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Examining the Dark Side of Financial Markets: Do Institutions Trade on Information from Investment Bank Connections?

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Institutions often have access to corporate inside information through their connections, but relatively little is known about the extent to which they exploit their informational advantage through short-term trading. We employ broker-level trading data to systematically examine possible cases of connected trading. Despite examining the issue from multiple angles, we are unable to find much evidence to support that investment bank clients take advantage of connections through takeover advising, IPO and SEO underwriting, or lending relationships. In contrast to recent academic literature and popular press, our findings suggest that institutional investors are reluctant to use inside information in traceable manners. (*JEL* G12, G14, G2)

Institutional investors are in constant and close contact with firms through their investment banking, lending, and asset management arms. At the same time that institutions are afforded access to information that can potentially

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be used for extremely profitable trading, they are told to not trade on it. Nevertheless, skeptics contend that the short-term profit motive is strong and informed institutions make substantial short-term trading profits by using their connections. They further argue that high-profile cases, such as the Galleon hedge fund, illustrate that insider trading is "rampant."[1] Institutions are quick to emphasize that they would not dare use such information, because their integrity is important and future business depends on reputation. Goldman Sachs' CEO has emphasized that they are "greedy, but long-term greedy."[2] The evidence for insider trading mainly consists of financial press articles about prosecuted cases and academic articles that find evidence of some form of connected and possibly illicit trading. Our article broadens this literature substantially by using daily broker-level data to provide a systematic examination of whether brokerage house clients trade on information through their informing investment banks prior to major announcements.

We first examine trading by clients of connected brokerage houses prior to takeover announcements for windows ranging from two to twenty days. Investment banks act as advisors to target and acquirer firms and, at the same time, have separate trading arms. If investment banks pass on valuable information to their clients, then the clients may purchase shares in takeover targets prior to public announcements. We test this proposition but find no evidence of buying ahead of takeover announcements by target or acquirer advisors' clients.

We then turn to clients of investment banks who had a previous initial public offering (IPO) underwriting relationship with the takeover target firm and fail to find client buying ahead of takeovers for IPO underwriters overall or various subgroups, such as book runners and past profitable underwriters. We do find some evidence of buying ahead of takeovers by clients of investment banks who are underwriters of recent IPOs, but this result is insignificant when using multiple significance tests that account for the many IPO connection types examined. Clients of investment banks with a previous IPO relationship do not trade in the right direction or earn abnormal returns prior to earnings announcements.

In a similar fashion, we examine if the clients of investment banks who were an advisor in a previous seasoned equity offering (SEO) engage in profitable trading prior to takeover or earnings announcements, but find little evidence of connected trading. Furthermore, there is little evidence that lending relationships are associated with profitable trading ahead of major events. In sum, clients of connected brokerage houses generally fail to make abnormal profits ahead of takeover and earnings announcements. We also examine

1 As discussed recently in *Bloomberg Businessweek* by Barrett, Burton, and Kishan (2011).

2 See Arlidge and Beresford (2009). The article also states, "Goldman dismisses charges of 'casino capitalism'…It emphatically denies it misuses information or acts unethically. Strict 'Chinese walls' between traders and advisers prevent any conflicts of interest."

market maker trading by connected brokerage houses but find little evidence that market making arms trade profitably before the announcements by firms to which they have investment banking or lending connections.

Our results to this point show that neither brokerage house clients nor the brokerage houses themselves trade on inside information through the brokerage house associated with the information. This finding may not seem surprising to some, but it would be surprising to careful readers of the rapidly growing body of academic literature that argues for evidence of links between investor holdings (or trading) and investment bank connections (Massa and Rehman 2008; Bodnaruk, Massa, and Simonov 2009; Jegadeesh and Tang 2010; Kedia and Zhou 2010; Ivashina and Sun 2011). For example, using quarterly 13f filings, Bodnaruk, Massa, and Simonov (2009) argue that funds affiliated with takeover bidder advisors take positions in target firms before a takeover announcement. In contrast, using higher-frequency data, Jegadeesh and Tang (2010) find no evidence of suspicious trading activity for bidder advisors but do find profitable trading through target advisors.[3] Massa and Rehman (2008) and Ivashina and Sun (2011) use quarterly mutual fund and 13f filings to provide evidence that access to confidential banking and loan renegotiation information is followed by profitable affiliated institutional trading. We find no support for any of these activities.

Why do our findings differ? Our data and approach have three potential advantages compared with the previously mentioned literature: high-frequency data, a direct study of brokerage house trading, and comprehensive analysis through multiple channels of relationships and connections. First, if institutions trade on short-term information, or they carefully avoid taking positions at the end of the quarter, then studies using quarterly government filings, such as 13f or N-30D holdings (e.g., Massa and Rehman 2008; Bodnaruk, Massa, and Simonov 2009; Ivashina and Sun 2011), may noisily observe or understate the importance of connections. Second, we are able to directly examine trading at the broker level and at a high frequency that leads to more power in detecting abnormal trading ahead of information events. Jegadeesh and Tang (2010) also use detailed trading data, except that their data comprise self-reported institutional trades that account for only about 8% of the market. In contrast, our data capture all reported NASDAQ trades. Third, we assemble an extensive list of connections through takeover advising, IPO and SEO underwriting, lending, and past trading profitability, whereas the previously mentioned articles focus on a single channel.

Next, we turn to further examining the information content of trading that is not directly linked to investment banking information. We investigate historical connections between brokerage houses and firms. The motivation is similar

[3] Kedia and Zhou (2010) suggest that bond dealers affiliated with takeover target advisors engage in suspicious bond trading prior to takeovers. Dai et al. (2011) argue that hedge funds take stakes in target firms prior to announcements.

to that employed by the Galleon hedge fund, where connections at firms are garnered and used repeatedly.[4] However, we find no evidence of consistently connected trading ahead of earnings announcements by historically connected brokerage house clients. We then examine whether clients of some brokerage houses consistently make profits ahead of announcements at the expense of less-connected or -sophisticated clients. We find that clients of some brokerage houses consistently earn profits during the twenty days prior to earnings announcements. Since the general trading through a brokerage house is not linked to any specific investment banking relationship, and the patterns are not statistically significant at short windows, the persistence in trading profits is also consistent with some investors processing public news better than others.

The lack of evidence for informed trading by clients of the connected brokerage houses or the brokerage houses themselves could be due to their routing informed trades through other brokerage houses. If this is true, then we expect to observe that aggregate institutional trading is informed prior to takeover and earnings announcements. However, institutions in the aggregate do not trade in the right direction ahead of takeover or earnings announcements. We do find evidence that some wealthy individuals are net buyers prior to takeover announcements. This finding suggests that connected individuals may choose to use inside information for themselves and/or their friends, instead of their firms.

Our article adds to the rapidly growing body of literature on connections and trading profitability. In addition to the articles discussed above, Irvine, Lipson, and Puckett (2007) use a high-frequency database of institutional trades and find that institutions trade in the same direction as impending analyst recommendations.[5] Acharya and Johnson (2010) find that stock price run-up prior to buyouts is increasing in the number of private equity participants and attribute this finding to information leakage through corporate connections. Cohen, Frazzini, and Malloy (2008) show that educational affiliations between mutual fund and corporate board managers are associated with more profitable mutual fund trading around corporate news announcements.[6]

Despite the fact that we examine a broader set of possible connections and have a more powerful data set compared with much of the literature, we find little evidence of connected trading. It seems intuitive that articles featuring evidence in favor of insider trading are more surprising, intriguing,

4 In October 2009, the SEC filed a complaint alleging that Raj Rajaratnam obtained nonpublic information, such as corporate earnings and takeover activity, at several companies, including Google, Hilton, Intel, and IBM. He then repeatedly traded on those tips on behalf of his Galleon hedge fund. In May 2011, Raj Rajaratnam was convicted on all 14 counts of insider trading.

5 Christophe, Ferri, and Hsieh (2010) also find evidence of “tipping” with NASDAQ short-sale data.

6 Cohen, Frazzini, and Malloy (2010) find that educational networks are associated with the profitability of analyst forecasts. Coval and Moskowitz (2001) find that funds make much larger profits in local stocks, which could be due to connections. Tang (2009) finds that mutual fund managers trade profitably based on former analyst connections.

and potentially publishable. We wonder whether this tendency contributes to the scarcity of literature showing situations in which institutions do not trade on connections. Our findings are more consistent with the literature examining trades of registered corporate insiders. Lee et al. (2011) find little evidence of informed trading by corporate insiders during recent periods.[7] From an international perspective, Griffin, Hirschey, and Kelly (2011) argue that the United States has one of the lowest levels of pre-announcement leakage and insider trading is much more prevalent in emerging and some small, developed markets.

We are not claiming that insider trading does not occur in the United States or that it is not a problem for financial market participants. Instead, our evidence indicates that brokerage houses naïvely exploiting in-house brokerage information seems to be the exception, rather than the norm. Investment banks and lenders may still use information gathered through connections but in ways that are difficult to trace. They may also trade on their own personal accounts, as illustrated by our finding of wealthy individuals buying prior to takeover announcements.

The outline of our article is as follows. Section 1 describes the trading data and construction of event samples. Section 2 examines trading prior to takeover and earnings announcements by clients of brokerage houses connected to firms through investment banking and lending relationships. Section 3 examines historical linkages between firms and brokerage house trading and persistence in client trading profits at the brokerage house level. Section 4 analyzes market maker trading and provides a brief examination of trading by aggregate investor groups prior to announcements. Section 5 concludes.

1. Data

1.1 Trading in NASDAQ stocks

The primary data set for this article consists of trading by brokerage houses in all NASDAQ-listed firms from January 2, 1997, to December 31, 2002. The data are derived from NASDAQ clearing records that include the date, time, ticker symbol, trade size, and price of each transaction for each stock. These clearing records also include market maker IDs from the settlement process that allow the volume to be assigned to investment banks. Hence, each trade can be linked to parties on both sides of the trade. Additionally, each trade is marked as to which party is buying or selling. The data also contain separate principal/agent flags, which allow us to identify whether the parties are trading for their own account or a client. For each brokerage house, we

[7] Cohen, Malloy, and Pomorski (2012) find evidence that registered insiders do make profits on their nonroutine trades. Through an examination of cases of prosecuted insider trading, Del Guercio, Odders-White, and Ready (2011) argue that insider trading in the United States is often by individual investors and overall relatively uncommon.

measure trading activity using imbalances defined as the difference between buy and sell volumes expressed as a fraction of shares outstanding.[8]

The data include both trades reported “to the tape” (tape report) and unreported NASDAQ clearing records (nontape report). We check and correct for two different issues. First, the same trade may be reported by both parties. In that case, there will be one entry in the tape report and another in the nontape report. We check for consistency between the reported and unreported records when assigning whether a market maker acted as a principal or an agent for each leg of the trade. We exclude unclassified trades that are inconsistently reported in any leg of the routing report. Second, when a trade between two non-ECN market makers A and B is facilitated by an electronic communication network (ECN), there is an A-to-ECN entry in the tape report and an ECN-to-B entry in the nontape report. To address this issue, we match an A-to-ECN trade in the reported file to an ECN-to-B trade in the nontape report and replace the A-to-ECN entry with an A-to-B trade. Unfortunately, we are unable to match parties in some ECN trades, likely because the data did not include complete details related to clearing (based on investigating unmatched ECN trades and consulting with NASDAQ officials). We exclude all unmatched trades executed through ECNs from our analysis. Unmatched ECN and unclassified trades account for 22.2% of trading volume. Elimination of these trades creates a small client net buying of 0.76% of total volume, which is concentrated in the first three years of our sample period (1997–1999). For robustness, we replicate many of our key findings for the 2000–2002 time period, where there is a minimal client buy imbalance (0.02% of total volume), and find extremely similar inferences.[9]

We match brokerage houses with takeover advisors, IPO and SEO underwriters, and lenders. Since most of our investment banks primarily deal with institutional clients during our sample period, our examination of connections is focused on institutional trading by construction. We obtain data on takeover advisors from SDC and complement that with the Mergerstat and CorpfinWorldwide databases. Data on IPO and SEO underwriters are from the Securities Data Company (SDC) database. We then manually match takeover advisors, IPO underwriters, and SEO underwriters with brokerage houses by name. Investment bank mergers are accounted for by using the list of mergers from Corwin and Schultz (2005). Data on lenders are obtained from Thomson Reuters’ DealScan database. Because of the large number of lenders, we pick the top 500 brokerage houses in terms of total NASDAQ trading volume during

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8 The brokerage level data have been used to examine trading after IPOs in Griffin, Harris, and Topaloglu (2007). They, along with Griffin et al. (2011), describe the NASDAQ clearing data in more detail.

9 We also repeat the tests in the article using the adjusted imbalance for a broker, which is computed by subtracting the average historical imbalance of the broker in the firm. To ensure that the benchmark window is before the event window but after the previous announcement, we choose the thirty-day period that ends twenty-one days prior to the announcement ([−50,−21] window). Our results are very similar when using the adjusted imbalances, confirming that the client buying bias has little impact on our results.

1997–2002 and then match with lenders by name. We address lender mergers following Sufi (2007).

For the very last subsection in our analysis, we use data classified into nine investor groups (four institutional groups, four individual groups, and a mixed group) following Griffin et al. (2011).[10] Following standard practice, in all of our analyses, we exclude a firm if it is not in the Center for Research in Security Prices (CRSP) database or its share code is not 10 or 11 (ordinary common shares).

1.2 Takeovers and earnings announcements

The samples used in this article consist of NASDAQ firms from January 1997 to December 2002 with takeovers and mergers and/or earnings announcements over the period. We drop announcements for which the stock is priced below five dollars on the twenty-first day prior to the announcement to control for microstructure effects.

For takeovers, we obtain information from the SDC's Mergers and Acquisitions database for all U.S. targets listed on NASDAQ over our sample period. We follow the literature to exclude leveraged buyouts, spinoffs, recapitalizations, self-tenders, exchange offers, repurchases, minority stake purchases, acquisitions of remaining interest, and privatizations. In addition to the "date announced" and "original date announced" variables from SDC, we also search Mergerstat, CorpfinWorldwide, and LexisNexis for the first news item about the target firm potentially being a takeover target. Since we want to focus on non-public-information-based trading before a merger has been announced, we take a conservative approach and choose the earliest of the four sources. Thus, some of our dates are "rumor" dates, as they occur prior to the official announcement dates.

Our final sample has 1,225 takeovers and mergers during 1997–2002, which we further match to brokerage houses that act as takeover advisors, IPO underwriters, SEO underwriters, and lenders. Panel A of Table 1 provides summary statistics for our takeover sample. The number of takeovers/mergers that are matched to connected brokerage houses ranges from 211 for lenders to 677 for takeover advisors. The two-day abnormal announcement return ranges from 14.89% (for IPO underwriters) to 18.11% (for lenders). The abnormal stock return during the twenty-day window prior to the takeover announcement (price run-up) ranges from 5.73% (for SEO underwriters) to 8.38% (for IPO underwriters). Our sample price run-up is less than the 11% documented by Jarrell and Poulsen (1989). This may be due to the fact that 1) we are conservative and choose the earliest date from four sources, 2) we

[10] The institutional groups include general institutions, largest investment banks, hedge funds, and derivative traders. The individual groups include general individuals, individual full-service, individual discount, and individual daytrading.

Table 1
Summary statistics

Panel A: Takeover Announcements

Relation Type	No. Brokers	No. Takeovers	Target Size ($ M)	Run-up (%)	Ann. Ret (%)
Takeover advisors	119	677	458.35	8.03	17.12
Acquirer advisors	92	353	503.86	8.89	17.47
Target advisors	102	499	449.01	8.04	17.08
IPO underwriters	204	288	571.79	8.38	14.89
Book runners	59	129	402.84	7.27	16.04
Comanagers	78	168	565.58	8.53	15.83
Syndicate members	190	170	652.27	9.33	13.23
SEO underwriters	163	225	988.08	5.73	14.95
Book runners	46	110	1,034.48	5.19	18.86
Comanagers	81	161	1,210.36	5.17	13.80
Syndicate members	154	108	1,510.96	5.23	16.22
Lenders	57	211	1,053.47	5.91	18.11
Lead lenders	53	180	1,166.31	6.67	17.41
Loan participants	49	103	1,494.26	3.45	15.79

Panel B: Earnings Announcements

		No. Earnings Announcements					
Relation Type	No. Brokers	Ret <−5%	−5% <Ret <0%	0% <Ret <5%	Ret >5%	Firm Size ($ M)	Ann. Ret (%)
IPO underwriters	404	3, 941	3,387	3,182	3,452	778.42	−0.41
Book runners	191	1, 681	1,577	1,497	1,404	613.45	−0.54
Comanagers	216	2, 343	1,968	1,892	2,037	861.22	−0.52
Syndicate members	351	2, 655	1,996	1,901	2,341	868.58	−0.42
SEO underwriters	329	3, 048	2,980	2,950	2,845	1,320.49	−0.38
Book runners	133	1, 475	1,519	1,487	1,323	1,455.69	−0.43
Comanagers	181	2, 225	2,108	2,066	2,109	1,251.47	−0.37
Syndicate members	311	1, 508	1,368	1,289	1,414	1,297.86	−0.50
Lenders	89	2, 959	3,529	3,522	3,269	1,808.46	0.23
Lead lenders	80	2, 603	3,015	2,971	2,865	1,925.27	0.20
Loan participants	77	1, 311	1,760	1,837	1,523	2,764.27	0.33
Hist. connected houses	1,432	6, 698	7,365	7,171	6,479	1,863.59	−0.08

Panel A reports summary statistics for takeover announcements. For each relation type, we present the number of brokers and takeover announcements; and average target firm size, price run-up, and announcement return. Target firm size is the market capitalization of the target measured twenty-one trading days prior to the announcement. Run-up is the buy-and-hold target return during the [−20,−1] window, where day −1 refers to the last trading day before the announcement. Announcement return is the buy-and-hold target return during the [0,1] window. Run-up and announcement returns are both in excess of the NASD index return. Panel B reports summary statistics for earnings announcements. For each relation type, we present the number of brokers; average firm size and announcement return; and the number of earnings announcements for four groups classified according to the announcement return: those with announcement returns below −5%, between −5% and 0%, between 0% and 5%, and greater than 5%. Firm size and announcement return are calculated as in Panel A. A brokerage house is classified as historically connected to a firm if that broker traded at least twice prior to the firm's earnings announcements in the previous year and traded in the same direction as the announcement return for each announcement. In both panels, we exclude the top 100 market makers, according to the 1997–2002 trading volume to control for liquidity trading.

exclude stocks priced below five dollars, or 3) information leakage has declined through time.

Our earnings announcement sample is the intersection of CompuStat quarterly accounting data and CRSP stock data. In particular, we obtain 62,804 quarterly earnings announcements from 1997–2002 from the CompuStat quarterly data file. Those earnings announcements are further matched to brokerage houses that act as IPO underwriters, SEO underwriters, and lenders for the announcing firms. Panel B of Table 1 provides summary statistics for our earnings announcement sample. The total number of earnings announcements for each connection category ranges from 11,823 (for SEO underwriters) to 27,713 (for historically connected brokers). To examine informed trading, we further divide the earnings announcements into four groups according to two-day abnormal announcement returns (i.e., $<-5\%$, between -5% and 0%, between 0% and 5%, and $>5\%$). Our sample of earnings announcements is relatively evenly distributed across the four groups, and the large sample size should allow for substantial power to detect connected trading.

2. Trading by Connected Brokerage Houses Prior to Major Announcements

We examine connections through traditional investment banking channels, including takeovers, IPOs, and SEOs, in addition to those through lending. There is theoretical (Fulghieri and Spiegel 1993; Loughran and Ritter 2002) and empirical (Reuter 2006; Griffin, Harris, and Topaloglu 2007) literature showing that investment banks may use their investment banking business, such as IPOs, to reward favored clients for past business or in exchange for excess fees in the future. The analogy here would be if investment banks rewarded favored clients with inside information regarding impending events. We examine abnormal trading primarily by looking at client imbalances of connected brokerage houses.

2.1 Investment banking connections

If a brokerage house handles diversified order flow from many clients, it would be more difficult to detect informed trading from a particular group of clients. Therefore, for our main analysis of client trading, we exclude the top 100 market maker codes (out of 2,904) in terms of total NASDAQ trading volume during 1997–2002. For robustness, we repeat our tests with those brokers included and obtain similar inferences. The intensity of insider or connected trading could vary across brokerage houses. Hence, among each connection category, we further identify a group of brokerage houses whose clients traded profitably in their connected firms in the previous period.[11]

[11] For the takeover sample, we identify past profitable brokers in year y as those whose clients traded at least once during the twenty-day window prior to takeovers they advised from 1997 to $y-1$ with positive twenty-day client imbalances prior to all such takeover announcements. To identify past profitable brokers for year y for the earnings announcement sample, we first sort connected brokers into terciles of success ratio (percentage of imbalances in the right direction) for their large twenty-day client imbalances (dollar imbalances above \$100,000) prior to connected firms' earnings announcements in year $y-1$. We further sort the top tercile of success ratio into terciles of trading frequency, which is the ratio of the number of large twenty-day client imbalances to the total number of twenty-day client imbalances for connected brokers prior to earnings announcements in year $y-1$, and identify the top tercile as past profitable brokers. For robustness, we identify past profitable brokers using alternative methods and obtain similar results.

For each connection category, we examine trading imbalances for the two-, five-, ten-, and twenty-day windows prior to takeover announcements and the four categories of earnings announcements. To control for clustering in trading during the same time period, we follow a calendar-time approach used in the earlier literature (e.g., Jaffe 1974; Mandelker 1974) and popularized by Fama (1998). Specifically, when we examine imbalances for a given window prior to events, we take all the daily imbalances for connected brokers during that window, calculate average imbalance for each calendar day during our sample period, and then report the time-series means for average daily imbalances.[12] We also report (unadjusted) p-values based on time-series t-statistics that are calculated using Newey-West robust standard errors with twenty lags.

Since we are examining many combinations of windows and subsamples for evidence of abnormal activity, it is possible that we obtain statistically "significant" results due to chance.[13] For example, when we examine trading by takeover advisors prior to takeover announcements (Table 2), we test the trading of all advisors, target advisors, acquirer advisors, and past profitable advisors in the two-, five-, ten-, and twenty-day windows prior to takeover announcements. To address the issue of multiple testing, for each table (and connection category) in our article, we report corrected p-values using the Holm-Bonferroni (Holm 1979) and the false discovery rate (FDR; Benjamini and Hochberg 1995) methods that consider all the tests in the table being examined. Holm-Bonferroni and FDR methods are both commonly used approaches in the literature to address the multiple testing issue, with the former typically being more stringent against finding significant results.[14]

2.1.1 Trading by takeover advisors prior to takeovers. We first analyze the most blatant form of connected trading—we examine if clients of brokerage houses acting as takeover advisors buy target shares prior to takeover announcements. Table 2 shows that client imbalances are all negative prior to takeover announcements. For example, the average ten-day client imbalance is −0.0079% when we examine all takeover advisors. In addition, for the

12 For example, when we examine the imbalances of IPO underwriters for the [−20,−1] window prior to earnings announcements, we first form a sample that includes all daily imbalances for IPO underwriters during the [−20, −1] windows. We then calculate average imbalances for each calendar day and report time-series means of daily average imbalances.

13 To illustrate the multiple testing problem, if a coin comes up on the same side at least nine out of ten flips, then we can reject the null of a fair coin at the 0.05 level because the probability of this outcome is only 0.0215 for a fair coin. However, if one tests twenty fair coins simultaneously but uses the same criterion, then she may falsely reject the null because the probability of at least one coin coming up on the same side at least nine out of ten flips is $1-(1-0.0215)^{20} \approx 0.35$.

14 To calculate Holm-Bonferroni p-values in a table, we first rank all the unadjusted p-values and then multiply each one by $n-k+1$, where n is the total number of significance tests in the table and k is the rank of the unadjusted p-value (1 for the lowest and n for the highest). FDR p-values are calculated using a similar approach except that we multiply unadjusted p-values by n/k. As discussed in Holm (1979), the Bonferroni method, another commonly used approach, is more stringent against finding significances than the Holm-Bonferroni method. Since our results show a lack of informed trading using the Holm-Bonferroni and FDR methods, it is redundant to examine the Bonferroni method.

Table 2
Trading by takeover advisors prior to takeover announcements

	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]
All advisors	−0.030	−0.035	−0.079	−0.089
Unadjusted *p*-value	(0.06)	(0.08)	(0.00)	(0.03)
Holm-Bonferroni *p*-value	(0.56)	(0.61)	(0.08)	(0.42)
FDR *p*-value	(0.12)	(0.14)	(0.08)	(0.16)
Target advisors	−0.027	−0.022	−0.092	−0.131
Unadjusted *p*-value	(0.42)	(0.57)	(0.05)	(0.12)
Holm-Bonferroni *p*-value	(0.57)	(0.61)	(0.55)	(0.57)
FDR *p*-value	(0.15)	(0.19)	(0.13)	(0.18)
Acquirer advisors	−0.009	−0.051	−0.041	−0.030
Unadjusted *p*-value	(0.11)	(0.01)	(0.09)	(0.39)
Holm-Bonferroni *p*-value	(0.63)	(0.18)	(0.62)	(1.00)
FDR *p*-value	(0.15)	(0.10)	(0.14)	(0.45)
Past profitable advisors	−0.062	−0.069	−0.123	−0.137
Unadjusted *p*-value	(0.05)	(0.05)	(0.06)	(0.14)
Holm-Bonferroni *p*-value	(0.85)	(0.57)	(0.55)	(0.60)
FDR *p*-value	(0.45)	(0.57)	(0.13)	(0.16)

This table reports average client imbalances prior to takeover announcements for brokerage houses acting as takeover advisors. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) p-values for the average daily imbalances. The unadjusted p-values are based on t-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We report results for all advisors, advisors of target firms, advisors of acquirer firms, and past profitable advisors. For year y, we identify past profitable advisors as those whose clients traded at least once during the twenty-day window prior to takeovers they advised from 1997 to $y-1$ with positive twenty-day client imbalances prior to all such takeover announcements. We exclude the top 100 market makers, according to the 1997–2002 trading volume to control for liquidity trading. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) p-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR p-values.

all advisors category, the negative imbalances in the ten- and twenty-day windows are significant at the 0.05 level using the unadjusted Newey-West standard errors before controlling for multiple testing. This result could be due to takeover advisors being careful not to clear trades for their clients in the same direction as future announcements out of fear of looking like they are engaging in insider trading. However, since Table 2 examines four different windows and four connection categories simultaneously, we further present Holm-Bonferroni and FDR p-values that show that none of the negative imbalances for the all advisors category is significant at the 0.05 level after controlling for multiple testing.

Table 2 also presents trading for clients of target advisors and acquirer advisors. Before controlling for multiple testing, imbalances are insignificant for most groups but significantly negative at the 0.05 level (and hence unprofitable) for the ten-day window for target advisors and the five-day window for acquirer advisors. Nevertheless, none of the negative imbalances are significant after controlling for multiple testing. We also investigate if there are particularly rogue brokers, who consistently pass on information to their clients, by examining brokers whose clients had trading profits in all advised deals in the previous years. There is weak evidence of negative imbalances using the unadjusted p-values, but the imbalances become insignificant after controlling for multiple testing.

To summarize, our findings show that clients of neither target nor acquirer advisors buy shares prior to takeover announcements. This is consistent with investment banking firms carefully monitoring the trading activity ahead of acquisitions. Even if clients do obtain information from the investment bank about the takeover, they are careful not to trade through the bank's brokerage arm.

2.1.2 Alternate measures of trading prior to takeovers. One natural reaction to our findings in the previous section is that it is obvious that investment banks do not allow trading based on their connections. However, as discussed previously, several other articles find explicit evidence of such trading. The most closely related article on takeovers is concurrent work by Jegadeesh and Tang (2010), which looks at similar broker-level trading patterns and documents significant buying by clients of target advisors (but not acquirer advisors) during the one month before takeovers. They also find heightened activity during the last seven days prior to takeover announcements. They employ Able/Noser data from the Plexus Group clients, which are predominantly large mutual fund and pension fund families that are interested in tracking their trading costs. Despite anonymity, they are able to track each institutional family to the brokerage house they often trade through. Able/Noser data account for only 8% of total trading volume, whereas our data cover 77.8% of NASDAQ trading volume. Other than different samples, we explore possible reasons for the discrepancy between our findings.

The first possibility is differences in test design.[15] We adopt their test design and report the imbalances in the left panel of Table 3, which shows that neither acquirer nor target advisors are significant buyers.[16] We further compute investment returns for takeover advisors following Jegadeesh and Tang (2010) and report the results in the right panel of Table 3, where none of the investment returns are significantly positive. These results suggest that the discrepancy is not due to test design.

15 Jegadeesh and Tang's (2010) test design differs from ours as follows: 1) when we classify takeover advisors, we adjust for investment bank mergers over the sample period, but they do not document adjusting for this; 2) their data only contain brokerage houses that execute institutional trades, while we include all categories of brokerage houses acting as takeover advisors; 3) they adjust imbalances using historical imbalances for the one-year window up to six months before takeovers; and 4) our primary tests on client trading exclude the top 100 market makers, according to the 1997–2002 trading volume, whereas they include all brokerage houses in their sample.

16 Specifically, we 1) do not adjust for investment bank mergers; 2) include institutional brokerage houses only (brokers classified as institutions, largest I-banks, hedge funds, or derivative traders, using the approach in Griffin et al. 2011); 3) report historically adjusted imbalances; and 4) do not exclude the top 100 market makers in terms of trading volume.

Table 3
Trading by takeover advisors prior to takeover announcements: Using the test design of Jegadeesh and Tang (2010)

	Imbalances				Investment Returns			
	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]
All advisors	−0.054	−0.103	−0.190	−0.251	−0.704	−0.278	−0.473	−0.425
Unadjusted *p*-value	(0.10)	(0.04)	(0.02)	(0.02)	(0.13)	(0.62)	(0.42)	(0.49)
Holm-Bonferroni *p*-value	(1.00)	(0.93)	(0.41)	(0.54)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.27)	(0.26)	(0.21)	(0.20)	(0.31)	(0.70)	(0.72)	(0.66)
Target advisors	−0.038	−0.108	−0.336	−0.548	−0.612	−0.555	−0.842	−0.013
Unadjusted *p*-value	(0.53)	(0.31)	(0.05)	(0.01)	(0.43)	(0.53)	(0.32)	(0.99)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(0.31)	(1.00)	(1.00)	(1.00)	(0.99)
FDR *p*-value	(0.64)	(0.68)	(0.24)	(0.31)	(0.68)	(0.67)	(0.65)	(0.99)
Acquirer advisors	−0.036	−0.046	−0.096	−0.016	−0.273	−0.480	0.976	−0.677
Unadjusted *p*-value	(0.06)	(0.08)	(0.09)	(0.89)	(0.63)	(0.47)	(0.33)	(0.46)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.25)	(0.28)	(0.27)	(0.93)	(0.69)	(0.66)	(0.62)	(0.69)

This table reports average client imbalances prior to takeover announcements for brokerage houses acting as takeover advisors. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We further calculate daily adjusted imbalance for a broker as the broker's daily imbalance minus its average daily imbalance for the firm during the [−360,−121] window. We scale the imbalances by 1,000. For comparison between our results and those of Jegadeesh and Tang (2010), we only include brokerage houses that are classified as institutions, largest I-banks, hedge funds, or derivative traders using the approach in Griffin et al. (2011). We do not adjust for broker mergers or exclude the top 100 market makers, according to the 1997–2002 trading volume. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We report results for all advisors, advisors of target firms, and advisors of acquirer firms. We also report average investment returns based on adjusted imbalances. We follow the same approach as Jegadeesh and Tang (2010) to calculate investment returns on client trading during the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows for each advisor prior to each takeover. Next, for the [−2,−1] and [−5,−1] windows, we first calculate average investment returns for all announcements during each week (Thursday to Wednesday) from January 2, 1997, to December 31, 2002, and then report time-series means and unadjusted *p*-values for the average weekly investment returns. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with four lags. We calculate average investment returns for the [−10,−1] and [−20,−1] windows in the same manner using two- and four-week windows, respectively. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

A second possibility is differences in sample construction. Specifically, Jegadeesh and Tang (2010) directly use SDC announcement dates as event dates, whereas we choose the earliest of the announcement dates from four data sources, including SDC, Mergerstat, CorpfinWorldwide, and LexisNexis. Our conservative approach enables us to focus on trading before the public announcement of a merger. However, in unreported results, we repeat our tests using SDC dates and find no significant buying by takeover advisors.

Third, it is possible that Able/Noser data used by Jegadeesh and Tang (2010) are tilted toward certain large brokerage houses that trade on information obtained as target advisors. We examine the trading activity across target advisors with at least five deals in our sample. Figure 1 sorts the brokerage

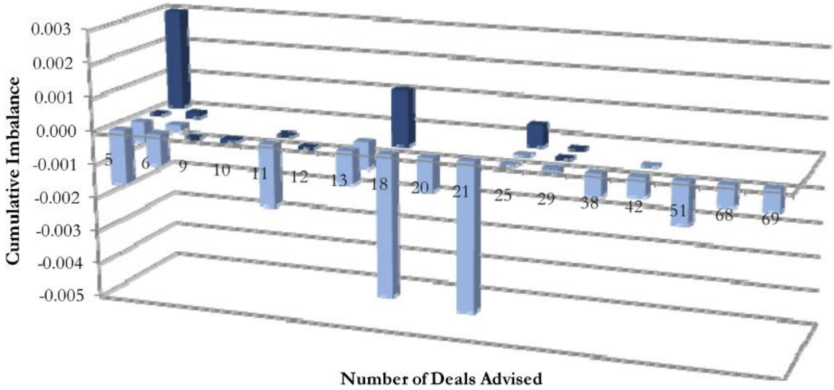

Figure 1
Client imbalances of target advisors by number of deals advised
This figure plots average cumulative client imbalances of target advisors prior to takeover announcements at the brokerage house level. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. We first calculate cumulative client imbalances for each target advisor during the [−20,−1] window prior to each takeover. We then classify brokerage houses according to the number of takeovers for which they act as target advisors and plot the average cumulative imbalances for each group. For example, the leftmost group reports average imbalances of the four brokers that advised five deals. For comparison between our results and those of Jegadeesh and Tang (2010), 1) we plot adjusted imbalances, where daily adjusted imbalance for a broker is the broker's daily imbalance minus its average daily imbalance for the firm during the [−360,−121] window; 2) we only include brokerage houses that are classified as institutions, largest I-banks, hedge funds, or derivative traders; 3) we do not adjust for broker mergers; and 4) we do not exclude the top 100 market makers, according to the 1997–2002 trading volume. We only include brokerage houses that act as target advisors for at least five takeovers. Positive imbalances are marked dark blue, and negative imbalances are marked light blue.

houses according to the number of deals advised and shows that only two advisors have large positive average client imbalances during the twenty-day window ahead of takeovers. Additionally, the net buying of brokerage house clients is not increasing in the number of deals advised by the investment bank.

Finally, since the Able/Noser data are from large institutions, such as pension funds and mutual funds, which would like to track their transaction costs, brokerage houses may reward these large clients at the expense of other clients. We partially investigate this possibility by examining whether large trades through takeover advisors are more informative than are small trades, but we find little evidence to suggest that this is the case. Nevertheless, it is still possible that Able/Noser clients are rewarded with inside information, whereas the average brokerage house client is not. From our discussions with academics using Able/Noser, it seems that the data do not capture hedge funds, which seem likely candidates if brokerage houses were passing information to their most sophisticated clients.

To summarize, our investigation suggests that the discrepancy between our findings and those of Jegadeesh and Tang (2010) is not likely driven by test design. Even though date identification does not affect our sample, we cannot rule out date identification or other differences in the events as being a factor for different results. Another likely cause for the discrepancy is the differences between the subsample of Able/Noser institutions and the broader set of brokerages in our NASDAQ data.

2.1.3 Trading by IPO underwriters prior to takeover and earnings announcements. We now turn to examining whether brokerage houses are more apt to use their information in settings not directly related to their deals. Specifically, brokerage houses may still be in contact with management of a firm with which they had a previous investment banking relationship, learn of impending events about the firm, and tip the information to their clients. We start by examining client trading prior to takeover and earnings announcements for the brokerage houses that were part of the firm's IPO underwriting syndicate. Table 4, Panel A, shows that clients of IPO underwriters as a whole are not significant buyers prior to takeover announcements. We then examine the trading activity ahead of takeovers for underwriters of recent IPOs (one year within the takeover announcement) and find evidence of significant buying (at the 0.05 level) for the ten- and twenty-day windows, before adjusting for multiple testing. However, the *p*-values are widely insignificant when we use either Holm-Bonferroni or FDR *p*-values to account for the multiple tests in the table. Book runners of IPOs may have more access to corporate insiders and information than do comanagers and syndicate members. We therefore examine book runners separately but find that their client imbalances are negative and insignificantly different from zero.[17] We also test for the possibility of differences in behavior across brokers by examining if any brokerage house clients exhibit evidence of consistent net buying prior to announcements. Inconsistent with informed trading, past profitable trading activity ahead of takeovers does not translate into future trading profits ahead of takeovers.[18]

We then investigate trading by clients of IPO underwriters prior to earnings announcements. Table 4, Panel B, presents imbalances prior to four categories of earnings announcements according to the announcement return. Inconsistent with informed trading, imbalances are not in the same direction as the pending announcement for any category, even when using unadjusted *p*-values. There is some evidence with unadjusted *p*-values that clients of IPO underwriters actually buy prior to negative announcements, but these numbers become insignificant when we control for multiple testing. Since the imbalances are reported for four earnings-announcement categories, we

[17] Although less privy to information, comanagers and other syndicate members may be subject to less scrutiny. We separately examine comanagers and syndicate members and find that their imbalances are insignificant as well.

[18] Since recent IPO underwriters exhibit some evidence of buying prior to takeovers, we further divide them into book runners, comanagers, and syndicate members. We find that the strongest buying is from the comanager group, which has significant buying in the two-day window (p-value $= 0.04$) and marginally significant buying in the five- and twenty-day windows (p-value $= 0.07$) before adjustment but not after adjusting for multiple testing.

Table 4
Trading by IPO underwriters prior to takeover and earnings announcements

Panel A: Imbalances Prior to Takeover Announcements

	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]
All underwriters	−0.013	0.009	−0.007	−0.031
Unadjusted *p*-value	(0.52)	(0.87)	(0.90)	(0.78)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.70)	(0.92)	(0.94)	(0.86)
Recent underwriters	0.101	0.385	0.334	1.057
Unadjusted *p*-value	(0.06)	(0.07)	(0.04)	(0.03)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.58)	(0.57)	(0.60)	(0.97)
Book runners	−0.039	−0.104	−0.138	−0.382
Unadjusted *p*-value	(0.24)	(0.18)	(0.23)	(0.13)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.59)	(0.49)	(0.59)	(0.46)
Past profitable underwriters	−0.006	−0.013	−0.009	0.020
Unadjusted *p*-value	(0.07)	(0.24)	(0.59)	(0.49)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.62)	(0.57)	(0.75)	(0.71)

Panel B: Imbalances and Investment Returns Prior to Earnings Announcements

	Imbalances				
	Ret <–5%	−5%<Ret<0%	0% <Ret <5%	Ret >5%	Investment Ret. (%)
Imbalance [–2,–1]	0.048	0.013	0.025	0.037	−0.24
Unadjusted *p*-value	(0.05)	(0.54)	(0.56)	(0.16)	(0.52)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.55)	(0.73)	(0.74)	(0.50)	(0.71)
Imbalance [–5,–1]	0.128	0.062	0.008	0.061	0.14
Unadjusted *p*-value	(0.03)	(0.11)	(0.85)	(0.12)	(0.73)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(0.51)	(0.91)	(0.48)	(0.85)
Imbalance [–10,–1]	0.093	0.069	0.008	0.076	−0.07
Unadjusted *p*-value	(0.10)	(0.17)	(0.79)	(0.26)	(0.86)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.58)	(0.50)	(0.87)	(0.54)	(0.91)
Imbalance [–20,–1]	0.075	0.093	0.033	0.051	−0.67
Unadjusted *p*-value	(0.23)	(0.12)	(0.56)	(0.26)	(0.04)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.61)	(0.50)	(0.73)	(0.55)	(0.70)

Panel C: Imbalances Prior to Earnings Announcements: Sub-Groups

	Ret <–5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%	Ret <–5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%
	Recent IPO Underwriters				IPO Book Runners			
Imbalance [–2,–1]	0.007	0.031	−0.040	0.013	0.081	−0.068	−0.151	0.097
Unadjusted *p*-value	(0.76)	(0.27)	(0.38)	(0.38)	(0.17)	(0.27)	(0.25)	(0.22)
FDR *p*-value	(0.87)	(0.52)	(0.62)	(0.60)	(0.52)	(0.53)	(0.55)	(0.59)
Imbalance [–5,–1]	0.066	0.104	0.031	0.063	0.294	0.112	−0.066	0.077
Unadjusted *p*-value	(0.11)	(0.08)	(0.64)	(0.11)	(0.06)	(0.39)	(0.61)	(0.47)
FDR *p*-value	(0.53)	(0.55)	(0.76)	(0.57)	(0.53)	(0.60)	(0.76)	(0.69)
Imbalance [–10,–1]	0.067	0.095	−0.043	0.067	0.405	0.177	0.022	−0.080
Unadjusted *p*-value	(0.29)	(0.11)	(0.34)	(0.34)	(0.12)	(0.45)	(0.92)	(0.76)
FDR *p*-value	(0.53)	(0.61)	(0.58)	(0.59)	(0.47)	(0.69)	(0.94)	(0.87)
Imbalance [–20,–1]	0.000	0.096	−0.001	0.055	0.199	0.414	0.272	−0.082
Unadjusted *p*-value	(1.00)	(0.25)	(0.99)	(0.50)	(0.46)	(0.34)	(0.50)	(0.72)
FDR *p*-value	(1.00)	(0.55)	(1.00)	(0.71)	(0.69)	(0.59)	(0.70)	(0.85)

(*continued*)

Table 4
Continued

	Past Profitable IPO Underwriters			
	Ret <–5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%
Imbalance [–2,–1]	0.083	0.118	−0.060	−0.008
Unadjusted *p*-value	(0.15)	(0.24)	(0.25)	(0.63)
FDR *p*-value	(0.49)	(0.56)	(0.56)	(0.77)
Imbalance [–5,–1]	0.324	0.365	−0.197	−0.032
Unadjusted *p*-value	(0.09)	(0.28)	(0.11)	(0.14)
FDR *p*-value	(0.57)	(0.54)	(0.55)	(0.49)
Imbalance [–10,–1]	0.632	0.737	−0.220	−0.240
Unadjusted *p*-value	(0.13)	(0.33)	(0.38)	(0.03)
FDR *p*-value	(0.45)	(0.59)	(0.61)	(1.00)
Imbalance [–20,–1]	0.218	1.008	0.144	−0.515
Unadjusted *p*-value	(0.62)	(0.30)	(0.77)	(0.04)
FDR *p*-value	(0.76)	(0.55)	(0.86)	(0.79)

Panel A reports average client imbalances prior to takeover announcements for brokerage houses acting as IPO underwriters. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We report results for all IPO underwriters, recent IPO underwriters (IPOs within one year of takeover announcements), IPO book runners, and past profitable IPO underwriters. For year y, we identify past profitable underwriters as those whose clients traded at least once during the twenty-day window prior to takeovers from 1997 to $y-1$ for which they acted as IPO underwriters for the target firm and have positive twenty-day client imbalances prior to all such takeover announcements. Panel B reports average imbalances and the corresponding value-weighted average investment returns prior to earnings announcements for IPO underwriters. Earnings announcements are classified into four groups according to two-day excess returns for the [0,1] window, where returns are in excess of NASD index return: those with announcement returns below −5%, between −5% and 0%, between 0% and 5%, and greater than 5%. Investment return is total dollar gain/loss divided by total dollar investment. To calculate dollar gain/loss for an announcement, we first multiply the daily dollar imbalance for each day with the buy-and-hold excess return from the next day until one day after the announcement day (day 1) and then sum the products across days in the selected window. Daily dollar imbalance for a stock is the difference between buy and sell volumes multiplied by the closing price for the day. To be conservative, we assume that buy and sell trades occur at the end of the trading day. Total dollar investment is the greater of the sum of daily dollar buy imbalances and the sum of daily dollar sell imbalances over the selected window. For average investment returns for the [−2,−1] and [−5,−1] windows, we first calculate value-weighted average investment returns for all announcements during each calendar week (Thursday to Wednesday) from January 2, 1997, using all announcement returns for that week in the sample of [−2,−1] and [−5,−1] windows, respectively, where the weights are dollar investments, and then report time-series means and unadjusted *p*-values for the average weekly investment returns. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with four lags. We calculate average investment returns for the [−10,−1] and [−20,−1] windows in the same manner using two- and four-week calendar windows, respectively. Panel C reports average imbalances prior to earnings announcements for recent IPO underwriters, IPO book runners, and past profitable IPO underwriters. To identify past profitable underwriters for year y, we first sort underwriters into terciles of success ratio (percentage of imbalances in the right direction) for their large twenty-day client imbalances (dollar imbalances above $100,000) prior to earnings announcements in year $y-1$. We require a broker to have at least ten large client imbalances. We then keep the top tercile of success ratio and further sort into terciles of trading frequency, which is the ratio of the number of large twenty-day client imbalances to the total number of twenty-day client imbalances (including zero imbalances) for underwriters prior to earnings announcements in $y-1$. We then identify underwriters in the top tercile of trading frequency as past profitable underwriters. We exclude the top 100 market makers, according to the 1997–2002 trading volume to control for liquidity trading. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

further combine inferences by examining investment returns. Consistent with results on imbalances, the value-weighted investment returns in Panel B of Table 4 are insignificant for all the windows examined. Table 4, Panel C, presents client imbalances prior to four categories of earnings announcements for underwriters of recent IPOs (one year within the earnings announcement), book runners, and past profitable underwriters. None of these groups' clients trade in the right direction prior to earnings announcements.

2.1.4 Trading by SEO underwriters prior to takeover and earnings announcements. Table 5 investigates whether clients of brokers that have

Table 5
Trading by SEO underwriters prior to takeover and earnings announcements

Panel A: Imbalances Prior to Takeover Announcements

	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]
All underwriters	−0.036	−0.062	−0.108	−0.216
Unadjusted *p*-value	(0.12)	(0.17)	(0.25)	(0.14)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.81)	(0.70)	(0.64)	(0.73)
Recent underwriters	−0.022	−0.104	−0.217	−0.274
Unadjusted *p*-value	(0.38)	(0.28)	(0.25)	(0.32)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.68)	(0.61)	(0.63)	(0.65)
Book runners	−0.087	−0.269	−0.351	−0.753
Unadjusted *p*-value	(0.16)	(0.03)	(0.05)	(0.02)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.75)	(1.00)	(1.00)	(1.00)
Past profitable underwriters	−0.003	−0.015	−0.064	−0.070
Unadjusted *p*-value	(0.60)	(0.41)	(0.17)	(0.32)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.75)	(0.69)	(0.66)	(0.64)

Panel B: Imbalances and Investment Returns Prior to Earnings Announcements

	Imbalances				
	Ret <−5%	−5%<Ret<0%	0% <Ret <5%	Ret >5%	Investment Ret. (%)
Imbalance [−2,−1]	−0.012	−0.015	−0.026	0.023	0.61
Unadjusted *p*-value	(0.45)	(0.12)	(0.26)	(0.17)	(0.06)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.70)	(0.79)	(0.64)	(0.63)	(0.98)
Imbalance [–5,–1]	0.063	0.096	−0.013	0.077	0.41
Unadjusted *p*-value	(0.48)	(0.32)	(0.60)	(0.20)	(0.16)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.73)	(0.62)	(0.76)	(0.63)	(0.72)
Imbalance [–10,–1]	0.069	0.038	−0.043	0.171	0.47
Unadjusted *p*-value	(0.63)	(0.49)	(0.20)	(0.07)	(0.07)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.77)	(0.72)	(0.63)	(0.92)	(0.76)
Imbalance [–20,–1]	−0.019	0.055	−0.001	0.340	0.37
Unadjusted *p*-value	(0.76)	(0.33)	(0.98)	(0.10)	(0.28)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.90)	(0.60)	(0.99)	(0.89)	(0.64)

(*continued*)

Table 5
Continued

Panel C: Imbalances Prior to Earnings Announcements: Sub-Groups

	Ret < –5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%	Ret <–5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%
	Recent SEO Underwriters				SEO Book Runners			
Imbalance [–2,–1]	−0.003	−0.039	−0.004	0.012	−0.052	−0.039	−0.048	0.009
Unadjusted *p*-value	(0.95)	(0.05)	(0.94)	(0.60)	(0.22)	(0.17)	(0.14)	(0.71)
FDR *p*-value	(0.97)	(1.00)	(0.97)	(0.76)	(0.64)	(0.68)	(0.69)	(0.85)
Imbalance [–5,–1]	0.099	0.346	0.002	0.098	−0.035	0.296	−0.113	0.085
Unadjusted *p*-value	(0.44)	(0.32)	(0.98)	(0.33)	(0.78)	(0.28)	(0.13)	(0.28)
FDR *p*-value	(0.71)	(0.63)	(0.98)	(0.61)	(0.91)	(0.62)	(0.80)	(0.60)
Imbalance [–10,–1]	0.090	0.019	−0.081	0.527	−0.157	0.120	−0.127	0.290
Unadjusted *p*-value	(0.51)	(0.85)	(0.57)	(0.24)	(0.40)	(0.59)	(0.24)	(0.11)
FDR *p*-value	(0.73)	(0.95)	(0.75)	(0.66)	(0.69)	(0.76)	(0.68)	(0.95)
Imbalance [–20,–1]	0.198	0.077	−0.025	1.107	−0.141	0.121	−0.200	0.441
Unadjusted *p*-value	(0.14)	(0.46)	(0.90)	(0.21)	(0.42)	(0.54)	(0.27)	(0.26)
FDR *p*-value	(0.77)	(0.70)	(0.99)	(0.64)	(0.70)	(0.75)	(0.64)	(0.63)
	Past Profitable SEO Underwriters							
Imbalance [–2,–1]	−0.041	0.003	0.075	0.018				
Unadjusted *p*-value	(0.18)	(0.92)	(0.45)	(0.43)				
FDR *p*-value	(0.65)	(0.97)	(0.71)	(0.72)				
Imbalance [–5,–1]	0.008	−0.084	0.150	0.016				
Unadjusted *p*-value	(0.92)	(0.19)	(0.55)	(0.80)				
FDR *p*-value	(0.96)	(0.63)	(0.75)	(0.91)				
Imbalance [–10,–1]	0.081	−0.113	−0.033	−0.026				
Unadjusted *p*-value	(0.55)	(0.12)	(0.90)	(0.84)				
FDR *p*-value	(0.73)	(0.89)	(0.99)	(0.96)				
Imbalance [–20,–1]	0.159	−0.208	−0.045	0.095				
Unadjusted *p*-value	(0.49)	(0.07)	(0.91)	(0.55)				
FDR *p*-value	(0.72)	(0.82)	(0.98)	(0.74)				

Panel A reports average client imbalances prior to takeover announcements for brokerage houses acting as SEO underwriters. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We report results for all SEO underwriters, recent SEO underwriters (SEOs within one year of takeover announcements), SEO book runners, and past profitable SEO underwriters. For year y, we identify past profitable underwriters as those whose clients traded at least once during the twenty-day window prior to takeovers from 1997 to $y-1$ for which they acted as SEO underwriters for the target firm and have positive twenty-day client imbalances prior to all such takeover announcements. Panel B reports average imbalances and the corresponding value-weighted average investment returns prior to earnings announcements for SEO underwriters. We calculate investment returns using the approach described in the header of Table 4. Panel C reports average imbalances prior to earnings announcements for recent SEO underwriters, SEO book runners, and past profitable SEO underwriters. To identify past profitable underwriters for year y, we first sort underwriters into terciles of success ratio (percentage of imbalances in the right direction) for their large twenty-day client imbalances (dollar imbalances above $100,000) prior to earnings announcements in year $y-1$. We require a broker to have at least ten large client imbalances. We then keep the top tercile of success ratio and further sort into terciles of trading frequency, which is the ratio of the number of large twenty-day client imbalances to the total number of twenty-day client imbalances (including zero imbalances) for underwriters prior to earnings announcements in $y-1$. We then identify underwriters in the top tercile of trading frequency as past profitable underwriters. We exclude the top 100 market makers, according to the 1997–2002 trading volume, to control for liquidity trading. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

a previous SEO underwriting relationship with a firm exhibit profitable trading prior to takeover and earnings announcements. Panel A shows that clients of SEO underwriters are not significant buyers prior to takeovers. In addition, clients of various subgroups of SEO underwriters, including underwriters of recent SEOs (within one year of takeovers), SEO book runners, and past profitable underwriters, are not informed of takeovers, either.[19]

Panel B examines trading prior to earnings announcements and presents evidence that the client trades of SEO underwriters are not informed. The strongest evidence for trading in the right direction is in the ten-day window, where client imbalance is 0.0171% (unadjusted p-value $=$ 0.07) for large positive announcements, but it becomes insignificant when using both Holm-Bonferroni and FDR p-values. The value-weighted investment return for client trades of SEO underwriters is marginally positive for the two- and ten-day windows using unadjusted p-values (0.06 and 0.07, respectively) but insignificant after considering multiple testing. Panel C further shows that most of the imbalances prior to earnings announcements are insignificant for recent SEO underwriters, SEO book runners, and past profitable underwriters.

2.1.5 Trading by lenders prior to takeover and earnings announcements. In Table 6, we study client trading for brokerage houses that act as lenders. We focus on lenders with ongoing loan contracts with a firm during any part of the three-month period prior to the announcement.[20] Panel A of Table 6 shows that none of the imbalances prior to takeover announcements are significantly positive. We further divide lenders into lead lenders and loan participants, because they can have different roles in information production (Bharath et al. 2007; Sufi 2007; Acharya and Johnson 2010). Panel A further shows that neither those two subgroups' clients nor clients of lenders with past trading profits buy a significant amount prior to takeovers.

Panel B of Table 6 presents client imbalances for lenders prior to earnings announcements and shows no evidence that clients of lenders trade in the direction of the announcement. Panel B also presents value-weighted investment returns on trading by lenders prior to earnings announcements, which are not significantly positive. Panel C further presents client imbalances prior to earnings announcements for lead lenders, loan participants, and lenders with past trading profits separately. There is no evidence to support that trading by any subgroup is informed prior to earnings announcements.

A potential explanation for the findings in Table 6 is that lenders might have sold their loans in the secondary market and therefore stopped acquiring information from the borrowing firm (Ivashina and Sun 2011). However, this

[19] We also examine trading by SEO comanagers and syndicate members and find little evidence that their clients are informed prior to takeovers.

[20] Our results are similar when we use a one- or six-month period prior to the announcement.

explanation is unlikely, because Nandy and Shao (2010) examine bank loans, which account for the vast majority of syndicated loans, and show that only 6% are traded in the secondary market.[21] Nevertheless, we examine this possibility by requiring lenders to enter a new loan contract during the three-month period prior to earnings announcements and find similar inferences in unreported results.

Table 6
Trading by lenders prior to takeover and earnings announcements

Panel A: Imbalances Prior to Takeover Announcements

	[−2, −1]	[−5, −1]	[−10, −1]	[−20, −1]
All lenders	−0.006	−0.003	0.018	0.014
Unadjusted *p*-value	(0.11)	(0.76)	(0.46)	(0.71)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
Lead lenders	−0.009	−0.002	0.027	0.019
Unadjusted *p*-value	(0.16)	(0.86)	(0.58)	(0.79)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(0.98)	(1.00)	(1.00)
Participating lenders	0.000	0.005	0.030	0.017
Unadjusted *p*-value	(0.97)	(0.60)	(0.31)	(0.56)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(0.99)	(1.00)	(1.00)
Past profitable lenders	0.013	0.024	0.030	0.042
Unadjusted *p*-value	(0.34)	(0.25)	(0.18)	(0.15)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)

Panel B: Imbalances and Investment Returns Prior to Earnings Announcements

	Imbalances				
	Ret <−5%	−5%<Ret<0%	0% <Ret <5%	Ret >5%	Investment Ret. (%)
Imbalance [–2,–1]	0.003	0.001	0.000	−0.001	−0.12
Unadjusted *p*-value	(0.36)	(0.60)	(0.73)	(0.54)	(0.74)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–5,–1]	0.004	−0.001	0.000	0.005	−0.05
Unadjusted *p*-value	(0.58)	(0.84)	(0.89)	(0.46)	(0.88)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(1.00)	(0.97)	(1.00)	(0.98)
Imbalance [–10,–1]	−0.005	−0.008	0.001	0.011	−0.06
Unadjusted *p*-value	(0.37)	(0.20)	(0.83)	(0.40)	(0.86)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	1.00)	(1.00)	(1.00)	(0.99)
Imbalance [–20,–1]	−0.008	−0.024	0.004	0.011	−0.61
Unadjusted *p*-value	(0.27)	(0.11)	(0.26)	(0.50)	(0.13)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)

(*continued*)

[21] Nandy and Shao (2010) argue that, compared to bank loans, lenders of loans that are syndicated only to institutions have a stronger motivation for information acquisition. We therefore examine trading prior to earnings announcements for lenders of institutional loans but find no evidence of informed trading for those lenders in unreported results.

Table 6
Continued

Panel C: Imbalances Prior to Earnings Announcements: Sub-Groups

	Ret < –5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%	Ret <–5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%
	Lead Lenders				Participating Lenders			
Imbalance [–2,–1]	0.003	0.002	−0.001	−0.003	0.000	0.000	−0.001	−0.001
Unadjusted *p*-value	(0.52)	(0.52)	(0.59)	(0.30)	(0.79)	(0.89)	(0.67)	(0.87)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(0.95)	(1.00)	(0.97)
Imbalance [–5,–1]	0.002	0.000	−0.004	−0.004	0.000	−0.001	0.003	0.007
Unadjusted *p*-value	(0.83)	(0.98)	(0.32)	(0.36)	(0.82)	(0.76)	(0.09)	(0.41)
FDR *p*-value	(1.00)	(0.99)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–10,–1]	−0.006	−0.009	0.000	0.000	−0.003	−0.006	0.004	0.016
Unadjusted *p*-value	(0.44)	(0.37)	(1.00)	(0.98)	(0.31)	(0.18)	(0.13)	(0.48)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–20,–1]	−0.018	−0.040	0.004	0.003	0.002	−0.001	0.007	0.015
Unadjusted *p*-value	(0.22)	(0.13)	(0.55)	(0.80)	(0.81)	(0.89)	(0.05)	(0.63)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(0.96)	(1.00)	(1.00)

	Ret < –5%	−5%< Ret<0%	0% < Ret <5%	Ret >5%
	Past Profitable Lenders			
Imbalance [–2,–1]	−0.002	0.006	0.003	0.002
Unadjusted *p*-value	(0.86)	(0.41)	(0.25)	(0.47)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–5,–1]	−0.005	0.026	0.004	0.011
Unadjusted *p*-value	(0.67)	(0.10)	(0.53)	(0.13)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–10,–1]	−0.007	0.040	0.005	0.006
Unadjusted *p*-value	(0.68)	(0.05)	(0.54)	(0.66)
FDR *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
Imbalance [–20,–1]	−0.003	0.057	−0.003	0.010
Unadjusted *p*-value	(0.93)	(0.17)	(0.74)	(0.73)
FDR *p*-value	(0.98)	(1.00)	(1.00)	(1.00)

Panel A reports average client imbalances prior to takeover announcements for brokerage houses acting as lenders of ongoing loans during the three-month period prior to the announcement date. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We report results for all lenders, lead lenders, participating lenders, and past profitable lenders. For year *y*, we identify past profitable lenders as those whose clients traded at least once during the twenty-day window prior to takeovers from 1997 to $y-1$ for which they are lenders to the target firm and have positive twenty-day client imbalances prior to all such takeover announcements. Panel B reports average imbalances and the corresponding value-weighted average investment returns prior to earnings announcements for lenders. We calculate investment returns using the approach described in the header of Table 4. Panel C reports average imbalances prior to earnings announcements for lead lenders, participating lenders, and past profitable lenders. To identify past profitable lenders for year *y*, we first sort lenders into terciles of success ratio (percentage of imbalances in the right direction) for their large twenty-day client imbalances (dollar imbalances above $100,000) prior to earnings announcements in year $y-1$. We require a broker to have at least ten large client imbalances. We then keep the top tercile of success ratio and further sort into terciles of trading frequency, which is the ratio of the number of large twenty-day client imbalances to the total number of twenty-day client imbalances (including zero imbalances) for lenders prior to earnings announcements in $y-1$. We then identify lenders in the top tercile of trading frequency as past profitable lenders. We exclude the top 100 market makers, according to the 1997–2002 trading volume to control for liquidity trading. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

3. Historically Connected Trading and Differential Trading Abilities

In this section, we examine relationships between brokerage houses and firms through trading activity. Specifically, we address two questions. First, do clients of brokerage houses who have traded profitably in a "linked" firm prior to past earnings announcements continue to make profits prior to future announcements? Second, is there evidence of differential trading ability across brokerage house clients prior to major announcements?

3.1 Trading by historically connected brokerage houses

We examine steady connections that a broker builds with a firm. For example, a hedge fund could develop a long-lasting relationship with a firm to obtain inside information prior to major announcements. Since most hedge funds have steady trading relationships with a main broker during our sample period, we could observe that client trades through the hedge fund's broker are consistently informed prior to the firm's earnings announcements.[22]

Each year, we classify a broker as historically connected to a firm if the broker's clients trade during the five-day window prior to at least two earnings announcements by the firm in the previous year and if they trade in the same direction as the two-day excess announcement return each time. We examine earnings announcements because we need frequent events to evaluate steady linkages between brokerage houses and firms around informational events. Table 7 examines short-term trading prior to earnings announcements for historically connected brokerage houses. We find that clients of historically connected brokerage houses do not trade in the same direction as earnings announcements. Table 7 further shows that the corresponding investment returns are small and insignificant. Therefore, there does not seem to be a group of brokers whose clients consistently make profits prior to announcements by their "linked" firms.

3.2 Persistence in trading profits

Clients of some brokerage houses might consistently predict major events better than others. This could be because they collect and trade on inside information more aggressively than do others. In this general analysis of profitability across brokerage houses ahead of major events, we use past trading profits in all earnings announcements as a signal of potential informedness.

We sort brokerage houses into four groups according to their total dollar gain/loss on client trading prior to all earnings announcements in year $y-1$ and then calculate average gain/loss for each group in year y. Panel A of Table 8 shows that profitability of client trades for the ten- and twenty-day windows prior to earnings announcements is increasing in past profitability,

[22] However, a large hedge fund and/or institution may trade with two or more brokers, making it difficult to track down the informed trading.

Table 7
Trading by historically connected brokerage houses prior to earnings announcements

	Imbalances				Investment
	Ret <–5%	−5%<Ret<0%	0% <Ret <5%	Ret >5%	Ret. (%)
Imbalance [–2,–1]	0.009	0.000	0.001	0.020	0.148
Unadjusted *p*-value	(0.04)	(0.90)	(0.72)	(0.33)	(0.45)
Holm-Bonferroni *p*-value	(0.67)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.35)	(0.94)	(0.96)	(1.00)	(1.00)
Imbalance [–5,–1]	0.011	0.002	−0.004	0.006	0.041
Unadjusted *p*-value	(0.02)	(0.60)	(0.61)	(0.27)	(0.82)
Holm-Bonferroni *p*-value	(0.43)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.43)	(1.00)	(0.94)	(1.00)	(0.91)
Imbalance [–10,–1]	−0.005	0.003	0.001	0.002	0.170
Unadjusted *p*-value	(0.72)	(0.52)	(0.96)	(0.64)	(0.31)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(0.96)	(1.00)	(1.00)
FDR *p*-value	(0.90)	(1.00)	(0.96)	(0.91)	(1.00)
Imbalance [–20,–1]	0.008	0.002	0.009	−0.005	0.192
Unadjusted *p*-value	(0.13)	(0.78)	(0.53)	(0.54)	(0.41)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.88)	(0.92)	(1.00)	(0.97)	(1.00)

This table reports average client imbalances prior to earnings announcements for historically connected brokerage houses. Earnings announcements are classified into four groups according to two-day excess returns for the [0,1] window, where returns are in excess of NASD index return: those with announcement returns below −5%, between −5% and 0%, between 0% and 5%, and greater than 5%. A brokerage house is classified as historically connected to a firm if that broker traded at least twice prior to the firm's earnings announcements in the previous year and traded in the same direction as the announcement return for each announcement. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. We further report value-weighted average investment returns on trading prior to earnings announcements for historically connected brokerage houses. We calculate investment returns using the approach described in the header of Table 4. We exclude the top 100 market makers, according to the 1997–2002 trading volume, to control for liquidity trading. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

and differences have significant unadjusted *p*-values. After adjusting for multiple testing by considering all the specifications in the table, the difference for the twenty-day window remains significant. This result provides evidence of a dichotomy within the trading universe, where those who traded profitably in the past continue to profit at the expense of the past losers. We further use average investment returns on client trades as an alternative measure for examining performance persistence. Brokerage houses are also sorted according to average investment returns during the previous year. Panel B of Table 8 shows that differences in investment returns are also positive. Consistent with Panel A, the difference is significant for the ten- and twenty-day windows using unadjusted *p*-values and the twenty-day window (at the 0.10 level) according to the FDR *p*-value that controls for multiple testing.

Table 8
Persistence in performance of brokerage houses: Trading prior to earnings announcements

	Loser Broker	2	3	Winner Broker	W–L	Unadjusted p-value	Bonferroni-Holm p-value	FDR p-value
Panel A: Annual Dollar Gain/Loss for Brokers Sorted by Past Performance ($1,000)								
Trading[–2,–1]	−294.94	11.28	34.92	−358.74	−63.81	(0.80)	(0.80)	(0.80)
Trading[–5,–1]	−599.58	32.71	34.51	114.20	713.78	(0.13)	(0.53)	(0.21)
Trading[–10,–1]	−1, 411.53	125.49	141.87	449.78	1, 861.31	(0.04)	(0.25)	(0.11)
Trading[–20,–1]	−1, 752.00	−31.98	92.30	848.59	**2,600.59**	**(0.00)**	**(0.02)**	**(0.02)**
Panel B: VW Investment Returns for Brokers Sorted by Past Performance (%)								
Trading[–2,–1]	−0.14	−0.03	−0.13	0.05	0.20	(0.14)	(0.43)	(0.19)
Trading[–5,–1]	−0.07	−0.09	0.05	0.10	0.16	(0.15)	(0.30)	(0.17)
Trading[–10,–1]	−0.20	−0.08	0.04	0.22	0.42	(0.03)	(0.19)	(0.11)
Trading[–20,–1]	−0.06	−0.14	0.13	0.22	0.28	(0.05)	(0.23)	(0.09)

Panel A reports dollar gain/loss on trading prior to earnings announcements for brokerage houses sorted by past profitability. To calculate dollar gain/loss for an announcement, we first multiply the daily dollar imbalance for each day with the buy-and-hold excess return from the next day until one day after the announcement day (day 1), and then sum the products across days in the selected window. Daily dollar imbalance for a stock is the difference between buy and sell volumes multiplied by the closing price for the day. To be conservative, we assume that buy and sell trades occur at the end of the trading day. Day –1 refers to the last trading day before the announcement. We calculate annual dollar gain/loss for a brokerage house in year y by summing that broker's dollar gains/losses for all announcements in year y. We classify brokerage houses into four groups according to annual dollar profits in year y, and calculate average annual dollar profit in year $y+1$ for each group. We then report time-series means and (unadjusted) p-values for annual dollar profits for each group. The unadjusted p-values are based on t-statistics calculated using Newey-West robust standard errors with four lags. Panel B repeats the tests in Panel A using investment returns. Investment return is total dollar gain/loss divided by total dollar investment, where total dollar investment is the greater of the sum of daily dollar buy imbalances or the sum of daily dollar sell imbalances over the selected window. We first calculate annual value-weighted investment return for a brokerage house in year y, where the weights are total dollar investments. We classify brokerage houses into four groups according to investment returns in year y, and calculate value-weighted investment return in year $y+1$ for each group. We then report time-series means and (unadjusted) p-values for annual investment returns for each group. The unadjusted p-values are based on t-statistics calculated using Newey-West robust standard errors with four lags. To control for outliers, in both panels, we require a brokerage house to trade prior to at least fifty earnings announcements in the previous year. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) p-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR p-values.

In sum, we find evidence that clients of some brokerage houses consistently make greater profits than do others. Since this finding is significant at the twenty-day frequency but not at the shorter frequencies, it is not clear whether the effect is due to insiders who have precise information about the earnings announcement or investors who simply have the ability to process public information more efficiently.

4. Trading by Market Makers and Investor Groups

In this section, we examine two questions that our analysis raises. First, if clients of connected brokerage houses are not making profits ahead of major announcements, is there evidence that brokerage houses themselves are making profits through proprietary trading ahead of such events? Second, who is trading ahead of major announcements? Perhaps institutions are careful and use the inside information in a manner that cannot be obviously linked to their connections. Do institutions in general profit from trading ahead of major announcements?

4.1 Market maker trading by connected brokers

In this section, we examine whether connected brokers make short-term trading profits on their own accounts prior to takeover and earnings announcements. Panel A of Table 9 presents market maker imbalances prior to takeover announcements for takeover advisors, IPO and SEO underwriters, and lenders. None of the subgroups are significant net buyers. Panel B of Table 9 further presents market maker imbalances and investment returns

Table 9
Market maker imbalances for various connection types prior to takeover and earnings announcements

Panel A: Market Maker Imbalances Prior to Takeover Announcements

	[–2,–1]	[–5,–1]	[–10,–1]	[–20,–1]
Takeover advisors	−0.026	−0.032	−0.013	−0.025
Unadjusted *p*-value	(0.12)	(0.31)	(0.70)	(0.53)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.35)	(0.50)	(0.84)	(0.72)
IPO underwriters	−0.022	−0.071	0.033	−0.036
Unadjusted *p*-value	(0.37)	(0.12)	(0.66)	(0.71)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.58)	(0.36)	(0.82)	(0.83)
SEO underwriters	0.000	−0.001	0.071	0.114
Unadjusted *p*-value	(0.99)	(0.98)	(0.19)	(0.10)
Holm-Bonferroni *p*-value	(0.99)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.99)	(0.99)	(0.39)	(0.32)
Lenders	−0.017	−0.027	−0.040	−0.051
Unadjusted *p*-value	(0.17)	(0.08)	(0.03)	(0.05)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.37)	(0.29)	(0.18)	(0.23)

Panel B: Market Maker Imbalances Prior to Earnings Announcements

	Imbalances				
	Ret <–5%	–5% < Ret < 0%	0% < Ret < 5%	Ret > 5%	Investment Ret. (%)
IPO underwriters					
Imbalance [–5,–1]	−0.046	−0.048	0.011	−0.037	−0.027
Unadjusted *p*-value	(0.02)	(0.13)	(0.54)	(0.09)	(0.90)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.16)	(0.36)	(0.70)	(0.31)	(0.95)
Imbalance [–20,–1]	−0.058	−0.023	0.017	−0.089	−0.356
Unadjusted *p*-value	(0.03)	(0.26)	(0.54)	(0.00)	(0.12)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(0.18)	(1.00)
FDR *p*-value	(0.19)	(0.45)	(0.71)	(0.06)	(0.37)
SEO underwriters					
Imbalance [–5,–1]	−0.078	−0.030	−0.004	−0.021	−0.116
Unadjusted *p*-value	(0.24)	(0.20)	(0.85)	(0.27)	(0.45)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.44)	(0.39)	(0.92)	(0.45)	(0.67)

(*continued*)

Table 9
Continued

	Imbalances				Investment
	Ret <–5%	–5%<Ret<0%	0% <Ret <5%	Ret >5%	Ret. (%)
Imbalance [–20,–1]	−0.076	0.003	−0.037	−0.126	0.150
Unadjusted *p*-value	(0.02)	(0.91)	(0.14)	(0.00)	(0.62)
Holm-Bonferroni *p*-value	(0.99)	(1.00)	(1.00)	(0.19)	(1.00)
FDR *p*-value	(0.18)	(0.94)	(0.35)	(0.05)	(0.78)
Lenders					
Imbalance [–5,–1]	−0.009	−0.004	−0.001	−0.005	0.348
Unadjusted *p*-value	(0.02)	(0.14)	(0.77)	(0.48)	(0.20)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.17)	(0.34)	(0.86)	(0.70)	(0.38)
Imbalance [–20,–1]	−0.024	−0.003	−0.039	−0.013	−0.167
Unadjusted *p*-value	(0.08)	(0.73)	(0.07)	(0.27)	(0.40)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.30)	(0.83)	(0.32)	(0.45)	(0.61)
Historically connected brokers					
Imbalance [–5,–1]	**−0.035**	−0.002	0.011	−0.005	0.345
Unadjusted *p*-value	**(0.00)**	(0.81)	(0.20)	(0.66)	(0.14)
Holm-Bonferroni *p*-value	**(0.01)**	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	**(0.01)**	(0.89)	(0.37)	(0.80)	(0.35)
Imbalance [–20,–1]	**−0.056**	−0.036	0.019	−0.038	−0.167
Unadjusted *p*-value	**(0.00)**	(0.01)	(0.19)	(0.05)	(0.49)
Holm-Bonferroni *p*-value	**(0.03)**	(0.71)	(1.00)	(1.00)	(1.00)
FDR *p*-value	**(0.02)**	(0.15)	(0.40)	(0.25)	(0.69)

Panel A reports average market maker imbalances prior to takeover announcements for brokerage houses acting as takeover advisors, IPO underwriters, SEO underwriters, and lenders. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. For the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, we first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows, respectively, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. Day −1 refers to the last trading day before the announcement. To ease comparison, we multiply the average daily imbalances for the [−2,−1], [−5,−1], [−10,−1], and [−20,−1] windows by 2, 5, 10, and 20, respectively. Panel B reports average market maker imbalances and the corresponding value-weighted average investment returns prior to earnings announcements for brokerage houses acting as IPO underwriters, SEO underwriters, lenders, and historically connected brokerage houses. Earnings announcements are classified into four groups according to two-day excess returns for the [0,1] window, where returns are in excess of NASD index return: those with announcement returns below −5%, between −5% and 0%, between 0% and 5%, and greater than 5%. A brokerage house is classified as historically connected to a firm if that broker traded at least twice prior to the firm's earnings announcements in the previous year and traded in the same direction as the announcement return for each announcement. We calculate investment returns using the approach described in the header of Table 4. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

prior to earnings announcements for IPO and SEO underwriters, lenders, and historically connected brokers. For brevity, we only report market maker imbalances and investment returns for the five- and twenty-day windows. The strongest evidence of informed trading lies in the market maker imbalances of historically connected brokerage houses prior to large negative earnings announcements. Both five- and twenty-day imbalances are significantly negative using unadjusted *p*-values, and they remain significant after adjusting for multiple testing. However, the twenty-day imbalances are also negative for large positive earnings announcements, which is consistent with the negative aggregate market maker bias in the data (as discussed in the data section). To address the concern that these imbalances might be affected by the aggregate bias in the data, we examine the market maker imbalances relative to the benchmark imbalances for the [−50,−21] window. In unreported results, we find that the adjusted market maker imbalances of historically connected brokers prior to large negative announcements are insignificantly negative. Additionally, the adjusted market maker trades of other connection types are unrelated to earnings announcements. The investment returns are also insignificant for all the connection categories, including the historically connected brokers. To summarize, there appears to be no evidence that connected market makers are trading profitably.

4.2 Trading by investor groups

Though we examine the most important connections, such as investment banking, lending, and historical linkages, our examination may not fully capture institutional informed trading if a connection is time-varying and comes in forms other than investment banking and lending relationships. Therefore, in this section, we further examine trading by various institutional and individual investor groups at the aggregate level ahead of major announcements.

Several studies have also examined the informedness of short-term trading by institutions. Campbell, Ramadorai, and Schwartz (2009) infer institutional trading using TAQ data and find that institutions trade profitably prior to earnings announcements. Hendershott, Livdan, and Schurhoff (2011) use a data set of NYSE trades and find that institutions trade in the same direction as future earnings announcements from Reuters. In contrast, Kaniel et al. (2012) examine individual trades in NYSE stocks and show that individuals trade profitably ahead of earnings announcements. Kelley and Tetlock (2011) separate individual trades into aggressive and passive orders and find that aggressive orders predict earnings news but passive orders do not. Additionally, Boehmer and Wu (2008) observe that institutional nonprogram imbalances predict next-day returns, and Puckett and Yan (2011) find that short-term institutional trading earns abnormal returns. Boehmer and Kelley (2009) suggest that short-term institutional trading helps improve stock price efficiency.

We use the data constructed by Griffin et al. (2011) to examine trading by different investor groups prior to takeover and earnings announcements. Their four institutional investor groups, four individual investor groups, and a mixed group mainly arise from classifying brokerage house clients according to details posted on broker Web sites regarding their client base. We further adjust investor group imbalances by measuring the imbalance for each firm in excess of an industry/size benchmark imbalance, where we are seeking to control for abnormal buying or selling of a particular set of stocks for extraneous reasons, e.g., institutions moving into or out of small Internet stocks.[23] The industry/size benchmark is based on the average investor group imbalance for all other firms that are in the same size tercile within the same two-digit SIC code industry.

Figure 2 plots the cumulative (buy-sell) imbalances for the institutional and individual investor groups during the twenty trading days prior to the first news of a takeover. Panel A of Figure 2 shows that the general institutional category is not a net buyer prior to takeovers. Clients of the three largest investment banks, 21 hedge funds, and derivative traders are not net buyers either, all having relatively small net activity. Interestingly, all four individual investor groups are net buyers. Panel A of Table 10 presents the twenty-day imbalances for the nine investor groups along with statistical significances. Consistent with Figure 2, all four individual investor groups have significantly positive imbalances before adjusting for multiple testing. The individual general and individual day-trader groups remain significant with both Holm-Bonferroni and FDR tests, whereas the individual full-service and discount groups are only marginally significant with the FDR test.

We further plot the client trading activity prior to earnings announcements for each group in Panel B of Figure 2. For brevity, we only plot the large negative announcements (left panel) and large positive announcements (right panel). In contrast to being informed, individual investors are net buyers prior to earnings announcements with large negative ($<-5\%$) returns, whereas clients of large investment banks and hedge funds sell. Panel B of Table 10 presents the statistical significances, showing that individuals' buying (except for day traders) and hedge funds' selling prior to large negative announcements ($<-5\%$) remain significant after controlling for multiple testing. Hedge funds are significant sellers ahead of positive announcements as well, indicating little evidence of special timing. In unreported results, small positive and negative announcements also show little evidence of informed trading by investor groups.

In general, the evidence for institutions at large is consistent with our connected trading findings. There is no evidence of institutions trading in the right direction prior to major announcements. The individual trading ahead of takeovers suggests that individuals are perhaps more brazen in trading on information for their own account, rather than for an institutional investor. For institutional investors, most of the volume is uninformative of future earnings announcements.

23 Additionally, if any systematic classification errors exist in the reporting of ECN trades (as discussed in Griffin et al. 2011), then to the extent that these errors are similar across similar stocks, benchmarking should help control for these issues. In order to calculate industry/size-adjusted imbalances, we drop a firm if no other firm falls in the same size tercile of the same two-digit industry. This filter eliminates about 0.6% of our sample. Our results are similar without benchmarking and with other benchmarks, such as past turnover and return momentum.

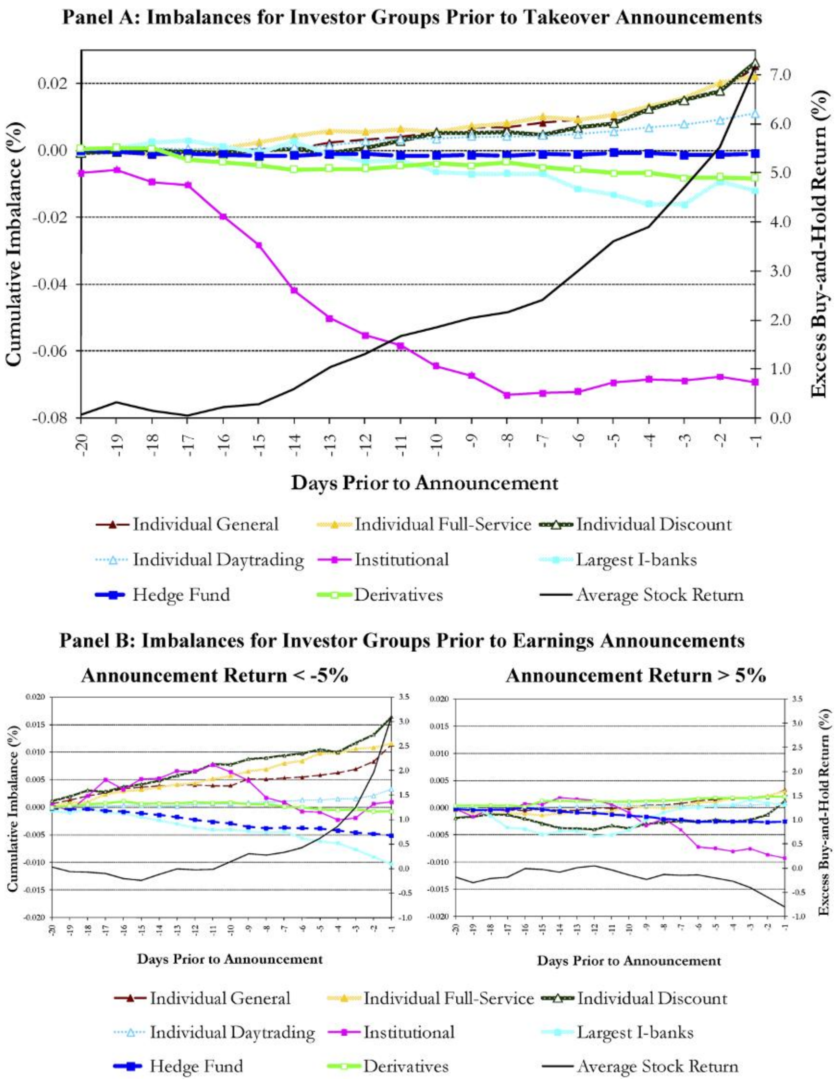

Figure 2
Imbalances for investor groups prior to takeover and earnings announcements
Panel A plots average cumulative industry/size-adjusted imbalances for various investor groups prior to takeover announcements. Our sample comprises 1,225 takeovers. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a percentage of shares outstanding. We further calculate daily industry/size-adjusted imbalance for a firm by subtracting the average daily imbalance for the portfolio containing all other firms that are in the same tercile of market capitalization within the firm's two-digit SIC industry. For each day during the [–20,–1] window, we plot the average cumulative imbalance across takeovers. Day –1 refers to the last trading day before the announcement. We also plot the buy-and-hold return in excess of the NASD index return. Panel B plots average cumulative industry/size-adjusted imbalances for various investor groups prior to earnings announcements. For brevity, we only plot imbalances prior to large negative announcements (two-day announcement return below –5%) in the left panel and prior to large positive announcements (two-day announcement return above 5%) in the right panel. The two-day announcement return is the buy-and-hold return in excess of the NASD index return in the [0,1] window, where day 0 is the earnings announcement date.

Table 10
Trading by investor groups prior to takeover and earnings announcements

	Inst.	Largest I-banks	Hedge Fund	Deriv.	Ind. Gen.	Ind. Full.	Ind. Disc.	Ind. Day.
Panel A: Imbalances Prior to Takeover Announcements								
Imbalance [–20,–1]	−0.490	−0.091	−0.020	−0.021	**0.221**	0.204	0.230	**0.111**
Unadjusted *p*-value	(0.11)	(0.50)	(0.37)	(0.71)	**(0.00)**	(0.02)	(0.02)	**(0.00)**
Holm-Bonferroni *p*-value	(1.00)	(1.00)	(1.00)	(1.00)	**(0.00)**	(0.35)	(0.40)	**(0.01)**
FDR *p*-value	(0.24)	(0.63)	(0.55)	(0.78)	**(0.00)**	(0.06)	(0.07)	**(0.00)**
Panel B: Imbalances Prior to Earnings Announcements								
Announcement return <–5%								
Imbalance [–20,–1]	−0.020	−0.058	**−0.060**	−0.010	**0.156**	**0.256**	**0.253**	0.017
Unadjusted *p*-value	(0.86)	(0.29)	**(0.00)**	(0.55)	**(0.00)**	**(0.00)**	**(0.00)**	(0.41)
Holm-Bonferroni *p*-value	(0.86)	(1.00)	**(0.01)**	(1.00)	**(0.00)**	**(0.00)**	**(0.00)**	(1.00)
FDR *p*-value	(0.86)	(0.50)	**(0.00)**	(0.63)	**(0.00)**	**(0.00)**	**(0.00)**	(0.55)
Announcement return >5%								
Imbalance [–20,–1]	−0.110	0.068	**−0.030**	0.013	0.035	0.076	−0.040	0.003
Unadjusted *p*-value	(0.36)	(0.19)	**(0.00)**	(0.55)	(0.20)	(0.07)	(0.37)	(0.78)
Holm-Bonferroni *p*-value	(1.00)	(1.00)	**(0.05)**	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
FDR *p*-value	(0.57)	(0.38)	**(0.01)**	(0.66)	(0.36)	(0.17)	(0.52)	(0.82)

Panel A reports average industry/size-adjusted imbalances for various investor groups during the [−20,−1] window prior to takeover announcements, where day −1 refers to the last trading day before the announcement. Our sample comprises 1,225 takeovers. Daily imbalance for a stock is the difference between buy and sell volumes expressed as a fraction of shares outstanding. We scale the imbalances by 1,000. We further calculate daily industry/size-adjusted imbalance for a firm by subtracting the average daily imbalance for the portfolio containing all other firms that are in the same tercile of market capitalization within the firm's two-digit SIC industry. We first calculate average imbalances for each calendar day using all imbalances for that calendar day within the sample of [−20,−1] windows, and then report the time-series means and (unadjusted) *p*-values for the average daily imbalances. The unadjusted *p*-values are based on *t*-statistics that are calculated using Newey-West robust standard errors with twenty lags. We further multiply the average daily imbalances by 20. Panel B reports average industry/size-adjusted imbalances for various investor groups during the [−20,−1] window prior to earnings announcements. Earnings announcements are classified into four groups according to two-day excess returns for the [0,1] window, where returns are in excess of NASD index return: those with announcement returns below −5%, between −5% and 0%, between 0% and 5%, and greater than 5%. For brevity, we only report results for large positive and large negative announcements. To examine statistical significance in the context of multiple tests, we further present the Holm-Bonferroni and the false discovery rate (FDR) *p*-values. Values are displayed in bold if they are significant at the 0.05 level, according to either the Holm-Bonferroni or the FDR *p*-values.

5. Conclusion

We examine if brokerage-level trading patterns indicate evidence of information leakage from the brokerage houses' investment banking and lending relationships to favored clients. Even though we examine a host of different connections through takeover advising, IPO and SEO underwriting, and lending, we find little evidence to support that trading by the clients of these brokerage houses reflects the brokers' extensive connections. We also find no evidence that client trading of certain brokerage houses can persistently predict earnings announcement returns for the same firm. Consistent with differential trading ability, we do find evidence that clients of some brokerage houses consistently make profits in the twenty-day period prior to earnings announcements. The fact that the patterns are not prevalent immediately prior to the announcement and are not linked to any specific investment banking or lending activity makes it unclear whether the profits are due to private signals or simply the better use of public information. We also find no evidence that market makers themselves are using their investment banking and lending connections to trade profitably prior to takeover and earnings announcements.

Our finding of little evidence for trading on connections is in direct contrast to a growing body of literature that finds that institutions widely exploit their connections (e.g., Massa and Rehman 2008; Bodnaruk, Massa, and Simonov 2009; Jegadeesh and Tang 2010; Ivashina and Sun 2011). Though we acknowledge that institutions may trade on connections in some settings, we also believe our data and empirical approach allow for more powerful and comprehensive tests than do those in previous articles.

There are several possible implications for our findings. First, there is likely a sizeable set of academics searching for evidence in favor of connected trading by brokerage houses, as well as a publication bias toward more shocking inferences, suggesting that the current literature may disproportionally represent the subset of articles that find such evidence. Second, though our findings do not rule out other forms of insider trading on connections, they do suggest that connected trading is less common than the picture presented in the academic literature. Third, institutions are aware of the possibility of their trades being monitored and thus may carefully avoid trading in a traceable manner through their own trading desk. Our finding of individual investors trading ahead of takeover announcements suggests that connected individuals may choose to use inside information for themselves, instead of for their firms.

Overall, our findings suggest that institutional trading on connected information is not as rampant as one might expect from reading the U.S. financial press and the academic literature. We are not implying that insider trading is not a problem or that it is not worth further monitoring or examining. Such examination may need to be more sophisticated than the common detection mechanisms employed by regulators in the past and should entail higher frequency and more detailed data. We hope to see additional research that further measures the potential scope of insider trading.

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