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Wealth Transfer Effects of Analysts' Misleading Behavior

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ABSTRACT

We investigate a sample of 50 firm-events, identified in the *Global Research Analysts Settlement*, in which analysts were discovered to have acted misleadingly ex post. In this setting, analysts' incentives caused them to issue public disclosures that differed from their private beliefs. We document that these firms' institutional holdings decline significantly during the period in which the analysts issued misleading disclosures. During this period daily small-size trades (a proxy for individual investors) are dominated by buy orders while daily large-size trades (a proxy for institutional investors) are dominated by sell orders. Short interest increases during the event period, consistent with the idea that sophisticated investors are selling. Our estimates of investors' trading losses show that individual investors lost about two and a half times the amount lost by institutions. Overall, the results suggest a wealth transfer from individuals to institutions that is likely attributable to analysts' misleading behavior.

1. Introduction

Economic theory predicts that sell-side analysts act strategically and in their best interests. In some cases, this strategic behavior results in analysts

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rationally issuing public disclosures to investors that differ from the analysts' beliefs. It is difficult, however, for researchers to identify the strategic behavior of analysts empirically because analysts' beliefs are private. Our study takes advantage of the *Global Research Analyst Settlement* (Settlement) to identify a "clean" set of firm-events in which analysts issued misleading stock research. We use this sample to test whether analysts' misleading behavior leads to a wealth transfer from individual investors to institutional investors (hereafter, "individuals" and "institutions," respectively).

In formal complaints, the SEC documents how analysts from 11 prominent Wall Street investment banks (sanctioned banks) provided misleading information to investors over the period April 1999 to July 2002, allegedly because of investment banking incentives.[1] A large part of the evidence consists of analysts' private communications recorded in emails. In some instances, analysts communicated positive public information that was inconsistent with their private negative views about the stocks. In other cases, analysts made exaggerated or unwarranted claims or failed to publicly disclose to investors fees received in exchange for publishing research. We draw our sample of 50 firm-events directly from the Settlement. We document that individuals adjust their holdings differently from institutions for poorly performing stocks in which analysts act misleadingly, resulting in a wealth transfer. We highlight that our inferences are unaffected by regulators' selection criteria because these firm-events were chosen for reasons unrelated to the wealth transfer we document. A summary of our tests and findings follows.

First, we predict and find that for Settlement firms, institutional share holdings, measured using their Securities and Exchange Commission (SEC) 13F filings, decreased over the event period, implying that institutions were selling these stocks. The decline is attributable mainly to decreases in the number of institutions holding the stock rather than to decreases in the ownership per institution. This is consistent with analysts disclosing their private information to select client institutions. Second, we investigate trading behavior using detailed New York Stock Exchange (NYSE) trade and quote (TAQ) information. We use trade size to determine whether the trade was initiated by institutions or individuals and the Lee and Ready [1991] algorithm to classify trades as buyer- or seller-initiated orders. "Net buy" is the difference between buyer- and seller-initiated trades. We find that individuals net buy relatively more shares than institutions over the event period, consistent with the institutional ownership results. Third, we use the TAQ-based trading measures in conjunction with actual stock returns to calculate the economic consequences of the different trading behaviors of individuals and institutions. Although institutions are involved in the bulk of the trading, we find that individuals lose $2.2 billion, an amount that is approximately two and a half times the amount that institutions lose. While

[1] The SEC complaints can be found at http://www.sec.gov/spotlight/globalsettlement.htm.

the TAQ-based trading measures are a noisy proxy, these results corroborate the institutional-holdings analysis. Fourth, we show that short interest increased during the event period. This result suggests that sophisticated investors, broadly defined to include hedge funds, sell ahead of the stock price declines and benefit over the event period. Because these short sellers do not typically file 13Fs, our institutional-ownership tests (which are based on 13F holding changes) understate the negative effect of analysts' misleading behavior on individuals.

We also conduct our tests on selected subsamples. Our results are generally stronger when there is evidence that analysts tipped off selected institutions, when sanctioned banks have larger numbers of individual investor clients, when firms have fewer analysts issuing recommendations, and when firms are larger. Our results are robust to comparisons with those for control firms that are matched on exchange listing, size, industry, and analyst consensus recommendation levels, and to the use of various fixed-length event windows. Overall, our findings show that institutions lose less than individuals during the event period, consistent with analysts' misleading disclosures causing a wealth transfer from individuals to institutions. Our evidence is less consistent with other scenarios, such as institutions placing less weight on analyst opinions or institutions unraveling analyst bias better than individuals.

We make two contributions. First, our study provides a direct test of whether analysts' misleading behavior harms investors, thereby contributing to the growing literature on *explanations* for the differential investment performance of investor classes, such as institutions and individuals. In other work, Iskoz [2003], Malmendier and Shanthikumar [2004, 2005], and Mikhail, Walther, and Willis [2005] study the effect of recommendations on the trading behavior of individuals and institutions. They provide mixed evidence on whether individuals suffer trading losses by following the recommendations of analysts who issue positive public recommendations and analysts employed by investment banks who issue firm's securities. While the effect of recommendations is interesting in its own right, these studies cannot distinguish between whether analysts behave strategically or whether they simply have overly optimistic views of the stock. Our sample only includes confirmed situations in which analysts behave strategically. Also, these other studies assume constant short (e.g., three- or five-day) or long (e.g., one-year) windows, which are not exogenous because analysts choose when to issue recommendations. We use medium-sized event periods exogenously defined by the Settlement. Neither the analysts engaging in the misleading behavior nor the investors who were trading during the event period knew that formal investigations would take place.

Second, our results provide support for recent regulation that has changed the incentives of analysts. For example, as part of the Settlement, sanctioned banks agreed to sever links between research and investment banking (SEC [2003a]). To the extent that these regulatory changes have significantly reduced, or eliminated, situations in which analysts act

misleadingly, the plight of individuals has improved. Addressing these issues is consistent with the aims of regulators and lawmakers to protect all investors. Our results complement other research on recent analyst regulation. Barber, Lehavy, and Trueman [2006] document that investment bank buy recommendations, particularly those issued by investment banks involved in the Settlement, on average underperform the buy recommendations of independent research firms. Barber et al. [2006] show that part of the decrease in the percentage of buy recommendations is a result of the analyst regulation. Further, Kadan et al. [2005] find that after the adoption of the regulation the probability of receiving an optimistic recommendation no longer depends on whether the analyst is affiliated.

Section 2 discusses prior research and motivates our research question. Section 3 provides institutional background on the Settlement and describes our sample. We then document how institutional investors respond to analysts' misleading behavior using institutional holdings (section 4) and TAQ data (section 5), respectively. In section 6, we document the wealth transfer from actively trading individuals to institutions during the event period. The examination of changes in short interest is presented in section 7. Section 8 provides additional analyses, including controls for the effect of returns and short sales, as well as the results of our tests on select subsamples. Section 9 provides some caveats of our analyses, as well as discusses how our findings could generalize to other situations.

2. Literature Review and Research Question

A large body of research studies the differential investment behavior of institutions and individuals. Most of these studies document a relation between categories of investors and stock returns (past, contemporaneous, and future).[2] Research that explains the relation between stock returns and investor trading behavior is more limited. Cohen, Gompers, and Vuolteenaho [2002] show that institutional trading decisions are correlated with firms' cash flow news. Barber and Odean [2006], Lee [1992], and Hirshleifer et al. [2003] show that individuals buy when any type of news (negative or positive) arrives. Malmendier and Shanthikumar [2004, 2005], Mikhail, Walther, and Willis [2005], and He and Mian [2005] find that institutions and individuals,

[2] Lakonishok, Shleifer, and Vishny [1992], Nofsinger and Sias [1999], Grinblatt, Titman, and Wermers [1995], Wermers [1999, 2000], Chen, Jegadeesh, and Wermers [2000], Grinblatt and Keloharju [2001], Griffin, Harris, and Topaloglu [2003], and others show that institutional buying is correlated with past and contemporaneous stock returns. Odean [1998] and Griffin, Harris, and Topaloglu [2003] provide evidence that individual stock buying is correlated negatively with past returns and correlated positively with contemporaneous returns. The evidence that institutional (individual) buying is correlated positively (negatively) with short-term expected returns or that institutions (individuals) produce superior (inferior) investment performance is mixed (Jensen [1968], Lakonishok, Shleifer, and Vishny [1992], Nofsinger and Sias [1999], Odean [1999], Barber and Odean [2000], Wermers [2000], Gompers and Metrick [2001], Griffin, Harris, and Topaloglu [2003], Barber et al. [2005]).

proxied by large- and small-size trades, respectively, react over three- to five-day windows to analyst disclosures, and that their relative reaction depends on the level, change, and type (recommendation or forecast) of disclosure. Two papers directly link postanalyst information event-return performance to investor types and analyst information. Malmendier and Shanthikumar [2004] show that long-term returns following analyst recommendations are negatively correlated with the buying behavior of individuals. In contrast, Iskoz [2003] uses 13F institutional holdings data to measure trading behavior and asserts that individuals do not lose money by following analyst advice.

None of these studies distinguish between analysts who strategically bias their recommendations and analysts who simply have positive views. Some of these studies partition analysts according to whether they work for affiliated or unaffiliated investment banks. Affiliated analysts issue more optimistic earnings growth forecasts and more favorable recommendations, and are slower to downgrade in the face of negative news (Dugar and Nathan [1995], Lin and McNichols [1998], Michaely and Womack [1999], Dechow, Hutton, and Sloan [2000], O'Brien, McNichols, and Lin [2005]). As Kolasinski and Kothari [2005] argue, however, affiliation does not necessarily measure strategic behavior. Firms about to issue equity may simply select investment banks with analysts who have truly positive views of the firm. Bradley, Jordan, and Ritter [2006] argue further that affiliation could be negatively correlated with strategic behavior. Because affiliated analysts are more likely to retain future investment-banking business, nonaffiliated analysts have stronger incentives to add optimistic bias to "curry favor" with management. Our sample relies on neither affiliation nor analyst recommendations to proxy for strategic behavior.

We use this setting to test whether analysts' misleading behavior leads to a transfer of wealth from individuals to institutions. We consider institutions to be more informed because they are more sophisticated, have access to larger information sets, and spend more resources on the investment process than individuals.[3] We predict that institutions gain at the expense of individuals for three reasons. First, we expect that institutions place less weight on signals from sell-side analysts so that analysts have a relatively smaller effect on institutional trading. Even if the analyst signal is unadjusted for any potential bias, institutional demand for the stock will deviate from the prior optimal level by a relatively smaller amount than the individual demand. Second, if institutions anticipate that individuals will follow analysts' recommendations to a greater extent, then institutions could take advantage of any temporary trading-related pressure on prices caused by misinformed individuals. Third, institutions could be better at unraveling the sell-side

[3] See Bhushan [1989], Lo and Mackinlay [1990], Hand [1990], Lee [1992], Cornell and Sirri [1992], Badrinath, Kale, and Noe [1995], Sias and Starks [1997], Walther [1997], Bonner, Walther, and Young [2003], Battalio and Mendenhall [2005], and Callen, Hope, and Segal [2005].

analysts' bias. Institutions could also receive less-biased information, and hence trade based on analysts' true view of the stock. Analysts have more incentives to serve institutions than individuals because institutions manage larger pools of funds, and hence trade more shares that increase brokerage commissions. In addition, analysts routinely communicate with institutions in less formal ways (such as by phone, by email, and in person), which provides analysts with an opportunity to directly or indirectly convey their true views.

3. Institutional Background and Sample

3.1 THE SETTLEMENT

The Settlement was first announced on December 20, 2002 (SEC [2002]) and is the culmination of investigations of potential conflicts of interest in equity research analysis by the New York (NY) Attorney General, U.S. SEC, National Association of Security Dealers, NYSE, and state regulators. The publicized civil charge was that affiliated analysts routinely issued overly optimistic disclosures to curry favor with corporate clients and to win lucrative investment-banking business, and that in the process misled investors (SEC [2003a, 2003b], Smith, Craig, and Solomon [2003]). As part of the Settlement, 11 sanctioned banks agreed to provide $432.75 million to compensate investors who purchased specific securities during a specific period defined by the Settlement from a specific sanctioned bank.[4] We describe below in detail how our sample of sanctioned banks was selected and investigated.

Investigation into analysts' conflicts of interest started with congressional hearings in the summer of 2001. The office of the NY Attorney General started its investigation of Merrill Lynch at the same time (Gasparino [2002a]).[5] These investigations immediately followed the general collapse in the stock prices of technology and telecommunication companies, suggesting that investor losses were a major catalyst for the investigation. The complete set of banks under investigation was first reported in the *Wall Street Journal* on May 23, 2002. The 12 sanctioned banks include the 10 largest banks in the U.S. ranked by capital according to *The Securities Industry Yearbook* (Securities Industry Association [2001]). The other two sanctioned banks were mid-size brokerage houses specializing in technology equity offerings. The NY Attorney General's stated goal was to effect major structural changes in the way Wall Street banks provide stock research to eliminate investment-banking influences and to achieve a global resolution that the industry would accept (Gasparino [2002b]). The focus on these 12 banks

[4] The Settlement includes other penalties and structural reforms to resolve conflict of interest issues at brokerage firms (see the link in footnote 1).

[5] To clarify, Merrill Lynch is one of the 12 sanctioned banks as part of the Settlement. However, Merrill Lynch is not one of the 11 sanctioned banks in our sample because its agreement did not include money allocated for investor restitution.

was consistent with this goal because these banks conducted the majority of telecommunications and technology equity offerings and so produced most of the research by analysts who experienced investment-banking conflicts. These banks were the industry leaders and hence a global resolution was more likely if they all agreed. These banks attracted the most media attention and hence investigators would benefit more politically by investigating these firms. Consistent with Watts and Zimmerman [1986], regulators likely took advantage of the bursting of the tech bubble to create a "crisis" which they could then "solve" to gain political advantage. These banks generally had the largest number of institutional and individual clients.[6] Last, focusing on a subset of banks was consistent with the significant costs of investigating each bank.

The investigative process consisted of interviews with analysts and executives at the sanctioned banks and reviews of emails and other internal documents (such as performance contracts). For example, five investigators reviewed 30,000 emails related to Merrill Lynch's Internet group (Gasparino [2002c]) and state regulators investigating Goldman Sachs reviewed more than 80,000 emails and other documents (Schroeder and Smith [2002]). During the settlement process, the SEC publicized the details of the complaint against each sanctioned bank (SEC [2003a, 2004]).

3.2 CLASSIFICATION OF MISLEADING ANALYST BEHAVIOR

The SEC announced the exact dates for the 53 selected stocks (some for multiple time periods) for which investors who were clients of the sanctioned banks would be eligible for compensation (SEC [2003b, 2004]). It is during these dates that analysts allegedly engaged in misleading behavior by communicating positive public information about the selected stocks while the prices of these stocks were generally falling.[7] The appendix lists the sanctioned banks, the equity securities involved, and the start and end dates of the eligible period—all as defined in the Settlement.[8]

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[6] These banks were unlikely to have been chosen solely because of the number of individual clients. Many other brokerage houses had much larger numbers of individual clients. To examine the size of the individual investor client base, we reviewed the number of broker offices per investment bank, a proxy for the number of individual-investor clients, as published in *The Securities Industry Yearbook* (Securities Industry Association [2001]). Only 4 of the 12 sanctioned banks are ranked in the top 10 in terms of broker offices.

[7] In some cases, the negative returns were quite dramatic and led directly to bankruptcy. For example, four of the Settlement firms went bankrupt and were delisted within three months of the end of the event period. All our results are robust to the exclusion of these observations. Untabulated analysis indicates that 46 of the 50 firm-events in the Settlement sample experienced a decrease in their stock price over the SEC event period, although not all returns are so dramatic.

[8] In some cases, we can reconcile the chosen start and end dates of the Settlement with dates specifically mentioned in the SEC complaint. For example, many of the start and end dates correspond with the date of the email indicating that the analysts' true views were negative and the date of the downward recommendation revision, respectively. However, it is impossible to

TABLE 1

Descriptive Statistics on the Investor Restitution Part of the Global Research Analyst Settlement

This table presents descriptive statistics on the classification of analysts' misleading behavior and distribution of the event-period lengths. Panel A presents the frequency of each allegation of misleading behavior as established in the final Settlement. Panel B presents the frequency distribution of event-window lengths by the number of months.

Panel A: Classification of analysts' misleading behavior

	Frequency	
Alleged Behavior	Number	Percentage of Total
Fraud	4	8%
Exaggerated, unwarranted, or no reasonable basis	34	68%
Receiving payment for research coverage but no disclosure	8	16%
Other (failing to update)	4	8%
Disclosed selectively	16	32%

Panel B: Distribution of event-period lengths

	Frequency		
Event-Period Length (Months)	Number	Percentage of Total	Cumulative Percentage
1	3	6%	6%
2	4	8%	14%
3	24	48%	62%
4	3	6%	68%
5	1	2%	70%
6	3	6%	76%
7	6	12%	88%
8	1	2%	90%
9	1	2%	92%
>9	4	8%	100%

Each security is associated with a specific allegation made by the SEC. Table 1, panel A tabulates the frequency of the allegations for the firms in our sample. The most severe allegation is "Fraud," in which analysts employed by the sanctioned banks were accused of publishing positive recommendations that were contrary to their true negative opinions. The most common allegation was of publishing reports that contained "exaggerated" or "unwarranted" claims and/or contained opinions for which there was "no reasonable basis." Another similar allegation was that analysts failed to keep recommendations current. The remaining allegation against the sanctioned banks was that they received payments for providing research coverage for some firms but failed to disclose these fees to investors. These payments were from other investment banks who conducted the firm's equity offering.

reconcile the dates systematically. Investigators had access to much larger sets of information and we assume that the start and end dates of the Settlement represent the periods in which the research was misleading.

Investigators identified 16 cases in which private "negative" signals were sent to institutions via email, even though the official public recommendation was positive. These cases are a subset of those in which analysts were accused of issuing fraudulent reports or making exaggerated or unwarranted claims. For example, the SEC complaint against Bear Stearns reports that in an email discussing some bad news about CAIS Internet, Inc., "the analyst stated: 'Any other scoop on this piece of sh–?'" and a few days later in response to an institutional investor query about CAIS fourth-quarter results, "the analyst stated: 'It's up a lot year to date . . . don't overstay your welcome on this one.'" In another example, the SEC complaint against Lehman Brothers Inc. reports that an analyst "wrote to an institutional investor, 'if it's in my group it's a short' despite the fact that the analyst maintained 1-Strong Buy ratings on all of his stocks."

3.3 SETTLEMENT FIRMS SAMPLE

In general, analysts appear in the sample because they worked for the sanctioned banks, covered investment-banking clients, and published allegedly misleading research.[9] The analyst behaviors correspond to specific stocks and periods. We rely on the start and end dates of the investor restitution period for each stock, as listed in the Settlement, to define our set of firm-event observations. Table 1, panel B shows the distribution of the event-period lengths. These range from seven days up to 14 months.

Of the 58 firm-events (53 firms) in the Settlement, with one exception, there is no overlap by security and time. Two brokers, Citigroup Global Markets and Thomas Weisel Partners, settled for the same firm, Level 3 Communications Inc., for almost identical time periods. These two events were combined into one observation. We exclude JDS Uniphase from our sample because it merged with another firm during the event period. We also exclude six other firms that had their initial public offering (IPO) within one quarter of the event period because the initial trading behavior of IPO firms may not be stable.[10] The final sample consists of 50

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[9] From reading the SEC complaints and reviewing actual investment-banking transactions (using the SDC database), we confirmed that, except for one firm, there was some form of investment-banking pressure placed on the analysts. For example, sanctioned banks participated in the IPOs and secondary offerings of 21 and 14 sample firms, respectively. In some cases, the SEC complaints state that the firm is an investment-banking client without detailing the relationship. Analysts also admit that their positive disclosures were motivated to increase the chance of obtaining an investment-banking assignment. In other cases, sanctioned banks were paid for research from the underwriting fees of specific firms but were not officially part of the underwriting team.

[10] Aggarwal, Prabhala, and Puri [2002] document that institutions dominate IPO allocations. Field and Hanka [2001] find that, when lockup agreements (typically 180 days after IPOs) expire, there is a permanent 40% increase in average trading volume and statistically significant three-day negative abnormal returns. Furthermore, IPO shares could be allocated to executives of firms unrelated to the IPO but who are clients of the investment bank that leads the IPO. This practice is discussed in the SEC complaints against the sanctioned banks and is banned as part of the Settlement.

firm-events (46 firms). The appendix specifically indicates those firms excluded from our sample. Untabulated analysis indicates that almost all Settlement firms are technology companies operating in communication, research and development (R&D)-intensive, or Internet-related industries and that the majority of these firms trade on NASDAQ (27), a small number on NYSE (9), and the remaining trade over-the-counter (14).

We highlight three notable aspects of our Settlement sample. First, neither the analysts acting misleadingly nor the investors trading during the event period knew that a formal investigation would take place. Thus, their behavior is untainted by the formal investigations and eventual settlement. While media articles and congressional hearings questioning analysts' conflicts of interest occurred in the spring and summer of 2001 and could have affected analyst behavior at this time, the likely effect would be to curtail the misleading behavior and hence would work against our findings.

Second, an important question is whether analysts were caught, and hence appear in our sample, *because* of the wealth transfers. Based on our analysis of the choice of banks investigated, the details of published formal allegations and the process that investigators followed, there is no evidence that investigators quantified investor losses in general, or the losses of institutions and individuals in particular, in any systematic way.[11] For example, the actual amounts for investor restitution in the Settlement are crude. Six sanctioned banks each paid $25 million. This coarseness is inconsistent with conjectures that investigators knew in advance the extent of investor losses, or whether losing investors were more likely to be individuals. Hence, while the sample could have been chosen for very specific reasons, (e.g., to maximize political gain), these reasons are exogenous to our research question—the effect of misleading analysts on wealth transfers. Thus, the selection should not affect inferences.

Third, our tests measure the trading of all investors and do not measure the specific trading of clients of the sanctioned banks (the only investors eligible for compensation). Research by the sanctioned banks was disseminated broadly through outlets such as First Call, Investext, Bloomberg, and the media (e.g., analysts were often interviewed on television). Other investors could have been influenced by the research (especially given the status of these investment banks), but purchased the stock via another broker, such as a competing full-service brokerage firm or an online trading entity.[12] To the extent that only a subset of investors is affected by the misleading behavior, the power of our tests is reduced.

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[11] We read all *Wall Street Journal* articles for the period January 1, 2000 to December 31, 2005 that were directly or remotely related to Wall Street analysts, research, conflicts of interest, and the Settlement. This encompassed several hundred articles, which we believe provides comprehensive coverage of the public details of these subjects.

[12] As an example, Credit Suisse First Boston (a sanctioned bank) distributed its research to Edward Jones, a bank not involved in the Settlement (Craig [2003]). Edward Jones has the largest number of broker offices, a proxy for the number of individual investor clients, in the United States among all banks (Securities Industry Association [2001]).

3.4 CONTROL FIRMS SAMPLE

We use a matched sample of firm-events to control for other potential factors that could affect our results. Control firms must satisfy the following criteria: be listed on the same exchange as the Settlement firm; have institutional ownership, I/B/E/S, and Compustat data available over the period examined; be in the same size (total assets) quintile; be in the same industry; and have a maximum difference in consensus recommendation between the Settlement firm and the control firm of less than or equal to one (the equivalent of a one-category difference, e.g., from strong buy to buy, or from buy to hold). Size quintiles are formed by sorting all firms that are in the same four-digit Standard Industrial Classification (SIC) industry in the calendar quarter prior to the event period. If more than one firm meets these criteria, we choose the control firm that is closest in the consensus recommendation to the Settlement firm. If no firm meets these criteria, we relax the industry constraint until we find a match. For the three sample firms that have more than one event period, we match each firm-event observation independently. We control for industry and size because these factors could affect the composition of individual and institutional investors. For example, many of our firms operate in technology industries, and institutions could have been leaving this sector in general. The advantage of controlling for consensus recommendation levels is that we can attribute any relative differences to analysts' misleading behavior rather than to analysts who simply have positive beliefs. This control group, however, could contain optimism that is a result of both true beliefs and strategic bias. The more that misleading behavior explains the optimism in the control group, the lower the power of our tests.

3.5 DESCRIPTIVE STATISTICS

Table 2 reports descriptive statistics for firm characteristics measured the quarter before the beginning of the Settlement event period. These variables (defined in the table) are computed using Compustat and I/B/E/S data. The variables *Institutional Ownership*, *Number of Institutions*, *Institutional Ownership/Institution*, *Short Interest*, and *Returns* are used in our tests and are discussed in the respective sections below. Panel A shows that the size of Settlement firms is skewed, with the mean larger than the median. Growth rates, proxied by percentage sales growth, are high and returns on assets are negative. Panel B reports descriptive statistics for the control-firm sample. Not surprisingly, given our matching process, the characteristics of the control sample are quite similar to those of the Settlement firms. The mean difference between Settlement and control firms for the Compustat and I/B/E/S variables is not different based on two-tailed *t*-tests.

3.6 RELATION BETWEEN ANALYST RECOMMENDATIONS AND RETURNS

Two conditions are necessary for a wealth transfer to exist: the investor groups must be systematically trading in different directions and there must be price changes following the trading that are consistent with the trading

TABLE 2

Descriptive Statistics on Sample Firms

This table reports descriptive statistics for our sample of 50 firm-events. Panel A(B) describes the Settlement (control) firms. Control firms are matched with the Settlement firms based on exchange listing, firm size, industry, and analyst consensus recommendation level. Variables are computed as of the quarter before the Settlement event period begins for all observations with available data. *Total Assets* is measured as total assets, *Market Capitalization* is the stock price at the end of the fiscal year multiplied by the number of common shares outstanding, *Leverage* is the ratio of long-term debt plus current debt to total assets, *Return on Assets* is the ratio of income before extraordinary items to total assets, *Growth* is the percentage change in net sales, and *Book to Market* is common equity divided by market value. These variables are computed using Compustat data. *Recommendation Levels* is the I/B/E/S consensus analyst recommendation level. From Thomson Financial, *Institutional Ownership* is the percentage of common stock held by institutional shareholders, *Number of Institutions* is the number of institutions owning the stock, and *Institutional Ownership/Institution* is the percentage of total common stock held by institutional shareholders divided by the number of institutions owning the stock. From Bloomberg and Barron's, *Short Interest* is the number of shares shorted as a percentage of total shares outstanding. From CRSP, *Returns* is the raw percentage returns over the SEC event period. Significant mean differences (at the 10% two-tailed level) in variables between the control and Settlement firms, calculated using *t*-tests, are indicated with an * in the mean column of panel B.

	Mean (1)	Median (2)	1st Quartile (3)	3rd Quartile (4)
Panel A: Settlement firms				
Total Assets	17,883	896	250	7,986
Market Capitalization	21,355	1,105	393	10,855
Leverage	0.23	0.15	0.01	0.39
Return on Assets	−0.21	−0.08	−0.25	0.05
Growth	0.96	0.82	0.13	1.63
Book to Market	0.68	0.28	0.08	0.68
Recommendation Levels	1.72	1.62	1.50	1.87
Institutional Ownership	40.98	39.37	24.83	56.32
Number of Institutions	237.7	101	49	245
Institutional Ownership/Institution	0.381	0.293	0.168	0.575
Short Interest	2.17	1.57	0.61	3.43
Returns	−31.81	−33.80	−60.53	−12.75
Panel B: Control firms				
Total Assets	5,846	1,034	164	2,962
Market Capitalization	12,545	854	392	14,255
Leverage	0.25	0.22	0.01	0.39
Return on Assets	−0.27	0.04	−0.17	0.10
Growth	0.78	0.27	0.01	0.77
Book to Market	0.39	0.26	0.14	0.46
Recommendation Levels	1.73	1.67	1.52	1.94
Institutional Ownership	35.0	36.6	7.7	55.3
Number of Institutions	141.1*	62	29	160
Institutional Ownership/Institution	0.473	0.311	0.162	0.793
Short Interest	2.55	1.48	0.25	3.04
Returns	−2.86*	−19.10	−36.46	−5.34

pattern. Analogously, if analysts' misleading behavior results in a wealth transfer then there must be a relation between analysts and price changes. The literature on analysts establishes that information from a single analyst can affect market prices, and hence can influence investors.[13] The literature also shows that analyst recommendations, while having modest investment value (as evidenced by long-term returns drifting in the direction of the analyst recommendation), do not produce large enough returns to cover transaction costs, consistent with an efficient market (Womack [1996], Barber et al. [2001]).

In untabulated analysis, we examine the effect of analysts' recommendations on returns for the period after the stock market "bubble" burst (March, 2000), which is for the period that aligns most closely with our sample event periods. As expected, the recommendation-return relation is negative for Settlement firms. In contrast, for control firms long-term returns following strong buy and buy recommendations are positive. Following the advice of these "nonmisleading" analysts would have produced investment gains.[14] We conjecture that Settlement analysts realized that the true values of Settlement firms were lower than the respective market prices, but failed to update their public recommendations because of investment-banking pressures—hence the long-window negative returns. In support of this idea, additional untabulated analysis indicates that for Settlement firms, analysts at sanctioned banks were slower than other analysts to update their recommendations during the nine-month period prior to the start of the event period. This finding is consistent with the finding of O'Brien, McNichols, and Lin [2005] that affiliated analysts are slower to downgrade their recommendations. In sum, we find evidence that analysts' activities led to mispricing of Settlement stocks, a condition necessary for an analyst-caused wealth transfer to exist.

4. Tests of Changes in Institutional Holdings

This section presents our primary tests, which focus on the change in institutional holdings over the event periods established by the Settlement.

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[13] Analyst studies show that individual analyst disclosures produce short-window abnormal returns consistent with the direction and magnitude of the forecast, recommendation revision, and target price (Lys and Sohn [1990], Abarbanell [1991], Womack [1996], Francis and Soffer [1997], Brav and Lehavy [2003], Asquith, Mikhail, and Au [2005]).

[14] The untabulated analysis includes the following. For both Settlement and control firms, using all I/B/E/S analyst recommendations on these firms for the period October 1993 until December 2002, we calculate cumulative value-weighted market-adjusted returns for the five-day window centered on the recommendation date and daily long-term abnormal returns following the recommendation using a four-factor model (Barber et al. [2006]). In addition to the postbubble results, we also calculate prebubble results and show that there is little difference in the recommendation-return relation between Settlement and control firms, consistent with the literature (e.g., Barber et al. [2001]).

We expect institutional ownership to decline for our Settlement sample during these periods.

4.1 DATA AND METHOD

We obtain institutional holdings from Thomson Financial, which gathers this information from SEC institutional 13F filings. Institutions with more than $100 million in equity holdings must report their equity ownership to the SEC quarterly via 13F filings. Noninstitutional investors are those investors who do not file 13F documents with the SEC. This group includes individuals as well as more sophisticated traders such as short sellers, management, and other firm insiders. We expect sophisticated investors to access information sets and trade in a fashion similar to institutions. Their exclusion from the institutional holdings measures weakens the power of our tests.

Our principal measure of institutional holdings is the total ownership of all institutions as a percentage of a firm's total common shares outstanding (*Institutional Ownership*). We also decompose this measure into the number of institutions holding the stock (*Number of Institutions*) and the average percentage holdings per institution (*Institutional Ownership/Institution*). Table 2 presents descriptive statistics on the levels of these variables, measured before the event period. Institutional ownership relative to shares outstanding and per institution is similar across the Settlement and control groups. We examine the change of each measure over time and report *t*-statistics that test whether the change differs from zero. We compute *p*-values using a bootstrap method (e.g., Yatchew [2003]).[15]

We use two types of event windows. The first approach, the "variable-length" window, matches the event window as closely as possible to the event period defined by the Settlement. This matching process is somewhat coarse because Settlement event periods tend to neither begin nor end on a calendar quarter basis. The start (end) of the variable-length event period is the last (first) calendar quarter end before (after) the first (last) day of the event period as specifically defined in the Settlement. Hence, variable-length event windows are always longer than those listed in the Settlement. The second approach assumes common "fixed-length" event windows. The

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[15] Given the small number of observations in our sample, the test based on the bootstrap method is more reliable than the parametric *t*-statistic. Results and inferences, however, are not affected if we use the *p*-values based on parametric tests. Each bootstrap *p*-value is computed using simulations (randomly drawing data with replacement) to construct the empirical distribution of the *t*-statistic, which is asymptotically standard normal. First, using the original sample (i.e., $n = 50$ observations) we compute the residuals $\varepsilon = \mathrm{y} - \mathrm{mean}(\mathrm{y})$. T is the *t*-statistic for this original sample $(\mathrm{mean}(\mathrm{y}) * n^{1/2}/\mathrm{standard\ deviation}(\varepsilon))$. We then take 20,000 repeated samples out of the residual pool. For each sample $B = 1 \ldots 20{,}000$, we obtain the bootstrap data $\varepsilon_i\,(B)$, $i = 1 \ldots \mathrm{n}$ and compute the *t*-statistic $(B) = n^{1/2} * \mathrm{mean}(\varepsilon_i(B))/\mathrm{standard\ deviation}(\varepsilon_i(B))$. Statistical significance (two-sided test H_0: sample mean = 0) is computed as: *p*-value = minimum$(N_1/20{,}000;\ N_2/20{,}000)$ where N_1 = number of times that *t*-statistic $(B) \geq T$ and N_2 = number of times that *t*-statistic $(B) \leq T$.

start of the event window is still the last calendar quarter ending before the first day of the Settlement period; however, the end is fixed at one, two, and three calendar quarters from the first day. The advantage is that we do not rely on the ending date of the Settlement, which is somewhat arbitrary. For example, 25 of the 58 firm-event periods listed in the Settlement are exactly 90 or 91 days long. It is possible that the differential-trading behavior of individuals and institutions ends prior to, or after, the Settlement-defined event period end date.

4.2 RESULTS

Table 3 presents the results of our analysis of changes in institutional holdings. Each panel presents the results of the same tests but for different event windows. Column 1 of panel A shows that institutional holdings for the Settlement firms decrease by 4.8% over the variable-length event period, consistent with our predictions. The average number of institutional investors also decreases by 21. The changes for these two variables are statistically significant. We find no evidence that ownership per institution changes. Column 2 of panel A shows that for control firms, none of the three measures suggest that institutional holdings change over the Settlement period. The third column shows that the matched-pair difference between the Settlement and control firms produces similar results to those of column 1.

Panels B to D present results using fixed-length event windows. Although there is no evidence of changes in institutional ownership in the one-quarter window, there is evidence of decreases in institutional holdings in the two- and three-quarter windows, which reinforces the results from our variable-length event-window analysis in panel A. Our lack of results for the one-quarter window is consistent with a lack of power to detect the changing trend. Recall that our mapping from the period specifically defined in the Settlement to the empirical window is imprecise and that only part of the Settlement period overlaps with the one-quarter event window. The institutional investor change results for the control sample, and for the difference between the Settlement and control samples for the fixed-length event windows, are consistent with the variable-length event-window analysis.

In summary, the evidence is consistent with our predicted effect of misleading behavior by analysts. The decrease in institutional ownership over the Settlement event period is mainly attributable to decreases in the number of institutions holding the stock rather than to decreases in the ownership per institution. This suggests that analysts disclosed their private information only to select institutions. These findings are less consistent with the idea that institutions mechanically sold for nonanalyst reasons, in which case we would have expected a more broad-based reaction by all institutions.

5. Tests of Detailed Large- and Small-Size Trading

In this section, we present additional analysis of the behavior of institutions and individuals using detailed trade and quote information from

TABLE 3

Changes in Institutional Ownership

This table presents an analysis of changes in Settlement and control firms' institutional ownership over the event period defined by the Settlement. Panel A computes the mean change from the last calendar quarter before the first day of the event period to the first calendar quarter after the last day of the event period. Panels B, C, and D compute the mean change from the last calendar quarter before the first day of the event period to the first, second, and third calendar quarter from the start of the event period. *Settlement* refers to the Settlement firms and *Control* refers to the matched-control sample. The sample consists of 50 firm-event observations for each group. Δ*Institutional Ownership* is the change in the percentage of total common stock held by institutional shareholders, Δ*Number of Institutions* is the change in the number of institutions owning the stock, and Δ*Institutional Ownership/Institution* is the change in the percentage of total common stock held by institutional shareholders divided by the number of institutions owning the stock. *t*-statistics are reported in parentheses. Statistical significance is based on bootstrapped *p*-values. *, **, and *** denote significance at the 10%, 5%, and 1% (two-tailed) levels, respectively.

	Settlement (1)	*Control* (2)	(*Settlement* – *Control*) (3)
Panel A: Variable-length event window			
Δ*Institutional Ownership*	−4.79	1.49	−6.28
	(−2.08)**	(1.24)	(−2.62)***
Δ*Number of Institutions*	−21.24	2.06	−23.30
	(−2.59)***	(0.30)	(−3.23)***
Δ*Institutional Ownership/Institution*	−0.023	0.006	−0.03
	(−1.27)	(0.19)	(−0.81)
Panel B: One-quarter event window			
Δ*Institutional Ownership*	−0.05	1.34	−1.39
	(−0.05)	(1.33)	(−0.96)
Δ*Number of Institutions*	−7.10	1.38	−8.48
	(−1.17)	(0.48)	(−1.42)
Δ*Institutional Ownership/ Institution*	−0.001	−0.02	0.02
	(−0.01)	(−0.91)	(0.69)
Panel C: Two-quarter event window			
Δ*Institutional Ownership*	−4.17	2.09	−6.26
	(−1.93)**	(1.55)*	(−2.69)***
Δ*Number of Institutions*	−16.9	1.74	−18.62
	(−2.34)**	(0.39)	(−2.75)***
Δ*Institutional Ownership/Institution*	−0.037	0.02	0.06
	(−2.77)***	(0.71)	(−1.91)**
Panel D: Three-quarter event window			
Δ*Institutional Ownership*	−5.60	0.88	−6.47
	(−2.17)**	(0.62)	(−2.35)**
Δ*Number of Institutions*	−31.14	−2.82	−28.32
	(−3.27)***	(−0.49)	(−3.08)***
Δ*Institutional Ownership/Institution*	−0.032	0.02	−0.05
	(−1.83)*	(0.43)	(−1.28)

the NYSE TAQ database. Although these TAQ-based variables contain more measurement error than institutional holdings, they provide an independent test of whether individuals are buying more shares of the Settlement firms, relative to institutions. In addition, these measures allow us to analyze separately the effects of trades by larger and smaller investors.

5.1 DATA AND METHOD

We continue to adopt both variable- and fixed-length event-window approaches. We take advantage of daily measures and let the variable-length window start and stop on the same days as those defined in the Settlement. The fixed-length window starts on the start day of the Settlement period. The basic intuition underlying our trading volume measures is that individuals (institutions) are more likely to trade using small- (large-)size trades. We define our TAQ-based trading measures as follows. First, we assign all trades into three groups based on the number of shares traded. The advantage of using cutoffs based on shares traded as opposed to trade dollar values (e.g., Barclay, Hendershott, and McCormick [2003]) is that these cutoffs are independent of the stock price level, thereby improving cross-sectional comparability. This is particularly important for our study because the sample firms demonstrate wide variation in stock prices. Trades of less than 1,000 shares are considered small and trades greater than 10,000 shares are considered large. Consistent with the literature, we exclude trades between these two cutoffs because these trades are more difficult to attribute to individuals or institutions. Lee and Radhakrishna [2000] show that eliminating medium-size trades increases the ability of the measures to identify individual from institutional trading.

Second, we use the Lee and Ready [1991] algorithm to classify trades as buyer initiated or seller initiated.[16] We define a net buying (*NetBuy*) measure as the difference between buyer- and seller-initiated trades. Our measures of small and large net buying are defined as follows:

$$Small\ NetBuy_{it} = \frac{Small\ Buy_{it} - Small\ Sell_{it}}{Small\ Buy_{it} + Small\ Sell_{it} + Large\ Buy_{it} + Large\ Sell_{it}} \quad \text{and} \tag{1}$$

$$Large\ NetBuy_{it} = \frac{Large\ Buy_{it} - Large\ Sell_{it}}{Small\ Buy_{it} + Small\ Sell_{it} + Large\ Buy_{it} + Large\ Sell_{it}}, \tag{2}$$

where *Small* (*Large*) Buy_{it} is the total number of firm-i buyer-initiated shares traded on day t classified as small- (large-)size trades, and *Small* (*Large*) $Sell_{it}$ is the total number of firm-i seller-initiated shares traded on day t classified as small- (large-)size trades. We define a third measure as the difference between (1) and (2) to summarize how individuals' trading behavior differs from that of institutions:

$$NetBuy\ Difference_{it} = \frac{(Small\ Buy_{it} - Small\ Sell_{it}) - (Large\ Buy_{it} - Large\ Sell_{it})}{Small\ Buy_{it} + Small\ Sell_{it} + Large\ Buy_{it} + Large\ Sell_{it}}. \tag{3}$$

As an additional benchmark, we estimate the net buying measures for individuals and institutions for the control firms and report differences

[16] Studies that validate this algorithm include Ellis, Michaely, and O'Hara [2000] and Odders-White [2000].

between the two samples. We present *t*-statistics using bootstrapped (two-tailed) *p*-values.

These TAQ-based variables suffer from measurement error and bias. First, our cutoffs are coarse. To the extent that our cutoffs fail to identify the type of investor and there is no systematic pattern in the measurement error, the power of our tests is reduced. As an example, brokers could aggregate individual trades into a large-block order, causing measurement error; but we do not expect that this practice occurs systematically in one direction, thereby introducing bias. Second, building on the theoretical intuition that investors with private information will likely trade gradually over time in order to profit before their trades fully reveal the information (e.g., Kyle [1985], Admati and Pfleiderer [1988]), Barclay and Warner [1993] and Chakravarty [2001] find that institutions are responsible for informed trading and this trading is concentrated in the medium-size category. In the context of this study, institutions would likely mask their informed selling by breaking sell orders into smaller lots. Thus, the probability of having a large sell order is smaller than the probability of having a large buy order (e.g., Harris [2003]). As a result, some sell orders of institutions are more likely to be misclassified in the middle- and small-size trade categories. Therefore, the *Large NetBuy* measure is positively biased while the *Small NetBuy* measure is negatively biased (i.e., true institutional and individual net buying is more negative and positive, respectively), which works against us.

We also investigate, but do not report, a number of alternative algorithms to determine whether the trade was seller or buyer initiated and whether an individual or an institution placed the order. First, we run our tests using the Ellis, Michaely, and O'Hara [2000] algorithm to distinguish between buyer- and seller-initiated trades. Their algorithm better classifies trades between the quotes than Lee and Ready [1991]. Second, we employ a large number of different cutoffs that have been used by extant literature. These cutoffs can be classified into three groups: (1) number of shares traded, (2) value of shares traded, and (3) a combination of both number and value of shares traded.[17] In general, the results from these alternative cutoffs are similar to those that we report.

5.2 RESULTS

Table 4 presents the results of using the TAQ-based variables to measure the trading behavior of individuals and institutions over the variable-length

[17] See, for example, Bhattacharya [2001], Lee and Radhakrishna [2000], Barclay, Hendershott, and McCormick [2003], Bhattacharya et al. [2004], Mikhail, Walther, and Willis [2005], and Malmendier and Shanthikumar [2004, 2005]. The actual trade-size cutoffs to estimate individual/institutional trading that we investigate, which take the form of (less than/more than) are as follows: (500 shares/1,000 shares), (500 shares/10,000 shares), ($7,000/$30,000), ($5,000/$35,000), ($10,000/$10,000), ($10,000 and 800 shares/$30,000), ($7,000 and 500 shares/$30,000), and ($5,000 and 500 shares/$30,000).

TABLE 4

TAQ-Based Analysis of Trading over Variable-Length Event Window

This table presents an analysis of trading by small-size and large-size traders over the event period defined by the Settlement. *Small NetBuy* is the difference between daily volumes of small-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *Large NetBuy* is the difference between daily volumes of large-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *NetBuy Difference* is *Small NetBuy* less *Large NetBuy*. *N* is the number of observations. Panel A presents mean measures based on daily pooled observations. Panel B presents mean results based on one observation (time-series average) per firm event. *Settlement* refers to the Settlement firms and *Control* refers to the matched-control sample. *t*-statistics are reported in parentheses. In panel B, statistical significance is based on bootstrapped *p*-values. *, **, and *** denote significance at the 10%, 5%, and 1% (two-tailed) levels, respectively.

	Settlement (1)	*Control* (2)	(*Settlement* – *Control*) (3)
Panel A: Daily observations			
Small NetBuy	0.013	−0.008	0.021
	(5.87)***	(−2.10)**	(4.84)***
Large NetBuy	−0.004	−0.005	0.002
	(−1.26)	(−1.72)*	(0.35)
NetBuy Difference	0.017	−0.003	0.020
	(4.43)***	(−0.54)	(3.15)***
N	4,606	4,600	4,600
Panel B: Firm-event observations			
Small NetBuy	0.019	−0.017	0.036
	(2.72)**	(−1.64)*	(2.92)***
Large NetBuy	−0.014	−0.011	−0.003
	(−1.28)	(−1.45)	(−0.24)
NetBuy Difference	0.033	−0.007	0.040
	(3.13)***	(−0.56)	(2.45)**
N	50	50	50

event window. Panel A presents the mean value of *Small NetBuy* and *Large NetBuy* measures in which each trading day during the sample firms' Settlement period is one observation. The sample consists of 4,606 pooled daily observations. Firms with longer Settlement periods have more weight in this analysis. The first column presents results for the Settlement firms. The positive and statistically significant value of the *Small NetBuy* mean for Settlement firms shows that individuals were generally buying during the event period, consistent with individuals naively following the misleading recommendations of analysts. The mean of the *Large NetBuy* measure, while negative, is not statistically significant and fails to provide evidence that institutions were buying or selling trades during the period. The difference between these two measures is significantly different from zero, consistent with our expectations. The second column shows the results of the same tests for the control sample. There is some evidence that both individuals and institutions for these firms were generally selling shares, although the difference between the two is not statistically significant. The third column shows the matched-pair difference between the Settlement and control samples. The

TABLE 5

TAQ-Based Analysis of Trading over Fixed-Length Event Windows

This table presents an analysis of trading by small-size and large-size traders over monthly periods relative to the first day of the Settlement event period. *Small NetBuy* is the difference between daily volumes of small-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *Large NetBuy* is the difference between daily volumes of large-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *NetBuy Difference* is *Small NetBuy* less *Large NetBuy*. *N* is the number of daily firm-event observations pooled each month. *Settlement* refers to the Settlement firms and *Control* refers to the matched-control sample. *t*-statistics are reported in parentheses. Statistical significance is based on bootstrapped *p*-values. *, **, and *** denote significance at the 10%, 5%, and 1% (two-tailed) levels, respectively.

	Settlement			*Control*	(*Settlement* – *Control*)
	Small NetBuy (1)	*Large NetBuy* (2)	*NetBuy Difference* (3)	*NetBuy Difference* (4)	*NetBuy Difference* (5)
Pre-event window					
3 months before starting	0.013	−0.002	0.014	−0.023	0.037
day ($N = 1{,}006$)	(2.63)***	(−0.27)	(1.88)*	(−2.11)**	(2.85)***
2 months before starting	0.010	−0.007	0.018	−0.034	0.052
day ($N = 1{,}000$)	(2.19)**	(−1.13)	(2.23)**	(−3.04)***	(3.99)***
1 month before starting	0.016	0.008	0.007	−0.032	0.039
day ($N = 1{,}033$)	(3.40)***	(1.32)	(0.96)	(−2.97)***	(2.97)***
Event window					
1 month after starting day	0.017	−0.017	0.034	−0.014	0.048
($N = 1{,}059$)	(3.89)***	(−2.57)**	(4.26)***	(−1.30)	(3.61)***
2 months after starting	0.014	−0.013	0.027	−0.018	0.038
day ($N = 1{,}003$)	(2.35)**	(−2.07)**	(2.58)***	(−1.56)	(2.85)***
3 months after starting	0.013	−0.012	0.024	0.006	0.017
day ($N = 1{,}004$)	(3.67)***	(−1.87)*	(2.99)***	(0.55)	(1.26)
4 months after starting	0.009	0.006	0.003	0.000	0.003
day ($N = 1{,}016$)	(1.75)*	(0.99)	(0.30)	(−0.02)	(0.20)
5 months after starting	0.016	−0.003	0.019	−0.033	0.052
day ($N = 926$)	(3.22)***	(−0.47)	(2.36)**	(−2.83)***	(3.67)***
6 months after starting	0.017	−0.011	0.028	−0.007	0.035
day ($N = 944$)	(3.73)***	(−1.61)*	(3.42)***	(−0.64)	(2.67)***

result shows that, relative to the control group, individuals in Settlement firms were net buyers relative to institutions. Panel B shows the same tests but aggregates the net buying trading measures over time per firm-event to create one measure (the time-series mean) for each firm-event observation. This measure weights each firm-event equally in the analysis. The results are consistent with the inferences in panel A.

Table 5 shows the results of the analysis in which we consider fixed-length monthly event windows starting three months before, and ending six months after, the first day of the Settlement period (i.e., day 0) for the pooled firm-day sample. We stop our analysis at six months because as panel B of table 1 shows, 76% of the observations have event windows that last six months or less. Each row in table 5 shows a different month and there is no overlap in the periods. The first, second, and third columns show the mean *Small NetBuy*, *Large NetBuy*, and *NetBuy Difference*, respectively, for the Settlement firms. For parsimony, we tabulate results only for the summary measure *NetBuy Difference* for the control and Settlement-less-control samples. This

monthly analysis produces results consistent with the variable-length window analysis above. For all six months of the event period, Settlement firms' *Small NetBuy* is positive and statistically significant, and for five months, Settlement firms' *NetBuy Difference* is positive and significant. Here we see evidence of institutional selling. In each of the first three months of the event period, Settlement firms' *Large NetBuy* is negative and significant. In the pre-event period, the *NetBuy Difference* is also positive and statistically significant in two of the three months, driven by the positive values of *Small NetBuy*, suggesting individuals followed the positive analyst recommendations that were issued before the event period started. In summary, the results of this TAQ analysis provide additional evidence consistent with our hypothesis that analysts' misleading behavior leads to a wealth transfer. In particular, the results suggest that individuals were following the public advice of misleading analysts while institutions were selling at the same time.

6. *Economic Consequence of Differential Trading Behavior*

In this section, we incorporate stock returns into our TAQ-based analysis. We first graphically examine the trading behavior of individuals and institutions as well as the stock returns over time. Then, in conjunction with various assumptions about holding periods, we present estimated dollar profits of individuals and institutions in order to gauge the economic magnitude of the wealth transfer.

6.1 TIMING OF TRADES

We examine the timing of individual and institutional trading as further evidence of a wealth transfer between the two groups. Figure 1 plots the cumulative individual net buying, cumulative institutional net buying, and the difference of these two for our Settlement firms starting about three months prior to day 0 (60 trading days) and ending six months after day 0 (120 trading days), where day 0 is the first day of each firm's event period as defined in the Settlement. The figure also plots the cumulative abnormal returns for the equal-weighted portfolio of sample firms. Abnormal returns are defined as cumulative raw returns less the cumulative value-weighted market return (NYSE/AMEX/NASDAQ).[18]

We discuss three distinct periods. In the pre-event period (day −60 to day −1), individuals consistently buy more than they sell, while institutions on average buy and sell in approximately equal amounts. Cumulative abnormal returns are −11.5% during this period. In the first part of the event period (day 0 to +60), individuals' net buying persists even though returns decline more rapidly (cumulative abnormal returns are −15.7%). Institutions seem astute in the timing of their trades in Settlement stocks. They start to sell more shares than they buy at the beginning of the event period, consistent

[18] The analyses in this section are robust to using NASDAQ index-adjusted abnormal returns.

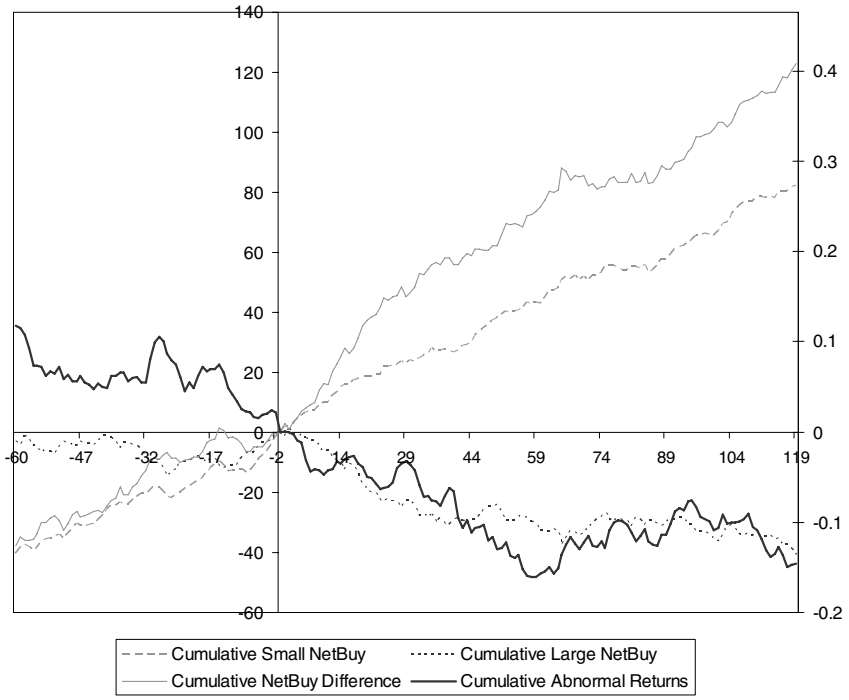

FIG. 1.—Time-series of stock returns and individual and institutional investors' net buying. This figure plots the cumulative small-size trades, cumulative large-size trades, and the cumulative difference of these two groups for the Settlement firms starting from three months prior to day 0 (60 trading days) and ending six months after day 0 (120 trading days). Day 0 is the first day of the Settlement period. *Small NetBuy* is the difference between daily volumes of small-size buy and sell trades scaled by the total number of shares traded by small and large traders that day. *Large NetBuy* is the difference between daily volumes of large-size buy and sell trades scaled by the total number of shares traded by small and large traders that day. *NetBuy Difference* is *Small NetBuy* less *Large NetBuy*. The figure also plots the cumulative abnormal returns for the equally weighted portfolio of Settlement firms, where abnormal returns are raw returns less the value-weighted market return. The first labeled vertical axis refers to cumulative net buying and the second labeled vertical axis refers to abnormal returns.

with analysts affecting institutional trading. Hence, during the first three months of the event period, the graphical evidence suggests a wealth transfer from institutions to individuals. This pattern appears to be driven mostly by individual buying and in small part by institutional selling. In the second part of the event period (day +61 to +120), individuals continue to (net) buy the Settlement stocks. Institutions are neither net buyers nor net sellers during this period. Returns, however, are more volatile, and hence there is less evidence of a wealth transfer between investors.

6.2 TRADING DOLLAR PROFITS

We develop a measure that proxies for the dollar profits of individuals and institutions based on the amount of daily net buying classified by trade

size and the closing stock price on each day. We calculate a measure of the economic gain (*Gain*) separately for small- and large-size trade groups and for each Settlement firm. The measure is defined as follows:

$$Gain = \sum_{t=1}^{N} (Buy_t - Sell_t)\,(P_N - P_t), \tag{4}$$

where Buy_t ($Sell_t$) is the total number of buyer- (seller-) initiated shares traded on day t that fall in the trade size category, N is the last day of the Settlement event window, and P_N and P_t is the closing stock price for the firm on date N and day t, respectively. Our measure is consistent with each investor group taking a trading position based on the net buys or sells, and holding this position until the end of the event period. This trading position can be long or short in the stock. Selling or buying with similar order size any time in the event period has no effect on the measured economic gain over the event period.

An alternative and equivalent interpretation of our measure is that each investor group develops a trading position in the security, based on whether they are net buyers or sellers each day. On day 0, the investor group starts with a zero trading position and each day the position is adjusted up or down based on whether the group is a net buyer or seller that day. The dollar gains of the trading position on day t are calculated based on the trading position on day $t-1$ multiplied by the change in price from day t to day $t-1$. *Gain* represents the aggregate dollar profit on this dynamic trading position, calculated until the event period ends.[19]

We define *Total Gain* as the sum of *Gain* for all firms and calculate it separately for each investor group. This measure represents the amount lost or gained by active traders in each investor group. For statistical tests, we define *Gain** as *Gain* scaled by the firm's market capitalization to facilitate comparisons across firms. We report the mean *Gain** for each investor group and test whether it differs from zero.

To account for the opportunity costs of investors, we also present results using a similar measure that adjusts for market returns:

$$Mkt\text{-}Adj\ Gain = \sum_{t=1}^{N} (Buy_{it} - Sell_{it})\,(P_N - P_t(1 + MktRet_{t,N})), \tag{5}$$

where *MktRet* is the value-weighted market return. The market was quite volatile during the event period and realized mostly negative returns, which could affect our calculation of *Gain* for each investor group.

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[19] To illustrate our calculation, consider an event period that is four days long, a single investor, and closing prices on day 1, 2, 3, and 4 of \$5, \$6, \$6.50, and \$7, respectively. On the first day, the investor buys 10 shares, on the second day the investor sells eight shares, and on the third day the investor buys three shares. Trades on the last day are ignored because $t = N$ on the last day so $P_N - P_t = 0$. According to our calculation, $Gain = 10 \times 2 - 8 \times 1 + 3 \times 0.5 = 13.50$. One may also calculate the investor's dollar profit on a daily basis based on the investor's daily portfolio, which would lead to an economic gain of $10 \times 1 + 2 \times 0.5 + 5 \times 0.5 = 13.50$, an equivalent amount to our calculation.

TABLE 6

Economic Gain for Small- and Large-Size Traders

This table presents a summary of the trading profit analysis for small-size and large-size traders over the event period specifically defined by the Settlement. Panel A presents the results using raw returns. The dollar profit is $Gain = \sum_{t=1}^{N} (Buy_t - Sell_t)(P_N - P_t)$, where P_t is the closing stock price for the firm on date t, P_N is the closing stock price for the firm at the ending date of the empirical event window, and N is the last day of the event window. Buy_t ($Sell_t$) is the total number of buy-initiated (sell-initiated) shares traded on day t that fall in the trade size category consistent with the investor-group classifications. Column 1 presents *Total Gain* ($million), which is the sum of *Gain* for all firms in the investor category. Column 2 presents the mean of *Gain** (in dollars), which equals each firms' *Gain* scaled by the firm's market capitalization ($million). Column 3 presents *t*-statistics that test whether mean *Gain** differs from zero. For each event period, results are presented for small-size and large-size traders and *t*-statistics for the difference between the mean *Gain** of the two groups. Event periods examined include the Settlement period and the first, second, and third months following the first day of the Settlement period. Columns 4 to 6 present results using market-adjusted returns. $Mkt\text{-}Adj\ Gain = \sum_{t=1}^{N} (Buy_{it} - Sell_{it})(P_N - P_t(1 + MktRet_{t,N}))$ where *MktRet* is the value-weighted market return. Statistical significance is based on bootstrapped *p*-values. * and *** denote significance at the 10% and 1% (two-tailed) levels, respectively.

		Raw Returns			Market-Adjusted Returns		
Event Window	Trade Size	*Total Gain* (1)	Mean *Gain** (2)	*t*-statistic (3)	*Total Gain* (4)	Mean *Gain** (5)	*t*-statistic (6)
Variable-length	Small	−2,232	−10,173	(−3.81)***	−2,025	−9,459	(−3.79)***
($N = 50$)	Large	−845	1,021	(0.32)	−762	1,012	(0.35)
	t-statistic		(−3.22)***			(−3.22)***	
First month	Small	−1,115	−5,653	(−3.09)***	−1,043	−5,273	(−3.10)***
($N = 50$)	Large	−539	1,158	(1.00)	−520	897	(0.84)
	t-statistic		(2.80)***			(−2.73)***	
Second month	Small	−605	−2,030	(−1.95)*	−501	−1,805	(−1.87)*
($N = 47$)	Large	−44	1,723	(0.72)	−79	1,884	(0.82)
	t-statistic		(−1.74)*			(−1.74)*	
Third month	Small	−256	−1,345	(−1.74)*	−231	−1,255	(−1.62)*
($N = 45$)	Large	−115	2,006	(1.02)	−80	1,967	(1.09)
	t-statistic		(−1.60)			(−1.64)*	

We again use two event-period approaches—variable-length and fixed-length monthly windows. The first, second, and third month are calculated from day 0 to day 30, day 31 to day 60, and day 61 to day 90, respectively. We focus on these three months to be consistent with our TAQ analysis above that shows that analysts' misleading behavior potentially had the strongest effect on the wealth transfer during this period. We continue to report *t*-statistics using bootstrapped (two-tailed) *p*-values.

We expect to observe an economic loss (i.e., negative values of *Gain*) for both institutions and individuals as the majority of our Settlement firms experience a decrease in their stock price over the event period. The emphasis, however, is on whether actively trading individuals lose more than actively trading institutions. Table 6 reports a summary of the trading-profit analysis for both small- and large-size trade groups. We first discuss the raw return analysis (columns 1–3). The first three rows present the results for the variable-length event window. *Total Gain* is negative for both groups, although investors with small-size trades lose more money. The total loss for small-size traders over the event period is estimated to be $2.2 billion, while

the loss for large-size traders is only $845 million. This result is not simply due to small-size traders trading more than large-size-traders, because untabulated analysis indicates that large-size traders on average trade more than small-size traders. The mean *Gain** by small-size traders is negative and significantly different from zero. Regardless of how the event window is defined, we note that mean *Gain** for small-size traders (large-size-traders) is always negative (positive). More importantly, the mean loss by small-size-traders is significantly greater in magnitude than for large-size traders and the results are robust to controls for market-wide stock movements (columns 4–6). Overall, the results in this section provide more direct support that small-size traders suffer greater losses than large-size traders and that the wealth transfer is driven by individual trading.

7. *Short-Interest Analysis*

In this section, we examine changes in Settlement firms' short interest and predict that it will be greater during the event period, consistent with short-sale investors taking advantage of analysts' misleading behavior. Short sellers (e.g., hedge funds) represent sophisticated investors who do not file 13Fs (unless they hold large stock portfolios). Hedge funds for the most part "view analysts as a waste of time" (Davis 2004b), which supports the idea that these sophisticated investors can independently determine the true value of the stock. Hedge funds, however, can have strong relations with sell-side analysts since they vote along with mutual and pension fund institutions to determine which analysts are placed on the *Institutional Investor* All-Star Team (Dini [2002]). Analysts have strong incentives to cater to hedge funds because they represent the most active traders and, more importantly, the most lucrative trading clients (Davis [2004a, 2004b]). This suggests that if analysts had negative opinions of a stock then they would privately inform a hedge-fund client. There is also no reason why other types of short sellers cannot talk to sell-side analysts. In either case, we predict that short interest will increase.

7.1 DATA METHOD

We collect data on the number of shares sold short for both our Settlement and control firms from Bloomberg and Barron's. These data are issued on a monthly basis by NYSE and NASDAQ. We find data for 39 Settlement and 31 control firm-events. The missing data are not surprising given the difficulty of obtaining short-sales data for the substantial number of firms in our sample that trade over-the-counter. Consistent with the literature (e.g., Asquith, Pathak, and Ritter [2005]), we define each firm's *Short Interest* as shares sold short, divided by the total number of common shares outstanding. We develop a quarterly measure per firm by taking the mean of the three respective monthly measures. Our analysis focuses on changes in Settlement and control firms' quarterly *Short Interest* (Δ*Short Interest*). Table 2 reports descriptive statistics for the level of *Short Interest* for the Settlement and control firms measured in the quarter prior to the beginning of the

TABLE 7

Short Interest Analysis

This table presents an analysis of changes in Settlement and control firms' short interest over the event period specifically defined by the Settlement. The first row computes the mean change from the last calendar quarter before the first day of the event period to the first calendar quarter after the last day of the event period. The following rows compute the change from the last calendar quarter before the first day of the event period to the first, second, and third calendar quarter, respectively, after the start of the event period. *Settlement* refers to the Settlement firms and *Control* refers to the matched-control sample. The sample consists of 39 firm-event observations for the Settlement group and 31 firm-event observations for the control group. Each firm's quarterly *Short Interest* is computed as the mean of the three monthly measures of the firm's shares sold short scaled by the total number of shares outstanding. *t*-statistics are reported in parentheses. Statistical significance is based on bootstrapped *p*-values. ** and *** denote significance at the 5% and 1% (two-tailed) levels, respectively.

Event Window	*Settlement* (1)	*Control* (2)	(*Settlement* – *Control*) (3)
Variable-length	0.0172	0.0005	0.0149
	(3.05)***	(0.14)	(2.49)**
One-quarter	0.0164	0.0018	0.0125
	(3.06)***	(0.55)	(2.37)**
Two-quarter	0.0225	0.0054	0.0167
	(2.89)***	(1.25)	(2.33)**
Three-quarter	0.0246	0.0044	0.0227
	(3.40)***	(0.98)	(3.34)***
N	39	31	31

Settlement event period. The difference in levels between the two samples is not statistically significant.

7.2 RESULTS

Table 7 shows that *Short Interest* for the Settlement firms increased during the event period, regardless of event period definition, and that these changes are statistically significant. In contrast, we do not find any significant changes for our control firms. Over the variable-length window, the mean percentage of shares sold short for the Settlement firms increases by 1.72% of shares outstanding, while there is no evidence of any change for control firms. All differences between Settlement and control firms are statistically significant. Thus, our results are robust to controlling for exchange listing, size, industry, and consensus recommendation levels. These results support our previous conclusions by showing that sophisticated investors, more broadly defined than just institutions, traded in the "right direction." Also, because these short sellers do not typically file 13Fs, our institutional-ownership tests (which are based on 13F holding changes) will understate the negative effect of analysts' misleading behavior on individuals.[20]

[20] It is possible that short selling costs are higher for control firms than for Settlement firms. However, the fact that we compare differences relative to the quarter prior to the event period mitigates this concern.

8. *Additional Analysis of the Mechanism by Which Analysts' Behavior Transferred Wealth*

Our objective in this section is to tighten the link between analysts' misleading behavior and the wealth transfer from individuals to institutions. We investigate the extent to which our full-sample results hold after conditioning on select control variables, either via regression or sample partitions.

8.1 REGRESSION ANALYSIS INCORPORATING RETURNS AND SHORT SALES

As with any univariate analysis, there could be potential factors affecting investor net buying that we do not measure. To the extent that these factors affect both the Settlement and control firms, we can rely on the difference between the two groups as a means to test our prediction. Returns for the Settlement firms, however, are more negative than those for the control group (see table 2) and the literature shows that institutional-investor trading behavior is correlated positively with returns. It is also possible that institutional-investor changes are a result of institutions simply following short sellers.

To investigate these alternative explanations, using the pooled sample of Settlement and control firms, we regress changes in our institutional-holding measures on a dummy variable indicating whether the firm is a Settlement firm, as well as on pre-event returns, event returns, and changes in short sales. Our regression is:

$$\Delta\, \textit{Institutional Ownership} = \beta_0 + \beta_1\, \textit{Settlement Firm} + \beta_2\, \textit{Current Period Returns} + \beta_3\, \textit{Pre-Event Returns} + \beta_4\, \Delta\, \textit{Short Interest} + \varepsilon. \tag{6}$$

Δ*Institutional Ownership* and Δ*Short Interest* are calculated in the same manner and using the same fixed-length calendar windows. *Settlement Firm* is an indicator variable that takes the value of one if the firm is in the Settlement group and zero otherwise. *Current Period Returns* are abnormal returns (raw returns adjusted by the value-weighted index) cumulated over the same calendar quarters used to calculate Δ*Institutional Ownership*. *Pre-Event Returns* are abnormal returns cumulated over the calendar quarter just prior to the start of the event period. Statistical significance is based on *p*-values calculated using a bootstrap method.

Table 8, panel A shows that the event and pre-event return coefficients are positive and statistically significant, consistent with the findings in the literature. We do not generally find a statistically significant coefficient on the change in short sales.[21] More importantly, we find that the

[21] This result holds in a regression excluding returns. Asquith, Pathak, and Ritter [2005] document a positive relation between short sales and institutional holding changes in a large-sample analysis, consistent with institutional ownership proxying for the "supply" of shares.

TABLE 8

Institutional Holdings Regression Analysis

This table presents an analysis that investigates the effect of returns and short sales as an alternative explanation for the negative changes in institutional holdings over the event period. Using the combined sample of Settlement and control firms, panel A presents the results of estimating the following regression:

$$\Delta Institutional\ Ownership = \beta_0 + \beta_1 Settlement\ Firm + \beta_2 Current\ Period\ Returns + \beta_3 Pre\text{-}Event\ Returns + \beta_4 \Delta Short\ Interest + \varepsilon.$$

Δ*Institutional Ownership* is the change in the percentage of total common stock held by institutional shareholders and is calculated using fixed-length (one-, two-, and three-quarter) calendar windows. *Settlement Firm* is an indicator variable that takes the value one if the firm is a Settlement firm and zero otherwise. *Current Period Returns* are abnormal returns cumulated over the same calendar quarters used to calculate Δ*Institutional Ownership*. *Pre-Event Returns* are abnormal returns cumulated over the calendar quarter just prior to the start of the event period. Abnormal returns are computed by adjusting firms' raw returns by the value-weighted market index returns. Δ*Short Interest* is the change in *Short Interest*, which is the number of shares sold short divided by the total number of common shares outstanding. Panel B shows the results of the same regression using Δ*Number of Institutions* as the dependent variable. Δ*Number of Institutions* is the change in the number of institutions owning the stock. *t*-statistics are reported in parentheses and are based on bootstrapped standard errors. *, **, and *** denote significance at the 10%, 5%, and 1% (two-tailed) levels, respectively

	Event Window					
	One Quarter		Two Quarter		Three Quarter	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dependent variable = Δ*Institutional Ownership*						
Intercept	1.67	2.52	3.95	3.89	2.35	2.60
	(1.61)*	(1.70)*	(2.81)***	(1.96)**	(1.61)*	(1.28)
Settlement Firm	−0.10	−2.25	−5.59	−6.18	−3.74	−4.20
	(−0.08)	(−1.42)	(−2.46)**	(−2.04)**	(−1.57)	(−1.65)*
Current Period Returns	5.22	4.03	7.47	7.27	5.17	3.73
	(2.35)**	(1.84)*	(3.48)***	(2.40)**	(3.18)***	(1.53)
Pre-Event Returns	4.29	5.21	7.49	6.91	9.03	7.97
	(3.17)***	(3.33)***	(2.72)***	(1.88)*	(2.58)**	(1.65)*
Δ*Short Interest*	–	0.25	–	−0.49	–	0.10
		(0.61)		(−0.49)		(0.13)
Adj. R^2	15.81	20.14	24.75	18.58	22.43	13.42
N	100	70	99	68	97	65

Panel B: Dependent variable = Δ*Number of Institutions*						
Intercept	3.76	4.23	6.44	9.31	4.12	4.88
	(1.35)	(1.00)	(1.36)	(1.25)	(0.61)	(0.51)
Settlement Firm	−4.56	−13.58	−16.73	−25.49	−21.97	−33.70
	(−0.74)	(−2.00)**	(−2.12)**	(−2.44)**	(−2.12)**	(−2.29)**
Current Period Returns	24.39	28.32	18.64	27.26	21.34	31.48
	(4.41)***	(4.23)***	(3.07)***	(3.30)***	(3.08)***	(2.74)***
Pre-Event Returns	3.82	8.66	17.48	19.03	20.75	18.25
	(0.52)	(1.40)	(2.18)**	(1.84)*	(2.02)**	(1.44)
Δ*Short Interest*	–	2.24	–	−0.73	–	2.28
		(2.47)**		(−0.24)		(1.14)
Adj. R^2	7.37	23.92	13.31	19.78	15.08	19.64
N	100	70	99	68	97	65

coefficient on the Settlement-firm indicator variable is significantly negative in the second quarter and marginally significant in the third quarter (10%, one tailed *t*-test). Hence, even after controlling for stock price performance and short sales, we find that the percentage institutional ownership drops over the event period for the Settlement firms. In panel B, we re-estimate equation (6) but use Δ*Number of Institutions* (defined previously) as our dependent variable. The results are similar and corroborate those in panel A.[22] In summary, our institutional ownership findings in table 3 are also robust to controls for stock price performance and changes in short sales. These findings show that analyst behavior is linked directly to the institutional-ownership changes and not indirectly via its effect on short sellers.

We also investigate whether differences in other firm characteristics such as total assets, return on assets, sales growth, and book-to-market ratio can explain our findings by including controls for these firm characteristics in the regression (results are not reported). As predicted, the Settlement firm indicator variable is negative and statistically significant. None of the control variable coefficients are statistically significant. Overall, these results indicate that other factors cannot fully explain the institutional holdings results and hence our conclusion that analysts' misleading behavior affected the wealth transfer.

8.2 PARTITION ANALYSIS

We investigate four partitions in table 9. The three panels show results for changes in institutional investor holdings, detailed large- and small-size trading, and trading dollar losses, respectively. The first column repeats the respective full-sample results from tables 3, 4, and 6 above as a benchmark while the last four columns present the results from the four partitions. We discuss our motivation and results for each partition in turn.

First, we predict that those allegations where analysts selectively disclosed their negative views to one or more investors (*Disclosed Selectively*) are more strongly associated with the differential trading behavior. In the other allegations, we simply conjecture that institutions received some form of a negative signal from the analyst. The results are much stronger in this partition. For example, the mean Δ*Institutional Ownership* for the variable-length window is -10.70, compared to the full-sample mean of -4.79. The comparative results for Δ*Number of Institutions* and for fixed-length windows are similar. This finding is consistent with the idea that institutions condition their

[22] In untabulated analyses, we also conduct a similar regression of TAQ-based trading measures on returns. Concurrent returns is a significant and positive predictor of *Small NetBuy* and *Large NetBuy* but does not predict *NetBuy Difference* consistent with returns affecting small- and large-size traders about the same. The economic magnitude of the coefficient on the event-period indicator variable and statistical significance remains similar to the corresponding TAQ analysis, and hence all inferences are similar.

TABLE 9

Tests on Selected Subsamples of Observations

This table presents results for selected partitions of Settlement firms. The first column repeats the respective full-sample results from tables 3, 4, and 6 above as a benchmark. The last four columns present the results from four partitions: (1) Firm-events in which the SEC documents that analysts selectively disclosed their negative news to select investors (*Disclosed Selectively*); (2) Firm-events in which analysts were employed by sanctioned banks with larger-than-the-median number of the broker offices (*Banks with Larger Retail*); (3) Firm-events with smaller-than-the-median number of unique analysts issuing a recommendation (*Lower Analyst Coverage*); and (4) Firm-events with larger-than-the-median market capitalization (*Larger Firms*). Panel A presents the results of changes in institutional investor holdings, panel B presents the results of detailed large- and small-size trading measures, and panel C presents the results of the trading dollar losses. Δ*Institutional Ownership* is the change in the percentage of total common stock held by institutions, Δ*Number of Institutions* is the change in the number of institutions owning the stock. *Small NetBuy* is the difference between daily volumes of small-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *Large NetBuy* is the difference between daily volumes of large-size buy- and sell-initiated trades scaled by the total number of shares traded by small and large traders that day. *NetBuy Difference* is *Small NetBuy* less *Large NetBuy*. The dollar profit is $Gain = \sum_{t=1}^{N} (Buy_{it} - Sell_{it})\ (P_N - P_t\ (1 + MktRet_{t,N}))$, where Buy_t ($Sell_t$) is the total number of buy-initiated (sell-initiated) shares traded on day t that fall in the trade size category consistent with the investor group classifications, P_t is the closing stock price for the firm on date t, P_N is the closing stock price for the firm at the ending date of the empirical event window, *MktRet* is the value-weighted market returns, and N is the last day of the event window. *Gain** equals each firm's *Gain* scaled by the firm's market capitalization. *t*-statistics are reported in parentheses. Statistical significance is based on bootstrapped *p*-values. *, **, and *** denote significance at the 10%, 5%, and 1% (two-tailed) levels, respectively.

	Full Sample (Benchmark)	*Disclosed Selectively*	*Banks with Larger Retail*	*Lower Analyst Coverage*	*Larger Firms*
Panel A: Changes in institutional ownership					
Variable-length event window					
Δ*Institutional*	−4.79	−10.70	−5.55	−8.20	−9.16
Ownership	(−2.08)**	(2.79)**	(2.07)**	(2.26)**	(2.47)**
Δ*Number of*	−21.24	−30.00	−24.57	−30.22	−41.32
Institutions	(−2.59)***	(−1.81)*	(2.49)**	(2.56)**	(3.04)***
One-quarter event window					
Δ*Institutional*	−0.05	−1.51	−1.73	−0.33	−1.31
Ownership	(−0.05)	(0.89)	(1.32)	(0.19)	(0.69)
Δ*Number of*	−7.10	−3.50	−8.21	−11.37	−11.76
Institutions	(−1.17)	(0.22)	(0.84)	(2.16)**	(1.01)
Two-quarter event window					
Δ*Institutional*	−4.17	−10.52	−5.52	−8.20	−8.12
Ownership	(−1.93)**	(2.39)**	(2.20)**	(2.60)**	(2.38)**
Δ*Number of*	−16.9	−25.19	−25.25	−21.93	−31.44
Institutions	(−2.34)**	(1.68)	(2.39)**	(2.83)***	(2.48)**
Three-quarter event window					
Δ*Institutional*	−5.60	−12.29	−8.02	−8.07	−12.21
Ownership	(−2.17)**	(2.59)**	(2.61)**	(2.14)**	(3.00)***
Δ*Number of*	−31.14	−46.69	−43.86	−30.67	−58.64
Institutions	(−3.27)***	(2.21)**	(3.28)***	(2.78)***	(3.68)***
N	50	16	28	27	25

(*Continued*)

TABLE 9 — *Continued*

	Full Sample (Benchmark)	*Disclosed Selectively*	*Banks with Larger Retail*	*Lower Analyst Coverage*	*Larger Firms*
Panel B: TAQ-based analysis of trading over variable-length event window					
Small NetBuy	0.013	0.024	0.026	0.005	0.025
	(5.87)***	(5.33)***	(8.23)***	(1.58)*	(13.44)***
Large NetBuy	−0.004	0.008	−0.019	−0.001	−0.005
	(−1.26)	(1.44)	(−4.25)***	(−0.36)	(−1.14)
NetBuy Difference	0.017	0.016	0.045	0.007	0.029
	(4.43)***	(2.30)**	(8.18)***	(1.26)	(6.59)***
N	4,606	1,158	2,042	2,859	2,187
Panel C: Economic gain using market-adjusted returns (mean *Gain)**					
Small Traders	−9,459	−13,177	−8,890	−11,674	−13,181
	(−3.79)***	(−2.66)**	(−3.22)***	(−3.08)***	(−3.15)***
Large Traders	1,012	−4,615	1,258	41.60	−2,250
	(0.35)	(−0.65)	(0.55)	(0.01)	(−0.47)
Difference	−10,471	−8,563	−10,149	−11,716	−10,931
	(−3.22)***	(−1.54)	(−3.05)***	(−2.40)**	(−2.09)**
N	50	16	28	27	25

trading on analysts' private information and suggests that analysts caused institutions to "flee" the stocks while at the same time encouraging individuals to buy.

Second, we expect that individuals access fewer information sources and are more likely to follow recommendations of analysts at banks in which they are clients. If so, sanctioned banks with larger retail operations should generate a stronger wealth transfer. The third column shows the results for those sanctioned banks with a larger-than-the-median number of broker offices (*Banks with Larger Retail*), a proxy for the number of individual-investor clients. For example, Morgan Stanley has 603 offices, the largest number among our sanctioned banks and is ranked #6 out of all (397) Securities Industry Association members. The results are slightly stronger for these banks, particularly for the three-quarter event window.

Third, assuming that investors approximately weight analysts' information equally, and that only the analyst of the sanctioned bank provides misleading information (while the analysts at other banks do not), we expect that misleading analysts will produce stronger wealth-transfer effects when fewer analysts provide stock recommendations. The fourth column shows the results for firms with a smaller-than-the-median number of unique analysts issuing a recommendation on I/B/E/S in the nine-month period prior to the start date (*Lower Analyst Coverage*). The results are stronger for firms with lower analyst coverage. Given that institutions are likely to rely less on recommendations than individuals, this result is consistent with analysts'

misleading behavior leading individuals astray, particularly when there are fewer recommendations available by other analysts.

Last, we investigate firm size. On one hand, we expect weaker results for larger firms because these firms have better information environments and it is less likely that investors in general place substantial weight on a single source of information, i.e., one misleading analyst. On the other hand, it is not clear that a better information environment is helpful to individuals, who potentially lack the access to, and ability to process, this larger information set. A superior information environment could exacerbate the wealth transfer if it allows institutions to react faster to new information than individuals. The last column shows the results by firms with a larger-than-the-median market capitalization (*Larger Firms*). Larger firms have stronger results, which suggests that individuals benefit less from the superior information environment than institutions. This result is also important because it shows that our results are not simply driven by smaller-size firms.[23]

Panel B presents the results of partitions using TAQ-based trading measures. The results for the two partitions of firms in which analysts disclosed selectively to institutions and in which firms are larger in size are stronger (than the full-sample results) and corroborate those documented in panel A. The results for the partition of banks with larger retail operations are particularly strong and the results for the analyst coverage partition are weak compared to the full-sample results. In the *Gain** measures, presented in panel C, generally we do not find differences in the test results between the partitioned and the full-sample results. Overall, we find some evidence that analysts' misleading behavior helped institutions. Most of the evidence is consistent with the wealth transfer caused by misled individuals.

9. Concluding Remarks

This study examines the responses of individual and institutional investors and the economic consequences of their different trading behaviors when analysts act strategically. We draw upon the *Global Research Analyst Settlement* to identify a set of firm-events in which analysts clearly issued misleading stock research. Our sample relies on neither affiliation nor analyst recommendations to proxy for strategic behavior. While investigators chose the sample for very specific reasons, these reasons are exogenous to our research question and hence should not affect our inferences.

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[23] There is a potential contradiction in our results because large firms generally have higher analyst coverage. In our sample, however, a large number of firms (one-fifth of the total) are large in size but have lower analyst coverage, and it is these firms that provide the strongest support for the full-sample results.

We use this sample to demonstrate that analysts' misleading behavior leads to a systematic wealth transfer from individual to institutional investors. We find that for the Settlement firms, institutional holdings decreased over the event period while short interest increased. We provide corroborating evidence using TAQ trade size data to proxy for institutions and individuals and the Lee and Ready [1991] algorithm to classify trades as buyer or seller initiated. We find that individuals were buying more than selling during the event period, relative to the trading of institutions. Although institutions are involved in the bulk of the trading, we find that actively trading individuals lose $2.2 billion, an amount approximately two and a half times the amount that actively trading institutions lose.

Our paper provides direct evidence on how analysts' misleading behavior harms individual investors and provides empirical support for the recent regulatory changes regarding analyst activities. To the extent that regulatory changes have significantly reduced the amount of strategic behavior by analysts, the plight of individual investors has improved. This objective is consistent with the aims of regulators and lawmakers to protect all investors.

One caveat is that the size of the Settlement sample is small and consists of technology-related firms that experienced large negative returns. Whether analysts would issue misleading disclosures of the same degree for firms experiencing positive returns or modest negative returns is less clear. Furthermore, it is likely that regulators investigated the sanctioned banks because they were visible and had "deep pockets," thus taking advantage of potential political gains (Watts and Zimmerman [1986]). Also, regulators likely investigated and presented the most extreme cases, so the sample is less representative of the population. Nevertheless, we believe our findings have broader implications. If regulators believed that the analyst behavior was limited, then regulation dealing with analysts' conflicts of interest that applied to the entire market, such as Section 501 of the *Sarbanes Oxley Act*, *Rule NASD 2711 and Rule NYSE 472*, would have been unnecessary.[24]

[24] These regulations can be found at http:/www.sarbanes-oxley.com and http://www.sec.gov/rules/sro/34-47110.htm.

APPENDIX

The Global Research Analyst Settlement List of Securities

Settling Party	Equity Security	Eligibility Period		
		Start	End	Length (Days)
Bear Stearns & Co. Inc.	CAIS Internet, Inc.	Nov 7, 2000	Apr 24, 2001	168
	Digital River, Inc.	Jan 30, 2002	Apr 1, 2002	61
	Micromuse, Inc.	Jul 18, 2001	Oct 16, 2001	90
	Sonic Wall	Jan 25, 2001	May 15, 2001	110
Citigroup Global Markets Inc.	Adelphia Business Solutions, Inc.	May 14, 2001	Aug 13, 2001	91
	AT&T Corp.	Nov 29, 1999	Jan 25, 2000	57
	Focal Communications Corp.	Apr 10, 2000	Oct 17, 2000	190
	Focal Communications Corp.	Feb 21, 2001	Aug 13, 2001	173
	Level 3 Communications, Inc.[a]	Apr 18, 2001	Jun 18, 2001	61
	Metromedia Fiber Networks, Inc.	Apr 30, 2001	Jul 25, 2001	86
	RCN Corp.	May 3, 2001	Aug 2, 2001	91
	Williams Communications Group, Inc.	May 1, 2001	Nov 1, 2001	184
	XO Communications, Inc.	Apr 26, 2001	Nov 1, 2001	189
Credit Suisse First Boston LLC	Agilent Technologies, Inc.	Jul 21, 2000	Feb 20, 2001	214
	Digital Impact, Inc.	Jul 23, 2001	Oct 2, 2001	71
	New Power Holdings, Inc.[b]	Oct 30, 2000	Dec 31, 2001	427
	Numerical Technologies, Inc.[b]	May 4, 2000	Aug 2, 2000	90
	Synopsys, Inc.	Jul 23, 1999	Jun 29, 2000	342
	Winstar Communications, Inc.	Jan 5, 2001	Apr 5, 2001	90
Deutsche Bank Securities Inc.	Emisphere Technologies, Inc.	Jan 26, 2000	Apr 25, 2000	90
	E-Prise Corporation	Apr 26, 2001	Jul 25, 2001	90
	Getty Images, Inc.	Apr 5, 2002	Jul 4, 2002	90
	Oracle Corp.	May 31, 2001	Aug 29, 2001	90
	Transkaryotic Therapeutics, Inc.	Jul 13, 2001	Oct 11, 2001	90
	Trimeris, Inc.	Dec 28, 2001	Mar 28, 2002	90
	United Therapeutics, Inc.	Mar 7, 2000	Jun 5, 2000	90
Goldman, Sachs & Co.	360networks, Inc.	Apr 27, 2001	May 15, 2001	18
	AT&T Corp.	Jul 26, 2000	Dec 19, 2000	146
	AT&T Corp.	Apr 25, 2001	Jun 30, 2001	66
	Exodus Communications, Inc.	Jun 11, 2001	Jun 20, 2001	9

(*Continued*)

APPENDIX— *Continued*

Settling Party	Equity Security	Eligibility Period		
		Start	End	Length (Days)
	Global Crossing Ltd.	Mar 21, 2000	Jun 19, 2000	90
	WorldCom, Inc.	Aug 7, 2000	Dec 5, 2000	120
	WorldCom, Inc.	Apr 26, 2001	Jun 30, 2001	65
Henry McElvey Blodget	Goto.com[b]	Jan 11, 2001	Jun 6, 2001	146
J. P. Morgan Securities Inc.	Epicor Software Corp.	Oct 22, 1999	Jan 30, 2001	466
	International Rectifier Corp.	Jul 1, 1999	Nov 1, 2000	489
Lehman Brothers Inc.	Broadwing, Inc.	Jan 25, 2001	Apr 25, 2001	90
	DDi Corp.[b]	Jun 30, 2000	Sep 28, 2000	90
	Razorfish, Inc.[b]	May 24, 1999	Aug 22, 1999	90
	Real Networks, Inc.	Jul 11, 2000	Oct 17, 2000	98
	RSL Communications	Mar 2, 2000	Sep 6, 2000	188
Morgan Stanley & Co. Inc.	Ask Jeeves Inc.	Apr 20, 2000	Jul 19, 2000	90
	drugstore.com Inc.	Apr 25, 2000	Jul 24, 2000	90
	Inktomi Corp.	Dec 7, 2000	Jan 4, 2001	28
	Ventro Corp.	Apr 5, 2000	Jul 4, 2000	90
Thomas Weisel Partners LLC	Hotjobs.com Ltd.[b]	Nov 4, 1999	Feb 2, 2000	90
	InfoSpace, Inc.	Jan 11, 2001	Jul 25, 2001	195
	Level 3 Communications, Inc.[a]	May 21, 2001	Jun 19, 2001	29
	Sprint FON Group	Jun 19, 2001	Jul 19, 2002	395
UBS Securities LLC	Interspeed, Inc.	Jan 3, 2000	Jul 21, 2000	200
	Triangle Pharmaceuticals, Inc.	Oct 8, 1999	Mar 10, 2000	154
	Atmel Corp.	Feb 9, 2000	May 9, 2000	90
	Flextronics International, Ltd.	Mar 2, 2001	Jun 1, 2001	91
US Bancorp Piper Jaffray Inc.	Esperion Therapeutics, Inc.	Oct 18, 2001	Jun 28, 2002	253
	Triton Networks Systems Inc.	Mar 30, 2001	May 1, 2001	32
	Just for Feet, Inc.	Apr 21, 1999	Jul 20, 1999	90
	JDS Uniphase Corp.[c]	Jul 27, 1999	Oct 25, 1999	90
	Comverse Technology, Inc.	Mar 28, 2001	Jun 26, 2001	90

[a]These two events are for the same firm and were combined into one sample observation that started on the earlier date (April 18, 2001) and ended on the later date (June 19, 2001).

[b]These six firms are excluded from the sample because they had their IPO within one quarter of the event period.

[c]This firm is excluded from the sample because it merged with another firm during the event period.

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