model with risk-averse investors. In their model, an investor who gets an early positive signal about a risky asset's value finds it optimal to buy the asset. Once the signal is revealed to the public, the informational advantage is lost, and the "early-informed" investor sells a portion of the asset. Brunnermeier (2005) develops a model with risk-neutral investors. In his model an early-informed investor receives a noisy signal about a short-term component of value. At that time it is optimal for this investor to trade in the direction of the signal, before it becomes known to the public. When the signal is publically revealed, the price tends to overshoot in the direction of the signal, and the early-informed investor finds it optimal, on average, to reverse some of her trade. Both models predict that the optimal way to exploit short-lived positive private information is to "buy the rumor and sell the fact," with a mirror image of this strategy applying when the private information is negative.

[1] A casual Google search shows more than five million hits for the term "buy the rumor and sell the fact," and yet there is not even one academic paper that empirically documents this trading strategy.

While these theories offer several well-defined predictions about how short-lived private information should be traded upon, there is limited empirical evidence on how informed investor exploit this, short-lived, information. There are two reasons for this. First, it is difficult to a priori identify events where one group of investors is likely to be informed while another group is not. Second, even when such a situation can be identified, obtaining detailed information about the trades of the informed and less-informed investor is often unavailable. Thus, documenting the specific strategies of informed investors and testing theories associated with their mechanics constitute a challenging task.[2]

To overcome these difficulties, in this paper we identify events for which short-lived differences in information across different classes of investors are plausible, and we analyze detailed data on trading patterns around these events. Specifically, we focus on sell-side analyst recommendation changes as times when information varies across investor classes because of analysts' practice of sharing information about their recommendations early with their institutional clients, known as "tipping" (e.g., Irvine, Lipson, and Puckett, 2007; Busse, Green, and Jegadeesh, 2012).[3] Institutional investors, who benefit from analyst tipping before recommendations are made public, are a plausible candidate to proxy for the early-informed investors in the models of Hirshleifer et al. (1994) and Brunnermeier (2005). Individual investors, who are unlikely to be tipped and thus learn about analyst recommendation changes when they are publicly announced, are our proxy for late-informed investors. To examine how such short-lived informational differences affect trading, we use a proprietary dataset that identifies the daily buying and selling volume of institutions and individuals on the New York Stock Exchange (NYSE).[4] In addition, the NYSE dataset separately identifies program trades (orders to trade multiple stocks, as in index arbitrage

[2] The literature often focuses on insider trading as a proxy for informed trading; however, insiders are legally prevented from speculating on their firm's stock, unlike informed investors in general.

[3] While Goldstein, Irvine, Kandel, and Wiener (2009) do not explicitly attribute their results to analyst tipping, they find evidence that institutional investors who execute trades through the broker issuing a recommendation execute at better prices, consistent with analysts tipping their own clients before releasing recommendations publicly. The practice of analysts tipping their clients is not illegal. Securities laws bar selective disclosure by companies to analysts or investors, but "no law prevents investors from trading on non-public information they have legally purchased from other private entities. Trading would be illegal only if the information was passed through a breach of trust," according to securities lawyers (Mullins, Rothfeld, McGinty, and Strassburg, 2013).

[4] It is important to note that individual trades are not simply the complement of institutional trades, because a third category, market makers (including specialists and non-designated market makers), also plays an active role in equity trading. In other words, it is not possible to back out individual trading from total trading and institutional trading.

strategies) from other institutional trades. This distinction is important because program trading is not typically motivated by specific news about one particular stock, and we use this feature to isolate the institutional trading that is more likely to receive private information in the form of analyst tips.

The heart of our analysis focuses on the direction and timing of trades of the two types of investors: the early-informed (institutions) and the late-informed (individuals). We examine each investor group's abnormal trade imbalance, measured as their buy minus sell trade imbalance in excess of their typical trade imbalance, to test empirical predictions derived from Hirshleifer et al. (1994) and Brunnermeier (2005).

Both theoretical models predict that early-informed investors will buy stocks prior to positive information releases and reverse their positions when the information is released publicly. Hirshleifer et al. (1994) also predict that late-informed investors will trade in the direction of the information when it is announced, but not before. Both models predict that early-informed investors' trading will be greater for information events with larger price changes on the day the information is made public. Indeed, both the early trading and the price change on the announcement date are driven by the strength of the underlying signal. The theories predict a mirror image of these trading patterns for negative information releases.

Our results concerning analyst upgrades are supportive of these predictions. Institutions (the early-informed group) buy into stocks during the four days before they are upgraded and then sell these stocks on the day of the upgrade. Thus, when it comes to upgrades, institutions appear to "buy the rumor and sell the fact," pocketing short-term profits in line with the theoretical predictions. Moreover, as predicted by the theories, during the days preceding analyst upgrades, institutions buy more of stocks that subsequently have bigger announcement-day price increases.[5] The converse trading pattern is not significant for downgrades, perhaps because short sale constraints restrict institutions' ability to execute the corresponding strategy to "sell the rumor and buy the fact" surrounding analyst downgrades.

[5] It is widely documented that, on average, analyst recommendation changes are accompanied by abnormal short-term returns in the direction of the recommendation change (e.g., Womack, 1996; Loh and Stulz, 2011; and Kecskes, Michaely, and Womack, 2013).

Our findings concerning the behavior of individual investors (the late-informed group) offer mixed support for the theoretical predictions. We find no abnormal buying by individuals prior to upgrades (nor selling prior to downgrades), consistent with the predictions of Hirshleifer et al. (1994). We also find positive, though insignificant buying by individuals on the day of upgrades (and no selling on downgrades).

Implicit in the Hirshleifer et al. (1994) and Brunnermeier (2005) models is that information events not associated with early information release to some investors do not induce the "buy the rumor, sell the fact" pattern of trading that occurs when some investors receive information in advance. Rather, we are likely to see trades in the direction of the news when it is publicly announced. To test this hypothesis we construct a placebo sample of stock-days exhibiting abnormal returns similar to those on recommendation days, but without any analyst recommendation changes. This sample captures significant information events that are unlikely to be associated with early information leakage to institutions. While in our main setting institutions are predicted to accumulate shares before the news becomes public and reverse their position at the announcement day, in the placebo sample the theory predicts no change in institutions' holdings prior to the placebo day.

In line with the theoretical predictions, the results for this placebo sample are starkly different from those for actual recommendation changes. Most notably, the "buy the rumor, sell the fact" pattern that institutions exhibit around actual analyst upgrades does not appear around the placebo positive events. Institutional investors exhibit no significant buying before the placebo positive events, and they significantly buy (rather than sell) on the day of placebo positive events. We thus find support for the theoretical predictions and conclude that the trading patterns we identify in our main tests are likely attributable to early-informed investors trading on private information before recommendation changes, unlike other events where such early information is less likely.

These results contribute to our understanding of how investors trade on private or proprietary signals. We offer an inside look at the way early-informed investors exploit their short-term information advantage and contrast their behavior with that of late-informed investors. This allows us to corroborate the theoretical papers describing how investors are expected to trade on short-lived information. It also allows us to offer the first formal piece of empirical evidence confirming the considerable anecdotal evidence of "buy the rumor sell the fact" trading strategies.

Our empirical design is predicated on prior findings of tipping before analysts initiate (e.g., Irvine et al 2007) Our reliance on theoretical predictions and the use of proprietary data enable us to bring novel evidence on institutional and individual trading patterns around analyst recommendation changes. Our study benefits from the use of a dataset that captures all NYSE trading over an 11-year period, with buy and sell volumes broken down by individuals, institutional non-program traders, institutional program traders, and market makers. Using a dataset of Plexus institutional trades, Irvine et al. (2007) find that institutions tend to buy before analysts initiate coverage with a buy recommendation, but they do not find evidence of institutional selling on the day the buy recommendation is announced. After conducting tests to reconcile our findings with theirs, we find that the difference seems to stem from the larger price changes accompanying recommendation changes (used in our main study) versus analyst coverage initiations (used in Irvine et al., 2007, and our robustness check).

Our findings also differ from those of Busse et al. (2012), who use a Plexus/Abel Noser dataset to examine the trades of institutions around analyst recommendation changes. They do not find evidence of institutions buying the rumor or selling the fact around analyst upgrades, while we do. The differences in our findings appear to be driven by the finer distinction our dataset allows between institutional program trades and institutional non-program trades, which are grouped together in Busse et al.'s dataset. By focusing on non-program trading, we isolate the institutional trades that are most likely to be attentive to analyst recommendations and tips. In fact, we find that institutional program trades are typically on the other side of non-program institutional trades (selling when non-program institutions are buying, and buying when they are selling); aggregating program and non-program trades together in our analyses would produce the non-result that Busse et al. (2012) find for analyst upgrades.

Additionally, our analyses of individual trading and of institutional trading around placebo events are entirely new and provide an informative contrast to the trading of potentially tipped institutional investors, supplying evidence for additional theoretical implications that could not be tested using smaller datasets containing only institutional trading.

Finally, our results shed new light on the behavior of both sell-side analysts and investors. It is important to understand the identity of the investors to whom analysts are talking, who is attentive to their outputs, and who trades and profits based on their recommendations. This information has far-reaching academic and policy implications. For example, it is often argued that institutions are sophisticated and are able to undo any biases of sell-side analysts such as those related to investment banking (e.g., Lin and McNichols, 1998; Michaely and Womack, 1999). Individuals, on the other hand, are often portrayed as suffering from chronic naïveté placing them on the losing sides of trades. Our results paint a more nuanced picture. It appears that institutions profit from analyst recommendations, not through their superior skill, but via a contrarian strategy associated with early tipping that yields speculative short-term profits. In contrast, individuals are not on the losing side of the trade around recommendation changes. (Moreover, in untabulated results we find that individual investors benefit from the long-term investment value associated with analyst recommendations changes.)

The remainder of the paper is organized as follows. In section 2 we review the theoretical literature and develop our hypotheses. Section 3 describes our sample and data. Section 4 presents our results. Section 5 details robustness checks, and Section 6 concludes.

2. Theoretical Motivation and Hypothesis Development

Our empirical analysis is motivated by two theoretical models of the way investors benefit from their informational advantages. The first is Hirshleifer et al. (1994). They offer a two-stage rational expectations model with risk-averse investors of two types: “early-informed” and “late-informed.” The early-informed investors learn information about the traded asset before the other investors, and they can trade on their information at date 1. At date 2, the rest of the investors learn the information and can trade on it. Noisy liquidity demand in the model ensures that prices are not completely revealing, and the market is cleared by risk-neutral market makers.

Proposition 2 in Hirshleifer et al. (1994) shows that it is optimal for the early-informed investors to trade in the direction of their information when this information becomes available to them. When the information becomes available to the public, it is then optimal for the early-informed to rebalance their positions and trade against their early information. Combining the two predictions produces "buy the rumor, sell the fact" behavior, with a mirror image of this strategy when the signal is negative.

Hirshleifer et al.'s (1994) Proposition 2 goes beyond just specifying the direction of trades, as it provides specific predictions regarding the amount of trade and how it is related to price changes on the day the information becomes public. Specifically, the amount of trading by the early-informed investors both before the announcement date and on that date is larger for more favorable information. That is, if the signal is more positive we expect to see the early-informed investors buying more before the signal becomes public and selling more when the signal becomes public. Empirically, since the signal is not observable, what we expect is that larger price changes when the information is announced are preceded by more intensive "buying the rumor" by early-informed investors.[6] As for the late-informed investors, they naturally do not trade early. Instead they trade in the direction of the signal when it becomes public.

The second theoretical model motivating our analysis is from Brunnermeier (2005), who develops a two-stage version of Kyle's (1985) model. Unlike Hirshleifer et al. (1994), all agents in Brunnermeier's model are risk-neutral. In this model the value of the firm has short-term and long-term components. Some investors are informed about the long-term component, while an early-informed investor gets a noisy signal on the short-term component. The early-informed investor trades in the direction of the signal before it becomes known to the public. A key feature of the model is that even after the signal is revealed to the public, the early-informed investor possesses an informational advantage compared to the market maker. The reason is that the early-informed investor is the only one who knows

[6] Another prediction of Hirshleifer et al.'s (1994) Proposition 2 is that there is a negative correlation between the price change on the day of the announcement and the amount of trading on that day by the early-informed investors. This prediction, however, is directly attributable to the simplifying assumption in their model (made for tractability) that "informed traders are individually infinitesimal and fall on a continuum, so that no informed trader can affect the price" (page 1669). We do not pursue this prediction because institutional investors (the early-informed investors in our setting) are not infinitesimal. On the contrary, institutional trading would put upward (downward) pressure on prices when institutions buy (sell) and is likely to induce a positive correlation between institutional trade imbalance and contemporaneous stock prices.

how to disentangle the actual signal from the noise in his own early trading. The market maker instead attributes some of the early noise to the long-term value, mistaking the trade as coming from investors who are informed about the long-term component. As a result, the price (set by the market maker) overshoots on average in the direction of the signal. Consequently, the early-informed investor finds it optimal to trade against the signal when it becomes public. Once again, this model predicts “buy the rumor, sell the fact” behavior. Furthermore, Propositions 1 and 2 in Brunnermeier (2005) show that the more favorable the signal is, the stronger is the speculative trading activity before the information announcement. Consequently, as in Hirshleifer et al. (1994), we expect a positive correlation between the pre-announcement trade of the early-informed investor and the price change when the information is announced.

To transform the theoretical predictions into workable hypotheses in our empirical setting, we identify the early-informed investors in these models as institutional investors. The premise of this assumption is that institutional investors are tipped by analysts and thus obtain an informational advantage several days before the analyst recommendation becomes public (e.g., Irvine et al., 2007).[7] We identify the late-informed investors in the models as individual investors. These investors are not known to be tipped early and thus receive the information about an analyst recommendation change only when it becomes public.

We thus have the following testable hypotheses:

- **Hypothesis 1 (institutional trading direction):** Institutional investors will exhibit abnormal trading in the direction of the analyst recommendation change in the few days preceding the recommendation change and will exhibit abnormal trading against the recommendation change when it is publicly announced.

[7] Our empirical design also works if institutional investors are not actually tipped by sell-side analysts, but rather do their own research and reach similar conclusions about stocks in the few days before analysts announce recommendation changes, inspiring them to trade as if they were tipped. Such an alternative would also lead to institutional investors being early-informed and would be consistent with previous evidence of institutional trading behavior commonly attributed to tipping.

- **Hypothesis 2 (individual trading direction):** Individual investors will not exhibit abnormal trading before the recommendation change is announced and will exhibit abnormal trading in the direction of the recommendation change when it is announced.[8]

The prediction of both models that the amount of trading by the early-informed investors prior to the day of the information release is larger for more favorable information leads to the following testable hypothesis:

- **Hypothesis 3 (magnitude of trade for institutions):** The amount of abnormal trading of institutional investors preceding the recommendation change will be positively correlated with the price change on the day of the recommendation announcement.

Analyst recommendations are unusual in that because of analyst tipping, different investors are informed about an analyst's views at different points in time. This practice is not illegal. In contrast, corporations must release material information at the same time to all analysts and investors, whether institutions or individuals, especially since the enactment of Regulation FD (in August 2000). Our next hypothesis concerns such information events, which are characterized by stock price movements but are not associated with early release of information in the form of analyst tipping. These information events are likely to be associated with significant price changes, yet we do not expect such events to induce "buy the rumor, sell the fact" trading activity by institutions, since they have no early informational advantage.

- **Hypothesis 4 (other information events):** For informational events that are not associated with short-term informational advantage, institutions will not exhibit the "buy the rumor, sell the fact" trading activity. Rather, they will simply trade in the direction of the information when it becomes available to the public.

[8] This prediction comes from Hirshleifer et al. (1994) only; in Brunnermeier (2005) long-term traders (corresponding to individuals in our setting) do not trade based on the private signal when it becomes public. The reason is that the market makers learn the signal as well. Since the market makers are risk-neutral they fully absorb any risk and all the information related to the signal is correctly reflected into the price. This risk neutrality, however, is assumed for tractability only. If the market makers in Brunnermeier's model were risk averse, then the picture would change and the long-run traders would trade in the direction of the signal when it is publically revealed, in line with the Hirshleifer et al. (1994). We thank Markus Brunnermeier for clarifying this point for us.

3. Data, Methodology, and Sample

In this section we detail our data sources, discuss how key variables are defined, and present descriptive statistics for our sample.

We employ stock recommendation changes as events around which some investors have early information and others do not. Our analysis uses analyst stock recommendation data from the Thomson Financial Institutional Brokers Estimate (I/B/E/S) U.S. Detail File,[9] data on institutional and individual daily buy and sell transaction volume from the NYSE Consolidated Equity Audit Trail Data (CAUD) database, stock data from the Center for Research in Securities Prices (CRSP) database, and institutional holdings data from the Thomson Financial 13F quarterly holdings database. We also use information on analyst rankings from Institutional Investor annual All-Star Analyst rankings. Our sample period is 1999 to mid-2010, and our sample includes all NYSE-listed domestic common stocks for which there are valid analyst recommendation changes in I/B/E/S within our sample period, as defined below.

3.1. Analyst recommendation changes

We define analyst recommendation changes based on the three-tier scale of buy, hold, and sell. We convert recommendations from the less common five-tier scale (strong buy, buy, hold, sell, strong sell) to the three-tier scale before identifying recommendation changes, so that our assessment of recommendation changes is not contaminated by the widespread change from five-tier to three-tier rating scales in 2002 prompted by the Global Analyst Research Settlement (Kadan, Madureira, Wang, and Zach, 2009). We define our recommendation changes as upgrades or downgrades within the three-tier scale for which the previous recommendation was issued by the same brokerage firm within the past year, to minimize the possibility of stale forecasts. We use the date and time stamps in I/B/E/S to identify the exact day of the recommendation change (the event day). To ensure that the recommendation date we consider is the relevant one in terms of the trading activity surrounding it, if a recommendation is released after 4:00 pm, we designate the next trading day as the recommendation change day.

[9] The data we use were pulled in early 2012 and so reflect the corrections Thomson made in 2007 in response to the findings of Ljungqvist, Malloy, and Marston (2009) that previous versions of the I/B/E/S database had been altered.

To separate the effect of analyst recommendation changes from firm-specific news (Altinkilic and Hansen, 2009), we apply two screens similar to Loh and Stulz (2011). First, we remove recommendation changes that occur on the same day as or the day following earnings announcements. Second, we remove recommendation changes on days when multiple analysts issue recommendations for the same firm, as clustering in recommendation changes may reflect the release of firm-specific news (Bradley, Jordan, and Ritter, 2008). Together these filters remove about 28% of the analyst recommendation changes in our sample period.

3.2. Investor-type trading volume and trade imbalance

We use proprietary data from the NYSE that allow us to precisely identify the trading activity of institutional investors (the early informed) and individuals (the late informed). The dataset is constructed from the NYSE's CAUD files, which are the result of matching trade reports to the underlying order data. CAUD contains information on all orders that execute on the NYSE, including both trades that are executed electronically and those that are executed manually (by floor brokers).

CAUD has two main advantages compared to other databases providing information on institutional trading. First is its coverage. CAUD covers a large portion of trading in NYSE stocks and is therefore likely to provide a representative picture of trading.[10] Second is the separate identification of individual and institutional trading.[11] CAUD is one of the few databases that identify both individual and institutional trading; because of the presence of market makers, individual volume is not simply the complement of institutional volume. Moreover, CAUD flags out institutional program trading which enable us to more precisely identify the early-informed traders. Since in this paper we are testing

[10] In the first half of our sample period, over 80% of trading in NYSE-listed stocks occurs on the NYSE and is therefore captured by CAUD. After the 2007 merger of NYSE with ARCA, our dataset includes trades on ARCA as well as on NYSE. We perform robustness checks for the early versus latter part of the sample period, when more trading occurs off the NYSE.

[11] Other papers use Trade and Quote (TAQ) data to identify large and small trades and then attribute large trades to institutions and small trades to individuals (e.g., Malmendier and Shantikumar, 2007). This categorization is less appropriate since the introduction of decimalization (trading in pennies rather than in sixteenths of a dollar) in 2000 and the growing use of computerized trading algorithms to break up institutional trades, both of which undermine the assumption that small trades are necessarily coming from individuals in recent years.

hypotheses related to institutional and individual trading behavior, these features of CAUD are crucial for us.

For each trade, CAUD shows the executed portion of the underlying buy and sell orders along with an account-type variable that identifies whether the trader who submitted an order is an institutional investor (and whether part of a program trade or not), an individual investor, or a market maker. Providing the account type classification is mandatory for brokers, although it is not audited by the NYSE on an order-by-order basis.[12] Because CAUD reports the buyer and seller for each trade based on actual order data, the classification of buy and sell volume in our data set is exact, and thus we do not have to rely on trade classification algorithms such as Lee and Ready (1991).

We note that given the aggregation inherent in our dataset (summing across all traders within each type), institutional investors are a noisy proxy for early-informed investors. It is likely that analysts tip institutional investors selectively, revealing information to their most valued institutional clients first and not revealing it to others (Goldstein, Irvine, Kandel, and Wiener, 2009; Nefedova, 2014). Furthermore, analysts may tip on just a subset of all recommendation changes. Thus it is reasonable to assume that our sample of early-informed institutions is contaminated by uninformed institutions. The presence of un-tipped institutions will add noise to our tests and would bias them against finding any differences between the group with early information (institutions) and the group without it (individuals). To the extent that we do find significant differences, they likely serve as a lower bound for the actual differences we would have observed had our proxy for the informed group been more precise. The NYSE dataset allows us to distinguish between the trades of institutions and individuals, but with one exception (described below) it does not enable us to distinguish further between types of institutions, nor does it allow us to identify a particular institution. Thus, we cannot make our distinction between the early-informed and late-informed groups any finer.

[12] Kaniel, Saar, and Titman (2008) point out that any abnormal use of the individual investor designation by brokers in hopes of gaining advantages is likely to draw attention, preventing abuse of the system.

Within the institutional category, we are able to separate institutional trading into program trades and non-program trades. The NYSE defines program trades as the trading of a basket of at least 15 NYSE securities valued at $1 million or more. We exclude program trades from our measures of institutional trading in order to focus on the institutional investor trading that is more likely to be attentive to analyst recommendations and tips.[13]

We construct daily measures of institutional and individual trading volume and trade imbalance for each stock, and we standardize the measures by the trading volume on the NYSE in the same stock the same day. Specifically, we define Raw Trading Volume for stock *i*, investor type *x*, on day *t* as:

$$Raw\ Trading\ Volume_{i,x,t} = \frac{\left(SharesBought_{i,x,t} + SharesSold_{i,x,t}\right)/2}{\left(SharesBought_{i,t} + SharesSold_{i,t}\right)/2} \quad (1)$$

where $SharesBought_{i,x,t}$ and $SharesSold_{i,x,t}$ are the number of shares of stock *i* bought and sold, respectively, by investor type *x* on day *t*, and $SharesBought_{i,t}$ and $SharesSold_{i,t}$ are cumulated across all buys and sells of stock *i* on day *t* on the NYSE. Similarly, we define Raw Trade Imbalance for stock *i*, investor type *x*, on day *t* as:[14]

$$Raw\ Trade\ Imbalance_{i,x,t} = \frac{SharesBought_{i,x,t} - SharesSold_{i,x,t}}{\left(SharesBought_{i,t} + SharesSold_{i,t}\right)/2} \quad (2)$$

To isolate the abnormal trading volume and abnormal trade imbalance surrounding analyst recommendation changes, we identify a benchmark period for each recommendation change. Our benchmark period is days -45 to -11 and +11 to +45 relative to the day of the analyst recommendation change. We calculate the Benchmark Trading Volume for stock *i*, investor type *x*, with analyst recommendation change on day *t* as the average Raw Trading Volume over days t-45 to t-11 and t+11 to t+45. Similarly, we calculate the Benchmark Trade Imbalance for stock *i*, investor type *x*, with analyst recommendation change on day *t* as the average Raw Trade Imbalance over days t-45 to t-11 and t+11 to t+45.

[13] Program trades are often part of index arbitrage strategies or rule-based algorithms that trade a basket of stocks for reasons that are unrelated to analyst recommendations (Boehmer and Kelley, 2009).
[14] Trade imbalance is sometimes referred to in the literature as "order imbalance." We use the term trade imbalance because we observe only executed trades, not all orders submitted, in our dataset.

Our main variables of interest are the abnormal trading volume and abnormal trade imbalance for each investor type and recommendation change, defined as:

$$Abnormal\ Trading\ Volume_{i,x,t} = Raw\ Trading\ Volume_{i,x,t} - Benchmark\ Trading\ Volume_{i,x,t} \qquad (3)$$

and

$$Abnormal\ Trade\ Imbalance_{i,x,t} = Raw\ Trade\ Imbalance_{i,x,t} - Benchmark\ Trade\ Imbalance_{i,x,t} \qquad (4)$$

To calculate the benchmark period volume and imbalance, and thus the abnormal volume and imbalance for each recommendation change, we require at least 45 days of data before and after the recommendation change, reducing our sample from the eleven and a half years (January 1, 1999 to July 1, 2010) for which we have CAUD data to recommendation changes occurring between March 10, 1999 and April 22, 2010.

3.3. Abnormal stock returns

We collect daily stock returns and value-weighted market returns from CRSP and define the abnormal stock return for stock *i* on day *t* as:

$$Abnormal\ Return_{i,t} = Return_{i,t} - ValueWeightedMarketReturn_t\ , \qquad (5)$$

where $Return_{i,t}$ is the return for stock *i* on day *t* and $ValueWeightedMarketReturn_t$ is the CRSP value-weighted market return on day *t*.

3.4. Descriptive statistics

Panel A of Table 1 presents basic descriptive statistics for the stocks in our sample. Because our sample is restricted to firms with at least one analyst recommendation change, the stocks in our sample are rather large, with an average market capitalization $6.490 billion. The average number of analysts covering a firm in our sample is seven (with a median of six), and the average institutional holdings percentage is 66.6%. On average, institutional trading (excluding program trading) accounts for 63.4% of the volume in our sample stocks, while individual trading accounts for 4.3%.[15] Clearly, institutional trading volume dwarfs that of individuals in these stocks on the NYSE. As for trade imbalance, a priori it is not clear we should expect either group of investors to be net buyers or sellers, and both groups' imbalances average near zero in our sample.

Panel B summarizes the distribution of analyst recommendation changes by year. Overall, there are about five percent more downgrades than upgrades in our sample (15,907 downgrades versus 15,101 upgrades). We also note considerable variation in the number of recommendation changes over time, so we include year fixed effects in all subsequent analyses.

[Table 1 here]

4. Results

We begin by documenting the trading volume of institutions and individuals around analyst recommendation changes, in Section 4.1. In Section 4.2 we study the trade imbalances of these two groups, allowing us to directly examine Hypotheses 1 and 2. In Section 4.3 we evaluate how the magnitude of imbalances is related to the price changes associated with recommendation changes (Hypothesis 3). In Section 4.4 we examine dates on which large stock price changes occur with no analyst recommendation changes, to conduct tests of institutional trading behavior without early information (Hypothesis 4). In Section 4.5 we complete the picture by examining what types of traders act as counterparties to the early-informed institutions.

4.1. Preliminary results: Institutional versus individual trading volume

Figures 1 and 2 provide a first look at institutional and individual trading volume surrounding analyst recommendation changes. Figure 1-A (2-A) shows the average Raw Trading Volume for institutions and individuals over the period from 45 days before to 45 days after an upgrade (downgrade); because the orders of magnitude for institutional and individual trading volumes are very different, we use

[15] Institutional program trades account for another 21% of trading volume on average, and the remaining 12% is executed by market makers, including specialists.

separate scales for the two groups of investors (left vs. right axis). Figure 1-B (2-B) shows the average Abnormal Trading Volume in the days immediately surrounding an analyst upgrade (downgrade).

[Figure 1 here]

[Figure 2 here]

Both institutional and individual trading volumes appear to spike around analyst recommendation changes. Critically, in selecting these recommendation changes we have removed all earnings announcement dates and dates of clustered stock recommendations from multiple analysts. Thus the spike in volume around recommendation changes is likely associated with the recommendation change itself, not other news such as earnings announcements.

To determine the statistical significance of the volume patterns displayed in Figures 1 and 2, we conduct analyses of the following form:

$$Volume_{i,x,t+k} = \alpha + \sum_{m=1}^{11} \delta_n YearDummy_{m,t} + \varepsilon_{i,x,t+k} \,, \qquad (6)$$

where $Volume_{i,x,t+k}$ is the abnormal trading volume for investor-type x (institutions or individuals) in stock i with a recommendation change on day t. The variable k takes values in {-4, 0, 4}. When k = 0 we are focusing on the abnormal volume on the day of the recommendation change (day t); when k = -4 we are focusing on the daily abnormal volume over the four days prior to the recommendation change (days t-4 to t-1); and when k = 4 we are focusing on the daily abnormal volume over the four days following the recommendation change (days t+1 to t+4). The variable of interest in this analysis is the intercept, α, which measures the daily abnormal volume related to the specific time period we are interested in (day of the recommendation change or four days preceding or following it). A positive intercept corresponds to a positive amount of abnormal volume. $YearDummy_{m,t}$ are year fixed effects to control for the variation in the number of recommendation changes over time observed in Table 1 and market changes over time. To adjust for potential cross-sectional correlation and idiosyncratic time-series persistence, we use standard errors double-clustered on stock and date in this and all subsequent analyses (Thompson, 2011).

Table 2 presents the regression results separately for upgrades (Panel A) and downgrades (Panel B). The results show clearly that volume is significantly higher on the recommendation change day (day 0) for both institutions and individuals and for upgrades and downgrades. We note that the day-0 abnormal volumes for the two groups of investors differ by an order of magnitude. For example, for upgrades the day-0 institutional abnormal volume is 0.0246 (2.46%, column (2)), more than 20 times individual abnormal volume of 0.0012 (0.12%, column (5)). On days -4 to -1 and +1 to +4, institutions exhibit abnormal volume on average, but individuals do not (columns (1) and (3) versus (4) and (6)) and the difference between the two groups is significant (last three columns).

[Table 2 here]

4.2. Results on institutional and individual trade imbalances

Figures 3 and 4 present the average buy-sell trade imbalances surrounding analyst recommendation changes. First consider Figure 3, which shows analyst upgrades. Panels A and B show that both institutional and individual trade imbalances are quite flat until four to five days before the upgrades. In the time period -4 to -1 we see a significant increase in positive imbalance by institutions, which reverses to a negative imbalance on the day the upgrade is announced. That is, institutions are net buyers of stocks in the four days prior to upgrades, and are net sellers on the day of the upgrade (day 0). This pattern is consistent with the predictions of Hypothesis 1, that institutional investors trade in the direction of the recommendation change prior to and trade in the opposite direction on the day of its announcement, giving rise to “buy the rumor, sell the fact” behavior.

The trade imbalances of individuals in Figure 3 tell a very different story. The abnormal imbalances of individual investors appear roughly flat before upgrades and then positive on the day the upgrade is announced. This pattern is consistent with Hypothesis 2, that individuals trade in the direction of the recommendation change when it is announced, but not before.

Figure 4 suggests similar but less pronounced behavior of institutions around downgrades. Individual imbalances are also more muted around downgrades than upgrades.

[Figure 3 here]

[Figure 4 here]

To determine the statistical significance of the trade imbalance patterns displayed in Figures 3 and 4, we conduct analyses of the following form:

$$Imbalance_{i,x,t+k} = \alpha + \sum_{m=1}^{11} \delta_n YearDummy_{m,t} + \varepsilon_{i,x,t+k} \ , \qquad (7)$$

where $Imbalance_{i,x,t+k}$ is the abnormal trade imbalance for investor-type x (institutions or individuals) in stock i with a recommendation change on day t. The variable k takes values in {-4, 0, 4}, as in Equation (6). The variable of interest in this analysis is the intercept, α, which measures the abnormal trade imbalance related to the specific time period we are interested in (day of the recommendation change or four days preceding or following it). A positive (negative) intercept corresponds to excess buying (selling) activity relative to the benchmark period.

[Table 3 here]

Table 3 presents the regression results separately for upgrades (Panel A) and downgrades (Panel B). Consider Panel A first. The results for institutional investors show abnormal buying activity during the four days prior to an upgrade (column (1)), and then abnormal selling activity on the day of the upgrade (column (2)). These results are consistent with the prediction in Hypothesis 1 that institutions "buy the rumor and sell the fact." In the four days following the upgrade we see no abnormal activity by institutions (column (3)).

The results in Panel B of Table 3 reveal that while institutions seems to be selling before downgrades and buying on the day of the recommendation change, consistent with "sell the rumor buy the fact", the selling before downgrades and buying on downgrade dates are not significant. The insignificant results for downgrades are likely due to practical considerations not considered by the models that motivate our analyses. It is well known that trading on negative information is often harder compared to trading on positive information due to short-sale constraints. Institutional investors can relatively easily respond to imminent upgrades by buying the recommended stocks, but downgrades can only be traded upon if the institution already holds a particular stock or through a short sale. The two models we rely on assume that short sales are allowed and are not associated with any costs or constraints. In practice, short sales are often costly, and many institutions' own policies prohibit short selling.[16]

Turning to individuals, Panels A and B of Table 3 indicate that individuals do not trade in the direction of information prior to recommend changes, consistent with Hypothesis 2. The other prediction of Hypothesis 2, that individuals should exhibit net buying on the day of upgrades and selling on the day of downgrades, is not supported, as the small positive imbalance in the upgrade graphs is not statistically significant.

Across different recommendation changes, we would expect that early-informed investors would trade more strongly on analyst recommendation changes that have higher information content. Furthermore, we would expect that institutions would be more likely to trade on early information regarding analyst downgrades when the stock involved has higher institutional ownership, which would increase the probability that the early-informed institution holds the stock and would make it easier to borrow the stock for a short sale. To investigate these cross-sectional implications, we run regressions of the following form:

$$Imbalance_{i,t+k} = \alpha + \beta_1 SmallFirm_{i,t} + \beta_2 AllStar_{i,t} + \beta_3 HighInst_{i,t} + \sum_{m=1}^{11} \delta_n YearDummy_{m,t} + \varepsilon_{i,t+k}\ , \qquad (8)$$

where $SmallFirm_{i,t}$ is an indicator variable that is equal to one if the firm is below the median firm size, else zero; $AllStar_{i,t}$ is an indicator variable that is equal to one if the analyst making the recommendation change is ranked as an all-star analyst by Institutional Investor in the prior year, else zero; and $HighInst_{i,t}$ is an indicator variable that is equal to one if the firm is above the median by institutional ownership percentage as of the previous quarter-end, else zero. As in prior equations, the variable k takes values in {-4, 0, 4}.

[16] The literature provides numerous examples of situations in which investors are prevented from fully exploiting negative information due to constraints on short selling. See, for example, Jones and Lamont (2002) and Nagel (2005). Engelberg, Reed, and Ringgenberg (2012) provide direct evidence of the costs associated with trading on negative information.

The regression results, presented in Table 4, show that the "buy the rumor, sell the fact" trading of institutions is not driven by the smallest firms, all-star analysts, or firms with the highest institutional ownership.[17] We find only two significant results for upgrades: The day-0 coefficient on *SmallFirm* for upgrades is significantly negative (Panel A, column (2)), indicating that institutions exhibit more selling for smaller firms on the day of analyst upgrades, and upgrades by all-star analysts are associated with significantly higher buying in the following days (Panel A, column (3)). We find no other evidence that institutional trading is larger in magnitude for small firms or all-star analyst recommendation changes, and we find no evidence of strategic trading by early-informed investors around analyst downgrades, even for stocks with above-median institutional ownership.

[Table 4 here]

4.3. Results on magnitude of institutional trading

We now turn to examine Hypothesis 3, which involves the link between institutional trade imbalances and stock returns. We consider the following specifications:

$$Imbalance_{i,t-4} = \alpha + \beta Return_{i,t} + \sum_{m=1}^{11} \delta_n YearDummy_{m,t} + \varepsilon_{i,t+k}\ , \qquad (9)$$

where $Imbalance_{i,t\text{-}4}$ cumulates over the four days prior to the analyst recommendation change and $Return_{i,t}$ is the abnormal return for stock *i* on the day of the analyst recommendation change.

We have already seen that institutions tend to buy prior to upgrades (Table 3). Our focus here will be on the coefficient β, which measures whether the buying activity of institutions is stronger for upgrades that are followed by high abnormal returns on the day of the upgrade. This approach allows us to test Hypothesis 3, which predicts that the trade imbalance of institutions prior to the recommendation change is positively correlated with the price change on the recommendation change day.

[Table 5 here]

[17] The results of similar regressions including additional cross-sectional explanatory variables are reported in Section 5, robustness checks.

Panel A of Table 5 presents the analysis for upgrades. As in Table 3, the intercept in this regression is positive (0.0059) and significant (*t*-statistic of 2.3), indicating that institutions net buy stocks in the four days before they are upgraded. In addition, the coefficient on *Return* is positive (0.0300) and significant (*t*-statistic of 2.4). Thus, institutions appear to be buying even more of those about-to-be-upgraded stocks whose prices rise more on the day of the upgrade. This is consistent with the idea underlying Hypothesis 3 that both the institutional buying and the following change in price are driven by the same signal. As in our main analysis, this effect is limited to upgrades; Panel B shows no equivalent result for downgrades.

4.4. Information events without tipping

The final prediction we test is that information events that are not accompanied by early information release to some investors should not induce "buy the rumor, sell the fact" trading patterns (Hypothesis 4). If such events do induce the buy/sell trading pattern, it would suggest that the trading pattern is not due to early-informed trading. For example, it may be that institutional investors always buy before and sell on days with large positive returns, and analyst upgrades are simply one cause of large positive returns. A related concern is that there may be omitted variables related to the behavior of individuals and institutions, which drive the differences in their trading behavior irrespective of whether they are early-informed or late-informed. To this end, we conduct a placebo test to examine the trading behavior of both institutions and individuals surrounding large-return days on which there are no analyst recommendation changes. These placebo events capture other informational events, which should not involve early informational advantages for institutional investors.

We construct the placebo sample as follows. For each analyst recommendation change in our sample, we identify a placebo event defined as the stock-day on which the same stock has the closest abnormal return to that of the actual analyst recommendation change (day 0).[18] We exclude from consideration the nine-day periods (days *t-4* to *t+4*) surrounding all actual analyst recommendation

[18] We define "closest abnormal return" as the return that has the same sign as and minimum absolute distance from the day-0 abnormal return of the actual analyst recommendation change.

change dates for that stock, to avoid overlap with analyst recommendation changes. Placebo events are chosen without replacement, so there are no duplicates in the placebo event set. Figure 5 shows the average abnormal return for the period surrounding the actual recommendation change dates and the placebo event dates. The average absolute difference between actual and placebo day-0 abnormal returns is an insignificant 0.0013%.

[Figure 5 here]

Figure 6 graphically examines institutional and individual investor trading volume surrounding placebo events compared to analyst recommendation changes.

[Figure 6 here]

The top two graphs in Figure 6 show that institutional investor volume is on average lower surrounding the placebo events than it is around the actual analyst recommendation changes. Individual investor volume also appears lower on the day of placebo events than on the day of analyst recommendation changes, although the average difference is smaller than for institutions (note the different vertical scales for institutions, above, versus individuals, below). To determine the statistical significance of the volume patterns surrounding the placebo events, we employ regression analyses identical to those in Table 2 except that we now perform the analyses on the placebo event sample. Table 6 presents the results.

[Table 6 here]

The variable of interest in these regressions is the intercept, which measures the abnormal volume in the days preceding, day of, and days following the placebo event (day with price movements of similar magnitude but without a recommendation change). Institutional abnormal trading volume is insignificant on the days before the placebo events (see column (1)). This is in contrast to the significantly higher institutional trading volume before actual recommendation changes. On the day of the placebo event, institutional trading volume is positive and significant (see column (2) of Panel A), consistent with the idea that institutions are often active traders on days of big price moves. But the day-0 volume is significantly lower than it is for actual upgrades (reported in Table 2). Individual abnormal trading volume is insignificant before the placebo event (column (4)), as it is before recommendation changes, and also insignificant on day 0 of the placebo event, compared to positive and significant trading volume on day 0 of recommendation changes. The results for downgrades (in Panel B) also suggest that trading volume patterns for institutions and individuals around analyst recommendation changes cannot be not fully explained by the large returns on day 0 alone, as captured in the placebo events.

Figure 7 provides a first look at institutional and individual investor trade imbalances surrounding placebo events compared to analyst recommendation changes.

[Figure 7 here]

Institutional trade imbalances display dramatically different patterns surrounding the placebo events compared to actual analyst recommendation changes. Most notably, the contrarian behavior of institutions (selling on upgrades, buying on downgrades) that appears on the day of actual recommendation changes is reversed for the placebo events: On average institutional investors appear to net buy on day 0 for placebo upgrades and net sell on day 0 for placebo downgrades. Table 7 presents the results of regression analyses for abnormal trade imbalances, analogous to those in Table 4 except now using the placebo sample.

[Table 7 here]

The results in Table 7 support Hypothesis 4. On information events other than analyst recommendation changes, institutions simply trade in the direction of the information when it is released, rather than exhibiting "buy the rumor, sell the fact" behavior associated with early information acquisition. Institutional investors are net buyers rather than sellers on the day of the upgrade (column (2) of Panel A), and they are net sellers on placebo downgrade days (column (2) of Panel B), in contrast to their insignificant imbalances on actual downgrade days (Table 4). These differences between actual and placebo recommendation changes are significant. Institutions do not demonstrate significant buying prior to the placebo upgrades (column (1) of Panel A). Interestingly, individuals exhibit net abnormal selling on placebo upgrades (column (5) of Panel A), consistent with individuals providing liquidity on high-return days that are not associated with analyst upgrades (although their activity is an order of magnitude smaller than institutions' net buying).[19]

Overall, these placebo tests show that institutions trade differently around information events when they do not possess short-lived private information. The increase in institutional trading volume is lower for the placebo events than when the same price change is associated with an analyst recommendation change. Even more revealing are the imbalance patterns, which show that the "buy the rumor, sell the fact" behavior of institutions is related specifically to analyst recommendation changes, not simply large-return events as captured by the placebo sample.

Taken together, these results lend further support to the predictions of Hirshleifer et al. (1994) and Brunnermeier (2005) that investors trade differently when they have a short-term informational advantage than when they do not. Before the information is revealed, early-informed investors buy stocks in which they have a positive informational advantage. Once the information is revealed, they reverse their position in those stocks. And the late-informed investors do not change their trading patterns before the information event. These findings lead to the natural question of who trades with the institutions before the information release. We address this question next.

4.5. Who is on the other side?

For every buyer there is a seller, and in aggregate the number of shares bought equals the number of shares sold. Thus a natural question to ask is who stands on the other side of the trading activity we document around analyst recommendation changes? In particular, when institutions are buying a stock a few days before an upgrade, some other traders must be selling to them. And when institutions are taking profits on the day of an upgrade, other traders must be buying. From the results so far it does not appear that individuals are the main the counterparties of institutions in their trades around analysts' recommendation changes. Even when individual trade imbalances are of the opposite sign than institutional imbalances, they are much smaller in magnitude.

[19] Similarly, Kaniel, Saar, and Titman (2008) find patterns of individual trading consistent with risk-averse individuals providing liquidity to institutions.

The other candidates for being "counterparties" are program traders and market makers. Program traders are institutions trading baskets of securities. In our main analysis, we separate active institutional investors from program traders because the latter are not likely to be trading on analysts' recommendations on individual stocks. The market maker category includes specialists and non-designated market makers.

Panel A of Table 8 reports univariate results on the abnormal trade imbalances of program traders and market makers around upgrades. The results for program traders are striking (columns (1) through (3)). The intercept on days -4 to -1 is negative and significant, whereas the intercept on day 0 is positive and significant. Comparing these results to the institutional trades in Panel A of Table 3, we find that the intercepts have similar magnitudes but opposite signs. The abnormal imbalance of program traders forms a mirror image to that of institutional investors, before and on the day of upgrades. In contrast, the analogous analysis for market makers shows no significant intercepts (columns (4) through (6)). Thus, program traders (and not market makers) emerge as the de-facto counterparties of institutions when institutions "buy the rumor and sell the fact" around analyst upgrades, serving the role of liquidity providers in those events.

[Table 8 here]

It is natural to ask what drives this result. Namely, what would induce program traders to trade against active institutional investors around analyst recommendation changes? Unfortunately, we do not have a clear answer to this question. Program traders comprise a variety of traders including index arbitragers, quantitative hedge funds, and other traders who trade baskets of securities based on a wide variety of quantitative models. The NYSE dataset pools all of these traders together under the program trader category, so it does not allow us to determine the motives of these traders. Thus this question remains open for now.

When considering abnormal imbalances around downgrades (Panel B), we find some activity on the date of the downgrade but not before or after. Here, program traders are net sellers and market makers appear to be net buyers on downgrades.

5. Robustness checks

We conduct several additional tests to confirm the robustness of our results (results from robustness checks are available on request). First, including analyst recommendation changes that occur near earnings announcement dates and those that occur on days with multiple analyst recommendation changes in our sample does not alter our results. Second, our results are robust to excluding from our sample all recommendation changes announced after 4:00 pm (16% of our original observations). Third, dropping the year fixed effects from our regressions does not change our inference, nor does dividing our sample into sub-periods, such as before versus after 2007, when Regulation NMS and the NYSE's Hybrid market structure were implemented (Hendershott and Moulton, 2011).

In multivariate regressions akin to those in Equation (8), we also test a number of other characteristics of recommendation changes, including the firm's book-to-market ratio, the number of analysts covering the firm, and whether the recommendation change is accompanied by an earnings forecast, issued by one of the 10 largest brokerages, issued by a brokerage firm that has an underwriting relationship with the firm, or occurs after the Global Settlement of 2002. None of these variables are found to be significantly related to the strength of the buy the rumor, sell the fact trading activity of institutions.

In our study we focus on analyst recommendation changes and so exclude initiations of analyst coverage. Thus our results are not directly comparable to those of Irvine et al. (2007), who focus on analyst initiations only. To reconcile our results with those of Irvine et al., we repeat our analysis for analyst initial recommendations. Using the methodology of Irvine et al., we identify 6,889 analyst coverage initiations for NYSE stocks during our sample period. Consistent with the findings of Irvine et al., we find that institutions are significant net buyers in the days prior to an analyst's initial positive recommendation. Also consistent with Irvine et al., but in contrast to our findings for analyst recommendation changes, we find no evidence of institutions net selling on the day the initial recommendation is announced. Thus, while institutions appear to be contrarians when it comes to positive recommendation changes, this behavior is not observed for positive initiations. The differential response of institutions to the announcement of initial positive recommendations versus upgrades may be attributable to the magnitude of the price effects. Hirshleifer et al. (1994) predict that the magnitude of early-informed buying before and selling the day of the announcement are positively related to the strength of the signal. The average abnormal return on the day of an analyst upgrade is 1.93% in our sample, compared to only 0.28% on the day of a positive analyst initiation. Thus, it may be that initiations are not powerful enough signals to induce contrarian trading by institutions. In accordance with this conjecture, we also note a less pronounced increase in institutional trading volume prior to and on the day of analyst initiations in our sample, compared to volume surrounding analyst recommendation changes.

6. Conclusion

In this paper we study how informed investors trade when they have short-lived private information. We are motivated by the theoretical work of Hirshleifer et al. (1994) and Brunnermeier (2005), who predict that early-informed investors will follow a "buy the rumor, sell the fact" trading strategy. Our empirical approach is to identify likely early-informed investors by relying on analysts' practice of tipping institutional clients several days before they issue a recommendation. We study how different types of investors trade around analyst recommendation changes, using a unique dataset that captures all NYSE trading by institutions and individuals from 1999 to mid-2010.

To our knowledge, this is the first study to analyze the differences in trading activity between early- and late-informed investors. Consistent with the theoretical predictions, we find that institutions (who are likely tipped) are significant net buyers before upgrades, and they buy more of stocks that ultimately have the biggest returns when analyst recommendation changes are announced. On the day of the upgrade, institutions trade in the opposite direction, selling upgraded stocks. In contrast, individuals, who are unlikely to be tipped, do not exhibit abnormal trade imbalances before recommendation changes. Institutions do not "buy the rumor and sell the fact" around equal-magnitude stock price moves that are unlikely to be accompanied by early access to information in the form of analyst tips. Finally, we find that program traders are the de-facto counterparties to institutional investors, buying and selling shares in a near mirror image to the pattern of institutional investors surrounding analyst upgrades.

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Table 1: Descriptive statistics

The sample consists of all domestic common stocks that were traded on the NYSE and had analyst recommendation changes between March 10, 1999 and April 22, 2010. Panel A presents descriptive statistics for the 2,122 stocks in the sample. *Market capitalization* is calculated annually from CRSP; Number of analysts covering (*# Analysts covering*) is calculated annually from I/B/E/S; and *Institutional holdings* are calculated quarterly as the percentage of shares held by institutional owners from Thompson 13F database; *Raw Trading Volume* and *Raw Trade Imbalance* are calculated daily from CAUD data files. All variables in Panel A are averaged for each stock over the sample period, and across-stock statistics are reported in Panel A. Panel B reports the number of analyst recommendation changes in the sample year-by-year, with *Upgrades* and *Downgrades* determined based on a three-tier scale (buy/hold/sell).

Panel A: Firms in sample

	Mean	Median	Std Dev
Market capitalization ($bn)	6.490	1.532	19.277
# Analysts covering	7.1	6.0	4.7
Institutional holdings (%)	66.6	69.4	22.6
Raw Trading Volume (%)			
Institutional	63.4	64.5	11.9
Individual	4.3	2.2	6.2
Raw Trade Imbalance (%)			
Institutional	0.0	0.4	1.2
Individual	-0.1	0.0	0.8
Number of firms	2,122		

Panel B: Recommendation changes per year

	Upgrades	Downgrades	All
1999	1,151	1,106	2,257
2000	386	598	984
2001	795	1,050	1,845
2002	1,194	2,124	3,318
2003	1,667	1,871	3,538
2004	1,414	1,437	2,851
2005	1,328	1,077	2,405
2006	1,157	1,189	2,346
2007	1,774	1,476	3,250
2008	2,016	1,981	3,997
2009	1,735	1,607	3,342
2010	484	391	875
All	15,101	15,907	31,008

Table 2: Univariate regressions of abnormal volume surrounding analyst recommendation changes

This table presents univariate analyses of abnormal trading volumes in the days surrounding analyst upgrades (Panel A) and downgrades (Panel B). The dependent variable is abnormal trading volume for institutional (three left columns) or individual (three center columns) traders. Abnormal volume is defined as volume as a percent of NYSE volume during the period of interest minus volume as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the day of the analyst recommendation change). *Day 0* is the day the analyst recommendation change is released if before 4:00 pm on a trading day, else the next trading day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Analyst upgrades

Dependent Variable	Institutional Volume			Individual Volume			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0121	0.0246	0.0141	-0.0005	0.0012	-0.0006	0.0126	0.0234	0.0147
	(3.7)	*(7.1)*	*(4.9)*	*(-1.2)*	*(2.6)*	*(-1.4)*	*(3.7)*	*(6.7)*	*(5.0)*
# Observations	15,101	15,101	15,101	15,101	15,101	15,101			

Panel B: Analyst downgrades

Dependent Variable	Institutional Volume			Individual Volume			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0082	0.0186	0.0132	0.0000	0.0024	-0.0002	0.0082	0.0162	0.0134
	(1.6)	*(5.2)*	*(4.8)*	*(0.0)*	*(4.1)*	*(-0.4)*	*(1.7)*	*(4.5)*	*(4.8)*
# Observations	15,907	15,907	15,907	15,907	15,907	15,907			

Table 3: Univariate regressions of abnormal trade imbalance surrounding analyst recommendation changes

This table presents univariate analyses of abnormal trade imbalances in the days surrounding analyst upgrades (Panel A) and downgrades (Panel B). The dependent variable is abnormal trade imbalance for institutional (three left columns) or individual (three center columns) traders. Abnormal imbalance is defined as shares bought minus shares sold as a percent of NYSE volume during the period of interest minus shares bought minus shares sold as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the day of the analyst recommendation change). *Day 0* is the day the analyst recommendation change is released if before 4:00 pm on a trading day, else the next trading day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Analyst upgrades

Dependent Variable	Institutional Trade Imbalance			Individual Trade Imbalance			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0065	-0.0123	-0.0009	0.0002	0.0010	0.0004	0.0063	-0.0133	-0.0013
	(2.5)	*(-3.1)*	*(-0.3)*	*(0.3)*	*(1.6)*	*(1.0)*	*(2.4)*	*(-3.2)*	*(-0.4)*
# Observations	15,101	15,101	15,101	15,101	15,101	15,101			

Panel B: Analyst downgrades

Dependent Variable	Institutional Trade Imbalance			Individual Trade Imbalance			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	-0.0011	0.0056	0.0013	-0.0002	0.0009	0.0011	-0.0009	0.0047	0.0001
	(-0.3)	*(1.3)*	*(0.3)*	*(-0.3)*	*(0.8)*	*(1.6)*	*(-0.2)*	*(1.0)*	*(0.0)*
# Observations	15,907	15,907	15,907	15,907	15,907	15,907			

Table 4: Multivariate regressions of institutional abnormal trade imbalances surrounding analyst recommendation changes

This table presents multivariate analyses of abnormal trade imbalances in the days surrounding analyst upgrades (Panel A) and downgrades (Panel B). The dependent variable is abnormal trade imbalance for institutional traders. Abnormal imbalance is defined as shares bought minus shares sold as a percent of NYSE volume during the period of interest minus shares bought minus shares sold as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the day of the analyst recommendation change). *Day 0* is the day the analyst recommendation change is released if before 4:00 pm on a trading day, else the next trading day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). *Small firm* is an indicator variable that is equal to one if the firm is below the median firm size, else zero; *All-star analyst* is an indicator variable that is equal to one if the analyst making the recommendation change is ranked as an all-star analyst by Institutional Investor in the prior year, else zero; and *High institutional ownership* is an indicator variable that is equal to one if the firm is above the median by insitutional ownership percentage as of the previous quarter-end, else zero. Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Analyst upgrades

Dependent Variable	Institutional Trade Imbalance		
	(1) Day -4 to -1	(2) Day 0	(3) Day +1 to +4
Intercept	0.0065	-0.0089	-0.0018
	(2.3)	*(-2.1)*	*(-0.5)*
Small firm	0.0016	-0.0068	-0.0008
	(1.0)	*(-3.0)*	*(-0.5)*
All-star analyst	0.0024	-0.0011	0.0044
	(1.2)	*(-0.4)*	*(2.2)*
High institutional ownership	-0.0015	-0.0001	0.0010
	(-0.9)	*(-0.1)*	*(0.6)*
# Observations	15,101	15,101	15,101
R^2	0.0020	0.0044	0.0065
Adj R^2	0.0011	0.0035	0.0056

Panel B: Analyst downgrades

Dependent Variable	Institutional Trade Imbalance		
	(1) Day -4 to -1	(2) Day 0	(3) Day +1 to +4
Intercept	-0.0013	0.0049	0.0002
	(-0.3)	*(1.1)*	*(0.1)*
Small firm	0.0002	0.0008	0.0024
	(0.1)	*(0.4)*	*(1.4)*
All-star analyst	0.0018	0.0023	-0.0032
	(0.9)	*(0.7)*	*(-1.5)*
High institutional ownership	-0.0002	0.0000	0.0005
	(-0.2)	*(0.0)*	*(0.3)*
# Observations	15,907	15,907	15,907
R^2	0.0032	0.0048	0.0073
Adj R^2	0.0023	0.0039	0.0065

Table 5: Regressions of institutional abnormal trade imbalances on returns

This table presents regression analyses of abnormal trade imbalances in the days prior to and of analyst upgrades (Panel A) and downgrades (Panel B). The dependent variable is abnormal trade imbalance for institutional traders. Abnormal imbalance is defined as shares bought minus shares sold as a percent of NYSE volume during the period of interest minus shares bought minus shares sold as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the day of the analyst recommendation change). *Day 0* is the day the analyst recommendation change is released if before 4:00 pm on a trading day, else the next trading day. *Day -4 to -1* reflects days -4 to -1 relative to day 0. *Return day 0* is the abnormal return for the stock on the day of the analyst recommendation change. Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Analyst upgrades	
Dependent Variable	Institutional Trade Imbalance
	Day -4 to -1
Intercept	0.0059
	(2.3)
Return day 0	0.0300
	(2.4)
# Observations	15,101
R^2	0.0020
Adj R^2	0.0012

Panel B: Analyst downgrades	
Dependent Variable	Institutional Trade Imbalance
	Day -4 to -1
Intercept	-0.0010
	(-0.3)
Return day 0	0.0057
	(0.3)
# Observations	15,907
R^2	0.0032
Adj R^2	0.0024

Table 6: Univariate regressions of abnormal volume surrounding placebo dates

This table presents univariate analyses of abnormal trading volumes in the days surrounding placebo positive-return days (Panel A) and negative-return days (Panel B). The dependent variable is abnormal trading volume for institutional (three left columns) or individual (three center columns) traders. Abnormal volume is defined as volume as a percent of NYSE volume during the period of interest minus volume as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the placebo day). *Day 0* is the placebo day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Placebo positive-return days

Dependent Variable	Institutional Volume			Individual Volume			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0058	0.0135	0.0096	-0.0007	0.0003	-0.0002	0.0065	0.0133	0.0099
	(1.5)	*(2.9)*	*(2.5)*	*(-1.6)*	*(0.5)*	*(-0.5)*	*(1.7)*	*(2.7)*	*(2.5)*
# Observations	15,101	15,101	15,101	15,101	15,101	15,101			
	Actual - Placebo Difference								
Actual - Placebo	0.0063	0.0111	0.0045	0.0003	0.0009	-0.0003			
	(1.9)	*(2.2)*	*(1.1)*	*(0.5)*	*(1.6)*	*(-0.7)*			

Panel B: Placebo negative-return days

Dependent Variable	Institutional Volume			Individual Volume			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0070	0.0025	0.0097	-0.0007	0.0012	0.0003	0.0078	0.0013	0.0094
	(1.9)	*(0.5)*	*(2.8)*	*(-1.4)*	*(1.9)*	*(0.5)*	*(2.0)*	*(0.2)*	*(2.6)*
# Observations	15,907	15,907	15,907	15,907	15,907	15,907			
	Actual - Placebo Difference								
Actual - Placebo	0.0012	0.0161	0.0036	0.0007	0.0012	-0.0004			
	(0.3)	*(3.5)*	*(1.0)*	*(1.4)*	*(1.3)*	*(-0.7)*			

Table 7: Univariate regressions of abnormal trade imbalance surrounding placebo dates

This table presents univariate analyses of abnormal trade imbalances in the days surrounding placebo positive-return days (Panel A) and negative-return days (Panel B). The dependent variable is abnormal trading volume for institutional (three left columns) or individual (three center columns) traders. Abnormal volume is defined as volume as a percent of NYSE volume during the period of interest minus volume as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the placebo day). *Day 0* is the placebo day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Placebo positive-return days

Dependent Variable	Institutional Trade Imbalance			Individual Trade Imbalance			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0045	0.0168	0.0058	-0.0001	-0.0022	0.0000	0.0045	0.0190	0.0058
	(1.1)	*(2.8)*	*(1.6)*	*(-0.1)*	*(-2.3)*	*(0.0)*	*(1.1)*	*(3.0)*	*(1.6)*
# Observations	15,101	15,101	15,101	15,101	15,101	15,101			
	Actual - Placebo Difference								
Actual - Placebo	0.0021	-0.0291	-0.0067	0.0003	0.0032	0.0004			
	(0.5)	*(-4.9)*	*(-1.5)*	*(0.4)*	*(2.8)*	*(0.6)*			

Panel B: Placebo negative-return days

Dependent Variable	Institutional Trade Imbalance			Individual Trade Imbalance			Institutional - Individual Difference		
	(1)	(2)	(3)	(4)	(5)	(6)			
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0084	-0.0128	-0.0022	-0.0004	0.0014	0.0014	0.0089	-0.0143	-0.0036
	(1.9)	*(-2.4)*	*(-0.6)*	*(-0.6)*	*(1.1)*	*(2.5)*	*(2.0)*	*(-2.5)*	*(-0.9)*
# Observations	15,907	15,907	15,907	15,907	15,907	15,907			
	Actual - Placebo Difference								
Actual - Placebo	-0.0095	0.0185	0.0035	0.0003	-0.0005	-0.0003			
	(-1.5)	*(2.8)*	*(0.8)*	*(0.3)*	*(-0.3)*	*(-0.3)*			

Table 8: Univariate regressions of abnormal program trade and market maker imbalance surrounding analyst recommendation changes

This table presents univariate analyses of abnormal trade imbalances in the days surrounding analyst upgrades (Panel A) and downgrades (Panel B). The dependent variable is abnormal trade imbalance for institutional program traders (three left columns) and market makers (three right columns). Abnormal imbalance is defined as shares bought minus shares sold as a percent of NYSE volume during teh period of interest minus shares bought minus shares sold as a percent of NYSE volume during the benchmark period (days *t-45* to *t-11* and days *t+11* to *t+45* relative to the day of the analyst recommendation change). *Day 0* is the day the analyst recommendation change is released if before 4:00 pm on a trading day, else the next trading day. *Day -4 to -1* (*Day +1 to +4*) reflects days -4 to -1 (+1 to +4). Regressions include year fixed effects, and *t*-statistics (in parentheses below parameter estimates) are based on double-clustered standard errors, clustered on stock and date.

Panel A: Analyst upgrades

Dependent Variable	Program Trade Imbalance			Market Maker Imbalance		
	(1)	(2)	(3)	(4)	(5)	(6)
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	-0.0070	0.0119	0.0009	0.0004	-0.0005	-0.0005
	(-2.6)	*(3.0)*	*(0.3)*	*(0.8)*	*(-0.7)*	*(-0.5)*
# Observations	15,101	15,101	15,101	15,101	15,101	15,101

Panel B: Analyst downgrades

Dependent Variable	Program Trade Imbalance			Market Maker Imbalance		
	(1)	(2)	(3)	(4)	(5)	(6)
	Day -4 to -1	Day 0	Day +1 to +4	Day -4 to -1	Day 0	Day +1 to +4
Intercept	0.0024	-0.0081	-0.0024	-0.0007	0.0021	0.0005
	(0.7)	*(-2.0)*	*(-0.6)*	*(-0.9)*	*(1.9)*	*(0.7)*
# Observations	15,907	15,907	15,907	15,907	15,907	15,907

Figure 1: Volume surrounding analyst upgrades

Daily *Raw Trading Volume* for each stock is defined as trader-type volume divided by total NYSE volume for each stock each day. Daily *Abnormal Trading Volume* for each stock is equal to Raw Trading Volume minus trader-type Benchmark Trading Volume, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change. Graphs depict averages across 15,101 analyst upgrades from March 10, 1999 to April 22, 2010.

Figure 1-A: Upgrades -45 days to +45 days

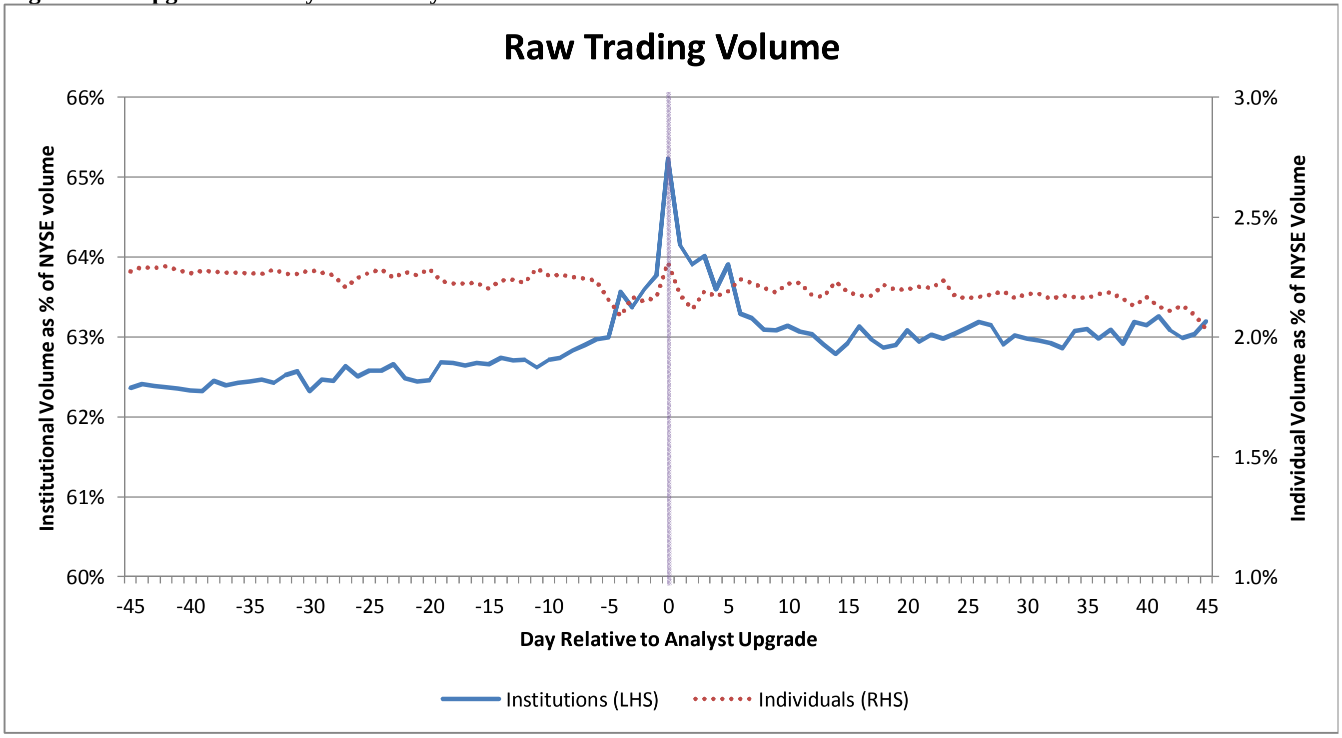


Figure 1-B: Upgrades -5 days to +5 days

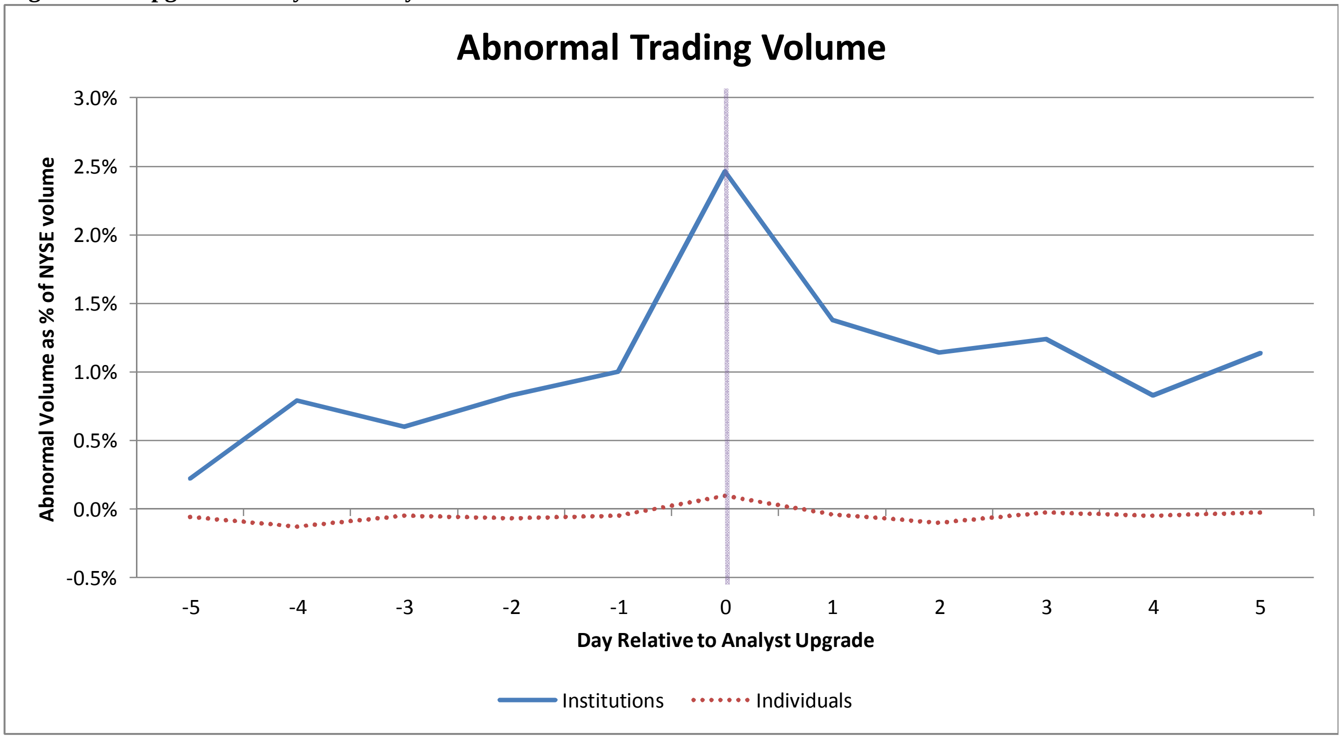


Figure 2: Volume surrounding analyst downgrades

Daily *Raw Trading Volume* for each stock is defined as trader-type volume divided by total NYSE volume for each stock each day. Daily *Abnormal Trading Volume* for each stock is equal to Raw Trading Volume minus trader-type Benchmark Trading Volume, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change. Graphs depict averages across 15,907 analyst downgrades from March 10, 1999 to April 22, 2010.

Figure 2-A: Downgrades -45 days to +45 days

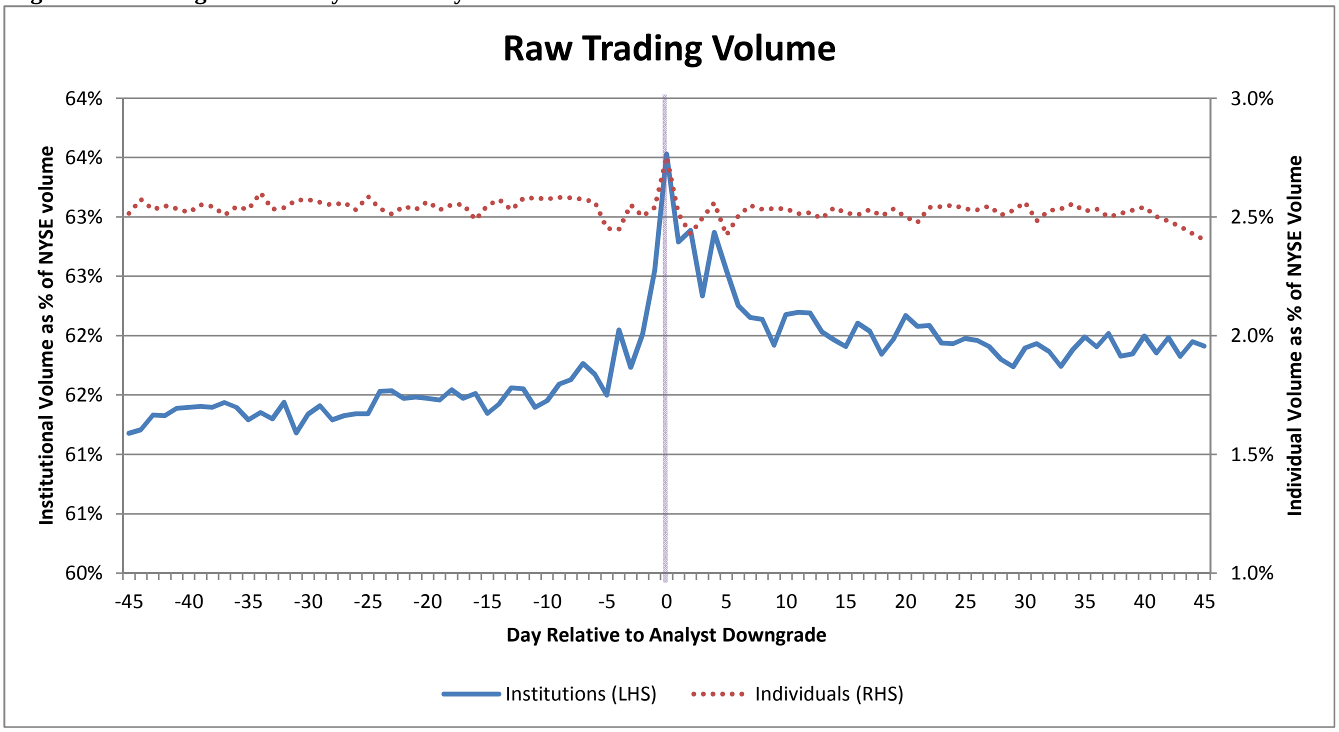


Figure 2-B: Downgrades -5 days to +5 days

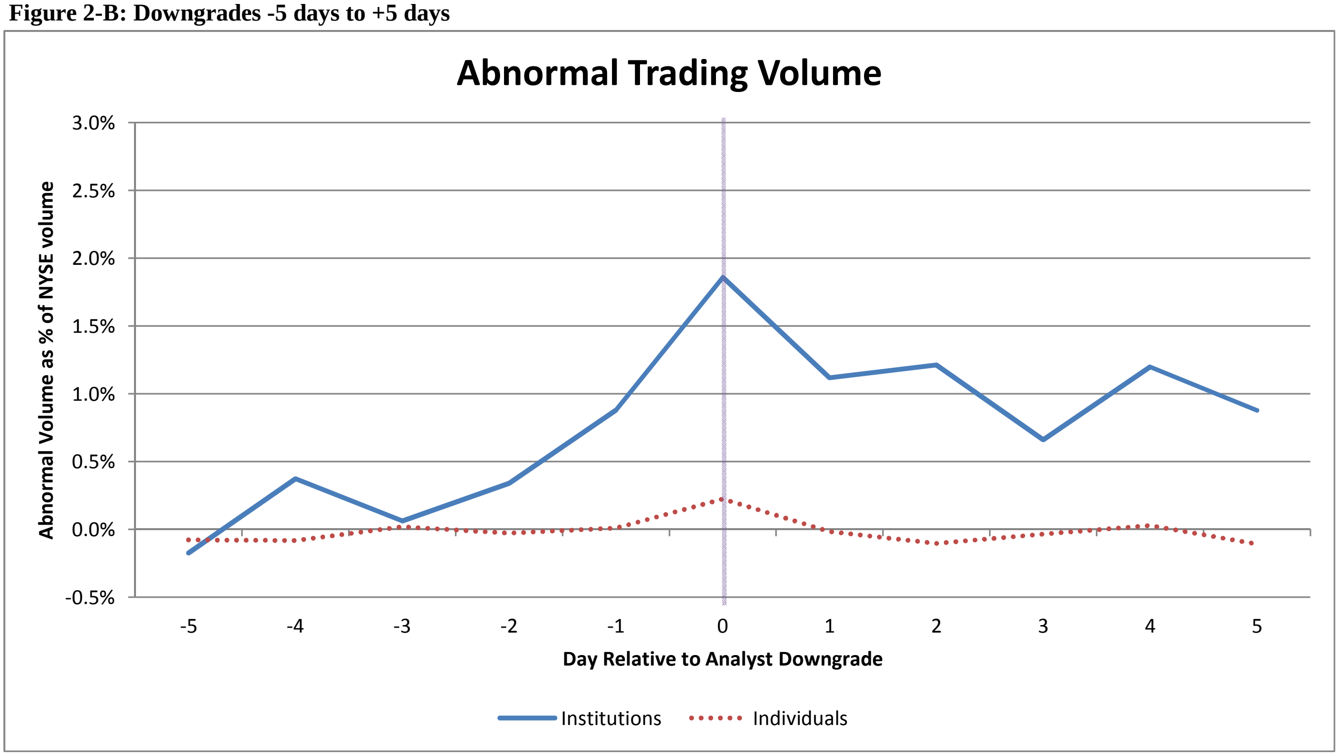


Figure 3: Imbalance surrounding analyst upgrades

Daily *Raw Trade Imbalance* for each stock is defined as trader-type buy minus sell imbalance divided by total NYSE volume for each stock each day. Daily *Abnormal Trade Imbalance* for each stock is equal to Raw Trade Imbalance minus trader-type Benchmark Trade Imbalance, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change. Graphs depict averages across 15,101 analyst upgrades from March 10, 1999 to April 22, 2010.

Figure 3-A: Upgrades -45 days to +45 days

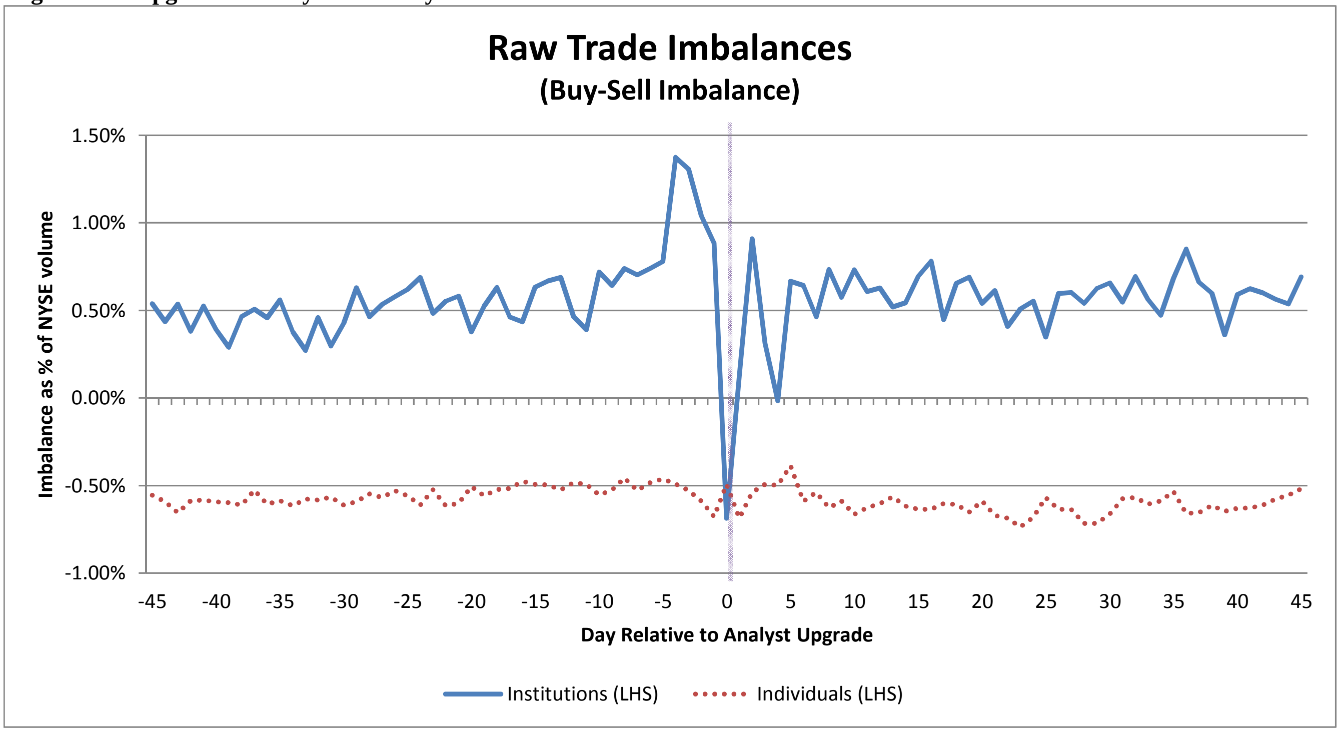


Figure 3-B: Upgrades -5 days to +5 days

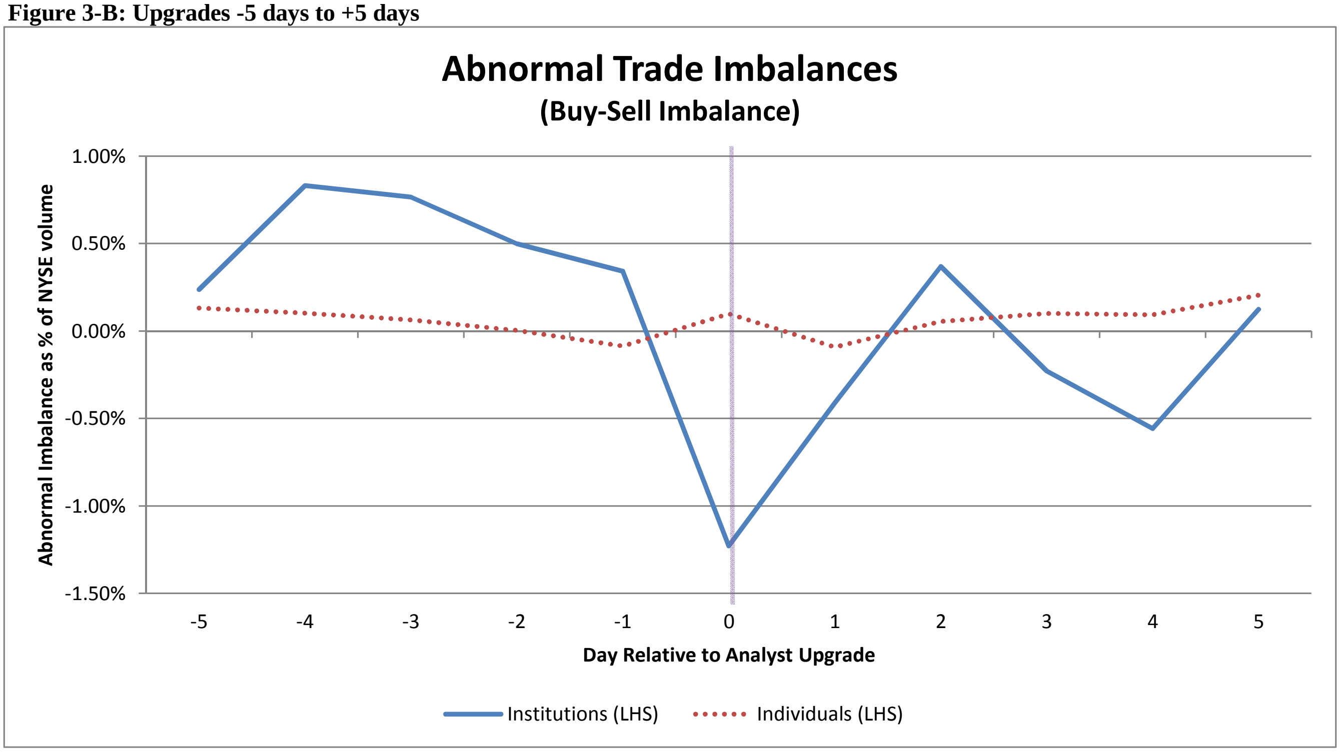


Figure 4: Imbalance surrounding analyst downgrades

Daily *Raw Trade Imbalance* for each stock is defined as trader-type buy minus sell imbalance divided by total NYSE volume for each stock each day. Daily *Abnormal Trade Imbalance* for each stock is equal to Raw Trade Imbalance minus trader-type Benchmark Trade Imbalance, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change. Graphs depict averages across 15,907 analyst downgrades from March 10, 1999 to April 22, 2010.

Figure 4-A: Downgrades -45 days to +45 days

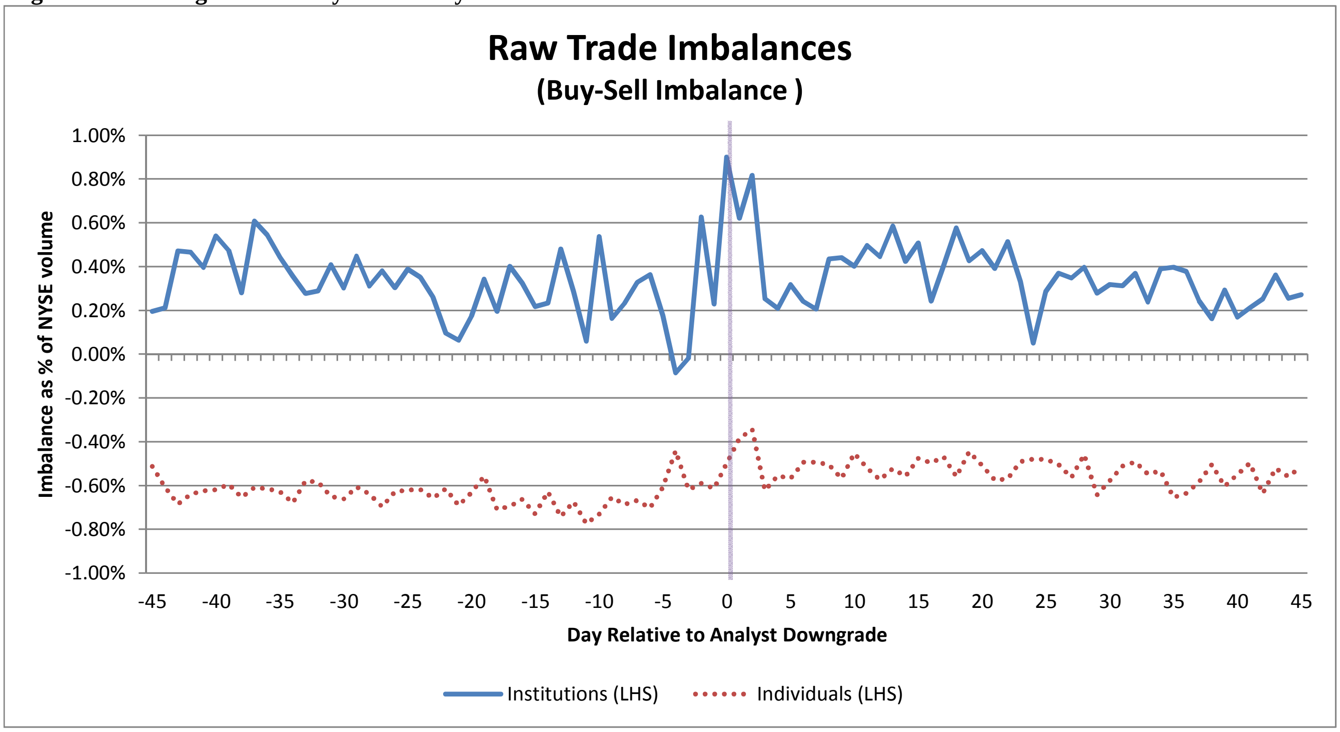


Figure 4-B: Downgrades -5 days to +5 days

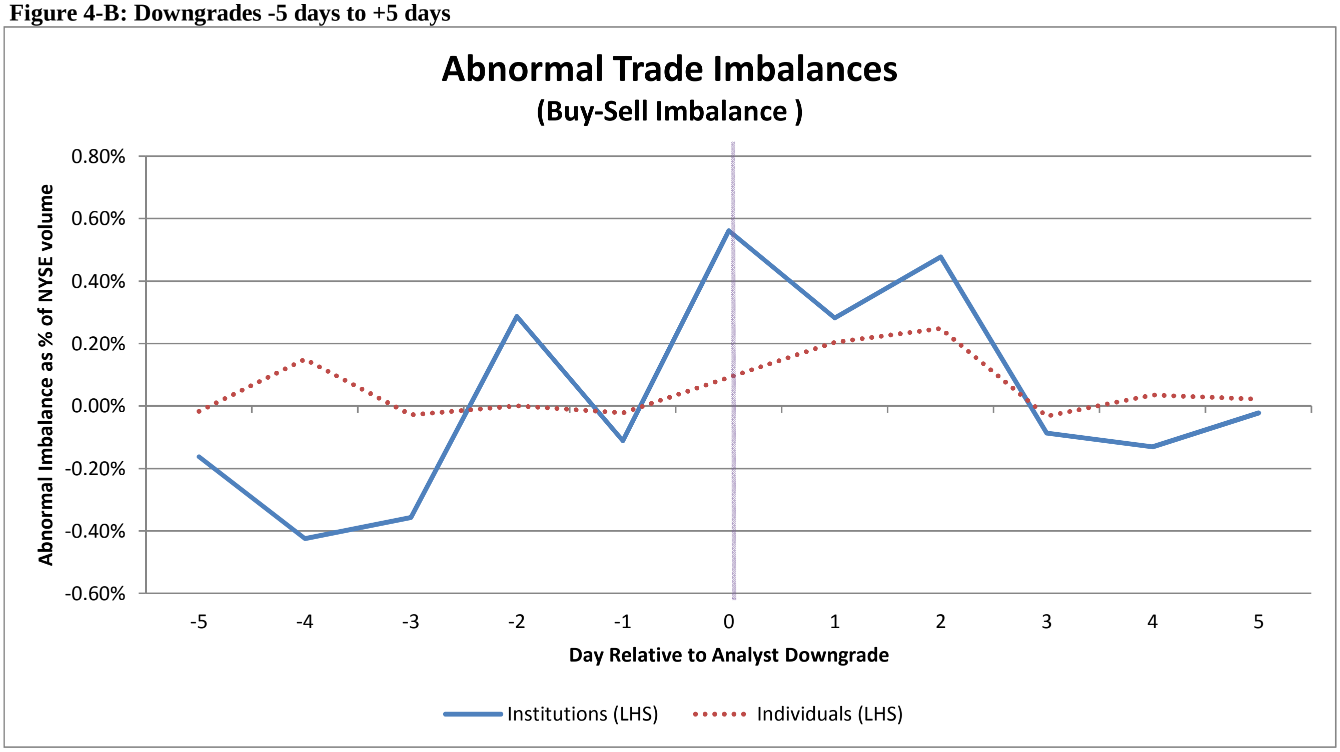
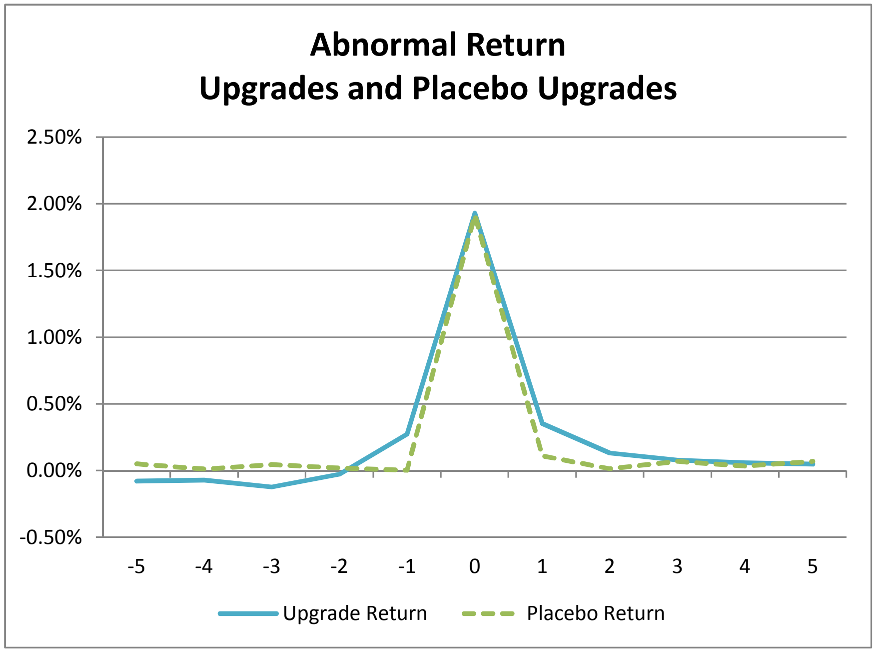


Figure 5: Abnormal returns surrounding analyst recommendation changes and placebo events

Graphs depict average abnormal returns across 15,101 analyst upgrades and placebo upgrades (left graph) and 5,907 analyst downgrades and placebo downgrades (right graph) from March 10, 1999 to April 22, 2010. For each analyst recommendation change, the placebo event is defined as the stock/day combination on which the same stock has the closest abnormal return to the stock's abnormal return on the day of the analyst recommendation change. Days within *t-4* to *t+4* of analyst recommendation changes are excluded, and placebo events are chosen without replacement.



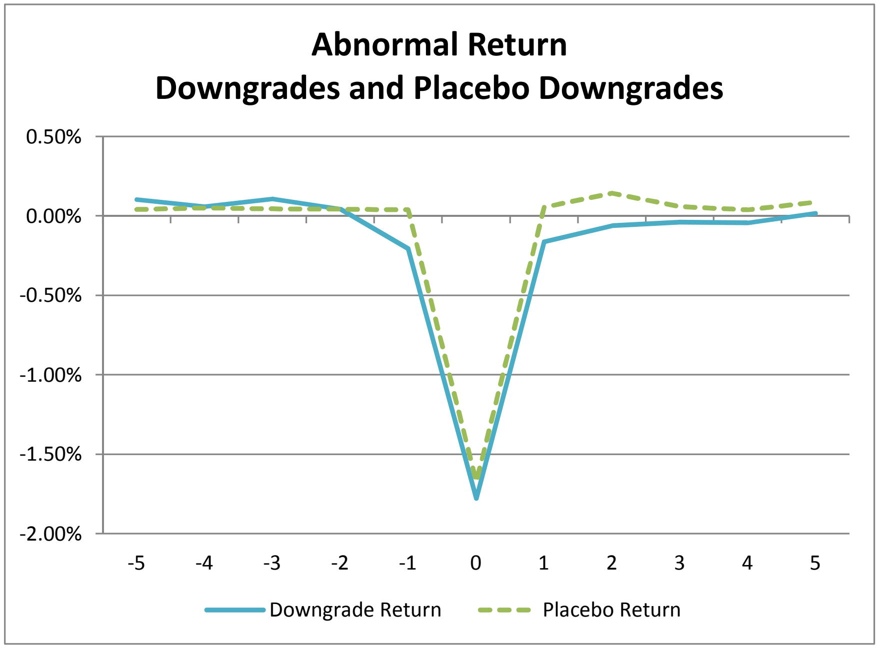


Figure 6: Abnormal trading volume surrounding analyst recommendation changes and placebo events

Graphs depict average *Abnormal Trading Volume* for institutional investors (top graphs) and individual investors (bottom graphs) across 15,101 analyst upgrades and placebo upgrades (left graphs) and 5,907 analyst downgrades and placebo downgrades (right graphs) from March 10, 1999 to April 22, 2010. For each analyst recommendation change, the placebo event is defined as the stock/day combination on which the same stock has the closest abnormal return to the stock's abnormal return on the day of the analyst recommendation change. Dates within *t-4* to *t+4* of analyst recommendation changes are excluded, and placebo events are chosen without replacement. Daily *Abnormal Trading Volume* for each stock is equal to Raw Trading Volume minus trader-type Benchmark Trading Volume, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change or placebo event.

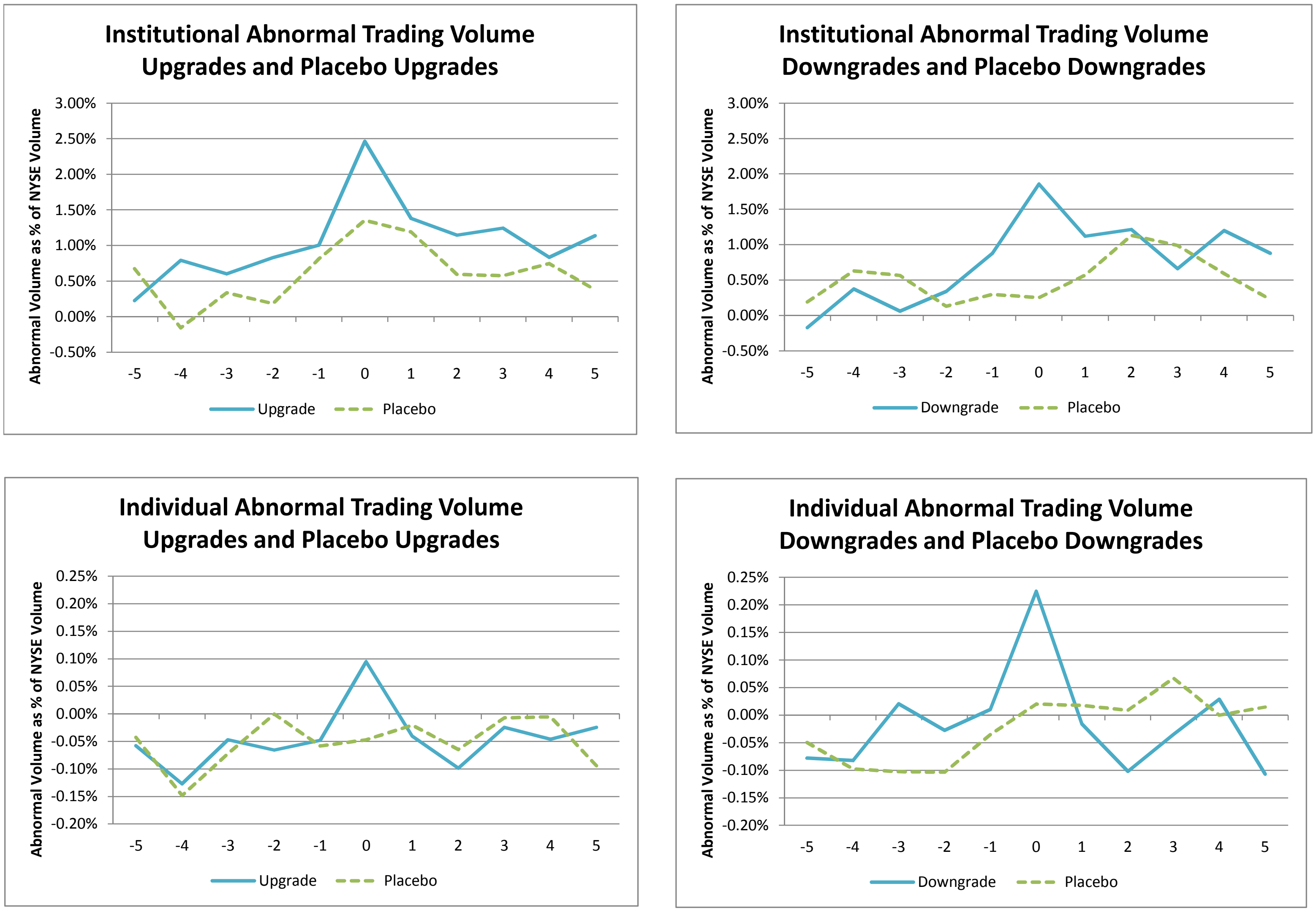


Figure 7: Abnormal trade imbalances surrounding analyst recommendation changes and placebo events

Graphs depict average *Abnormal Trade Imbalances* for institutional investors (top graphs) and individual investors (bottom graphs) across 15,101 analyst upgrades and placebo upgrades (left graphs) and 5,907 analyst downgrades and placebo downgrades (right graphs) from March 10, 1999 to April 22, 2010. For each analyst recommendation change, the placebo event is defined as the stock/day combination on which the same stock has the closest abnormal return to the stock's abnormal return on the day of the analyst recommendation change. Dates within *t-4* to *t+4* of analyst recommendation changes are excluded, and placebo events are chosen without replacement. Daily *Abnormal Trade Imbalance* for each stock is equal to Raw Trade Imbalance minus trader-type Benchmark Trade Imbalance, measured over the period from -45 to -11 and +11 to +45 days relative to each analyst recommendation change or placebo event.