Non-Deal Roadshows, Informed Trading, and Analyst Conflicts of Interest

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Abstract

Non-deal roadshows (NDRs) are private meetings between management and institutional investors, typically organized by sell-side analysts. We find that around NDRs, local institutional investors trade heavily and profitably, while retail trading is significantly less informed. Analysts who sponsor NDRs issue significantly more optimistic recommendations and target prices, coupled with more "beatable" earnings forecasts, consistent with analysts issuing strategically biased forecasts in order to win NDR business. Our results suggest that NDRs result in a substantial information advantage for institutional investors and create significant conflicts of interests for the analysts that organize them.

Keywords: Non-deal roadshows, informed trading, analyst conflicts of interest, private meetings

JEL classifications: G20, G23

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1. Introduction

The 2000 Regulation Fair Disclosure ("Reg FD") and the 2003 Global Analyst Research Settlement ("Global Settlement") are two of the most significant regulatory actions designed to protect retail investors in the past few decades. Reg FD was introduced to level the information playing field for retail investors by prohibiting the disclosure of nonpublic, material information to selected parties, and the Global Settlement was designed to reduce the conflicts of interest that are inherent when financial institutions engage in both investment banking activities and equity research. Existing evidence suggests that these regulations have been at least somewhat successful in achieving their stated objectives.[1] However, there is concern that the effectiveness of both regulations is being eroded by corporate managers' tendency to meet privately with institutional investors, particularly when such private meetings are undisclosed to the public.

In this paper, we examine whether "non-deal roadshows" (NDRs), a pervasive and secretive activity for brokerages, corporate managers, and institutional investors, impact the informativeness of trading (both institutional and retail) and amplify analyst conflicts of interest. A company "roadshow" is a series of targeted private meetings over several days across different cities where firm management meets with investors to provide them with information regarding their firm. Roadshows are commonly associated with presentations given by firms seeking to issue securities, such as in an initial public offering. However, firms frequently go on roadshows unrelated to securities issuance, which are referred to as non-deal roadshows (NDRs). NDRs involve one-on-one meetings between corporate managers and investors, held at the offices of current and potential institutional investors. As a recent *Wall Street Journal* article points out, unlike other corporate access events such as broker-hosted conferences or analyst days, these meetings are not disclosed to the public nor are webcasts or transcripts provided.[2] Further, these meetings are often arranged by sell-side analysts as a corporate access service to their institutional clients.

The secretive nature of NDRs exacerbates concerns relating to both conflicts of interest and information asymmetries. In particular, anecdotal evidence suggests that sell-side analysts have strong incentives to issue overly optimistic research in order to organize firms' NDRs.[3] The lack of disclosure

[1] For example, Koch, Lefanowicz, and Robinson (2013) conclude that Reg FD resulted in more equal access to information among investors, and Corwin, Larocque, and Stegemoller (2017) find that the Global Settlement led to a significant reduction in investment-banking related conflicts of interest for sanctioned banks.

[2] Hoffman (March 4, 2020). https://www.wsj.com/articles/in-boston-money-managers-fire-shot-at-wall-street-brokers-11583323502

[3] For example, the *Wall Street Journal* reports, "Securities firms have struggled ever since the settlement to make their research profitable. As a result, analysts' relationships with company executives, including the ability to line up private

surrounding NDRs makes it more difficult for investors to detect and adjust for this possible bias, which increases the risk that these conflicts ultimately distort market prices and reduce economic efficiency. In addition, the private nature of NDRs makes it far more difficult for smaller investors to recognize that they may be at an informational disadvantage, amplifying the potential trading losses incurred by uninformed investors around NDRs.

A primary challenge in empirically examining NDRs is that NDR data are generally not observable. We overcome this challenge by collecting a novel sample of more than 40,000 NDRs from 2013 to 2019 from TheFlyOnTheWall.com (*FLY*). *FLY* is a subscription-based publisher of real-time financial news that obtains data on NDRs through a variety of non-public sources, including leaks from employees within the brokerage firm.[4] For each NDR, *FLY* reports the date, the firm, the location, and the brokerage firm organizing the NDR.

We begin by examining the consequences of NDRs for institutional investors headquartered in or near the city where a firm conducts an NDR (local institutional investors). We find that local institutional investors increase their trading in the NDR firm by a highly significant 85% during the quarter of the NDR. Moreover, this trading is highly informed. The tercile of stocks most heavily purchased by local institutions outperforms the tercile of stocks most heavily sold by 1.43% over the subsequent quarter, which is more than six times larger than the corresponding estimate for non-local institutional investors. Both the intensity and informativeness of institutional trading is significantly greater for local institutions who have high ownership stakes in the NDR firm, consistent with firms using NDRs to visit their largest shareholders.

We also investigate the informativeness of retail trading around NDRs. Using the method of Boehmer, Jones, Zhang, and Zhang (2020), we find retail trading is significantly less informed in the weeks following an NDR. Our findings are consistent with NDRs placing retail investors at an informational disadvantage, particularly relative to local institutional investors. In contrast to NDRs, we find no evidence that retail investor trading is less informed in the weeks following an investor conference. This finding is consistent with the view that the more secretive nature of NDRs puts smaller investors at a larger informational disadvantage.

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meetings for investor clients, have become an increasingly vital revenue source. And that is increasing the pressure for analysts to be bullish on the publicly traded companies they follow" (https://www.wsj.com/articles/new-wall-street-conflict-analysts-say-buy-to-win-special-access-for-their-clients-1484840659).

[4] *FLY* only reports a subset of all NDR activity, which raises potential concerns regarding sample selection. We explore this concern in greater detail in Section 3.2 of the paper. We find little evidence that our results are biased based on *FLY's* NDR coverage.

We next examine the implications of NDRs for the brokerage firm that organizes the event. Prior work finds that institutional investors reward brokerage firms that provide valuable services with greater trading commissions (e.g., Nimalendran, Ritter, and Zhang, 2007; and Goldstein, Irvine, Kandel, and Weiner, 2009), which suggests that NDR brokers should experience an increase in commission revenue following the NDR. Consistent with this prediction, we find that commission revenues increase substantially for the sponsoring broker during the week of the NDR and remain elevated over the subsequent month.

Given that NDRs are valuable to the broker sponsoring the NDR, we examine the possible conflicts that they may create for sell-side analysts. The incentives created by NDRs are similar to investment banking conflicts. That is, analysts may issue overly optimistic forecasts for NDR clients, like banking clients, to secure business. Consistent with this view, we find that brokers who take a firm on an NDR (NDR brokers) issue substantially more optimistic investment recommendations and target prices for the firm compared to other brokers. This difference in optimism peaks in the period immediately surrounding the NDR, and it holds when we include broker and analyst characteristics and include firm-time fixed effects. The magnitude of the bias is also substantial. For example, the optimism of NDR brokers is typically at least three times as large as the optimism associated with having an investment banking affiliation or hosting an investor conference. The magnitude of the optimism is also larger for NDRs that are likely to generate greater trading commissions for the brokerage firm, including NDRs that span multiple days, NDRs that visit cities with greater institutional ownership, and NDRs for firms with higher share turnover.

The optimism of NDR brokers is consistent with analysts attempting to gain favor with management to increase their likelihood of taking the firm on an NDR. However, an alternative view is that analysts behave honestly and NDR firms gravitate towards analysts who have sincerely optimistic views of the company. To distinguish between strategic versus sincere optimism, we follow Malmendier and Shanthikumar (2014), who argue that sincerely optimistic analysts should issue both optimistic recommendations and optimistic short-term earnings forecasts, while analysts aiming to curry favor with management will issue optimistic recommendations coupled with more pessimistic (or "beatable") short-term earnings forecasts. We find that NDR brokers issue substantially more pessimistic earnings forecasts, consistent with NDR brokers' bias being motivated, at least in part, by strategic considerations.

Our paper has important implications for Regulation Fair Disclosure (Reg FD). Reg FD prohibits managers from disclosing material, non-public information to analysts and institutional

investors. However, it continues to allow for private meetings between investors and management, provided that material non-public information is not disclosed. While NDRs do not necessarily violate Reg FD, our findings suggest that they run counter to Reg FD's stated objective of creating a more level informational playing field. Further, our findings that retail investor trading is significantly less informed around NDRs relative to investor conferences suggest that disclosure of NDR activity might mitigate some of the adverse effects of NDRs for retail traders.

Our paper also contributes to our understanding of conflicts of interest in financial institutions (see Mehran and Stulz [2007] for a review). It has long been recognized that investment research creates conflicts of interest for investment banks. For example, an internal Morgan Stanley memo from the 1990s stated that their objective "is to adopt a policy, fully understood by the entire firm, including the Research Department, that we do not make negative or controversial comments about our clients as a matter of sound business practice" (Mishkin and Eakins, 2018, page 158). Lin and McNichols (1998) and Michaely and Womack (1999) document that the relationship between investment banking and analyst optimism is systematic. Regulators responded to such abuses by imposing severe fines on major financial institutions (2003 Global Analyst Research Settlement), requiring that there be a "Chinese wall" between investment banking and investment research, and mandating explicit disclosure of banking relationships. As part of the Global Settlement, research analysts were prohibited from participating, either directly or indirectly, in roadshows where security issuances are pitched to investors. Our findings suggest that non-deal roadshows also pose serious conflicts of interest that result in optimistic equity research. Yet, NDRs do not fall under the Global Settlement or other regulatory purviews and thus should be of great interest to policy makers.

Finally, our paper contributes to the literature on the consequences of private meetings in capital markets. Much of the prior research focuses on private meetings at widely disclosed events such as analyst investor days (Kirk and Markov, 2016) and investor conferences (Bushee, Jung, and Miller, 2011, and Green et al., 2014a and 2014b) or relies on proprietary data from a single firm (Soltes, 2014 and Solomon and Soltes, 2015). Perhaps closest in spirit to our work, Bushee, Gerakos and Lee (2018) develop a clever approach to identify a large sample of possible NDR activity—they track corporate flight patterns by forming non-overlapping three-day flight windows to financial money centers and non-money centers where firm-specific institutional ownership is high. They find that their proxy for NDR activity is associated with elevated trading of local institutional investors, but

they fail to uncover systematic evidence that the trading is profitable.[5] Our paper differs in three important ways. First, our data include information on the broker sponsoring the NDR. This allows us to examine the impact of NDRs on trading commissions and analyst conflicts of interest. Second, we offer direct evidence on the consequences of NDRs for retail investors, which should be of particular interest to policy makers.[6] Lastly, we exploit a unique, large sample of NDRs that is less susceptible to measurement error. This likely explains why we find significantly stronger results for the intensity of local institutional trading around NDRs, as well as why we are able to uncover widespread and economically large trading gains for local institutional investors.

2. Institutional details of NDRs

Executives generally know more about the economic conditions of their firm than do outside investors who provide capital. To mitigate this information asymmetry, managers spend a significant amount of time disclosing information to investors. Many disclosures simply involve disseminating news to a wide audience (e.g., financial reports, press releases, and conference calls). However, managers also regularly meet with investors at private events like investor conferences, analyst/investor (AI) days, and non-deal roadshows.

Regulation Fair Disclosure (Reg FD) prohibits managers from disclosing material nonpublic information during private meetings. Existing work suggests that Reg FD has been effective in reducing selective disclosure (see Koch, Lefanowicz, and Robinson, 2013 for a summary). However, there is good reason to believe that private meetings still convey at least some informational benefits. While Reg FD bans the disclosure of material private information, it explicitly permits the disclosure of non-material information that could help an investor complete a "mosaic of information that, taken together, is material."[7] Further, in practice, the demarcation between material and non-material information is subtle and subject to interpretation. In fact, survey evidence highlights considerable heterogeneity in what both executives and regulators view as appropriate disclosure under Reg FD (Soltes, 2018). Perhaps unsurprisingly, the lack of clarity regarding "materiality" has made Reg FD difficult to enforce, which has likely undermined its effectiveness in curtailing selective disclosure.[8]

[5] In particular, Bushee, Gerakos, and Lee (2018) conclude, "Thus, there is no widespread evidence of institutional investors being able to earn trading gains based on roadshow meetings, but there is some evidence that trading gains exist when the firm's information is more complex and private meetings between managers and investors were infrequent." (p. 374).
[6] See, for example, https://www.sec.gov/news/speech/mjw-speech-032114-protecting-retail-investor.
[7] See SEC Release Number 33-781: https://www.sec.gov/rules/final/33-7881.htm.
[8] As of 2019, there have been only thirteen Reg FD enforcement cases (Soltes, 2018).

Consistent with this view, recent research suggest that private meetings provide an informational advantage to attending investors (Solomon and Soltes, 2015) and analysts (Green et al., 2014b).

Existing work on private meetings has focused primarily on investor conferences (e.g., Bushee, Jung, and Miller 2011, 2017; and Green et al., 2014a, and 2014b) and analyst/investor (AI) days (Kirk and Markov, 2016). Much less is known about NDRs, which have several distinct features relative to other types of private meetings. NDRs are more private and discreet than other investor relation activities. While broker-hosted conferences and AI days are private in the sense that investors must be invited to attend, the calendars for these events are publicly disclosed well in advance of the event, and the firms participating and the executives attending are known. In addition, transcripts of the events are released with little delay. In fact, many brokers and firms have begun to webcast not only the presentations, but also the more private break-out sessions.[9] In contrast, the dates and locations of NDRs are almost never disclosed, and to our knowledge, transcripts of NDRs do not exist. In a regulatory world aimed at leveling the playing field for small investors with more disclosure and transparency, NDRs appear to have fallen below the radar where virtually no disclosure is provided.

NDRs also tend to allow for more intimate and in-depth discussions with managers. For example, it's common for hundreds (and occasionally thousands) of investors to attend both AI days and investor conferences. While these events typically offer time for Q&A and less formal discussions, they generally do not provide time for investors to ask in-depth private questions with management in a one-on-one setting.[10] In contrast, in an NDR, managers privately meet with one buy-side firm at the investor's office.

To get further insight into the importance of these differences, we interviewed a senior buy-side analyst at an investment company with over $200 billion in assets. He emphasized that NDRs are the most valuable channel for access to management for two reasons. First, unlike broker-hosted conferences or AI days that he also attends, NDRs are one-on-one meetings on his home turf, and the face time he gets with management is significantly longer compared to broker conferences or AI days. He suggested that at the latter venues, interactions with executives typically occur in breakout sessions after the firm presentation and this is shared with many other investors in a different room. The questions he asks in these semi-public forums are much different than the ones he would ask when a company is visiting his office. For instance, during broker conferences and AI days, he refrains

[9] For example, http://investors.alnylam.com/events/event-details/37th-annual-jp-morgan-healthcare-conference-qa-breakout-session.

[10] Bushee, Jung and Miller (2017) report that only 14.7% of conference presentations are accompanied by one-on-one meetings, while 41.1% have breakout sessions, and the remaining 44.2% have no formal offline meetings.

from asking tough questions or questions that might give a competing firm additional insight, but indicated he would ask anything during an NDR meeting. Second, he noted that the typical break-out session at a conference is only 30-minutes long, while NDR meetings tend to be at least one hour. He feels that the longer duration of NDR meetings allows for him to dig into topics more deeply.

The views of this buy-side analyst are consistent with ample anecdotal and survey-based evidence on the importance of NDRs. For example, Ryan and Jacobs (2005) quote investor relations officers (IROs) as stating, "the non-deal roadshow is the most effective forum to develop interest in a stock because the portfolio manager can ask questions, look management in the eye, and share concerns in a private setting" (p.205). Brown et al. (2019) poll IROs at 610 publicly-traded firms and find that out of the 12 most common information disclosure channels that firms use to convey the company's message to institutional investors, NDRs rank as the 2nd most valuable form of investor outreach channels just behind earnings conference calls (and ahead of press releases, private phone calls, sell-side analysts, 10-K/10-Q/8-K filings, on-site visits, media, management forecasts, informal settings such as golf, and social media). Further, NDRs are growing in importance relative to other investor outreach channels. For example, a 2018 survey of investor relation officers by Citigate Dewe Rogerson finds that 45% of firms plan to dedicate more time to NDRs while only 4% plan to dedicate less time to NDRs. In contrast, only 23% of firms plans to dedicate more time to investor conferences, compared to 18% who plan to dedicate less time.[11]

3. Data

3.1. Data source and descriptive statistics

We collect NDR data from TheFlyOnTheWall.com (*FLY*). *FLY* is a news aggregator of financial data.[12] *FLY* relies on a variety of non-public sources, including leaks from employees at brokerage firms and buy-side funds, to obtain information on NDRs. We capture the firm, the date(s), the location(s), and the brokerage firm sponsoring the NDR for all NDRs from 2013, the first full year for which *FLY* reports NDR data, through 2019.[13] We also collect information on investor conferences from the Bloomberg Corporate Events Database over the same period. The conference

[11] See: https://citigatedewerogerson.com/wp-content/uploads/2018/09/CDR-IR-Survey-2018.pdf

[12] Additional details on the *FLY* are provided in Bradley, Clarke, and Zeng (2020), and Section 2 of Barclays Capital Inc., Merrill Lynch, Pierce, Fenner & Smith Incorporated, and Morgan Stanley & Co. Incorporated v. TheFlyOnTheWall.com, 700 F. Supp. 2d 310 (D.N.Y. 2010), available at: https://scholar.google.com/scholar_case?case=2571947736946721031&q=Barclays+Capital,+Inc.+v.+Theflyonthewall.com,+700+F.&hl=en&as_sdt=4000006&as_vis=1

[13] Table IA.1 of the Internet Appendix provides an example of the NDR data for Microsoft in 2013.

data include information on the date of the conference, the names of each of the presenting companies, and the brokerage firm organizing the conference.

Panel A of Table 1 provides descriptive statistics of our NDR sample after merging the sample with all common stocks (share codes 10 and 11) that are contained in the intersection of the CRSP monthly return file and the Compustat fundamentals annual file. The sample contains 43,799 unique firm-date-city observations (hereafter: NDRs). The NDRs are organized by 112 brokerage firms on behalf of 3,541 firms. We are able to match 89 of the 112 NDR brokers to the I/B/E/S database. The 23 unmatched brokers are small and account for only 0.6% of all the NDRs in the *FLY* sample.

Panel B of Table 1 provides similar descriptive statistics for the conference sample. The conference sample includes 109,486 conference presentations, hosted by 368 different organizers, including 71,095 presentations at conferences organized by I/B/E/S brokers.

The majority of the NDRs in our sample occur in major US cities. In Panel C, we provide statistics on the top 30 cities visited by firm management during NDRs.[14] For each city, we also compute the fraction of total institutional trading that is driven by local institutional investors. Specifically, we merge institutional quarterly holdings from Form 13-F with the data on fund headquarters location provided on Form ADV.[15] Following Coval and Moskowitz (2001), we define an institutional investor as local to a city if it is headquartered within 100 kilometers of the downtown of the city. For each institution-firm-quarter, we compute *Total Trading* as the absolute value of the change in the institution's holdings across the adjacent quarters scaled by shares outstanding, and we aggregate to a city-firm-quarter level by summing across all local institutions. *Total Trading* provides a lower bound on institutional trading since it does not capture intra-quarter roundtrip trades, short sales, or confidential filings (Agarwal, Jiang, Tang, and Yang, 2013). We report the average *Total Trading* for all firm-quarters for each city. Not surprisingly, typical financial centers where institutional investors are concentrated dominate the most visited cities. For instance, New York City accounts for more than 20% of all NDRs and accounts for 37% of total institutional trading. This is followed by Boston, which is also one of the largest locales for institutional trading. More generally, across the 30 cities, we document a correlation between *NDRs* and *Total Trading* of 92%.[16]

[14] The remaining observations include non-US observations (e.g., London), broad US regions (e.g., "Mid Atlantic"), smaller US cities with infrequent NDR activity (e.g., Buffalo), or missing data.

[15] We thank Stephen Dimmock, Will Gerken and Joe Farizo for making the Form ADV data available here: https://uknowledge.uky.edu/finance_data/1/. Additional details are available in Dimmock and Gerken (2012) and Dimmock, Farizo, and Gerken (2018).

[16] In Section IA.1 of the Internet Appendix, we provide additional descriptive statistics about the timing of NDRs relative to earnings announcements and the returns around NDRs.

3.2. Database Representativeness

A limitation of our sample is that *FLY* only reports a subset of NDR activity. This raises the important question of whether *FLY*'s NDR coverage has any systematic biases that would influence our results.

One potential concern is that *FLY* may redact or disclose more important NDRs *ex post*. To explore this possibility, every day during the month of August 2020 we recorded all NDRs that occurred or were scheduled to occur between August 1, 2020 and December 31, 2020. During this process, we found zero cases where *FLY* either redacted or added NDRs post-event.

A more general concern is that *FLY* coverage might not be representative of the universe of NDRs. To examine this possibility, we collected NDR data from two alternative sources. First, we purchased the email addresses of Fortune 1000 firms' Investment Relations Officers (IROs). After eliminating private firms and invalid email addresses, we were left with 557 IROs. We emailed all 557 of these IROs asking for their NDR calendars so that we can compare our data with theirs. Most IROs did not respond to our email, and the majority that did respond told us that they are unwilling to share this data. The lack of response is consistent with the view that NDRs are a secretive event that firms try to conceal. Despite the general lack of support, 22 firms provided us with NDR data that contain 324 NDRs spanning 67 firm-years.

We also expanded this sample through a contact at a large buy-side fund, who provided his full calendar of NDRs (N= 237) for 2018. Three NDRs appear in both samples, so our final sample of "hand-collected" NDRs includes 558 NDRs of which 34% (189) are reported in *FLY*.

Using this sample, we explore two main questions. First, what are the determinants of *FLY* coverage? Second, to what extent does *FLY*'s incomplete NDR coverage affect the central findings of the paper? We offer a brief summary of our findings below and delegate a more detailed discussion to Section IA.2 of the Internet Appendix.

We find very limited evidence that *FLY* coverage is correlated with firm characteristics (see Table IA.3). Of the 17 firm characteristics considered, only two are statistically significant at a 5% level: *Intangibles* (−) and # *Institutions* (+).[17] We do find that brokerage fixed effects have significant explanatory power for *FLY*'s coverage. For example, more than two-thirds of all NDRs sponsored by JP Morgan, Deutsche Bank, and UBS are reported in *FLY* while no NDRs sponsored by Bank of

[17] In untabulated analysis, we find that our main findings generally do not vary significantly with *Intangibles or # Institutions* which suggests that FLY's tilt towards firms with these characteristics is unlikely to meaningfully impact out findings.

America, Credit Suisse, Morgan Stanley, Goldman Sachs, Barclays, and Citi are reported (see Table IA.4). This finding is consistent with the view that *FLY* primarily relies on leaks from select brokerage firms to obtain their data. Importantly, however, we find no evidence that these brokerage effects are correlated with broker reputation. In particular, *FLY* coverage is virtually identical across bulge bracket and non-bulge bracket brokerages. Finally, we also directly compare our main findings for NDRs in the hand-collected sample that are reported in *FLY* versus those that are unreported. The results of this analysis provide no evidence that our main results are biased upwards due to *FLY*'s incomplete coverage of NDRs (see Tables IA.5, IA.6, and IA.7).

3.3. Determinants of NDRs

We next examine the factors associated with firms' decisions to conduct an NDR. We expect a firm's NDR activity to be determined in equilibrium by both institutional investor demand for information and the firm's incentives to supply information. Following Green et al. (2014a), we expect that institutional investors' demand for management access is likely greater for firms that are harder to value with more complex information environments. We thus conjecture that firms with higher levels of recognized intangibles (*Intangibles*), high R&D expenses ((*R&D* + *ADV)/OE*), greater growth opportunities as proxied by market-to-book ratios (*MB*), and high idiosyncratic volatility (*IVOL)* are more likely to conduct NDRs.

Given that NDRs provide opportunities for firms to meet with current institutional investors, we expect the number of NDRs to be positively correlated with the percentage of the firm owned by institutional investors (*Institutional Ownership).* In addition, we expect that the benefits of NDRs might be larger for younger firms with lower visibility (*Age*), firms that will issue new shares in the next two years (*SEO*), and firms that will make an acquisition in the next two years (*M&A - Acquirer*).

We also control for analyst coverage (*Coverage)* as a proxy for demand for published analyst research, as well as several additional factors known to influence the magnitude of published analyst research including the number of institutional investors who own the stock (# *Institutions*), market capitalization (*Firm Size*), share turnover (*Turnover)*, and the *r*-squared from a market model regression (*R-squared*) (Bhushan, 1989). Finally, we explore whether a firm's tendency to go on an NDR varies with recent performance as measured by its stock's return over the prior month (Ret_{m-1}) or prior two to twelve months ($Ret_{m-12, m-2}$). A detailed description of all variables can be found in Appendix A.

To examine the relation between NDRs and the set of firm characteristics discussed above, we estimate a linear probability model where the dependent variable, *NDR*, equals 1 if the firm participated in an NDR in the firm-month, and zero otherwise. All continuous independent variables

are standardized to have zero mean and unit variance. We include either month fixed effects or month and firm fixed effects. Standard errors are double clustered by firm and month.

Table 2 reports the results. Specification 1 provides the results with month fixed effects. As predicted, harder-to-value firms are more likely to participate in NDRs. For instance, we find that NDR activity is correlated with valuation difficulty as proxied by *Intangibles,* $(R\&D + ADV)/OE$, and *MB*. The other estimates are also largely consistent with our predictions. For example, we find that firms that will make an acquisition or SEO within the next two years are more likely to conduct an NDR. The results from Specification 2, which augments Specification 1 by including firm fixed effects, are qualitatively similar.

Overall, the results from this section are generally consistent with expectations. Firms are more likely to participate in NDRs when the demand for private access to management is high and when the expected benefits to the firm of providing private management access are greater. Our findings are also broadly consistent with Bushee, Gerakos, and Lee (2018), who examine the determinants of corporate jet visits to money centers (a proxy for NDRs). Like us, they find that NDR activity is increasing in intangibles, firm size, and for firms about to raise capital.

4. NDRs and informed trading

In this section, we examine the impact of NDRs on the trade informativeness of institutional investors headquartered in or near the city where a firm conducts an NDR (local institutional investors) and retail investors who are unlikely to be aware that an NDR is taking place.[18]

4.1. NDRs and local institutional trading

We begin by examining the trading of institutions located in close proximity to the NDR. For instance, on January 9-10, 2017, Community Healthcare (CHCT) participated in a two-day NDR to St. Louis, Dallas and Houston. We ask two questions. First, do local institutions increase their trading activity in Community Healthcare in Q1, 2017, relative to non-local institutional investors? Second, is the net trading of local institutions informed about future returns?

4.1.1. The intensity of local institutional trading around NDRs

For each fund *f*, firm *i*, and quarter *t*, we measure $Trading_{fit}$ as the absolute value of the difference in split-adjusted shares held from quarter *t-1* to quarter *t*, scaled by the firm's total shares outstanding. For each of the top 30 NDR destinations (see Panel C of Table 1), we aggregate $Trading_{fit}$

[18] Note that we use the term "local institutional investors" to refer to institutional investors that are located near the location of the NDR, *not* institutional investors that are located near the firm's headquarters.

to a city-level measure (*Total Trading*$_{cit}$) by summing across all local institutions, defined as any institutional investor headquartered within 100 kilometers of the downtown of the city. Similarly, for each fund *f*, firm *i*, and quarter *t*, we measure *Net Trading*$_{fit}$ as the signed value of the difference in split-adjusted shares held from quarter *t-1* to quarter *t* (scaled by the firm's shares outstanding), and we aggregate this measure to a city-level measure (*Total Net Trading*$_{cit}$) by summing across all local institutions and then taking the absolute value. Thus, *Total Trading*$_{cit}$ measures whether local institutional investors are trading in any direction, while *Total Net Trading*$_{cit}$ measures whether local institutional investors are trading in a correlated direction.[19]

We split all firm-city-quarters into those where the firm visited the city in the quarter (Local NDR =1) and all others (Local NDR =0). Panel A of Table 3 shows that average *Total Trading* is much greater around Local NDRs (1.71% of the firm's total shares outstanding) compared to firm-quarters in which there was no Local NDR (0.33% of shares outstanding). Likewise, *Total Net Trading* is larger when Local NDR=1 compared to Local NDR=0 (1.03% versus 0.25%, respectively).

To more carefully examine the relation between NDRs and local institutional trading, we next estimate the following regression:

$$\text{Trading}_{cit} = \alpha + \beta_1 Local\ NDR_{cit} + \beta_2 Non\text{-}Local\ NDR_{cit} + FE + \varepsilon_{cit}. \quad (1)$$

The dependent variable is either *Total Trading* or *Total Net Trading*. Since the distribution of both variables is highly skewed, we also consider log transformations of each variable (*Log Trading*), defined as Log (1×10^{-6} + *Trading*). The independent variable of interest is *Local NDR*. *Non-Local NDR* is also included, which equals one if a firm participates in an NDR in quarter *t* but does not visit city *c*. *FE* always includes city fixed effects and either firm and quarter fixed effects or firm-quarter fixed effects. Standard errors are double clustered by firm and quarter.

Panel B of Table 3 presents these results. In Specifications 1 through 3, the coefficients on *Local NDR* are highly significant, both statistically and economically. The point estimates imply an increase of at least 85% in local institutional trading.[20] Specifications 4 through 6 show that local NDRs are also strongly correlated with *Total Net Trading,* suggesting that local institutional investors are often on the same side of a trade (either buying or selling) during an NDR quarter.

[19] For example, if one local institution purchased 1% of shares outstanding in a firm and a second local institution sold 1% of the share outstanding in the firm, *Total Trading* would equal 2% while *Total Net Trading* would equal 0%.

[20] For example, the estimate of 0.29% in Specification 1 is a roughly 85% increase relative the average value of local institutional trading of 0.34%. Similarly, the estimate in Specification 3 implies an 88% increase (e^0.63−1).

Bushee, Gerakos, and Lee (2018) also examine local institutional trading around private meetings and find results that are directionally consistent, but economically weaker. For example, they find that when a firm's corporate jet visits a money center city, *Total Net Trading* for local institutions increases by 0.054%, roughly one third of our estimated effect of 0.16% in Specification 5. Our larger economic magnitudes are not surprising; while the approach employed by Bushee, Gerakos, and Lee (2018) is a clever proxy for NDRs, it likely suffers from considerable measurement error.

4.1.2. The informativeness of local institutional trading around NDRs

We next investigate whether NDRs give local institutional investors an information edge. We begin by benchmarking the informativeness of local institutional trading during the NDR quarter to non-local institutional trading during the same quarter. We limit the sample to firm-quarters with NDR activity and we require non-zero trading by local and non-local institutional investors in the firm-quarter. We then compare the informativeness of local and non-local institutional trading during the NDR quarter by examining the extent to which local and non-local order imbalances forecast future returns. We define local institutional order imbalance (*Local OIB*) as the total shares of firm *i* bought by all local institutions in quarter *t* less the total shares of firm *i* sold by all local institutions in quarter *t*, scaled by total local institutional trading volume of firm *i* in quarter *t*. Non-local institutional order imbalance (*Non-Local OIB*) is defined analogously.

We first consider simple portfolio sorts. At the end of each quarter, we place stocks into portfolios based on *Local OIB* and *Non-Local OIB* terciles, and we report the average return to the strategy of buying stocks in the top tercile of *Local* (or *Non-Local*) *OIB* and selling stocks in the bottom tercile of *Local* (or *Non-Local*) *OIB*. Figure 1 plots the returns to this strategy over the subsequent 12 months.[21] We find that the stocks most heavily bought by local institutions (tercile 3) outperform the stocks most heavily sold (tercile 1) by 1.43% over the subsequent three months and this difference grows to 2.02% over the 12-month holding period.[22] In contrast, the analogous long-short spread based on *Non-Local OIB* is 0.22% over a 3-month holding period and -0.68% over a 12-month holding period. These results are consistent with local institutions gaining an economically large informational advantage from NDRs relative to non-local institutions. Importantly, these findings are a significant

[21] Return data on CRSP ends in December of 2019. Thus, here and throughout the remainder of the paper, we compute returns either to the specified horizon or until the end of December 2019. The six-month return sample is computed for all institutional trading ending as of Q2 2019 or before, the 12-month return sample is computed for all institutional trading ending as of Q4 2018 or before, etc.

[22] Using various risk adjustments, including Fama-French (1993) three-factor alphas, Carhart (1997) four-factor alphas, and Fama-French (2015) five-factor alphas yields virtually identical estimates (untabulated).

contrast to the evidence in Bushee, Gerakos, and Lee (2018) who find, at best, very weak evidence of trading gains around NDRs (pg. 374, Table 7). As discussed in the prior section, our stronger results are likely a consequence of measuring NDR activity with much greater precision.

We next estimate the informativeness of local institutional investors using the panel regression:

$$Ret_{it+x} = \alpha + \beta_1 Local\ OIB_{it} + \beta_2 Non\text{-}Local\ OIB_{it} + \beta_3 Char_{it} + Qtr_t + \varepsilon_{it.} \quad (2)$$

Ret_{it+x} is the quarterly return for firm i in quarter $t+x$, where quarter t is the NDR quarter. We let x vary from one to four quarters. *Local OIB* and *Non-Local OIB* are defined as above. *Char* is a vector of firm characteristics taken from Boehmer, Jones, Zhang, and Zhang (2020) and includes past one-week returns (Ret_{w-1}), past one month returns (Ret_{m-1}), returns over the prior two to seven months ($Ret_{m-7,m-2}$), market capitalization (*Size*), share turnover (*Turnover*), volatility of daily returns (*Vol*), and book-to-market (*BM*). All independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and quarter.

Specification 1 of Table 4 reports the estimates from Equation 2 for the one-quarter ahead returns. We find that a one standard-deviation increase is *Local OIB* is associated with a statistically significant 0.66% higher one-quarter ahead return. In contrast, the coefficient on *Non-Local OIB* (0.10%) is statistically insignificant, and less than one-sixth of the estimated effect for *Local OIB*. In the last row of the table, we also confirm that the difference between *Local OIB* and *Non-Local OIB* (0.56%) is statistically significant.

Specifications 2 through 4 report analogous results for quarters two, three, and four, respectively. The average estimate of *Local OIB* in quarters two through four is positive but statistically insignificant. The lack of reversal over longer horizons is inconsistent with the returns following local institutional trading being attributable to uninformed price pressure. Instead, the results support the view that NDRs provide new information to local institutional investors, and this information is subsequently impounded into prices, with the majority of the effect occurring within one quarter.

The results from Table 4 indicate that institutions located near the NDR are more informed than other institutions during the NDR quarter. One concern, however, is that local institutions may generally be more informed about NDR firms than non-local institutions even in the absence of an NDR. Thus, we next consider an alternative benchmark that compares the informativeness of local institutional trading during the NDR quarter to their trading in the quarters prior to the NDR. Specifically, we examine the informativeness of local (and non-local) institutional trading in each of the three quarters prior to the NDR. For example, for quarter (-3) we examine local and non-local

institutional trading in the NDR firm three quarters prior to firm conducting the NDR. We also impose the filter that the firm must not have conducted an NDR in the city in the given quarter to ensure that our benchmark is not impacted by previous NDR activity. If the large coefficient on *Local OIB* documented in Table 4 is attributable to local institutions having a general informational advantage in NDR firms, then their trading should be equally informative in non-NDR quarters. In contrast, if the effects are attributable only to the NDR, the estimated effect should be small in non-NDR quarters.

Figure 2A reports the estimates for *Local OIB* for quarters -3 to -1. All three estimates are statistically insignificant and the average value across the three estimates is 0.001%. Thus, there is little evidence that local institutions have a general information advantage in NDR stocks prior to the NDR.

Figure 2A also reports the estimate for *Local OIB* for the three quarters after the NDR (+1 to +3). We find modest evidence of informed trading in the post period. The average estimates across the three post-quarters are 0.16%. The estimates across the three quarters are jointly insignificant, although the point estimate for quarter 2 is significant at a 5% level. The positive estimate is consistent with anecdotal reports from a buy-side manager who suggests that at least some of the information conveyed in NDRs does not represent an urgent trading opportunity but rather valuable contextual information that helps institutions better interpret new information. We also repeat the above analysis for *Non-Local OIB*. The results, reported in Figure 2B, indicate that non-local order imbalances are not significantly related to future returns across any of the quarters.

4.1.3. The intensity and informativeness of local institutional trading around NDRs – investor and firm heterogeneity

In this section, we explore whether the intensity and informativeness of local institutional trading varies significantly with investor and firm characteristics. Heterogeneity in institutional investors' intensity of trading around NDRs provides insight into both the types of investors that firms are more likely to meet with and the types of investors that are more likely to trade following an NDR meeting. Similarly, variation in the informativeness of institutional trading speaks to both the type of investors that firms are more likely to meet with and the types of investors that extract the most useful information from NDRs.

We first compare hedge fund trading to the trading of all other institutions (non-hedge funds). Solomon and Soltes (2015) find that hedge funds are more likely to privately meet with management and more likely to trade in informed ways following the meeting. While the latter finding is consistent with the conventional view of hedge funds as sophisticated investors, the former finding runs counter to ample survey and anecdotal evidence which suggests that firms tend to avoid meeting privately with

hedge funds due to their shorter holding periods and ability to short sell. For example, Brown et al. (2019) report that while 70% of IROs are very likely to grant private access to investors working for a mutual fund, only 39% are willing to provide access to investors working for a hedge fund.

We identify hedge funds using Form ADV. Following much of the prior literature (e.g., Brunnermeir and Nagel, 2004; Griffin and Xu, 2009) we classify an institution as a hedge fund if it meets the following two criteria: (1) at least 50% of its clients are "Other pooled investment vehicles" or "High net worth individuals" and (2) it charges performance-based fees. We then re-estimate Specification 3 of Table 3 after partitioning the sample into hedge funds and other funds. Panel B of Table 5 reports the results. We find that local non-hedge fund trading increases by 112% (e^0.75−1) compared to only 45% for local hedge fund trading, and the difference between the two estimates is highly significant. This finding is consistent with survey evidence but is inconsistent with the results of Solomon and Soltes (2015). This difference may be related to idiosyncrasies in the meeting policies of the one firm analyzed in Solomon and Soltes (2015). For example, Solomon and Soltes (2015) report that the firm analyzed "accommodates all requests to meet with management" (p. 332), a stark contrast to the survey evidence suggesting that the majority of firms are reluctant to privately meet with hedge funds (Brown et al., 2019).

We next compare the informativeness of hedge fund and non-hedge fund trading by repeating Specification 1 of Table 4 for the two groups. The results, reported in Specifications 2 and 3 of Table 5, indicate that a one standard deviation increase in *Local HF OIB* is associated with a statistically insignificant 0.34% increase in returns in the subsequent quarter, while the corresponding estimate for non-hedge funds is a statistically significant 0.68%. However, the difference between the two estimates is not reliably different from zero. This result appears inconsistent with Solomon and Soltes (2015), who find that hedge funds benefit the most from private meetings. However, Solomon and Soltes (2015) examine the informativeness of institutional trading conditional on the firm meeting with the investor, while we examine the informativeness of institutional trading conditional on the firm visiting the institution's city. If firms are less likely to visit hedge funds than other local institutional investors, as suggested by both survey evidence and our intensity of trading results, the two sets of findings need not be inconsistent.

We next sort on fund turnover. It is unclear whether firms will be more or less likely to meet with high turnover funds. Firms generally prefer to meet with longer-term investors, but sell-side analysts organizing the NDRs have an incentive to arrange meetings with high-turnover institutions, where the increase in trading commissions to the brokerage firm are likely to be larger. Conditional

on meeting with the firm, we expect that high-turnover funds may benefit more from NDRs. Existing work suggests that higher turnover funds are more informed (Yan and Zhang, 2009). Further, short-term institutions are presumably more likely to trade on short-lived information conveyed during NDRs. This is particularly important given the finding from Table 4, which suggests that much of the informational advantage of local investors is impounded into prices within one quarter of trading.

We rank funds based on the average quarterly turnover in the prior year, where quarterly turnover is computed as the dollar volume traded by the fund scaled by the total dollar value of the fund's holdings. We define funds in the top (bottom) half of turnover as *High Turnover* (*Low Turnover*). Specification 1 of Panel C shows that the intensity of trading does not significantly vary with fund turnover. However, Specifications 2 and 3 show that the informativeness of *Local OIB* is significantly greater for high turnover funds. In fact, the coefficient on *Local OIB* is actually negative (albeit insignificant) for *Low Turnover* funds. These results suggest that high turnover funds, which are typically viewed as more skilled and more short-term focused, are likely better able to interpret (or extract) more subtle and short-lived information during an NDR.

Existing work suggests that firms are more likely to privately meet with investors who have a large ownership stake in the firm (Solomon and Soltes, 2015; Brown et al. 2019). Accordingly, we expect that both the intensity and informativeness of local institutional trading will be greater for funds with high ownership in the firm. We measure ownership based on the fund's holdings of the stock in the quarter prior to the NDR. We define funds in the top quintile of ownership as *High Ownership* and all other funds as *Low Ownership*.[23] Consistent with NDRs being targeted towards the firm's largest investors, we report in Panel D that local trading is significantly larger for *High Ownership* funds. The informativeness of local institutional trading is also significantly greater for *High Ownership* funds. In particular, a one standard deviation increase in *High Ownership Local OIB* is associated with an increase in returns of 0.63% compared to -0.09% for *Low Ownership Local OIB*. Lastly, we find that *Low Ownership Non-Local OIB* is significantly negative, which suggests that institutional investors who are presumably least familiar with the firm are harmed the most by NDRs.

Finally, in Panel E, we sort on the size of the firm conducting the NDR. We expect that NDRs will be more valuable to investors when they meet with managers of harder-to-value firms, such as smaller firms. We partition the sample into two groups based on the median NYSE market

[23] We define *High Ownership* using quintiles rather than the median breakpoint, because the distribution of ownership is heavily skewed. Although *High Ownership* funds represent 20% of the sample of funds, they account for roughly 85% of trading in the firm's stock.

capitalization breakpoint at the end of each year. We find that the increase in the intensity of trading is significantly larger for small firms relative to large firms. The informativeness of trading is also somewhat larger for smaller firms (0.77% versus 0.41%); however, both estimates are statistically significant, and the estimates are not significantly different from each other. Thus, while NDRs induce substantially more trading for smaller firms, the informational advantages associated with NDRs are not confined to small firms.

4.2. The informativeness of retail trading around NDRs

We next examine the impact of NDRs on the informativeness of retail investor trading. We identify retail trading using the approach of Boehmer, Jones, Zhang, and Zhang (BJZZ, 2020), which exploits two key institutional features of retail trading. First, most equity market orders by retail investors do not occur on an exchange. Instead, a broker typically fills retail trades internally from its own inventory or sends the trades to a wholesaler. These types of trades are classified as exchange code "D" in TAQ. Second, retail traders typically receive a small fraction of a one cent price improvement over the National Best Bid or Offer (NBBO) for market orders (ranging from 0.01 to 0.2 cents), while institutional orders tend to be executed at whole or half-cent increments.

Thus, following BJZZ (2020), we classify trades with TAQ exchange code "D" and prices just below a round penny (fraction of a cent between 0.6 and one) as retail purchases, while trades on exchange code "D" and prices just above a round penny (fraction of a cent between zero and 0.4) are classified as retail sales. This classification is conservative in the sense that is has a low type 1 error (i.e., trades classified as retail are very likely to be retail). However, this classification does omit retail trades that occur on exchanges as well as limit orders that are not immediately executable.[24]

Our objective is to examine how the relationship between retail order imbalances and future returns changes around NDRs. We measure retail order imbalances as retail buy volume less retail sell volume divided by the sum of retail buy and sell volume (*Retail OIB*). We define a trade as occurring around an NDR if an NDR took place at any point from day *t* to day *t-10*.[25]

We examine the informativeness of retail trading around NDRs using the following regression:

$$Ret_{it+x} = \alpha + \beta_1 Retail\ OIB_{it} + \beta_2 Retail\ OIB_{it} \times NDR_{it,t-10} + \beta_3 NDR_{t,t-10} + \beta_4 Retail\ OIB_{it} \times Conf_{t,t-10} \quad (3)$$

[24] Kelley and Tetlock (2013) find that retail market orders are more informed than limit orders, and Linnainmaa (2010) find that limit orders are more likely to be picked off by informed traders. Thus, excluding limit orders likely understates the adverse consequences of NDRs on retail investor trading gains.

[25] We focus on a 10-day event window because we expect that local institutional trading may persist for a couple of weeks following the NDR. For example, in Table 7, we find elevated trading commissions for the sponsoring brokerage firm for two weeks following NDRs and investor conferences. We explore alternative event windows in Table IA.8 of the Internet Appendix. We find qualitatively similar results.

$$+ \beta_5 Conf_{t,t-10} + \beta_6 Char_{it} + \beta_7 Retail\ OIB_{it} \times Char_{it} + Day_t + \varepsilon_{it}.$$

Ret_{it+x} is the weekly (i.e., five-day ahead) return for firm *i* during week *x*, and day *t* is the day in which *Retail OIB* is constructed. We let *x* vary from one to four weeks. *NDR* is an indicator equal to one if firm *i* conducted an NDR in the past 10 trading days. As a benchmark, we also examine the informativeness of retail trading around investor conferences (*Conf*), defined analogously. *Char* is a vector of firm characteristics taken from BJZZ (2020), as defined in Equation 2. We also include *Retail OIB* × *Char* to control for the possibility that the informativeness of retail trading may vary with firm characteristics. All specifications also include calendar day fixed effects. All continuous independent variables are standardized to have mean zero and unit variance.

Table 6 reports the slope coefficients from Equation 3 and the *t*-statistics computed from standard errors double clustered by month and firm. Column 1 reports the results for the five-day ahead returns. Consistent with BJZZ (2020), we find that retail order imbalances are strongly related to returns over the following week. However, this pattern is significantly weaker around NDR days. In particular, relative to non-event days, a one-standard deviation increase in retail order imbalances around NDRs is associated with a decline of 4.1 basis points (bps) in one-week ahead returns.[26] Summing the coefficients on *Retail OIB* and *Retail OIB* × *NDR* yields an estimate of 0.1 bps (t=0.07), indicating that retail trade informativeness shifts from highly positive on non-event days to essentially zero on NDR days.

In contrast to the NDR findings, the coefficient on *Retail OIB* × *Conf* is economically small and statistically insignificant. Further, in untabulated tests, we confirm that the difference between *Retail OIB* × *NDR* and *Retail OIB* × *Conf* is statistically significant (*t*=2.10). This suggests that some of the differences between NDRs and conferences have a significant impact on retail trade informativeness. For example, the structure of NDRs (e.g., longer visits and more private meetings) may create larger informational advantages for institutions that meet with management. Alternatively, it is possible that the greater disclosure surrounding investor conferences, including publishing the date/time of the conference and providing detailed transcripts, benefits retail investors. This interpretation is consistent with recent evidence suggesting that retail investors are skilled at processing public information (e.g., Farrell, Green, Jame, and Markov, 2020; and Akbas and Subasi, 2019).

[26] Similar to Figure 2, we benchmark retail trading around the NDR to their trading in the same stock in the three quarters before (or after) the NDR. Figure IA.2 shows that the coefficient on *Retail OIB* × *NDR* in the placebo quarters is always statistically insignificant and economically small relative to the estimate during the 10 days around the NDR.

Following BJZZ (2020), Specification 1 measures returns under the assumption that all retail trades are executed at the closing price on the day of the trade. This likely overstates retail trading gains since it ignores bid-ask spreads.[27] We next repeat Specification 1 after incorporating execution prices (*Day 0 Returns*). Specifically, for stocks with positive (negative) retail order imbalances, we measure day 0 returns assuming that all trades occurred at the retail-volume-weighted purchase (sale) price. Thus, the inclusion of the day 0 return incorporates the bid-ask spread as well as any subsequent intraday-return. Specification 2 reports the results after including *Day 0 Returns*. We find that the coefficient on *Retail OIB* drops substantially and is no longer significantly different from zero, while the coefficient on *Retail OIB* × *NDR* remains significantly negative. Further, the sum of the coefficients (i.e., *Retail OIB* + *Retail OIB* × *NDR*) is now significantly negative, which is consistent with retail investors incurring trading losses around NDRs over a one-week holding period.

Columns 2, 3 and 4, report the results for week 2, week 3, and week 4, respectively. The estimates on *Retail OIB* × *NDR* are always negative, but generally statistically insignificant. To further explore whether retail investors benefit from NDRs over longer horizons, we estimate the results for each week up to week 12. Figure 3 plots the cumulative estimates on *Retail OIB* and *Retail OIB* × *NDR*. The results indicate that the coefficient on *Retail OIB* × *NDR* remains stable after week 3. The results suggest that the impact of NDRs on retail trade informativeness is concentrated over short horizons, but permanent.

5. NDRs and trading commissions

Sections 4.1.1 and 4.1.2 document that NDRs generate a substantial increase in trading by nearby institutions, and such trading is particularly informed. We expect institutional investors to reward brokers for arranging these face-to-face meetings with firm management through increased commission revenue (Goldstein et al., 2009). Specifically, we expect an increase in commission revenue for the organizing brokers surrounding NDRs. As a benchmark, we also examine the impact of broker-hosted conferences on commissions.

We measure brokerage commissions using transaction data from Abel Noser Solutions (formerly known as Ancerno), a consulting firm that helps institutional investors monitor their transaction costs. Each observation in Abel Noser corresponds to an executed trade. For each trade, Abel Noser provides information on the date of the trade, the stock traded, the commission paid, and

[27] The measure also ignores several other factors that would reduce trading gains including trading commissions and taxes.

the broker that executed the trade.[28] The data stop in June of 2014, so the analysis of trading commissions is limited to broker-sponsored NDRs between January 2013 and June of 2014. We merge the Abel Noser dataset with I/B/E/S by broker name, resulting in a merged sample of 42 I/B/E/S brokers. We drop broker-firm pairs when there is zero trading volume for the firm through the broker during the sample period. We are able to match 1,732 broker-sponsored NDRs and 7,452 broker-hosted conferences with the Abel Noser transaction data.

We estimate the following panel regression:

$$Com_{jit} = \beta_1 NDR_{jit} + \beta_2 Conf_{jit} + \beta_3 Turnover_{it} + Broker\text{-}Firm_{ji} + \varepsilon_{jit} \quad (4)$$

The dependent variable, *Com,* is a measure of commissions for brokerage firm *j*, in stock *i*, in week *t*. We consider two measures of commissions: *$Commissions*, defined as the natural log of 1 plus the total dollar commissions for broker *j* in stock *i* during week *t*, and *Commission Share*, defined as the total commissions for broker *j* in stock *i* during week *t* scaled by total Abel Noser commissions across all I/B/E/S-Abel Noser matched brokers for stock *i* in week *t*. Thus, *$Commissions* allows us to examine whether NDR brokers generate an increase in commission revenue, either due to increased aggregate commissions or a higher percentage of total commissions, while *Commission Share* focuses exclusively on the percentage of total commissions.

Our independent variables of interest are *NDR*, an indicator equal to one if brokerage firm *j* organized an NDR for firm *i* in week *t*, and *Conf*, an indicator equal to one if brokerage firm *j* hosted firm *i* at an investor conference in week *t*. In some specifications, we also include the weekly share turnover (*Turnover*) in the stock to control for the fact that NDRs may take place when general interest in the firm is greater.[29] Finally, all specifications include broker-firm fixed effects to control for the fact that some brokers tend to have persistently higher levels of commissions in certain stocks..

Specifications 1 and 2 of Table 7 report the results for $*Commissions* and *Commission Share*, respectively. We find that *$Commissions* increases by roughly 30% (e^0.26−1) and *Commission Share* increases by 1.15 percentage points during the week of the NDR. Both estimates are economically large and statistically significant. The magnitudes are also similar to the estimates for *Conf*. The comparable magnitudes are perhaps surprising since a much smaller set of investors attend NDRs

[28] Prior vintages of the Abel Noser data included information on the identity of the institutional investor making the trade, allowing for tests of institutional trading skill (see, e.g., Jame, 2018). However, more recent vintages that overlap with our NDR sample time period are anonymous. See Hu, Jo, and Wang and Xie (2018) for additional details of the Abel Noser dataset.

[29] If the NDR itself is the cause of increased trading volume, controlling for total trading likely understates the commission benefits of the NDR. For this reason, we report results that both include and exclude *Turnover* as a control.

relative to conferences, and they highlight the perceived value of NDRs to institutional clients. Specifications 3 and 4 indicate that the results are very similar after including *Turnover* as a control.

To paint a more complete picture of the dynamics of commissions around NDRs, we re-estimate Equation 4 after including indicator variables for whether there was an NDR over the prior two weeks (*NDR [-1,-2]*), prior three to four weeks (*NDR [-3,-4]*), or prior five to eight weeks (*NDR [-5,-8]*). We also examine whether institutions reward brokers for organizing an NDR in advance of the meeting by adding indicators for whether there will be an NDR in the subsequent two weeks (*NDR [1,2]*), subsequent three to four weeks (*NDR [3,4]*), or subsequent five to eight weeks (*NDR [5,8]*). We also include analogous measures for conferences. Specifications 5 and 6 report the results for \$*Commissions* and *Commission Share*, respectively. We find some evidence of elevated \$*Commissions* in the weeks following an NDR or investor conference, but no evidence that institutions reward brokers prior to the NDR or conference. This is consistent with the view that institutions reward brokers for value-added services with realizations only known *ex post*. In other words, if an institution participated in the NDR, but it was not valuable (e.g., poorly organized, uninformative, etc.), it is unlikely that the institution would reward the broker.[30]

6. NDRs and analyst conflicts of interest

In the previous two sections, we demonstrated that NDRs are valuable to institutions, and, in exchange for valuable access to management, institutions allocate commission dollars as payment to the brokerage houses for providing these services. In this section, we examine if NDRs are associated with analyst bias. The broker's analyst that covers the firm is the responsible agent for NDRs—they organize the logistics, determine invitation lists, and make sure the meetings run smoothly. As a result, any commission revenue allocated to the broker from institutions is credited to the sponsoring analyst.[31] Because analyst compensation is based upon the revenue they generate for the broker firm (Groysberg, Healy and Maber, 2011), organizing NDRs can be lucrative to the analyst.

The incentives created by NDRs are similar to the misaligned incentives created by investment banking business. That is, banking business (or NDR business) has the potential to cloud analysts'

[30] A related question is whether the increased trading through the sponsoring broker is informed. In Section IA.3.2 of the Internet Appendix, we compare the informativeness of trades executed through the sponsoring and non-sponsoring broker. We find evidence that is directionally consistent with trades made through the sponsoring broker being more informed than trades through other brokers; however, the estimates are generally not significantly different from each other.

[31] Our discussions with a buy-side investor confirmed the commission allocation dynamics. He noted that he allocates trades for broker services that he finds valuable. He has the ability to insert "notes" that the Director of Research can observe. For instance, if he was invited to participate in an NDR that he found valuable, he would direct trades to the sponsoring analyst's firm and indicate the reason (i.e., analyst *A's* NDR with firm *X*).

opinions because analysts may use optimistic ratings as a way to curry favor with management and increase their likelihood of being selected for the next deal (Bradley, Jordan and Ritter, 2008; Corwin, Larocque, and Stegemoller, 2017). Recent reforms such as the Global Analyst Research Settlement are intended to mitigate these biases. For example, as part of the Global Settlement, analyst compensation cannot be explicitly tied to banking business. No such policies apply to NDRs. Further, in comparison to banking deals or broker-hosted conferences that are well publicized, NDRs are under the radar, making it much more difficult for investors (particularly smaller, less-sophisticated investors) to detect and adjust for possible biases.

6.1. Univariate statistics of NDR versus Non-NDR brokers

We begin by reporting univariate statistics of analyst and broker characteristics, including measures of analyst bias. The sample consists of all broker-firm-months where the broker issued at least one recommendation or price target for the firm within the prior 24 months. The final sample includes roughly 2.16 million observations, of which 1.57 (1.96) million have non-missing recommendation (target price) data. We split the sample into firm-months where a broker will take the firm on an NDR in the subsequent three months (*NDR3* =1 or *NDR broker*) versus all other broker-firm-months (*NDR3* =0 or *Non-NDR brokers*). We examine the three-month horizon prior to the NDR because conversations with a CFO indicated that his firm tends to plan NDRs roughly three months in advance. The CFO also confirmed that his firm would never select an analyst that had pessimistic views about the company to sponsor the NDR. As he put it, how could a pessimistic analyst market his company to investors? Thus, the three-month period prior to the NDR is likely a period when the chosen brokerage has an especially strong incentive to cater to management.

Table 8 reports analyst and broker characteristics. Detailed definitions of the analyst and broker characteristics are available in Appendix A. We find that NDR brokers are significantly more likely to host the firm at a conference in the subsequent three months (8.15% versus 5.01%). Notably, there is no meaningful difference between NDR brokers and Non-NDR brokers with respect to banking affiliation status (1.04% versus 1.11%, respectively).

Panel B provides statistics on three measures of analyst optimism: *Rec Level, Target Return*, and *Target Return Bias*. *Rec Level* is the analyst's current recommendation, converted to a numeric value using the following scale: 1=strong buy, 2=buy, 3=hold, 4=sell/underperform, and 5=strong sell. *Target Return* is the 12-month expected return (excluding dividends) implied from broker *j*'s most recent 12-month price forecast of firm *i* as of month *t*, computed as (Forecast $\text{Price}_{jit}/\text{Price}_{it-1}$)−1.

Lastly, *Target Return Bias* is the difference between the *Target Return* and the 12-month realized return (excluding dividends).

Across all three measures, we find that NDR brokers are significantly more optimistic than Non-NDR brokers. For instance, the mean average recommendation level for *NDR brokers* is 1.96 compared to 2.38 for *Non-NDR brokers*. This difference is economically large, particularly relative to the cross-sectional standard deviation of *Rec Level* of 0.89. Similarly, NDR brokers' price targets imply an expected return of 28.01% compared to 19.18% for *Non-NDR Brokers*, a spread of 8.83%. The spread in *Target Return Bias* is slightly smaller but still very large (7.41%), suggesting that differences in realized returns cannot explain the majority of the difference in target price optimism.

To offer a richer description of the dynamic relation between analyst optimism and NDRs, we also examine differences in the *Rec Level* of NDR brokers relative to Non-NDR brokers covering the same firm at the same time (*Abnormal Rec Level*) in event time. Figure 4A plots *Abnormal Rec Level* from months –36 to +36, when month 0 is the month of the NDR. Across all months, we find that NDR brokers issue more optimistic recommendations. The change in *Abnormal Rec Level* is fairly small in Year −3 (-0.02), somewhat larger in Year −2 (-0.04), and substantially larger in Year −1 (-0.12). The optimism then sharply declines in the year following the NDR and continues to slowly decline over longer horizons. Figure 3B documents a very similar pattern for *Target Returns*.[32] The event-time patterns are consistent with NDR brokers attempting to curry favor with management in the period immediately prior to the NDR by issuing even more optimistic research.[33]

6.2. Multivariate regressions of analyst optimism

We now consider a multivariate regression that controls for other determinants that are likely to influence analyst research optimism. The formal model is below:

$$Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 Conf3_{jit} + \beta_3 Affiliated3_{jit} + \beta_4 Controls + \text{FE} + \varepsilon_{jit}, \quad (5)$$

where $Optimism_{jit}$ is either *Rec Level* (Specifications 1 and 2) or *Target Return* (Specification 3 and 4). The main variable of interest is *NDR3*. We also include other brokerage activities that have the potential to impact analyst bias. *Conf3* (*Affiliated3*) is an indicator variable equal to one if the firm will participate

[32] Because we compare bias across brokers for the same firm and month, the results for *Target Return* and *Target Return Bias* are identical.

[33] It is perhaps surprising that we observe elevated levels of optimism up to three years prior to an NDR. It is worth noting that brokers frequently sponsor the same firm's NDRs, and thus brokers might also have sponsored NDRs for the firm in months −36 through −1. We find that changes in optimism for brokers that only sponsor a firm's NDR once are far more concentrated over shorter windows around the NDR (see Section IA.3.3 of the Internet Appendix).

in the broker's conference (will become a banking client) in the next three months, and zero otherwise. Not only are *Conf3* and *Affiliated3* important controls, but they also provide a useful benchmark for gauging the magnitude of the bias associated with NDRs.

The remaining variables in the specification (*Controls*) are common broker and analyst-specific controls. *Log (Broker Size)* is the natural log of the number of analysts that a broker employs and is used as a measure of broker prestige and reputation. *Log (Firm experience)* and *Log (Experience)* are the natural logs of the analyst's firm-specific forecasting experience and overall analyst experience, respectively. Both are designed to capture expertise and accuracy. *Log (Firms Followed)* is the natural log of the size of the analyst's coverage portfolio. Analysts with larger coverage portfolios, i.e., busy analysts, have less time to allocate to each individual firm in their portfolio and therefore their accuracy may be hindered. Finally, *All-Star* is an indicator variable equal to one if the analyst was chosen for *Institutional Investor's* annual all-star poll, zero otherwise. All-stars have reputational capital to protect and generally are thought to be less inclined to issue biased forecasts (Stickel, 1992, Fang and Yasuda, 2009). All specifications also include either month or firm-month fixed effects, and standard errors are double clustered by firm and month. All continuous variables are standardized to have mean zero and unit variance.

Table 9 reports the estimates. In specification 1, *NDR3* has a coefficient value of −0.39 with a *t*-statistic of −38.9. This implies that analysts are close to one-half recommendation–level more optimistic about firms that they will take on an NDR in the next three months. The coefficients on *Conf3* and *Affiliated3* are also highly significant. However, the magnitude is less than half of the estimated effect for NDR brokers. The coefficients on the remaining controls are largely consistent with prior research. For example, more reputable analysts, as proxied by broker size or all-star status, issue less optimistic ratings, while more experienced analysts tend to issue more optimistic recommendations.

In Specification 2, we include firm-month fixed effects. This specification compares NDR brokers' research to Non-NDR brokers' research for the same firm at the same time, thereby controlling for a number of important differences that could potentially justify different levels of optimism, including future realized performance. However, if other brokers also issue optimistic research in hopes of winning the firm's NDR business, the inclusion of firm-month fixed effects could understate the extent to which NDRs induce bias. We find that the estimate on *NDR3* declines but remains economically large at −0.29 and highly statistically significant. The inclusion of firm-month fixed effects has a more severe impact on the coefficients *Conf3* and *Affiliated3*. The point estimate

now suggests that the excess optimism for NDR brokers is nearly five times as large as the excess optimism for brokers with a conference-hosting relation, and nearly six times as large as brokers with an investment banking affiliation.

Specifications 3 and 4 present analogous results where *Target Return* is the dependent variable. Similar to recommendation levels, NDR brokers issue significantly more optimistic target prices. For example, in Specification 4, the coefficient estimate implies that NDR analysts issue 12-month target prices that are 4.46% more optimistic than non-NDR analysts. The economic magnitudes continue to be substantially larger than the optimism associated with hosting a firm at a conference (1.47%) or being the lead underwriter for an investment banking deal (1.14%). In the Internet Appendix, we also repeat the tests after replacing the level of recommendation optimism with either *Upgrade,* an indicator variable equal to one if the analyst revises his (or her) recommendation level upward (e.g., from a buy to a strong buy) for a firm in that month, or *Downgrade*, defined analogously. The results of this analysis, reported in Table IA.10 of the Internet Appendix, confirm that *NDR3* is significantly positively associated with *Upgrade* and significantly negatively associated with *Downgrade.*

6.3. Multivariate regressions of analyst optimism – cross-sectional patterns

We next examine whether analyst optimism around NDRs varies systematically with analyst, firm, and NDR characteristics. In choosing a level of optimism prior to an NDR, we conjecture that analysts trade off the benefits in the form of greater trading commissions (Section 5) and valuable management access (Green et al., 2014b) with the costs of reputation loss and diminished long-term career prospects (Fang and Yasuda 2009; Altinkilic, Balashov, and Hansen, 2019). Similarly, in selecting the analyst to sponsor the NDR, we conjecture that firms value analyst optimism as well as the analyst's ability to add value when organizing the NDR.

Based on these tradeoffs, we make the following predictions. First, analysts will compete more aggressively (i.e., issue more optimistic research) for NDRs when the expected trading commissions associated with sponsoring the NDR are larger. We consider three proxies for the expected trading commissions associated with sponsoring the NDR: 1) *Multi-Day NDR* - an indicator equal to one if the NDR trip spans multiple days; 2) *Big Inst. NDR* - an indicator equal to one if the firm is visiting a city that has a top five concentration of institutional ownership, and 3) *Turnover* - a proxy for the intensity of trading in the firm's shares. We also expect that management access may be more valuable for institutional investors (and thus, too, the sell-side analysts who have incentives to please them) for hard-to-value firms, such as small firms (*Firm Size*) and more volatile firms *(Volatility*). In addition, holding other firm characteristics constant, analysts may need to compete more aggressively when the

firm has a larger pool of analysts to choose from, as measured by existing analyst coverage (*Coverage*). Finally, we expect that analysts with greater reputation, as proxied by all-star status (*All-Star*), experience as an analyst (*Experience*), and the size of the brokerage firm employing the analysts (*Broker Size*), are less likely to issue biased research in order to win an NDR for two reasons. First, the potential reputation costs associated with issuing biased research are likely larger for analysts that have built a strong reputation for themselves (Fang and Yasuda, 2009). Second, more reputable analysts are likely able to add more value when organizing NDRs, and thus may not need to inject as much bias to win NDR business.[34]

We test these predictions by estimating the following regression:

$$Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 NDR3_{jit} \times CV + \beta_3 Conf3_{jit} + \beta_4 Affiliated3_{jit} + \beta_5 Controls + FE + \varepsilon_{jit.} \quad (6)$$

Optimism, NDR3, Conf3, Affiliated3, and *Controls* are defined as in Equation 5, and *CV* is a vector of the following conditioning variables: *Multi-Day NDR, Big Inst. NDR, Turnover*, *Firm Size*, *Volatility*, *Coverage*, *All-Star, Experience,* and *Broker Size*. More detailed definitions of all the conditioning variables are provided in Appendix A. Finally, FE denotes firm-month fixed effects.

Specifications 1 through 3 of Table 10 report the results where *Rec Level* is the dependent variable. Specification 1 includes the conditioning variables associated with NDR or firm attributes, Specification 2 reports the results for analyst-level attributes, and Specification 3 reports the results for all attributes. Turning to Specification 3, we find the results are generally consistent with the predicted effects. For example, all three of the proxies for expected trading commissions (*NDR3* × *MultiDay, NDR3* × *Big Inst. NDR,* and *NDR3* × *Turnover*) are at least marginally significant ($p \leq 0.10$) and in the predicted direction. The relation between optimism and valuation difficulty is more mixed, with optimism decreasing with firm size (as predicted) but also decreasing in volatility (in contrast to the predicted effect). We find strong support for the prediction that recommendation level optimism is increasing in potential NDR competition, as proxied by analyst coverage. The point estimate implies that a one-standard deviation increase in analyst coverage is associated with 0.08 increase in recommendation optimism. Finally, consistent with optimism declining with analyst reputation, we find that recommendation level optimism is weaker among analysts with greater experience and all-star analysts. Specifications 4 through 6 report analogous results for target price optimism. While there are some differences (e.g., the correlation between target price optimism and analyst reputation is

[34] In a survey of investor relations officers, Brown et al. (2019) find that experience, brokerage size, and all-star status are three of the most important characteristics associated with analysts' ability to help firms convey their companies' message to institutional investors (see their Table 3).

weaker), the patterns are generally similar. For example, we continue to find strong evidence that analyst optimism is correlated with proxies for expected trading commissions and analyst coverage.

6.4. NDR broker optimism: Strategic or sincere?

The findings from the prior sections are consistent with NDR brokers strategically issuing optimistically biased research in order to gain favor with management and increase the likelihood that they take the firm on an NDR (hereafter "strategic optimism"). However, an alternative explanation is that some analysts are sincerely optimistic about a firm's prospects, and firms simply select these optimistic analysts to organize their NDRs (hereafter "sincere optimism").[35]

To disentangle strategic versus sincere optimism, we follow Malmendier and Shantikumar (2014), who argue that sincerely optimistic analysts will issue both optimistic recommendations and optimistic earnings forecasts, while strategically optimistic analysts will issue optimistic recommendations coupled with more negative (or "beatable") earnings forecasts. Intuitively, since earnings forecasts are a critical input into recommendation levels (e.g., Brown et al., 2015), an analyst with a sincerely optimistic recommendation will tend to have more optimistic earnings projections as well. On the other hand, since managers generally like both optimistic recommendations and beatable earnings targets (Richardson, Teoh, and Wysocki, 2004), analysts attempting to curry favor with management have incentives to issue optimistic recommendations coupled with more pessimistic short-term quarterly earnings forecasts.

We examine NDR brokers' short-term earnings forecast bias by re-estimating Equation 5 after replacing the dependent variable with two measures of pessimism from quarterly earnings forecasts. The first, *MBE*, is an indicator variable equal to one if the firm's realized earnings meets or beats the analyst's estimated earnings. The second, *Relative Earnings Pessimism*, is computed as: [(*Rank* – 1)/ (*Number of Analysts* -1)]. *Rank* is the rank of the analyst's forecasted earnings estimate, where the highest estimate is given a rank of 1, the second highest estimate is given a rank of 2, etc., and *Number of Analysts* is the number of analysts issuing a forecast for the firm-quarter. Thus, higher values of *MBE* and *Relative Earnings Pessimism* indicate greater pessimism.

Table 11 reports the results. Specifications 1 and 2 document a significant positive relation between *NDR3* and *MBE*. Similarly, Specifications 3 and 4 document a positive relation between *NDR3* and *Relative Earnings Pessimism*. Both results suggest that NDR brokers tend to issue more pessimistic quarterly earnings forecasts, which is inconsistent with sincere optimism. This finding,

[35] We note that even the more innocuous *Sincere Optimism* explanation implies that brokers face strong incentives to issue optimistic research to win NDRs, but it argues that brokers (for whatever reason) do not respond to these incentives.

coupled with the evidence from the previous section linking analyst optimism to analysts' incentives to win NDR business (e.g., increased trading commission benefits), suggests that strategic analyst behavior is responsible for at least a portion of the observed correlation between NDR activity and analyst optimism.

7. Conclusion

We examine the effects of non-deal roadshows (NDRs) on the informativeness of institutional and retail trading, and we investigate the conflicts of interest that they create for analyst equity research. We show that institutional investors located close to a city where a firm attends an NDR substantially increase their trading in the firm, and this trading becomes significantly more informed, while retail investor trading becomes significantly less informed in the weeks following an NDR.

We also document that institutions reward brokers who organize NDRs through increased commission revenues, which suggests that NDRs can be lucrative for analysts and thus create conflicts of interest in the same fashion as investment banking business. Consistent with this view, we show that brokerages that are about to take a firm on an NDR have significantly more optimistic recommendations and target price forecasts, and the optimism peaks in the NDR event month. In addition, we document that while NDR brokers issue *more* optimistic recommendations and target prices, they issue *less* optimistic short-term earnings forecasts. This seemingly incongruent pattern is consistent with NDR brokers' research suffering from strategic distortions aimed at currying favor with management (Malmendier and Shanthikumar, 2014).

Our findings have direct implications regarding two of the most important regulatory reforms pertaining to sell-side analysts in the past several decades: Regulation Fair Disclosure (Reg FD) and the Global Analyst Research Settlement. In the interest of providing more equal access to information across investors, Reg FD prohibits the selective disclosure of material information. However, it does continue to allow for private meetings between investors and management provided that material, non-public information is not disclosed. Our results suggest that NDRs are providing an informational advantage to local institutional investors. We acknowledge that this information advantage need not relate to material information. For example, private meetings may allow institutional investors to benefit by combining public information with nonmaterial nonpublic information (i.e., the "mosaic theory"). Nevertheless, at a minimum, our results suggest that NDRs run counter to Reg FD's stated objective of creating a more level playing field.

The Global Settlement (and other related regulations including NYSE Rule 472 and NASD Rule 2711) aims to minimize analyst conflicts of interest by severing the ties between the corporate

finance and research divisions of investment banks, including analyst compensation tied to generating banking business. The regulations also mandate improved disclosure, including disclosing whether the brokerage house has an investment banking affiliation with the firm. Importantly, NDRs do not fall under the Global Settlement or related regulations, yet our evidence suggests the potential conflicts are just as economically large. Further, in comparison to banking deals where the identity of the organizing broker is publicly available, NDRs are generally not publicly disclosed, making it much more difficult for investors to recognize this bias. This raises the important question of whether brokers should also be required to disclose their NDR affiliations with firms.

Appendix A: Variable Definitions

A.1 Firm Characteristics

- *Intangibles*: recognized intangibles (33) divided by total assets (6) Winsorized at the 99th percentile. (Source: Compustat).
- (*R&D + ADV)/OE*: R&D expense (46) plus advertising expense (45) divided by total operating expenses. Missing values of R&D and advertisting are set equal to zero. Winsorized at the 99th percentile. (Source: Compustat).
- *Market-to-Book* (*MB*)*:* the market-to-book ratio computed as the market capitalization at the end of the calendar year scaled by book value of equity during year *t-1*. Positive values are winsorized at the 99th percentile. Negative values are set equal to zero and we include a corresponding negative book value indicator (*Negative Book*, often untabulated). (Source: CRSP/Compustat).
- *Idiosyncratic Risk:* the square root of the mean squared residual from an annual regression of a firm's daily returns on the market (value-weighted CRSP index) returns. (Source: CRSP).
- *Institutional Ownership*: the percentage of the firm's shares held by institutions at year end. Winsorized at 100%. (Source: Thomson Reuters S34).
- *Firm Age*: the number of years since the firm first appeared on CRSP. (Source: CRSP).
- *Net Shares*: the natural log of the ratio of the split-adjusted shares outstanding at the fiscal year end in *t-1* divided by the split adjusted shares outstanding at the fiscal year end in *t-2*. (Source: Compustat).
- *SEO*: a dummy variable equal to one if a firm will issue a Seasoned Equity Offering in the next two years. (Source: SDC).
- *M&A Acquirer*: a dummy variable equal to one if a firm will announce the acquisition of another firm in the next two years (Source: SDC).
- *Coverage:* the number of analysts issuing earnings forecasts for firm *i* during quarter *t*.
- # *Institutions*: the number of institutions holding firm shares at year end. (Source: Thomson Reuters S34).
- *Firm Size*: the market capitalization computed as share price times total shares outstanding at the end of June (Source: CRSP).
- *Turnover*: the average daily turnover (i.e., share volume scaled by shares outstanding) over all trading days in the year. Winsorized at 99th percentile. (Source: CRSP).
- *R-squared*: the r-squared from an annual regression of a firm's daily returns on the market (value-weighted CRSP index) returns. Winsorized at the 99th percentile. (Source: CRSP).
- *Ret (m-1):* the return in the prior month. (Source: CRSP).
- *Ret (m-12, m-2):* the return in the prior two to twelve months. (Source: CRSP).
- *Ret (m-7, m-2):* the return in the prior two to seven months. (Source: CRSP).
- *Ret (w-1):* the return in the prior week (Source: CRSP).
- *Vol:* the standard deviation of daily returns over the prior calendar year (Source: CRSP).
- *Book-to-Market (BM):* the book-to-market ratio computed as the book value of equity during year *t-1* scaled by the market capitalization at the end of the calendar year. Positive values are winsorized at the 99th percentile. Negative values are set equal to zero and we include a corresponding negative book value indicator (untabulated). (Source: CRSP/Compustat).

A.2 Institutional Trading Measures and Institutional Investor Characteristics

- *Total Trading:* the absolute value of the difference in split-adjusted shares held from quarter *t-1* to quarter *t,* scaled by the firm's total shares outstanding. This measure is aggregated to a city-quarter level by summing *Total Trading* across all institutions local to city *c* in quarter t. (Source: Thomson Reuters S34).
- *Total Net Trading:* the (signed) value of the difference in split-adjusted shares held from quarter *t-1* to quarter *t,* scaled by the firm's total shares outstanding. This measure is aggregated to a city-quarter level by summing *Total Net Trading* across all institutions local to city *c* in quarter t and then taking the absolute value. (Source: Thomson Reuters S34).
- *Local (Non-Local) OIB:* Local (non-local) institutional buy volume less local (non-local) institutional sell volume, scaled by total institutional volume. (Source: Thomson Reuters S34).
    - An institutional investor is classified as local (non-local) to a city if the investor is headquartered within (outside of) 100 kilometers of the city's downtown.
- *Local NDR:* a dummy variable equal to one if firm *i* participated in an NDR in city *c* during the previous quarter, and zero otherwise.
- *Non-Local NDR:* a dummy variable equal to one if firm *i* participated in an NDR during the previous quarter, but not in city *c*, and zero otherwise.
- *Hedge Fund (*HF): an indicator equal to one if (1) at least 50% of its clients are "Other pooled investment vehicles" or "High net worth individuals" and (2) it charges performance-based fees. (Source: Thomson Reuters S34 and Form ADV).
    - *Non-Hedge Fund (Non-HF):* Any 13F filing institution not classified as a *Hedge Fund.*
- *Fund Turnover:* the average of a fund's quarterly turnover in the prior calendar year, where quarterly turnover is computed as the dollar volume traded by the fund scaled by the total dollar value of the fund's holdings. (Source: Thomson Reuters S34).
    - *High (Low) Turnover:* indicators equal to one if the fund is in the top (bottom) half of *Fund Turnover*.
- *Fund Ownership:* the total shares of the stock owned by a fund scaled by total shares outstanding. (Source: Thomson Reuters S34).
    - *High (Low) Ownership:* indicators equal to one if the fund is in the top quintile (bottom four quintiles) of *Fund Ownership*.
- *Sponsor OIB*: the total shares of the firm bought through the sponsoring broker on the date minus the total shares of the firm sold through the sponsoring broker on the date, scaled by total trading volume in the firm through the sponsoring broker on the date. (Source: Abel Noser).
    - *Sponsor Buy:* an indicator equal to one if *Sponsor OIB* is greater than zero, and zero if less than zero.
- *Non-Sponsor OIB*: the total shares of the firm bought through the non-sponsoring broker on the date minus the total shares of the firm sold through the non-sponsoring broker on the date, scaled by total trading volume in the firm through the non-sponsoring broker on the date. (Source: Abel Noser).
    - *Non-Sponsor Buy:* an indicator equal to one if *Non-Sponsor OIB* is greater than zero, and zero if less than zero.

A.3 Retail Trading Measures

- *Retail OIB:* Daily retail buy volume less retail sell volume, scaled by total retail volume. Retail trading is estimated using the approach outlined in Boehmer, Jones, Zhang, and Zhang. (2020) (Source: TAQ).

A.4 Commission Measures

- *$Commissions ($Com.):* the natural log of 1 plus the total dollar commissions for broker *j* in stock *i* during week *t*. (Source: Abel Noser).
- *Commission Share (CS):* the total commissions for broker *j* in stock *i* during week *t* scaled by total Abel Noser commissions across all I/B/E/S-Abel Noser matched brokers for stock *i* in week *t*. (Source: Abel Noser).

A.5 Analyst and Broker Characteristics

- *NDR3:* an indicator variable equal to one if broker *j* will take firm *i* on an NDR in the subsequent three months (i.e., t through t+2), and zero otherwise. (Source: *FLY*).
- *Conf3:* an indicator variable equal to one if broker *j* will host firm *i* at one of its investor conferences over the next three months (i.e., t through t+2), and zero otherwise. (Source: Bloomberg Corporate Events Database).
- *Affiliated3:* a dummy variable equal to one if broker *j* will be a lead underwriter for firm *i* for an equity (i.e., SEO) offering or debt offering, or will be lead advisor on an M&A in the next three months, and zero otherwise. (Source: SDC).
- *Broker Size:* the total number of analysts issuing earnings forecasts for brokerage firm *j* during year *t*. (Source: I/B/E/S).
- *Firm Experience:* the number of years since analyst *j* first issued earnings forecasts for firm *i*. (Source: I/B/E/S).
- *Experience:* the number of years since analyst *j* first issued earnings forecasts for any firm. (Source: I/B/E/S).
- *Firms Followed*: the number of firms followed by analyst *j* in year *t*. (Source: I/B/E/S).
- *All-Star:* a dummy variable equal to one if analyst *j* is ranked as an All-American (first, second, third, or runner-up) in the annual polls. (Source: *Institutional Investor Magazine*).
- *NDR Broker:* a broker that sponsors at least one NDR for a given firm during the sample period. (Source: *FLY*).
    - *Single Sponsor:* an NDR broker that sponsors only one NDR for a given firm during the sample period.
    - *Multiple Sponsor:* an NDR broker that sponsors more than one NDR for a given firm during the sample period.
- *Bulge Bracket*: an indicator equal to one if the brokerage firm is one of the nine bulge bracket banks according to the Corporate Finance Institute (JP Morgan, Deutsche Bank, UBS, Bank of America, Credit Suisse, Morgan Stanley, Goldman Sachs, Barclays, and Citi).

A.6 Research Characteristics

- *Rec Level:* the most recent outstanding recommendation of broker *j* for firm *i* in month *t*. If the brokerage firm *j* has not issued a recommendation for firm *i* in the previous 24 months, we set the value to missing. Recommendations are converted to numeric values using the following scale: 1 for strong buy, 2 for buy, 3 for hold, 4 for sell/underperform, and 5 for strong sell. (Source: I/B/E/S).
    - *Abnormal Rec Level:* the *Rec Level* of a broker less the *Rec Level* of all other brokerage firms covering the same firm during the same month.
    - *Lag (Rec Level):* the *Rec Level* of broker j for firm *i* in month *t-1*.
- *Upgrade*: an indicator variable equal to one if the recommendation level was more favorable in month *t* than month *t-1* (e.g., moving from a hold to a buy). (Source: I/B/E/S).
- *Downgrade*: an indicator variable equal to one if the recommendation level was less favorable in month *t* than month *t-1* (e.g., moving from a buy to a hold). (Source: I/B/E/S).
- *Target Return:* the 12-month expected return (excluding dividends) implied from broker *j*'s most recent price forecast of firm *i* as of month *t*, computed as (Forecast $\text{Price}_{jit}/\text{Price}_{it-1}$)−1. The sample is limited to 12-month ahead forecasts. If the brokerage firm *j* has not issued a target price for firm *i* in the previous 24 months, we set the value to missing. We winsorize at the 1st and 99th percentile. (Source: I/B/E/S).
    - *Abnormal Target Return:* the *Target Return* of a broker less the *Target Return* of all other brokerage firms covering the same firm during the same month.
- *Target Return Bias:* the difference between the *Target Return* and the 12-month realized return (excluding dividends). We winsorize at the 1st and 99th percentile. (Source: I/B/E/S).
- *Meet or Beat Earnings (MBE)*: a dummy variable equal to one if firm *i's* realized quarterly earnings are greater than analyst *j's* most recent quarterly earnings forecast for firm *i* as of month *t*. (Source: I/B/E/S).
- *Relative Earnings Pessimism:* [(*Rank* – 1)/ (*Number of Analysts* -1)]. *Rank* is the rank of the analyst's forecasted earnings estimate, with the highest estimate value being given a ranking of 1, the second highest estimate is given a rank of 2, etc., and *Number of Analysts* is the number of analysts issuing a forecast for the firm-quarter. (Source: I/B/E/S).

A.7 NDR Characteristics

- *Multi Day NDR*: an NDR trip that spans more than one day. (Source: *FLY*).
- *Big Inst. NDR*: an indicator equal to one if the firm is visiting a city that has a top five concentration of institutional ownership. (Source: *FLY*).
- *Hand*: an indicator equal to one if the NDR was obtained from our emails to Fortune 1000 IROs (and/or our phone conversations with them) or our contact at a large institutional investor.
- *FLY Missing*: an indicator equal to one if the NDR we obtained from the hand-collected sample (described above) was not reported by *FLY*. (Source: *FLY*).

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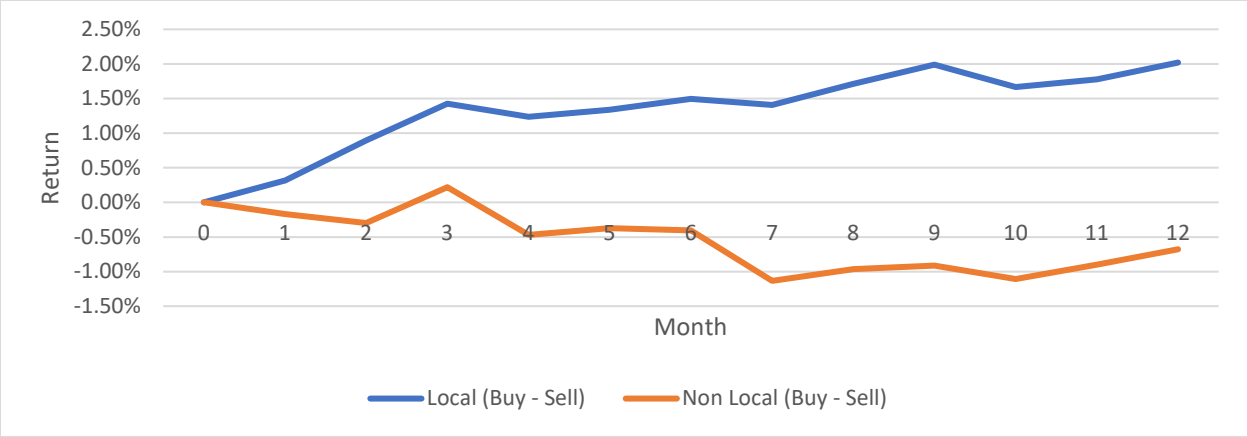
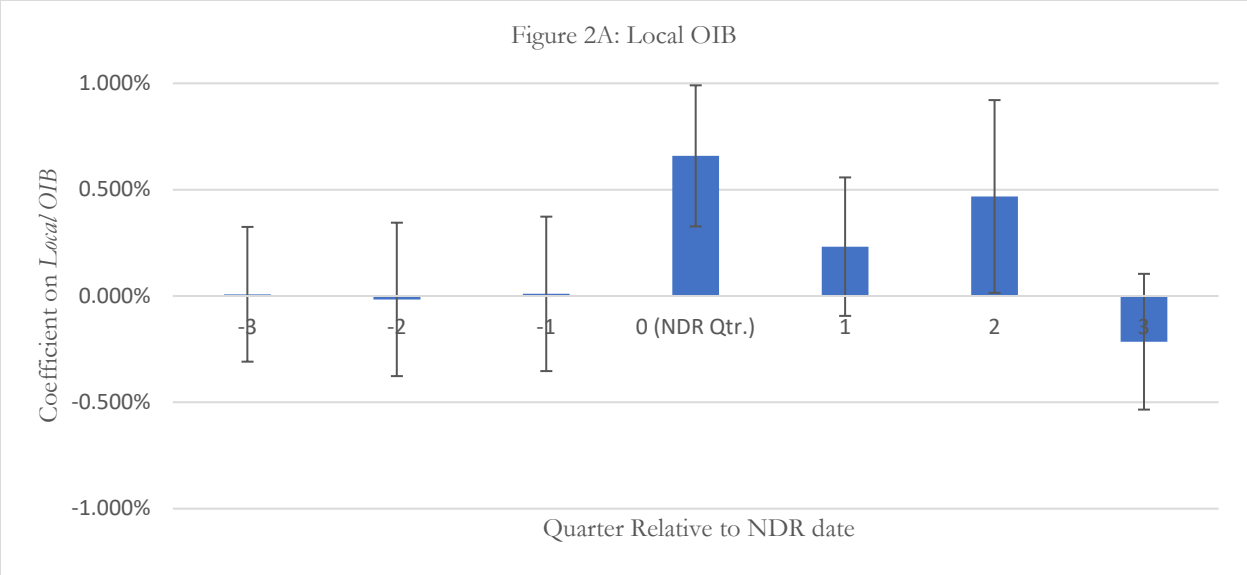


Figure 1: NDRs and the Informativeness of Institutional Trading
At quarter end from March 2013 through December 2019, we sort all NDR firms during the quarter into terciles based on the order imbalances of institutions who are headquartered within 100 kilometers of the NDR location (*Local*) and all other institutions (*Non-Local*). We define *Local OIB* as the total shares of firm *i* bought by all local institutions in quarter *t* less the total shares of firm *i* sold by all local institutions in quarter *t*, scaled by total local institutional trading volume of firm *i* in quarter *t*. *Non-Local OIB* is defined analogously. *Local* reports the cumulative market-adjusted return to a strategy that buys stocks in the top tercile of *Local OIB* and sells stocks in the bottom tercile of *Local OIB* for horizons ranging from one month to 12 months after the end of the quarter. *Non-Local* reports the returns to the analogous strategy based on *Non-Local OIB*.



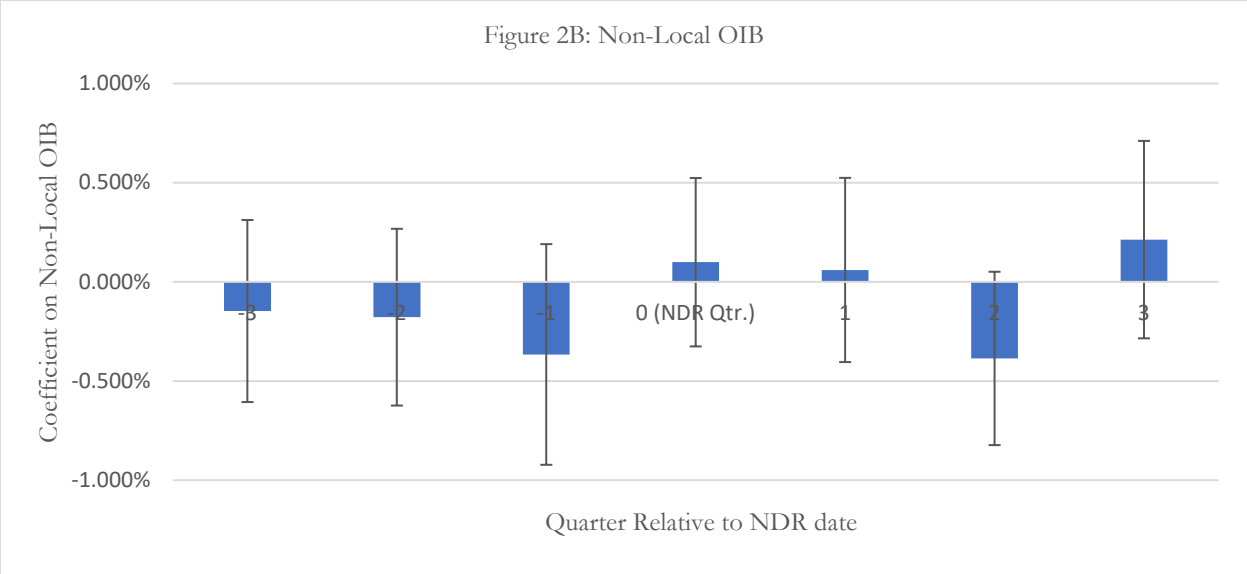


Figure 2: NDRs and the Informativeness of Institutional Trading – Pre and Post NDR
This figure examines the informativeness of local and non-local institutional trading in each of the three quarters prior to and after the NDR quarter. We repeat the regression in Specification 1 of Table 4, after shifting the timing of the NDR. For example, for quarter (-3) we examine local and non-local institutional trading three quarters prior to the firm conducting the NDR. Figure 2A (2B) plots the estimates and 95% confidence intervals for *Local OIB* (*Non-Local OIB)* for each of the seven separate regressions over the [-3,3] interval. The confidence intervals are computed from standard errors double clustered by firm and quarter.

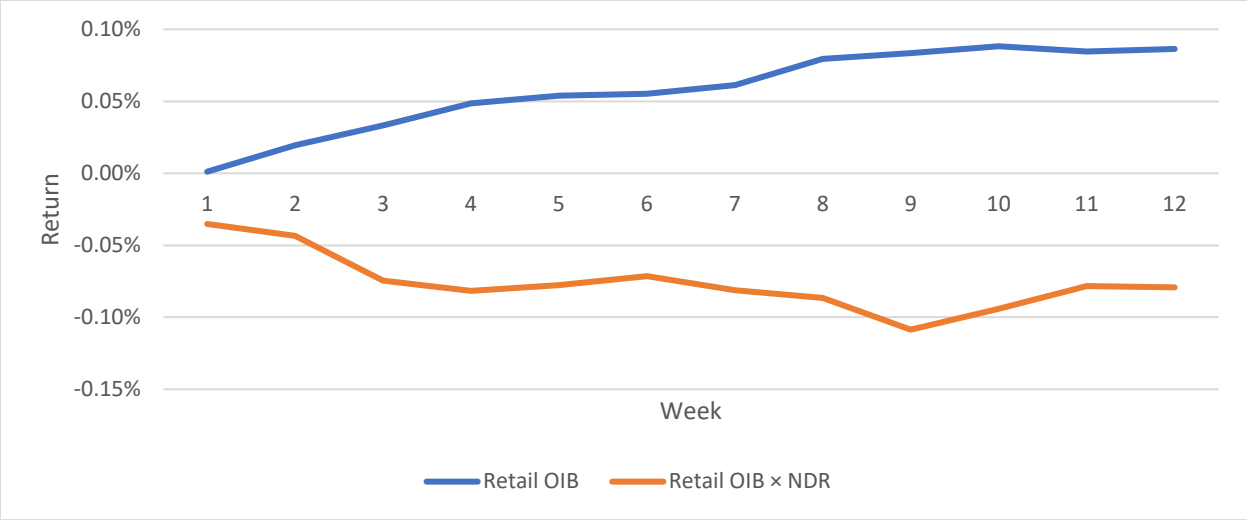
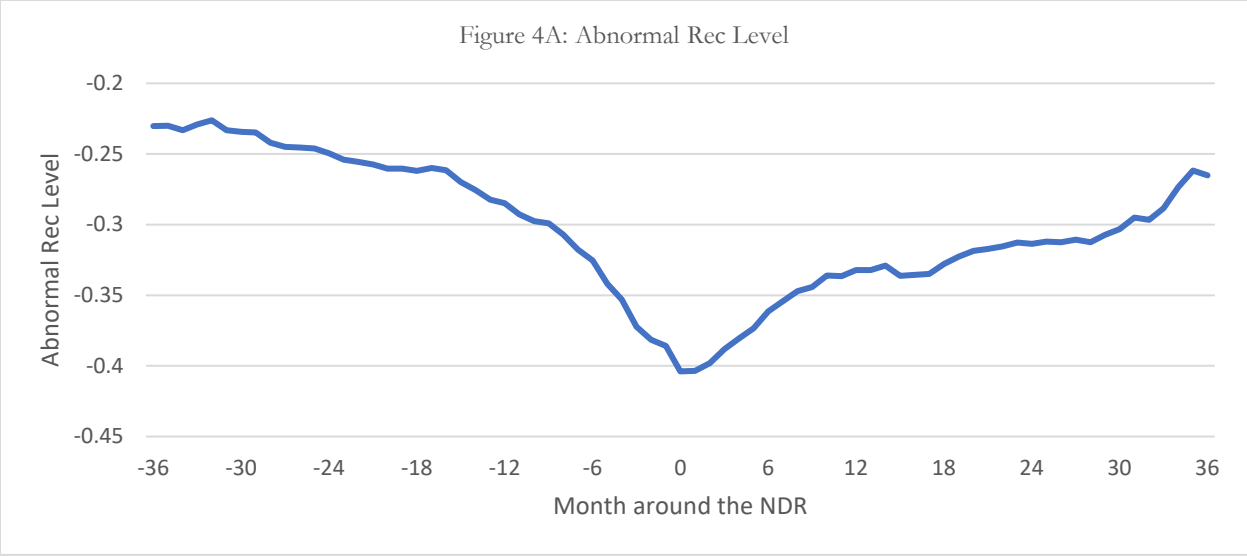


Figure 3: NDRs and the Informativeness of Retail Trading
This figure repeats the regressions reported in Table 6, after replacing the dependent variable with cumulative returns from weeks 1 through 12. Returns are computed assuming that all trades for stock with positive (negative) retail order imbalances are executed at the retail-volume weighted average purchase (sale) price. The figure plots the estimates on *Retail OIB* and *Retail OIB x NDR* from 12 separate regressions (weeks 1 through 12).



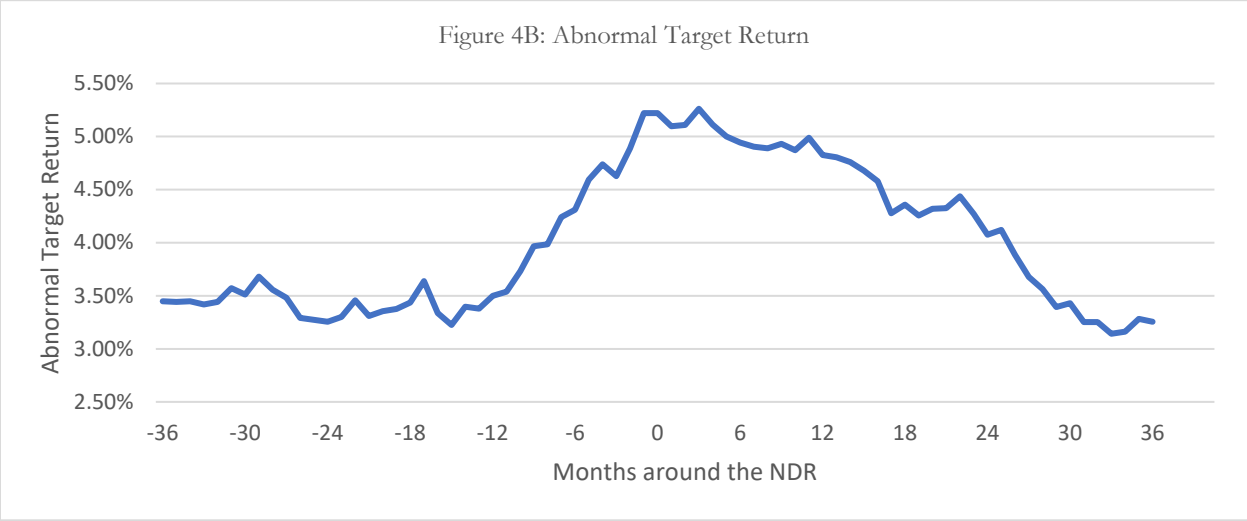


Figure 4: Relative Optimism of NDR Brokers around Non-Deal Roadshows
For each NDR, we plot the optimism of the brokerage firm that takes the firm on the NDR (*NDR Broker)* relative to all other brokerage firms covering the same firm during the same month (*Abnormal Optimism)*. Panel A plots the results where the optimism measure is computed using recommendation levels (*Abnormal Rec Level*), where strong buy =1 and strong sell = 5 (and thus a more negative recommendation level indicates greater optimism). Panel B plots the results where the optimism measure is computed using target returns (*Abnormal Target Return*). We plot *Abnormal Optimism* from three years prior to the NDR (-36) to three years after the NDR (+36).

Table 1: Non-Deal Roadshow (NDR) Summary Statistics

This table reports summary statistics for the sample of non-deal roadshows and investor conferences from January 2013 to December 2019. In Panel A, *Firm-Day-City* reports the total number of unique firm-date-location NDRs (i.e., Apple, 1/3/2013, New York City), *Firm-Month* reports the number of NDRs at the firm-month level (i.e., Apple January 2013), *Firms* is the number of firms that went on an NDR, and *Brokers* is the number of brokerage firms that organized an NDR. *Full Sample* includes all common stocks in the intersection of CRSP and Compustat with a price above $1 as of the end of the previous month. *I/B/E/S Matched* imposes the filter that the NDR was sponsored by a brokerage firm in the *I/B/E/S* database, and *Top 30 Cities* includes NDRs that visit one of the top 30 US cities (reported in Panel C). In Panel B, *Conf. Presentations* reports the number of firm presentations at investor conferences, Panel C reports the frequency of NDRs for the top 30 NDR destinations in the US and reports the percentage of total institutional trading that is driven by institutional investors that are located within 100 kilometers of the city (*Total Trading*), averaged across all stocks.

Panel A: NDRs

	Firm-Day-City	*Firm-Months*	*Firms*	*Brokers*
Full Sample	43,799	24,809	3,541	112
I/B/E/S Matched	43,550	24,656	3,526	89
Top 30 Cities	32,324	20,100	3,433	106

Panel B: Investor Conferences

	Conf. Presentations	*Firm-Months*	*Firms*	*Brokers*
Full Sample	109,486	73,364	4,418	368
I/B/E/S Matched	71,095	53,482	4,180	93

Panel C: Frequency of NDRs (Top 30 Destinations)

	NDRs (Firm-Days)	*Total Trading*	*Firms*	*Brokers*
New York	8,881	36.84	2,771	92
Boston	5,389	11.30	2,238	77
San Francisco	2,927	3.91	1,471	74
Chicago	2,774	13.51	1,556	72
Los Angeles	1,743	3.03	1,032	68
Denver	1,313	1.28	800	52
Minneapolis	1,146	0.95	778	51
Kansas City	1,135	0.95	722	47
Milwaukee	1,033	0.88	708	51
Dallas	828	1.13	554	58
Baltimore	690	5.25	509	48
Philadelphia	648	2.60	485	47
San Diego	506	0.40	371	43
Houston	452	1.02	309	46
Portland	432	0.33	313	42
Atlanta	371	1.32	246	44
St. Louis	336	1.27	261	31
Seattle	304	1.20	219	43
Detroit	285	0.29	224	33
Salt Lake City	209	0.48	155	24
Austin	185	0.67	116	33
Orlando	129	0.44	88	28
Las Vegas	121	0.01	62	24
Cleveland	103	0.13	77	31
Charlotte	89	0.29	72	23
Columbus	67	0.17	46	22
Washington, D.C.	61	5.12	52	20
Cincinnati	58	0.41	36	19
Richmond	57	0.34	39	20
Tampa	52	0.77	49	21

Table 2: Determinants of Non-Deal Roadshows (NDRs)

This table reports estimates from OLS linear probability models. The sample includes the universe of CRSP-Compustat firms from 2013-2019 with non-missing data for all the independent variables and a price greater than $1 at the end of the previous month. The dependent variable is an indicator variable equal to one if the firm attends at least one non-deal roadshow (NDR) in the given month and zero otherwise. All independent variables are defined in Appendix A. All continuous variables are standardized to have mean zero and unit variance. Specification 1 includes month fixed effects, and Specification 2 includes month and firm fixed effects. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Broker NDRs - Dummy	Broker NDRs - Dummy
	[1]	[2]
Intangibles	0.90%	0.95%
	(6.79)	(3.19)
(R&D + ADV)/OE	1.06%	0.51%
	(7.40)	(2.57)
Log (MB)	1.61%	0.65%
	(7.06)	(2.67)
Negative Book	4.74%	2.23%
	(4.89)	(2.08)
Idiosyncratic Risk	0.12%	0.11%
	(0.92)	(0.75)
Institutional Ownership	1.49%	0.22%
	(6.72)	(0.69)
Log (Firm Age)	-0.21%	-0.96%
	(-1.82)	(-3.52)
Net Shares	0.13%	(0.00)
	(0.77)	(0.30)
Log (Coverage)	2.12%	-0.18%
	(10.67)	(-0.71)
Log (# Institutions)	-0.66%	0.12%
	(-2.87)	(0.39)
Log (Firm Size)	1.06%	3.95%
	(4.07)	(10.34)
Log (Turnover)	0.17%	0.01%
	(1.25)	(0.04)
R-squared	0.32%	0.15%
	(1.84)	(1.00)
Ret (m-1)	0.43%	0.35%
	(8.12)	(7.21)
Ret (m-12, m-2))	1.01%	0.64%
	(11.02)	(8.19)
SEO	0.75%	0.12%
	(2.59)	(0.37)
M&A - Acquirer	0.44%	0.57%
	(1.84)	(2.44)
Fixed Effects	Month	Month & Firm
R-squared	4.50%	9.84%
Observations (Firm-Months)	277,364	277,364
Mean of Dependent Variable	8.21%	8.21%

Table 3: Intensity of Institutional Trading around NDRs

This table examines the intensity of institutional trading around local NDRs. The unit of observation is a firm-city-quarter, where the sample of cities includes the 30 cities reported in Panel C of Table 1. For each firm-city-quarter, we compute *Total Trading* as the total volume traded by institutions located within 100 kilometers of the city (*Local Institutions*), scaled by shares outstanding, and we compute *Total Net Trading* as Abs(*Total Buying – Total Selling*), where *Total Buying (Total Selling*) is the total volume purchased (sold) by local institutions, scaled by shares outstanding. Panel A presents a univariate comparison of *Total Trading* and *Total Net Trading* when the firm went on an NDR to that city in that quarter (i.e., Local NDR =1) versus all other firm-city-quarters (i.e., Local NDR =0). Panel B reports results from the following panel regression:

$$\text{Trading}_{cit} = \alpha + \beta_1 Local\ NDR_{cit} + \beta_2 Non\text{-}Local\ NDR_{cit} + FE + \varepsilon_{cit.}$$

The dependent variable is either *Total Trading* or *Total Net Trading*. *Local NDR* is defined as above, and N*on-Local NDR*$_{cit}$ is an indicator variable equal to one if firm *i* attended an NDR in quarter *t* but did not visit city *c*. *FE* includes city fixed effects and either firm and quarter fixed effects (Specifications 1 and 4) or firm-quarter fixed effects (Specifications 2, 3, 5, and 6). Standard errors are double clustered by firm and quarter, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate. The sample spans from January 2013 through December 2019.

Panel A: Univariate Comparison

	Local NDR = 1 (N = 21,727)			Local NDR = 0 (N = 2,137,826)		
	Mean	Median	Std Dev.	Mean	Median	Std Dev.
Total Trading	1.71%	0.34%	2.87%	0.33%	0.00%	1.24%
Total Net Trading	1.03%	0.19%	1.97%	0.25%	0.00%	0.97%

Panel B: Regression Results

	Total Trading	*Total Trading*	*Log (Total Trading)*	*Total Net Trading*	*Total Net Trading*	*Log (Net Trading)*
	[1]	[2]	[3]	[4]	[5]	[6]
Local NDR	0.29%	0.31%	0.63	0.15%	0.16%	0.57
	(12.76)	(12.71)	(14.24)	(9.96)	(9.99)	(12.90)
Non-Local NDR	-0.01%			0.00%		
	(-2.45)			(-1.01)		
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	No	No	Yes	No	No
Firm Fixed Effects	Yes	No	No	Yes	No	No
Firm-Quarter Fixed Effects	No	Yes	Yes	No	Yes	Yes
R-squared	35.14%	36.43%	55.90%	21.88%	23.64%	54.28%
Obs. (Firm-City-Quarters)	2,144,809	2,144,809	2,144,809	2,144,809	2,144,809	2,144,809

Table 4: NDRs and the Informativeness of Local Institutional Trading

This table reports estimates from the following panel regression:

$$Ret_{it+x} = \alpha + \beta_1 Local\ OIB_{it} + \beta_2 Non\text{-}Local\ OIB_{it} + \beta_3 Char_{it} + Qtr_t + \varepsilon_{it}.$$

The sample includes all firm-quarters with NDR activity and non-zero trading by local and non-local institutional investors in the firm-quarter from January 2013 through December 2019. Ret_{it+x} is the one-quarter return for firm *i* in quarter *t*+*x*, where quarter *t* is the quarter when the firm conducted an *NDR*. *Local OIB* is the total shares of firm *i* bought by all local institutions in quarter *t* less the total shares of firm *i* sold by all local institutions in quarter *t*, scaled by total local institutional trading volume of firm *i* in quarter *t,* and *Non-Local OIB* is defined analogously. *Char* is a vector of firm characteristics taken from Boehmer, Jones, Zhang and Zhang (2020) and defined in Appendix A. *Qtr* denotes quarter fixed effects. All independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and quarter, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate. The last row also reports a formal test of whether the coefficient on *Local OIB* is significantly different from *Non-Local OIB*.

	Qtr. 1	Qtr. 2	Qtr. 3	Qtr. 4
Local OIB	0.659%	0.020%	0.357%	0.013%
	(3.89)	(0.14)	(1.55)	(0.06)
Non-Local OIB	0.099%	-0.190%	-0.081%	0.133%
	(0.46)	(-0.87)	(-0.35)	(0.56)
Log (Firm Size)	-0.473%	-0.213%	-0.159%	0.147%
	(-1.27)	(-0.66)	(-0.43)	(0.41)
Log (Turnover)	0.140%	-0.306%	-0.257%	-0.540%
	(0.47)	(-0.80)	(-0.82)	(-1.79)
Log (Vol)	-0.793%	-0.110%	-0.454%	-0.387%
	(-1.25)	(-0.20)	(-0.65)	(-0.63)
Ret (m-1)	-0.081%	0.753%	-0.367%	-0.550%
	(-0.28)	(1.88)	(-0.95)	(-1.27)
Ret (m-7, m-2)	-0.239%	0.066%	0.486%	0.207%
	(-0.62)	(0.26)	(1.07)	(0.59)
Log (BM)	-1.429%	-1.774%	-1.257%	-0.817%
	(-1.53)	(-2.47)	(-1.53)	(-1.03)
Local OIB – Non-Local OIB	0.560%	0.210%	0.438%	-0.120%
	(2.34)	(0.93)	(1.24)	(-0.42)
R-squared	13.63%	12.36%	12.53%	12.92%
Observations (Firm-Quarters)	11,240	10,797	10,301	9,845

Table 5: NDRs and the Informativeness of Local Institutional Trading - Investor and Firm Heterogeneity

This table repeats the analysis from Tables 3 and 4 after partitioning the sample by either institution or firm attributes. The sample includes all firm-quarters with NDR activity and non-zero trading by local and non-local institutional investors (for each investor group) in the firm-quarter from January 2013 through December 2019. Specification 1 reports the estimates on *Local NDR* from Specification 3 of Table 3. Specifications 2 and 3 report the estimates on *Local OIB* and *Non-Local OIB* from Specification 1 of Table 4. For reference, Panel A reports the baseline results from the full sample. Panels B through D partition investors into *Hedge Funds* versus *Non-Hedge Funds, High Turnover* versus *Low Turnover* funds, and funds with *High Ownership* or *Low Ownership* in the NDR firm. Panel E splits the sample of NDR firms into *Small Firms* versus *Large Firms* based on the median NYSE market capitalization. More detailed variable definitions are provided in Appendix A. We report the estimates for each group, as well as the difference in the estimates across the two groups. Standard errors are double clustered by firm and quarter, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Log (Total Trading)	*Qtr. 1 Return*	
	Local NDR	*Local OIB*	*Non-Local OIB*
	[1]	[2]	[3]
Panel A: Full Sample			
Full Sample	0.67	0.659%	0.099%
	(14.67)	(3.89)	(0.46)
Panel B: Hedge Funds			
Hedge Fund (HF)	0.37	0.339%	-0.078%
	(8.70)	(1.32)	(-0.31)
Non-Hedge Fund	0.75	0.680%	-0.245%
	(17.91)	(3.44)	(-0.88)
HF - Non-HF	-0.38	-0.341%	0.167%
	(-8.30)	(-0.49)	(0.11)
Panel C: Fund Turnover			
High Turnover	0.57	0.592%	-0.118%
	(6.68)	(4.42)	(-0.57)
Low Turnover	0.61	-0.145%	0.109%
	(12.63)	(-0.68)	(0.41)
High - Low Turnover	-0.04	0.737%	-0.227%
	(-0.35)	(2.36)	(-0.70)
Panel D: Firm-Level Ownership			
High Ownership	0.81	0.631%	0.112%
	(16.41)	(3.15)	(0.51)
Low Ownership	0.45	-0.090%	-0.580%
	(10.66)	(-0.39)	(-2.49)
High - Low Ownership	0.36	0.721%	0.692%
	(7.78)	(2.60)	(2.82)
Panel E: Firm Size			
Small Firms	0.84	0.771%	0.095%
	(15.45)	(2.82)	(0.32)
Large Firms	0.18	0.409%	0.121%
	(5.55)	(2.92)	(0.46)
Small - Large	0.66	0.350%	-0.026%
	(9.05)	(1.06)	(-0.15)

Table 6: NDRs and the Informativeness of Retail Trading

This table reports estimates from the following panel regression:

$$Ret_{it+x} = \alpha + \beta_1 Retail\ OIB_{it} + \beta_2 Retail\ OIB_{it} \times NDR_{it,t-10} + \beta_3 NDR_{t,t-10} + \beta_4 Retail\ OIB_{it} \times Conf_{t,t-10} + \beta_5 Conf_{t,t-10} + \beta_6 Char_{it} + \beta_7 Retail\ OIB_{it} \times Char_{it-} + Day_t + \varepsilon_{it..}$$

The sample includes 5,257,844 firm-days from January 2013 through December 2019. Ret_{it+x} is the weekly (i.e., five-day) return for firm *i* on day *t*+*x*, where day *t* is the day in which *Retail OIB* is constructed. Specification 1 reports the one-week ahead return assuming all trades are executed at the closing price on day *t,* and Specification 2 report the one-week ahead return assuming all trades for stocks with positive (negative) retail order imbalances are executed at the retail-volume weighted average purchase (sale) price. Specifications 3 through 5 report the results for one-week returns for weeks 2 through 4, respectively. *Retail OIB* is defined as (Retail Buy Volume - Retail Sell Volume)/Total Retail Volume. Retail buys and sells are classified as in Boehmer et al. (2020). *NDR* (*Conf*) is an indictor variable equal to one if the firm attended an NDR (Conf) in the previous 10 days and zero otherwise. Detailed variable definitions are provided in Appendix A. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Week 1 (Exclude 0)	Week 1 (Include 0)	Week 2	Week 3	Week 4
Retail OIB	0.042%	0.001%	0.018%	0.014%	0.015%
	(7.66)	(0.19)	(3.95)	(2.96)	(2.99)
Retail OIB x NDR	-0.041%	-0.035%	-0.008%	-0.031%	-0.007%
	(-3.01)	(-2.45)	(-0.61)	(-2.10)	(-0.45)
NDR	0.067%	0.067%	0.042%	0.028%	0.052%
	(2.43)	(2.31)	(1.78)	(1.01)	(1.76)
Retail OIB x Conf	-0.006%	-0.001%	0.006%	0.004%	-0.029%
	(-0.57)	(-0.07)	(0.54)	(0.41)	(-1.40)
Conf	0.020%	0.036%	-0.006%	-0.021%	-0.012%
	(0.59)	(1.02)	(-0.15)	(-0.62)	(-0.33)
Log (Turnover)	0.008%	0.046%	0.021%	0.020%	0.017%
	(0.33)	(1.67)	(0.83)	(0.82)	(0.67)
Log (Vol)	-0.071%	-0.104%	-0.063%	-0.059%	-0.054%
	(-3.23)	(-4.28)	(-2.87)	(-2.71)	(-2.63)
Log (Firm Size)	-0.059%	-0.064%	-0.061%	-0.058%	-0.063%
	(-1.39)	(-1.44)	(-1.31)	(-1.24)	(-1.46)
Log (BM)	-0.024%	-0.014%	-0.021%	-0.027%	-0.030%
	(-0.51)	(-0.28)	(-0.42)	(-0.51)	(-0.63)
Ret (w-1)	-0.029%	-0.061%	-0.030%	-0.028%	-0.038%
	(-1.25)	(-2.47)	(-1.44)	(-1.37)	(-1.34)
Ret (m-1)	-0.060%	-0.068%	-0.024%	0.004%	0.025%
	(-1.94)	(-2.13)	(-0.85)	(0.16)	(0.85)
Ret (m-7, m-2)	0.034%	0.042%	0.039%	0.031%	0.021%
	(1.21)	(1.42)	(1.24)	(0.96)	(0.71)
Retail OIB x Log (Turnover)	-0.021%	0.002%	0.006%	-0.008%	0.007%
	(-2.54)	(0.20)	(0.73)	(-1.14)	(0.95)
Retail OIB x Log (Vol)	-0.007%	0.007%	-0.008%	0.004%	0.000%
	(-1.37)	(1.16)	(-1.53)	(0.75)	(-0.05)
Retail OIB x Log (Firm Size)	0.037%	0.010%	0.020%	0.002%	0.011%
	(5.73)	(1.53)	(3.20)	(0.27)	(1.92)
Retail OIB x Log (BM)	0.002%	-0.007%	-0.005%	0.007%	-0.008%
	(0.17)	(-0.67)	(-0.47)	(0.76)	(-0.96)
Retail OIB x Ret (w-1)	-0.005%	0.000%	0.008%	-0.009%	-0.006%
	(-0.52)	(-0.05)	(1.27)	(-1.33)	(-0.72)
Retail OIB x Ret (m-1)	-0.010%	-0.005%	-0.012%	0.007%	0.012%
	(-1.17)	(-0.56)	(-1.37)	(0.82)	(1.55)
Retail OIB x Ret (m-7, m-2)	-0.009%	0.003%	-0.021%	-0.005%	0.003%
	(-1.29)	(0.46)	(-3.10)	(-0.70)	(0.51)
(*Retail OIB + Retail OIB x NDR*)	0.001%	-0.034%	0.010%	-0.017%	0.008%
	(0.07)	(-2.37)	(0.79)	(-1.20)	(0.54)

Table 7: Weekly Commissions around NDRs

This table presents the estimates from the following weekly panel regression:

$$Com_{jit} = \beta_1 NDR_{jit} + \beta_2 Conf_{jit} + \beta_3 Turnover_{it} + Broker\text{-}Firm_{ji} + \varepsilon_{jit}$$

The sample spans from January 2013 to June 2014 and includes all broker-firm pairs with non-zero trading during the sample period (3,058,195 broker-firm-weeks). In Specifications 1, 3, and 5, the dependent variable is *$Com*, defined as the log (1 + Commissions) of broker *j* in firm *i* during week *t*. In Specifications 2, 4, and 6, the dependent variable is *Commission Share$_{jit}$ (CS),* computed as the total commission of broker *j* in firm *i* during week *t* scaled by total Abel Noser commissions (across all I/B/E/S-Abel Noser matched brokers) for stock *i* in week *t*. The independent variables are indicators equal to one if broker *j* took (or will take) firm *i* on an NDR or a conference during week *t+x*, and zero otherwise. For example, *NDR [0]* equals one if broker *j* took firm *i* on an NDR in week *t, NDR [-1,-2]* equals one if broker *j* took firm *i* on an NDR in week *t-1* or *t-2,* and *NDR [1,2]* equals one if broker *j* will take firm *i* on an NDR in weeks *t+1* or *t+2*. Some specifications also include the average weekly share turnover in the stock *(Turnover*), and all specifications include broker-firm fixed effects. Standard errors are double clustered by firm and week, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	$ Com.	*CS*	*$ Com.*	*CS*	*$ Com.*	*CS*
	[1]	[2]	[3]	[4]	[5]	[6]
NDR [0]	0.26	1.15%	0.24	1.14%	0.25	1.19%
	(4.30)	(2.77)	(4.02)	(2.75)	(4.23)	(2.89)
Conf [0]	0.20	1.36%	0.19	1.36%	0.19	1.38%
	(3.26)	(5.18)	(3.24)	(5.17)	(3.29)	(5.17)
Turnover [0]			0.34	0.13%	0.34	0.13%
			(38.89)	(11.20)	(38.99)	(11.18)
NDR [-1, -2]					0.15	0.49%
					(1.95)	(0.95)
NDR [-3, -4]					0.20	0.90%
					(2.31)	(1.60)
NDR [-5, -8]					0.02	0.14%
					(0.60)	(0.64)
NDR [1,2]					0.14	0.83%
					(1.20)	(1.46)
NDR [3,4]					0.05	0.32%
					(0.96)	(0.78)
NDR [5,8]					0.01	0.19%
					(0.25)	(1.10)
Conf [-1, -2]					0.15	0.47%
					(2.48)	(1.41)
Conf [-3, -4]					0.05	0.52%
					(0.95)	(1.53)
Conf [-5, -8]					-0.01	-0.07%
					(-0.29)	(-0.40)
Conf [1,2]					-0.01	0.31%
					(-0.11)	(0.69)
Conf [3,4]					0.00	-0.27%
					(0.00)	(-0.75)
Conf [5,8]					-0.02	-0.24%
					(-0.77)	(-1.46)
R-squared	41.61%	16.38%	42.08%	16.38%	42.08%	16.38%
Obs. (Firm-Broker-Week)	3,058,195	3,058,195	3,058,195	3,058,195	3,058,195	3,058,195

Table 8: Characteristics of NDR and Non-NDR Brokers

This table compares analyst/broker characteristics and measures of research optimism for *NDR* and *Non-NDR Brokers*. The full sample includes all broker-firm-months from 2013-2019 where broker *j* issued at least one recommendation or target price for firm *i* in the past 24 months. We split this sample into broker-firm-months where broker *j* will take firm *i* on an NDR in the subsequent three months (i.e., months t, t+1, or t+2) [*NDR3 =1*], and all other broker-firm-months [*NDR3 =0*]. The *NDR3* =1 (*NDR3* =0) sample includes 63,605 (2,095,859) firm-broker-month observations. For each sample, we report the mean of analyst and broker characteristics (Panel A) and measures of research optimism (Panel B). All variables are defined in Appendix A. We also report the difference between the two means (Column 3), the standard deviation of the variable across the combined sample (Column 4), and the differences in the means scaled by the standard deviation (Column 5).

	NDR3 =1	NDR3 =0	Difference	Std. Dev	Scaled Difference
	[1]	[2]	[3]	[4]	[5]
Panel A: Analyst/Broker Characteristics					
Broker Size	49.97	60.13	-10.16	47.02	-21.61%
Firm Experience	4.53	4.71	-0.18	5.23	-3.42%
Total Experience	13.78	13.38	0.40	9.60	4.14%
Firms Followed	19.84	19.51	0.33	8.61	3.86%
All-Star	8.95%	10.56%	-1.60%	30.67%	-5.23%
Conf3	8.15%	5.01%	3.14%	30.02%	10.45%
Affiliated3	1.04%	1.11%	-0.07%	7.87%	-0.95%
Panel B: Research Optimism					
Rec Level	1.96	2.38	-0.42	0.89	-47.19%
Target Return	28.01%	19.18%	8.83%	34.28%	25.76%
Target Return Bias	18.86%	11.45%	7.41%	50.25%	14.75%

Table 9: NDRs and Analyst Optimism

This table reports estimates from the following panel regression:

$$Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 Conf3_{jit} + \beta_3 Affiliated3_{jit} + \beta_4 Controls + FE + \varepsilon_{jit}.$$

The sample consists of all broker-firm-months from 2013 through 2019 where the broker issues at least one recommendation (columns 1-2) or target price (columns 3-4) for the firm in the prior 24 months. The dependent variable is a measure of optimism for analyst *j* for firm *i* in month *t*. In Specifications 1 and 2 the optimism measure is *Rec Level,* a rating from 1 to 5 using the following scale: 1=strong buy, 2=buy, 3=hold, 4=sell/underperform, and 5=strong sell (and thus a more negative recommendation level indicates greater optimism). In Specifications 3 and 4 the optimism measure is *Target Return,* the 12-month expected return implied from the most recent 12-month price forecast of the firm, computed as (Forecast $Price_{jit}/Price_{it-1}$)-1. *NDR3* is an indicator variable equal to one if the broker will take the firm on an NDR over the subsequent three months. *Conf3* and *Affiliated3* are indicator variables equal to one if the broker hosts the firm at a conference or has an investment banking relation with the firm in the subsequent three months. *Controls* include the following broker/analyst related controls: Log *(Broker Size)*, Log (*Firm Experience*), Log (*Experience*), Log (*Firms Followed*), and *All-Star*. Detailed variable definitions are provided in Appendix A. The regressions include either month fixed effects or firm-month fixed effects. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Rec Level		Target Return	
	[1]	[2]	[3]	[4]
NDR3	-0.39	-0.29	7.66%	4.46%
	(-38.88)	(-31.11)	(17.55)	(22.72)
Conf3	-0.16	-0.06	7.62%	1.47%
	(-15.59)	(-6.35)	(15.69)	(9.42)
Affiliated3	-0.11	-0.05	4.51%	1.14%
	(-6.23)	(-2.96)	(6.03)	(4.46)
Log (Broker Size)	0.07	0.05	-5.03%	-1.88%
	(19.87)	(14.26)	(-22.78)	(-19.84)
Log (Firm Experience)	0.00	0.00	2.76%	0.46%
	(-0.38)	(-0.34)	(6.31)	(3.02)
Log (Experience)	-0.03	-0.01	0.97%	0.34%
	(-7.61)	(-3.00)	(4.04)	(3.97)
Log (Firms Followed)	0.10	0.00	-5.08%	0.44%
	(20.64)	(1.21)	(-18.60)	(4.92)
All-Star	0.10	0.09	-2.05%	-0.65%
	(8.36)	(8.70)	(-5.00)	(-3.28)
Fixed Effects	Month	Firm-Month	Month	Firm-Month
R-squared	2.57%	29.67%	4.57%	71.85%
Obs. (Broker-Firm-Month)	1,565,813	1,565,813	1,955,800	1,955,800

Table 10: NDRs and Analyst Optimism - Cross Sectional Patterns

This table reports estimates from the following panel regression:

$$Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 NDR3_{jit} \times CV + \beta_3 Conf3_{jit} + \beta_4 Affiliated3_{jit} + \beta_5 Controls + FE + \varepsilon_{jit,}$$

The sample consists of all broker-firm-months from 2013 through 2019 where the broker issues at least one recommendation (columns 1-3) or target price (columns 4-6) for the firm in the prior 24 months. *Optimism* is either *Rec Level* (Specifications 1-3) or *Target Return* (Specifications 4-6). *NDR3* is an indicator variable equal to one if the broker will take the firm on an NDR over the subsequent three months. *Conf3* and *Affiliated3* are indicator variables equal to one if the broker hosts the firm at a conference or has an investment banking relation with the firm in the subsequent three months. *CV* is a vector of conditioning variables defined in Appendix A. The regressions include firm-month fixed effects. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Rec Level			Target Return		
	[1]	[2]	[3]	[4]	[5]	[6]
NDR3	-0.28	-0.30	-0.29	3.89%	4.58%	3.94%
	(-18.72)	(-29.24)	(-18.61)	(14.13)	(21.94)	(14.09)
NDR3 × Multi Day NDR	-0.03		-0.03	0.56%		0.61%
	(-1.73)		(-1.81)	(1.87)		(2.00)
NDR3 × Big Inst. NDR	-0.02		-0.02	0.80%		0.87%
	(-1.22)		(-1.65)	(2.40)		(2.60)
ND3 × Turnover	-0.02		-0.02	0.50%		0.50%
	(-1.73)		(-1.71)	(1.91)		(1.91)
NDR3 × Firm Size	0.07		0.07	-0.70%		-0.59%
	(4.91)		(4.26)	(-1.69)		(-1.41)
NDR3 × Volatility	0.02		0.02	1.96%		1.95%
	(1.84)		(1.98)	(5.82)		(5.80)
NDR3 × Coverage	-0.08		-0.08	1.65%		1.70%
	(-5.17)		(-5.38)	(4.82)		(4.96)
NDR3 × Broker Size		0.00	0.01		-0.38%	-0.32%
		(0.17)	(0.86)		(-1.30)	(-1.05)
NDR3 × Experience		0.05	0.05		-0.37%	-0.18%
		(5.18)	(5.31)		(-1.79)	(-0.88)
NDR3 × All Star		0.05	0.06		-0.54%	-0.41%
		(1.72)	(2.15)		(-1.07)	(-0.83)
Conf3	-0.06	-0.06	-0.06	1.46%	1.47%	1.46%
	(-6.46)	(-6.42)	(-6.45)	(9.46)	(9.50)	(9.47)
Affiliated3	(-0.05)	-0.05	-0.05	1.13%	1.15%	1.13%
	(-2.98)	(-2.99)	(-2.98)	(4.42)	(4.49)	(4.42)
Log (Broker Size)	(0.05)	0.05	0.05	-1.93%	-1.92%	-1.92%
	(14.31)	(14.18)	(14.16)	(-19.71)	(-19.61)	(-19.61)
Firm Experience	(0.00)	0.00	0.01	0.42%	0.42%	0.41%
	(1.30)	(1.31)	(1.34)	(4.84)	(4.85)	(4.83)
Experience	(-0.01)	-0.01	-0.01	0.35%	0.35%	0.35%
	(-3.10)	(-3.36)	(-3.38)	(4.10)	(4.15)	(4.13)
Firms Followed	0.00	0.00	0.00	0.43%	0.44%	0.43%
	(-0.27)	(-0.28)	(-0.28)	(2.85)	(2.89)	(2.84)
All-Star	0.09	0.09	0.09	-0.66%	-0.65%	-0.65%
	(8.73)	(8.53)	(8.52)	(-3.33)	(-3.24)	(-3.25)
R-squared	29.65%	29.65%	29.66%	71.79%	71.78%	71.79%
Obs. (Broker-Firm-Month)	1,555,701	1,555,701	1,555,701	1,947,749	1,947,749	1,947,749

Table 11: NDRs and Quarterly Earnings Forecast Pessimism

This table reports estimates from the following panel regression:

$$Qtr.\ Forecast\ Pessimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 Conf3_{jit} + \beta_3 Affiliated3_{jit} + \beta_4 Controls + FE + \varepsilon_{jit}.$$

The sample consists of all broker-firm-months from 2013 through 2019 where the broker issues at least one recommendation or target price. In addition, we require that the broker issues at least one forecast for one-quarter ahead earnings. The dependent variable is a measure of pessimism in quarterly forecasts of analyst *j* for firm *i* in month *t*. In Specifications 1 and 2, the dependent variable is *MBE,* an indicator variable equal to one if the firm's realized earnings meet or beat the analyst's estimated earnings. In Specifications 3 and 4, the dependent variable is *Relative Earnings Pessimism*, computed as: [(*Rank* – 1)/ (*Number of Analysts* -1)], where *Rank* is the rank of the analyst's forecasted earnings estimate, where the highest estimate is given a rank of 1, the second highest estimate is given a rank of 2, etc., and *Number of Analysts* is the number of analysts issuing a forecast for the firm-quarter. *NDR3* is an indicator variable equal to one if the broker took the firm on an NDR over the subsequent three months. *Conf3* and *Affiliated3* are indicator variables equal to one if the broker hosts the firm at a conference or has an investment banking relation with the firm in the subsequent three months. Detailed variable definitions are provided in Appendix A. The regressions include either month fixed effects or firm-month fixed effects. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	MBE		Relative Earnings Pessimism	
	[1]	[2]	[3]	[4]
NDR3	2.55%	1.30%	1.10	1.41
	(5.77)	(5.61)	(6.00)	(6.16)
Conf3	1.21%	0.53%	0.24	0.31
	(3.31)	(2.71)	(1.65)	(1.77)
Affiliated3	-0.39%	-0.31%	-0.37	-0.49
	(-0.48)	(-0.84)	(-1.58)	(-1.62)
Log (Broker Size)	1.19%	0.19%	0.30	0.35
	(9.13)	(2.76)	(5.13)	(4.84)
Firm Experience	-1.32%	0.19%	0.38	0.48
	(-4.23)	(1.33)	(3.69)	(3.50)
Experience	1.08%	0.31%	0.29	0.35
	(6.55)	(4.09)	(5.15)	(4.93)
Firms Followed	1.22%	0.14%	-0.03	-0.06
	(5.60)	(1.83)	(-0.66)	(-0.75)
All-Star	0.57%	0.22%	-0.31	-0.37
	(1.57)	(1.14)	(-1.66)	(-1.75)
Fixed Effects	Month	Firm-Month	Month	Firm-Month
R-squared	0.48%	59.95%	0.04%	0.12%
Obs. (Broker-Firm-Month)	1,507,564	1,507,564	1,507,564	1,507,564

Non-Deal Roadshows, Informed Trading, and Analyst Conflicts of Interest

INTERNET APPENDIX

This internet appendix accompanies the article, "Non-Deal Roadshows, Informed Trading, and Analyst Conflicts of Interest." It consists of three sections. Section IA.1 provides more details on non-deal roadshows (NDRs) including a typical NDR calendar in our sample and firm characteristics around NDRs. Section IA.2 explores the representativeness of the *FLY's* NDR coverage, with a particular emphasis on whether FLY's incomplete coverage of NDRs could bias our main conclusions. Section IA.3 reports and discusses additional results briefly discussed in the body of the paper.

IA.1. Additional NDR characteristics

Table IA.1 provides a sample calendar for Microsoft during the 2013 calendar year. Microsoft participates in four distinct trips, sponsored by three different brokerage firms. They visit institutions in Europe, the US Northeast, US Midwest. and Toronto. When reporting the summary statistics in Table 1, we consider each firm-date-location as an NDR. Thus, these four trips would be classified as 14 NDRs because there are 14 distinct firm-date-location triples.

Figure IA.1 reports the timing of NDRs relative to the most recent earnings announcement. Specifically, for each NDR, we count the number of calendar days between the most recent earnings announcement date and the NDR. We then examine the fraction of all NDRs that occur between zero to 10 days after the earnings announcement, 11 to 20 days after the earnings announcement, etc. Figure IA.1 shows 46% of all NDRs occur within 30 days of the most recent quarterly earnings announcement, while only 13% occur more than 60 days after the earnings announcement. These findings are consistent with firms using NDRs as an opportunity to provide more context around their recent earnings report. The dearth of NDRs immediately prior to the earnings announcement is consistent with firms managing litigation risk by generally avoiding NDRs during periods where they may have relatively more material private information that they cannot legally disclose.

Table IA.2 examines the distribution of market-adjusted returns in event time around NDRs. We examine returns over various event windows starting three months (i.e., 63 trading days) prior to the NDR and extending three months subsequent to the NDR. Firms that go on NDRs have typically experienced positive returns over the past several months. For example, the average return for NDR firms over the [-63, -1] window is 1.84%. We also find that the market-adjusted returns for firms conducting NDRs are 0.23% over the [0,1] window and 0.36% over the [0,5] window, suggesting that the market reaction to NDRs is typically positive. However, there is substantial dispersion in NDR

returns. For example, over the [0,1] event window, the interquartile range is -1.22% to +1.54% and the median firm has a return of -0.22% over the [0,63] day window. Overall, this evidence is inconsistent with the view that managers choose to go on NDRs only when they have positive private information that they wish to convey to investors.

IA.2. Representativeness of the FLY Sample

While our sample of more than 40,000 NDRs is large, a limitation of our data is that *FLY* only reports a subset of NDR activity. This raises the important question of whether *FLY*'s NDR coverage has any systematic biases that would influence our results. In this section, we explore the severity of these concerns.

One potential concern is that *FLY* may redact or disclose more important NDRs *ex post*. To explore this possibility, every day during the month of August 2020 we recorded all NDRs posted on *FLY* that occurred or were scheduled to occur between August 1, 2020 and December 31, 2020. During this process, we found zero cases where *FLY* either redacted or added NDRs post-event.

A more general concern is that *FLY* coverage may not be representative of the universe of NDRs. To examine this possibility, we collected NDR data from two alternative sources. First, we purchased the email addresses of Fortune 1000 firms' Investment Relations Officers (IROs). After eliminating private firms and invalid email addresses, we were left with 557 IROs. We emailed all 557 of these IROs asking for their NDR calendars so that we could compare our data with theirs. Most IROs did not respond to our email, and the majority that did respond told us that they are unwilling to share this data. The lack of response is consistent with the view that NDRs are a secretive event that firms try to conceal. Despite the general lack of support, 22 firms provided us with their NDR calendars. The sample includes 324 NDRs spanning 67 firm-years. Second, we expanded this sample through a contact at a large buy-side fund, who provided his full calendar of NDRs for 2018 (N= 237). Three NDRs appear in both samples, so our combined hand-collected sample includes 558 NDRs, of which 34% (189) are reported in *FLY*.

Using this sample, we explore two main questions. First, what are the determinants of *FLY* coverage? Second, to what extent does *FLY*'s incomplete NDR coverage affect the central findings of the paper?

IA.2.1. Determinants of FLY Coverage

We begin by examining whether *FLY* coverage is systematically correlated with firm characteristics. Specifically, for our hand-collected sample of 558 NDRs, we regress *FLY Coverage*, an indicator equal to one if the NDR was reported in *FLY,* onto the 17 firm characteristics included in

Table 2. As in Table 2, we standardize all continuous variables to have mean zero and unit variance, and we cluster standard errors by firm and month. The results of this analysis are reported in Specification 1 of Table IA.3. Across the 17 variables considered, only two are statistically significant at a 5% level. Specifically, we find that *FLY* coverage is decreasing in *Intangibles* and increasing in the number of institutional investors holding the stock (#*Institutions*).[36]

In our email correspondences with IROs, some suggested that *FLY* primarily relies on leaks from brokerage firms. To test this conjecture, in Specification 2, we add brokerage fixed effects. We find that the *r*-squared jumps from 14.5% to 54.3%, confirming that *FLY* coverage is strongly related to the brokerage firm sponsoring the NDR. Further, after including brokerage fixed effects, only one firm characteristic (# *Institutions*) is significant at the 5% level, and no other variables are significant at the 10% level. The fact that # *Institutions* remains positive and significant is consistent with the idea that *FLY* obtains some of their data from leaks from the buy-side, with the additional assumption that firms with more institutional owners meet with more investors on their NDRs.

We next explore whether the explanatory power of the brokerage fixed effects is related to the brokers' reputations. In Specification 3, we drop broker fixed effects, and instead include *Bulge Bracket*, an indicator equal to one if the brokerage firms is ranked as a bulge bracket bank by the Corporate Finance Institute.[37] We find that the coefficient on *Bulge Bracket* is small and statistically insignificant. Thus, while *FLY* coverage varies significantly across brokerage firms, there is no evidence that it varies with broker reputation. To provide a better sense for which brokers are included in the FLY sample, in Table IA.4 we tabulate the mean of *FLY Coverage* across the 9 bulge bracket brokerage firms and the 16 non-bulge bracket firms who sponsored at least 10 NDRs in our hand-collected NDR sample. The table further reinforces our view that *FLY* coverage is primarily a broker effect. For example, more than two-thirds of all NDRs sponsored by JP Morgan, Deutsche Bank, and UBS are reported in *FLY*. In contrast, none of the NDRs sponsored by Bank of America, Credit Suisse, Morgan Stanley, Goldman Sachs, Barclays, or Citi are reported by *FLY*.

[36] In untabulated tests, we also examine whether institutional trade informativeness, retail trade informativeness, and analyst optimism varies with *Intangibles* and # *Institutions*. We find virtually no evidence that institutional or retail trading informativeness varies with either variable. We find that target return bias is decreasing in both *Intangibles* and # *Institutions* (with very similar magnitudes). Thus, *FLY's* tilt towards firms with high intangibles and low # *Institutions* is unlikely to meaningfully bias our main findings.

[37] The indicator equals one for the following nine brokerage firms: Bank of America, Barclays, Citigroup, Credit Suisse, Deutsche Bank, Goldman Sachs, JPMorgan Chase, Morgan Stanley, and UBS.

IA.2.2. The impact of FLY's coverage on our results

In this section, we investigate the extent to which our central findings are affected by *FLY*'s incomplete NDR coverage by exploring whether our main findings vary with *FLY* coverage. In particular, within our hand-collected sample of 558 NDRs, we compare whether our results differ for NDRs reported by *FLY* (N=189) relative to NDRs excluded from *FLY* (N=369).

We examine whether *FLY*'s incomplete NDR coverage affects our analysis of institutional informed trading by estimating the following panel regression:

$$Ret_{it+x} = \beta_1 Local\ OIB_{it} + \beta_2 Local\ OIB_{it} \times Hand_{it} + \beta_3 Local\ OIB_{it} \times FLY\ Missing_{it} + B_4\, Hand_{it} + \beta_5 FLY\ Missing_{it} + B_6\, Non\text{-}Local\ OIB_{it} + \beta_7 Char_{it} + Qtr_t + \varepsilon_{it}. \quad \text{(IA.1)}$$

Ret_{it+x}, *Local OIB, Non-Local OIB*, and *Char* are all defined as in Equation 2. *Hand* is an indicator equal to one if the NDR is included in the hand-collected NDR sample described in Section IA.2.1, and *FLY Missing* is an indicator equal to one if the NDR was included in the hand-collected NDR sample but was not included in *FLY*. The coefficient of primary interest is β_3, which measures whether the informativeness of local institutional trading around NDRs differs significantly for NDRs excluded from *FLY* relative to the other NDRs in the hand-collected sample.

Specification 1 of Table IA.5 reports the results for one-quarter ahead returns. We find that the coefficient on *Local OIB*× *FLY Missing* is economically small and statistically insignificant.[38] The point estimate implies that a one-standard deviation increase in *Local OIB* is associated with 0.10% higher returns for NDRs not reported in *FLY* relative to those NDRs reported in *FLY*. The estimate for two- to four-quarter ahead returns are also always statistically insignificant. While the statistically insignificant results may be a consequence of the relatively small number of NDRs within the hand-collected sample, the typically positive point estimates on β_3 suggests that, if anything, the informativeness of local institutional trading is slightly larger for NDRs omitted from the FLY.

We next conduct analogous tests for retail trading informativeness by estimating the following regression:

$$Ret_{it+x} = \beta_1 Retail\ OIB_{it} + \beta_2 Retail\ OIB_{it} \times NDR_{it,t-10} + \beta_3 Retail\ OIB_{it} \times Hand_{it} + \beta_4 Retail\ OIB_{it} \times FLY\ Missing_{it} + \beta_5 NDR_{t,t-10} + B_6\, Hand + \beta_7 FLY\ Missing_{it} + B_8 Retail\ OIB_{it} \times Conf_{t,t-10} + \beta_9 Conf_{t,t-10} + \beta_{10} Char_{it} + \beta_{11} Retail\ OIB_{it} \times Char_{it} + Day_t + \varepsilon_{it}. \quad \text{(IA.2)}$$

[38] The coefficient on *Local OIB*× *Hand* is negative and marginally significant ($p < 0.10$). This could be consistent with IROs who work for firms that tend to provide more valuable information during NDRs being more reluctant to provide us with their full calendar of NDRs.

All variables are defined as in Equation 3 or Equation IA.1. The estimates from Equation IA.2 are reported in Table IA.6. We find that the coefficient on *Retail OIB* × *FLY Missing* is always statistically insignificant.

Finally, we consider analogous tests for analyst optimism. Specifically, we estimate the following regression:

$$Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 Hand_{jit} + \beta_3 FLY\ Missing_{jit} + \beta_4 Conf3_{jit} + \beta_5 Affiliated3_{jit} + \beta_6 Controls + FE + \varepsilon_{jit.} \quad \text{(IA.3)}$$

All variables are defined as in Equation 5 and Equation IA.1. The results from Equation IA.3 are reported in Table IA.7. We find that in all four specifications the coefficient on *FLY Missing* always suggests that optimism is greater for NDRs not reported in *FLY*, and the point estimate is marginally significant ($p \leq 0.10$) in two of the four specifications. It is possible that the brokers that allow their NDR activity to most strongly influence their equity research might be the least willing to leak their NDR activity to *FLY*.

Summarizing, our analysis in this section suggests that any bias due to *FLY*'s incomplete coverage of NDRs is small, and if anything, likely slightly attenuates our main findings.[39]

IA.3. Additional Results

IA.3.1. Retail Trading Informativeness – Robustness Tests

In Table 6, we define *NDR* as an indicator variable equal to one if the firm has participated in an NDR over the past 10 trading days. In this section, we explore the sensitivity of our findings to alternative event windows. Specifically, we re-estimate Equation 3 after defining *NDR* (and *Conf*) using a one-week (i.e., 5-day), one-month (i.e., 21-day), or one-quarter (i.e., 63-day) event window. The results are reported in Table IA.8. We find the estimate on *Retail OIB* × *NDR* is at least marginally significant ($p \leq 0.10$) for all horizons. The coefficient on *Retail OIB* × *NDR* tends to decline (in absolute value) as the horizon increases. Specifically, the coefficients for the 5-day, 10-day, 21-day, and 63-day windows are: -0.028%, -0.041%, -0.025%, and -0.012%, respectively. The generally declining point estimates suggest that most informed institutional trades occur within a two-week window after the NDR. However, the (marginally) significantly negative estimates for horizons of up to one-quarter are consistent with at least some institutions obtaining a relatively long-lived informational advantage following the NDR.

[39] Across the three tables, we test 13 coefficients. Of the 13 coefficients, 11 of the estimates are insignificant at a 10% level. The remaining two variables are marginally significant ($p \leq 0.10$) and suggest our main findings are understated.

As an additional robustness test, we compare the informativeness of retail trading in NDR stocks during the NDR period to their trading in NDR stocks in periods prior to and after the NDR. We create placebo NDR dates by shifting the timing of the NDR plus or minus three quarters. For example, the plus 1 (2) quarter placebo date shifts the NDR date by 63 (126) trading days. We then re-estimate Specification 1 of Equation 3 around each of the placebo periods and report the estimate on *Retail OIB* × *NDR* and the 95% confidence intervals in Figure IA.2. The estimates are always statistically insignificant and economically small (with a mean of -0.7 bps) relative to the estimate during the 10-days following the NDR (-4.1 bps).[40] These findings suggest that the large negative coefficient on *Retail OIB* × *NDR* is attributable to NDR itself rather than some omitted variable (e.g., retail investors being particularly bad at trading the types of firms that attend NDRs).

IA.3.2. NDRs and the Informativeness of Trades through the Sponsoring Broker

Section 5 of the paper documents a spike in trades routed through the sponsoring broker in the weeks following the NDR. In this section, we examine whether the trades routed through the sponsoring broker are more informed than trades routed through other brokers. We define a trade as occurring around an NDR if an NDR took place at any point between days *t-10* and *t*. We limit the sample to NDR days with non-zero trading in the sponsoring broker. The final sample includes 1,368 unique NDRs and 5,471 firm-days.

For each day, we compute *Sponsoring Broker OIB* as the total shares of firm *i* bought through the sponsoring broker on day *t* less the total shares of firm *i* sold through the sponsoring broker on day *t*, scaled by total trading volume in firm *i* through the sponsoring broker on day *t*. We compute an analogous measure based on trades through all other brokers (*Non-Sponsoring Broker OIB*).

We first consider portfolio sorts. At the end of each day, we place stocks into two portfolios based on whether *Sponsoring Broker OIB* is greater than zero (*Sponsor Buys*) or less than zero (*Sponsor Sells*), and we report the average return to the strategy of buying stocks in the *Sponsor Buy* portfolio and selling stocks in the *Sponsor Sell* portfolio. We also consider analogous tests based on *Non-Sponsoring Broker OIB*. Figure IA.3 plots the returns to these strategies over the subsequent 12 weeks. We find *Sponsor Buys* outperform *Sponsor Sells* by 0.17% over the subsequent 12 weeks, while *Non-Sponsor Buys* underperform *Non-Sponsor Sells* by 0.48% over the same period.

[40] Similar to Figure 2, we find no evidence that retail investors are at an informational disadvantage in the three quarters prior to the NDR (mean = 0.0 bps), and very weak evidence of an information disadvantage in the three quarters after the NDR (mean = -1.4 bps).

We also examine the informativeness of trades executed through the sponsoring and non-sponsoring brokers using the following panel regression:

$$Ret_{it+x} = \alpha + \beta_1 Sponsoring\ Buy_{it} + \beta_2 Non\text{-}Sponsoring\ Buy_{it} + \beta_3 Char_{it} + Day_t + \varepsilon_{it..} \quad \text{(IA.4)}$$

Ret_{it+x} is the monthly (i.e., 21 trading day) return for firm i in month $t+x$. We let x vary from one to three months. *Sponsor Buy* (*Non-Sponsor Buy)* is an indicator equal to one if the *Sponsoring Broker OIB* (*Non-Sponsoring Broker OIB*) is greater than zero, and zero if the OIB measure is less than zero. *Char* is the same vector of firm characteristics from Equation 2. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month.

Table IA.9 reports the estimates from Equation IA.4. Most of the estimates of *Sponsor Buy* and *Non-Sponsor Buy* are insignificant. However, there is evidence that the stocks purchased through non-sponsoring brokers underperform the stocks sold during the 2nd month, both in absolute terms (−0.41%) and relative to trades executed through the sponsoring broker (−0.70%). In sum, there is weak evidence that trades through the non-sponsoring broker are less informed than trades through the sponsoring broker. However, the differences in informativeness of trades through sponsoring and non-sponsoring brokers is less dramatic than the differences in the informativeness of local and non-local institutional investors (Table 4). One potential explanation for this finding is that investor location is a better proxy for whether the investor attended the NDR than the trading activity routed through the sponsoring broker.[41] Consistent with this view, we find that the increase in local institutional trading during the NDR quarter (Table 3) is much greater in magnitude than the increase in trading through the sponsoring broker in the weeks following the NDR (Table 6).[42]

IA.3.3. NDRs and Analyst Optimism - Robustness

Figure 4 documents that NDR brokers issue optimistic recommendations and target prices for at least three years prior to the NDR. It is perhaps surprising that we observe elevated levels of optimism up to three years prior to an NDR because it seems unlikely that brokers would engage in such large strategic behavior so far in advance of the NDR. One potential explanation is that brokers might repeatedly sponsor the firm's NDRs, and the observed optimism long before an NDR might be capturing optimism that is proximate to another NDR. For example, consider a broker that took a

[41] An alternative possibility is that the Abel Noser sample has greater noise since it captures a much smaller fraction of total institutional trading. During our sample period, Abel Noser trading accounts for roughly 4% of CRSP total trading volume.

[42] The results in Table 6 examine total commissions rather than total trading volume. However, in unreported tests, we find similar results after replacing commissions with trading volume.

firm on an NDR in January 2014 and January 2016. In this example, months −36 through −25 for the January 2016 NDR are also months −12 through −1 for the January 2014 NDR. To explore this possibility, we repeat the analysis in Figure 4 after partitioning the sample into brokers who sponsor an NDR for the firm only once during the sample period (*Single Sponsor*) and brokers who sponsor multiple NDRs for the firm (*Multiple Sponsor*).

Figures IA.4A and IA.4B report the results of this analysis for recommendations and target returns, respectively. The partition reveals two clear differences. First, across all periods, the level of optimism is significantly smaller for brokers that only organize one NDR compared to those that organize multiple NDRs. Second, the increase in optimism is far more concentrated in a shorter window around the NDR for single sponsors. The patterns suggest that brokerages that regularly organize NDRs for the firm persistently maintain a very high level of optimism.

The results in Table 9 focus on the levels of NDR broker optimism. However, the evidence from Figure 4 suggests that analyst optimism is also increasing in the period immediately prior to the NDR. To more formally examine changes in recommendation optimism around the NDR, we re-estimate Equation 5 after replacing the dependent variable with either *Upgrade,* an indicator variable equal to one if the analyst revises his (or her) recommendation level upward (e.g., from a buy to a strong buy) for a firm in that month, or *Downgrade*, defined analogously. We also add an additional control variable, *Lag Rec,* defined as the recommendation level of the analyst in the prior month. This variable controls for the fact that upgrades (downgrades) are far more common when the existing recommendation level is more pessimistic (optimistic). Specification 1 of Table IA.10 shows that NDR brokers are 0.68 percentage points more likely to issue an upgrade in the three months prior to the NDR. This estimate reflects a 49% increase relative to the base probability of issuing an upgrade (1.38%). Specification 3 reports even more dramatic results for downgrades. Specifically, NDR brokers are 1.25 percentage points less likely to issue a downgrade, a 78% decrease relative to the base probability (1.61%). The inclusion of firm-month fixed effects yields similar, albeit slightly weaker, estimates.

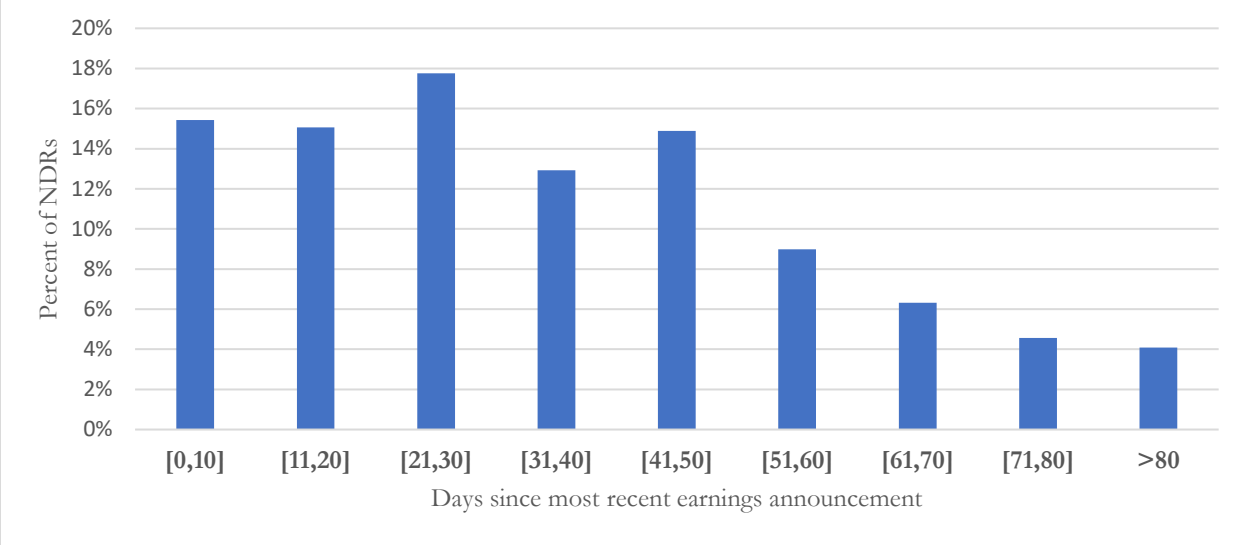


Figure IA.1: Timing of NDRs Relative to Most Recent Earnings Announcement
We sort all NDRs based on the timing of the NDR relative to the most recent earnings announcement. The figure reports the fraction of all NDRs that occur over different event windows. For example, [0,10] reports the fraction of all NDRs that occur within 10 calendar days after an earnings announcement, [11,20] reports the fraction of all NDRs that occur between 11 and 20 calendar days after the earnings announcement, etc.

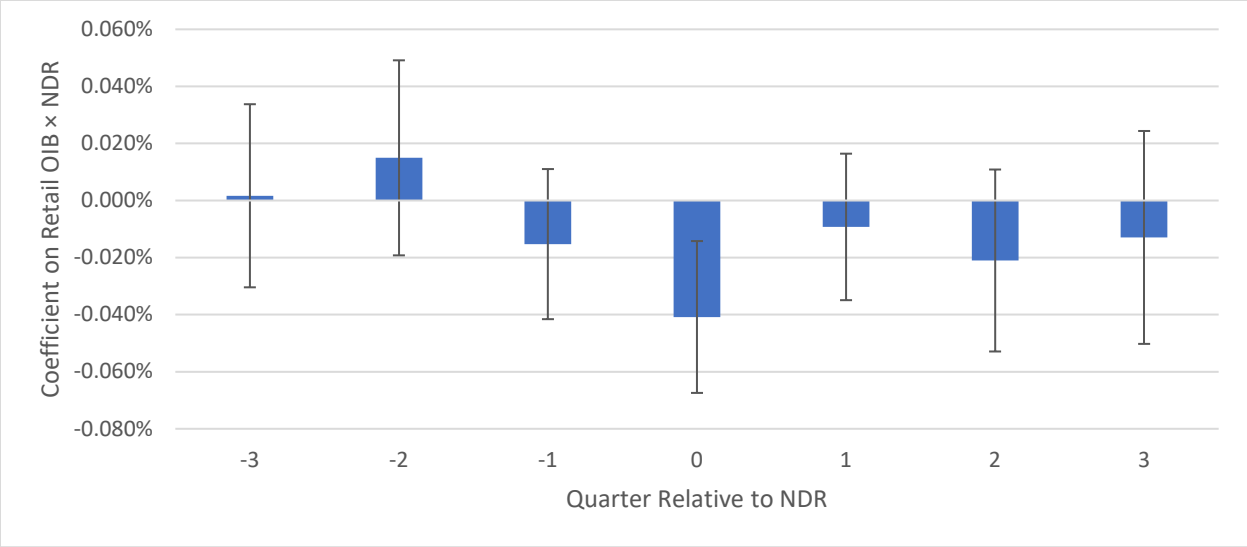


Figure IA.2: NDRs and the Informativeness of Retail Trading – Placebo NDR Dates
This figure repeats the regressions reported in Specification 1 of Table 6 of the paper after altering the timing of the NDR +/- three quarters. For example, in Quarter -1, we set the NDR as occurring one-quarter (63 trading days) prior to the actual NDR date. The figure plots the estimate and 95% confidence intervals for *Retail OIB x NDR* for each of the seven separate regressions.

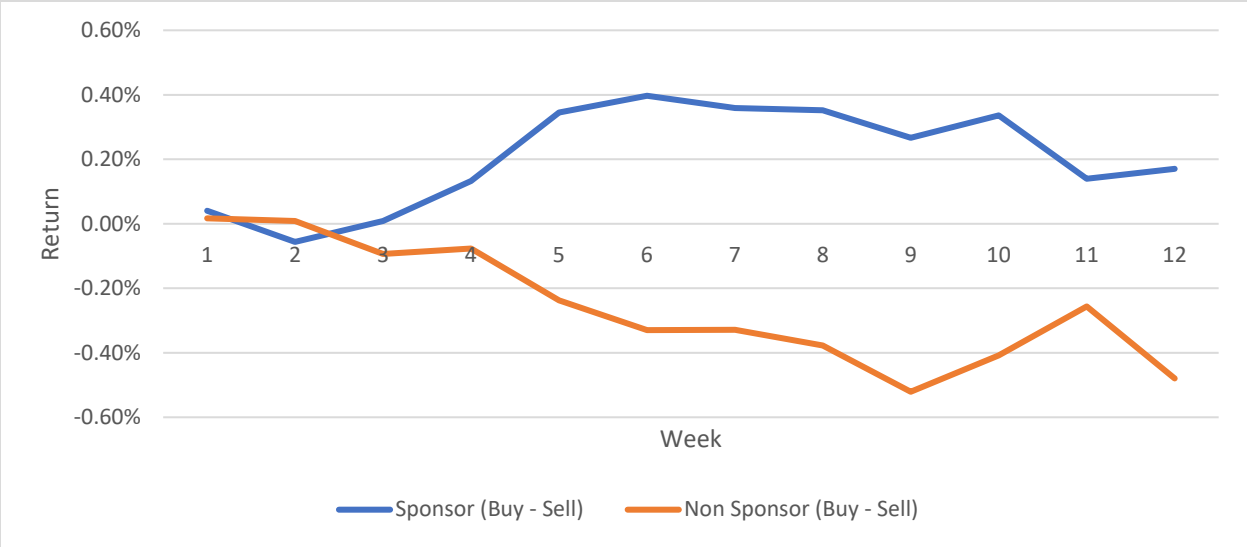
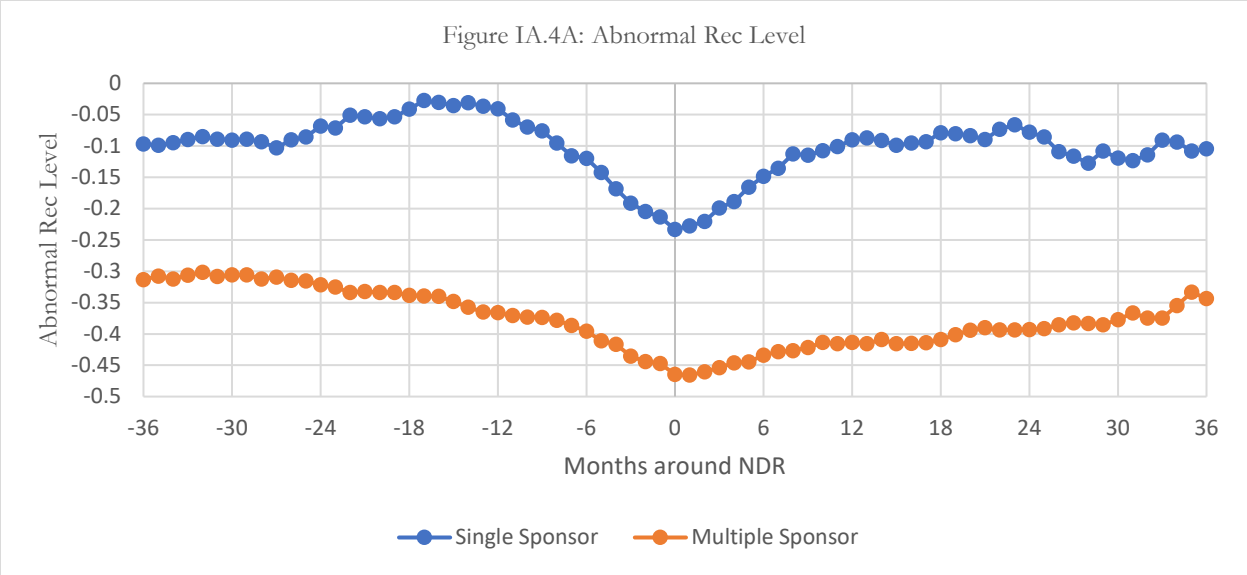


Figure IA.3: NDRs and the Informativeness of Institutional Trading through the Sponsoring Broker
At the end of each day, from January 2013 through June 2014, we sort all firms that conducted an NDR during the past 10 days into two groups based on the order imbalances of institutions who executed their trades through the sponsoring broker (*Sponsor)* and institutions who executed their trades through all other brokers (*Non-Sponsor*). We define *Sponsor OIB* as the total shares of firm *i* bought through the sponsoring broker on day *t* less the total shares of firm *i* sold through the sponsoring broker on day *t*, scaled by total trading volume in firm *i* through the sponsoring broker on day *t*. *Non-Sponsor OIB* is defined analogously. *Sponsor* (*Non-Sponsor*) reports the cumulative market-adjusted return to a strategy that buys stocks with positive *Sponsor OIB (Non-Sponsor)* and sells stocks with negative *Sponsor OIB* (*Non-Sponsor OIB*) for horizons ranging from one week to 12 weeks after the day of the trade.



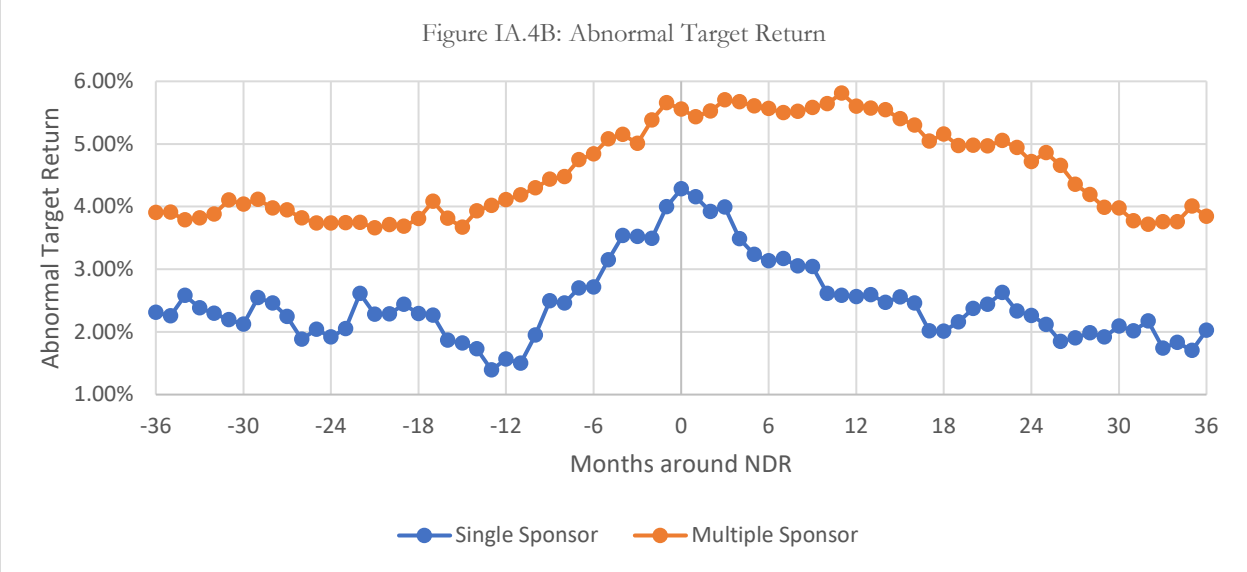


Figure IA.4: Relative Optimism of NDR Broker around Non-Deal Roadshows by Sponsoring Frequency
Figures IA.4A and IA.4B repeat Figures 4A and 4B after partitioning the sample of NDRs into brokers who sponsor an NDR for the firm only once during the sample period (*Single Sponsor*) and brokers who sponsor multiple NDRs for the firm (*Multiple Sponsor*).

Table IA.1: NDR Data Example

This table provides an example of the NDR data collected from TheFlyOnTheWall.com (*FLY*). This snapshot includes all observations for Microsoft over the 2013 calendar year.

Date	Location	Broker Name
25-Feb-13	Europe	UBS
27-Feb-13	Europe	UBS
1-Mar-13	United Kingdom	UBS
19-Mar-13	New York	JPMorgan
20-Mar-13	Philadelphia	JPMorgan
20-Mar-13	Trenton	JPMorgan
30-Apr-13	Cleveland	Pacific Crest
1-May-13	Columbus	Pacific Crest
2-May-13	Chicago	Pacific Crest
4-Nov-13	Toronto	JPMorgan
5-Nov-13	Toronto	JPMorgan
5-Nov-13	Chicago	JPMorgan
6-Nov-13	Chicago	JPMorgan
7-Nov-13	Minneapolis	JPMorgan

Table IA.2: Returns around NDRs

This table reports the distribution of equally weighted marked-adjusted returns over different event-time windows around the NDRs. The sample includes 43,799 NDRs from January 2013 through December 2019.

	Mean	Std. Dev	Q1	Median	Q3
[-63, -21]	0.94%	18.52%	-7.32%	-0.10%	7.41%
[-20, -6]	0.54%	11.41%	-4.48%	0.17%	4.89%
[-5, -1]	0.32%	6.29%	-2.19%	0.16%	2.56%
[0,1]	0.23%	3.49%	-1.22%	0.11%	1.54%
[2,5]	0.13%	4.65%	-1.90%	0.02%	2.00%
[6,21]	0.04%	9.87%	-4.23%	-0.11%	3.97%
[22, 63]	0.29%	19.33%	-8.17%	-0.29%	7.68%
[-63, -1]	1.84%	24.06%	-9.27%	0.28%	10.10%
[0,63]	0.70%	23.36%	-9.67%	-0.22%	9.53%

Table IA.3: Determinants of *Fly* Coverage

This table reports estimates from OLS linear probability models. The sample includes the 558 NDRs in the hand-collected sample described in Section IA.2. The dependent variable is an indicator equal to one if the NDR is reported in *FLY* and zero otherwise. All independent variables are defined in Appendix A. All continuous variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	(1)	(2)	(3)
Intangibles	-5.62%	-2.33%	-5.62%
	(-3.43)	(-1.24)	(-3.43)
(R&D + ADV)/OE	-5.39%	-4.07%	-5.40%
	(-1.84)	(-1.37)	(-1.84)
Log (MB)	5.93%	1.95%	5.98%
	(1.06)	(0.36)	(1.08)
Negative Book	4.52%	-7.40%	4.67%
	(0.19)	(-0.35)	(0.20)
Idiosyncratic Risk	-10.60%	-3.25%	-10.50%
	(-1.67)	(-0.59)	(-1.67)
Institutional Ownership	0.46%	-1.62%	0.48%
	(0.11)	(-0.58)	(0.11)
Log (Firm Age)	-1.03%	1.03%	-1.02%
	(-0.34)	(0.36)	(-0.33)
Net Shares	-0.84%	-0.76%	-0.85%
	(-0.59)	(-0.57)	(-0.59)
Log (Analyst Coverage)	-5.84%	-1.40%	-5.84%
	(-1.61)	(-0.39)	(-1.61)
Log (# Institutions)	7.88%	4.96%	7.87%
	(2.37)	(2.10)	(2.36)
Log (Firm Size)	-8.38%	-3.06%	-8.41%
	(-1.82)	(-0.54)	(-1.83)
Log (Turnover)	4.35%	-1.31%	4.33%
	(0.92)	(-0.31)	(0.92)
R-squared	-4.14%	-1.86%	-4.11%
	(-1.28)	(-0.73)	(-1.27)
Mom1	0.85%	1.33%	0.84%
	(0.57)	(0.99)	(0.57)
Mom12	3.88%	3.12%	3.87%
	(0.95)	(0.88)	(0.95)
SEO	32.30%	0.33%	32.20%
	(1.52)	(0.02)	(1.49)
M&A - Acquirer	-30.70%	-0.50%	-30.70%
	(-1.49)	(-0.04)	(-1.46)
Bulge Bracket			0.36%
			(0.11)
Fixed effects	Year	Year and Broker	Year
R-squared	14.50%	54.30%	14.50%
Observations (NDRs)	558	558	558

Table IA.4: FLY NDR Coverage by Brokerage Firm

This table provides descriptive statistics of NDR activity at the brokerage level. The sample includes the 558 NDRs in the hand-collected sample described in Section IA.2. For each broker, we report the total number of NDRs in the hand-collected sample (#*NDR*s), the total number of NDRs reported in FLY (*FLY*), the total number of NDRs not reported in FLY (*Non-FLY*), and the percentage of NDRs reported in FLY (*Percent FLY*). We report the results separately for the nine bugle bracket banks (Panel A) and 16 non-bulge bracket banks that sponsored at least 10 NDRs (Panel B).

Panel A: Bulge bracket banks

	# *NDRs*	*FLY*	*Non-FLY*	*Percent FLY*
JPMorgan	27	23	4	85%
Deutsche Bank	24	17	7	71%
UBS	19	13	6	68%
Bank of America	17	0	17	0%
Credit Suisse	16	0	16	0%
Morgan Stanley	14	0	14	0%
Goldman Sachs	13	0	13	0%
Barclays	13	0	13	0%
Citi	7	0	7	0%

Panel B: Non-bulge bracket banks

	# *NDRs*	*FLY*	*Non-FLY*	*Percent FLY*
Stephens	26	21	5	81%
SunTrust	22	12	10	55%
RBC	21	11	10	52%
William Blair	21	10	11	48%
Jefferies	21	19	2	90%
Piper Jaffray	19	11	8	58%
Wells Fargo	19	0	19	0%
Oppenheimer	19	15	4	79%
Stifel Nicolaus	18	0	18	0%
Evercore ISI	16	4	12	25%
Cowen	15	1	14	7%
Sidoti	13	2	11	15%
Strategas	13	0	13	0%
Raymond James	12	0	12	0%
Baird	11	0	11	0%
Guggenheim	11	3	8	27%

Table IA.5: NDRs and the Informativeness of Local Institutional Trading by Fly Coverage

This table repeats the analysis in Table 4 after interacting *Local OIB* with *Hand* and *FLY Missing*. The sample includes all firm-quarters with NDR activity (as reported in either the FLY sample or the hand-collected sample) and non-zero trading by local and non-local institutional investors in the firm-quarter from January 2013 through December 2019. *Hand* is an indicator equal to one if the NDR was included in the hand-collected NDR sample described in Section IA.2. *FLY Missing* is an indicator equal to one if the NDR was included in the hand-collected NDR sample but was not reported by *FLY*. All other independent variables are defined in Appendix A. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and quarter, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Qtr. 1	Qtr. 2	Qtr. 3	Qtr. 4
Local OIB	0.653%	0.006%	0.352%	0.021%
	(3.93)	(0.04)	(1.52)	(0.10)
Local OIB × Hand	-0.422%	0.114%	-0.331%	-0.560%
	(-1.93)	(0.55)	(-1.27)	(-1.16)
Local OIB × FLY Missing	0.101%	-0.216%	0.195%	0.638%
	(0.38)	(-0.96)	(0.99)	(1.41)
Hand	3.201%	-0.829%	2.577%	4.901%
	(1.95)	(-0.87)	(1.20)	(3.51)
FLY Missing	0.437%	1.527%	-1.425%	-3.985%
	(0.20)	(0.96)	(-0.60)	(-2.54)
Non-Local OIB	0.103%	-0.175%	-0.079%	0.137%
	(0.48)	(-0.83)	(-0.35)	(0.59)
Log (Firm Size)	-0.486%	-0.220%	-0.164%	0.139%
	(-1.30)	(-0.69)	(-0.45)	(0.39)
Log (Turnover)	0.170%	-0.299%	-0.241%	-0.538%
	(0.57)	(-0.76)	(-0.77)	(-1.80)
Log (Vol)	-0.786%	-0.124%	-0.447%	-0.378%
	(-1.24)	(-0.22)	(-0.64)	(-0.61)
Ret (m-1)	-0.086%	0.736%	-0.365%	-0.593%
	(-0.30)	(1.84)	(-0.94)	(-1.37)
Ret (m-7, m-2)	-0.244%	0.050%	0.491%	0.209%
	(-0.63)	(0.19)	(1.08)	(0.60)
Log (BM)	-1.458%	-1.838%	-1.303%	-0.839%
	(-1.57)	(-2.57)	(-1.58)	(-1.07)
Observations (Firm-quarters)	11,342	10,897	10,399	9,943

Table IA.6: NDRs and the Informativeness of Retail Trading by Fly Coverage

This table repeats the analysis in Table 6 after interacting *Retail OIB* with *Hand* and *FLY Missing*. *Hand* is an indicator equal to one if the NDR was included in the hand-collected NDR sample described in Section IA.2. *FLY Missing* is an indicator equal to one if the NDR was included in the hand-collected sample but was not reported by *FLY*. All other independent variables are defined in Appendix A. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Week 1 (Exc. 0)	Week 1 (Inc. 0)	Week2	Week 3	Week 4
Retail OIB	0.042%	0.001%	0.018%	0.014%	0.015%
	(7.68)	(0.21)	(3.95)	(2.95)	(2.98)
Retail OIB x NDR	-0.040%	-0.035%	-0.009%	-0.031%	-0.006%
	(-2.98)	(-2.41)	(-0.63)	(-2.13)	(-0.41)
Retail OIB x Hand	-0.098%	-0.140%	0.072%	0.093%	-0.126%
	(-1.21)	(-1.76)	(0.59)	(1.32)	(-0.71)
Retail OIB x FLY Missing	-0.041%	0.012%	-0.070%	0.100%	0.242%
	(-0.33)	(0.09)	(-0.48)	(0.96)	(0.95)
NDR	0.065%	0.065%	0.041%	0.029%	0.054%
	(2.36)	(2.23)	(1.72)	(1.03)	(1.80)
Hand	0.260%	0.345%	0.170%	-0.106%	-0.180%
	(1.88)	(1.30)	(0.80)	(-0.61)	(-1.60)
FLY Missing	-0.105%	-0.235%	0.119%	0.304%	0.196%
	(-0.60)	(-0.86)	(0.55)	(1.57)	(1.01)
Retail OIB x Conf	-0.006%	-0.001%	0.006%	0.004%	-0.029%
	(-0.57)	(-0.07)	(0.54)	(0.41)	(-1.40)
Conf	0.020%	0.036%	-0.006%	-0.021%	-0.012%
	(0.59)	(1.02)	(-0.15)	(-0.62)	(-0.33)
Log (Turnover)	-0.071%	-0.104%	-0.062%	-0.059%	-0.054%
	(-3.23)	(-4.28)	(-2.87)	(-2.71)	(-2.63)
Log (Vol)	-0.059%	-0.063%	-0.061%	-0.058%	-0.063%
	(-1.39)	(-1.44)	(-1.31)	(-1.24)	(-1.46)
Log (Firm Size)	0.008%	0.045%	0.021%	0.020%	0.017%
	(0.33)	(1.66)	(0.82)	(0.81)	(0.67)
Log (BM)	-0.024%	-0.014%	-0.021%	-0.027%	-0.030%
	(-0.52)	(-0.28)	(-0.43)	(-0.51)	(-0.63)
Ret (w-1)	-0.029%	-0.061%	-0.030%	-0.028%	-0.038%
	(-1.25)	(-2.47)	(-1.44)	(-1.37)	(-1.34)
Ret (m-1)	-0.060%	-0.068%	-0.024%	0.004%	0.025%
	(-1.94)	(-2.13)	(-0.85)	(0.16)	(0.85)
Ret (m-7, m-2)	0.034%	0.042%	0.039%	0.031%	0.021%
	(1.21)	(1.42)	(1.24)	(0.96)	(0.71)
Retail OIB x Log (Turnover)	-0.007%	0.007%	-0.008%	0.004%	0.000%
	(-1.37)	(1.16)	(-1.53)	(0.75)	(-0.05)
Retail OIB x Log (Vol)	0.037%	0.010%	0.020%	0.002%	0.011%
	(5.73)	(1.53)	(3.21)	(0.27)	(1.91)
Retail OIB x Log (Firm Size)	-0.021%	0.002%	0.006%	-0.008%	0.007%
	(-2.53)	(0.21)	(0.72)	(-1.16)	(0.94)
Retail OIB x Log (BM)	0.002%	-0.007%	-0.005%	0.007%	-0.008%
	(0.17)	(-0.67)	(-0.47)	(0.75)	(-0.96)
Retail OIB x Ret (w-1)	-0.005%	0.000%	0.008%	-0.009%	-0.006%
	(-0.52)	(-0.05)	(1.27)	(-1.33)	(-0.72)
Retail OIB x Ret (m-1)	-0.010%	-0.005%	-0.012%	0.007%	0.012%
	(-1.17)	(-0.56)	(-1.37)	(0.82)	(1.55)
Retail OIB x Ret (m-7, m-2)	-0.009%	0.003%	-0.021%	-0.005%	0.003%
	(-1.29)	(0.46)	(-3.10)	(-0.70)	(0.51)

Table IA.7: NDRs and Analyst Optimism by Fly Coverage

This table repeats the analysis in Table 9 after including *Hand* and *FLY Missing*. *Hand* is an indicator equal to one if the NDR was included in the hand-collected NDR sample described in Section IA.2. *FLY Missing* is an indicator equal to one if the NDR was included in the hand-collected NDR sample but was not reported by *FLY*. All other independent variables are defined in Appendix A. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Rec Level		Target Return	
	[1]	[2]	[3]	[4]
NDR3	-0.39	-0.29	7.71%	4.47%
	(-39.20)	(-31.32)	(17.65)	(22.65)
Hand	0.03	-0.03	-8.41%	-0.67%
	(0.36)	(-0.35)	(-5.33)	(-0.57)
FLY Missing	-0.11	-0.17	3.93%	0.63%
	(-1.12)	(-1.77)	(1.70)	(0.41)
Conf3	-0.16	-0.06	7.62%	1.47%
	(-15.59)	(-6.35)	(15.68)	(9.42)
Affiliated3	-0.11	-0.05	4.51%	1.14%
	(-6.23)	(-2.97)	(6.03)	(4.46)
Log (Broker Size)	0.07	0.05	-5.03%	-1.88%
	(19.88)	(14.27)	(-22.78)	(-19.84)
Firm Experience	0.00	0.00	2.76%	0.46%
	(-0.38)	(-0.34)	(6.31)	(3.01)
Experience	-0.03	-0.01	0.97%	0.34%
	(-7.61)	(-3.00)	(4.04)	(3.97)
Firms Followed	0.10	0.00	-5.08%	0.44%
	(20.64)	(1.21)	(-18.60)	(4.92)
All-Star	0.10	0.09	-2.06%	-0.65%
	(8.37)	(8.70)	(-5.00)	(-3.28)
Fixed Effects	Month	Firm-Month	Month	Firm-Month
R-squared	2.57%	29.67%	4.57%	71.85%
Obs. (Broker-Firm-Month)	1,565,813	1,565,813	1,955,800	1,955,800

Table IA.8: NDRs and the Informativeness of Retail Trading - Alternative Event Windows

This table repeats Specification 1 of Table 6 using an alternative event window when defining *NDR* and *Conf*. Specification 1 repeats the analysis after redefining *NDR* (*Conf*) equal to one if the firm attended an NDR (Conference) over the past five trading days. Specifications 2, 3, and 4 report analogous results using event windows of 10 trading days, 21 trading days, and 63 trading days, respectively.

	5-Days	10-Days	21-Days	63-Days
Retail OIB	0.040%	0.042%	0.045%	0.043%
	(7.37)	(7.66)	(7.77)	(6.21)
Retail OIB x NDR	-0.028%	-0.041%	-0.025%	-0.012%
	(-1.84)	(-3.01)	(-2.40)	(-1.64)
NDR	0.068%	0.067%	0.052%	0.089%
	(2.13)	(2.43)	(2.27)	(4.98)
Retail OIB x Conf	-0.006%	-0.006%	-0.013%	-0.002%
	(-0.44)	(-0.57)	(-1.49)	(-0.29)
Conf	0.040%	0.020%	0.000%	0.031%
	(1.00)	(0.59)	(0.01)	(1.24)
Log (Turnover)	0.008%	0.008%	0.009%	-0.001%
	(0.34)	(0.33)	(0.37)	(-0.02)
Log (Vol)	-0.071%	-0.071%	-0.071%	-0.075%
	(-3.22)	(-3.23)	(-3.24)	(-3.41)
Log (Firm Size)	-0.059%	-0.059%	-0.058%	-0.062%
	(-1.39)	(-1.39)	(-1.38)	(-1.47)
Log (BM)	-0.024%	-0.024%	-0.024%	-0.017%
	(-0.52)	(-0.51)	(-0.52)	(-0.38)
Ret (w-1)	-0.030%	-0.029%	-0.029%	-0.029%
	(-1.26)	(-1.25)	(-1.25)	(-1.25)
Ret (m-1)	-0.060%	-0.060%	-0.060%	-0.060%
	(-1.93)	(-1.94)	(-1.93)	(-1.95)
Ret (m-7, m-2)	0.034%	0.034%	0.034%	0.032%
	(1.21)	(1.21)	(1.20)	(1.15)
Retail OIB x Log (Turnover)	-0.021%	-0.021%	-0.020%	-0.020%
	(-2.61)	(-2.54)	(-2.41)	(-2.50)
Retail OIB x Log (Vol)	-0.007%	-0.007%	-0.007%	-0.007%
	(-1.41)	(-1.37)	(-1.30)	(-1.34)
Retail OIB x Log (Firm Size)	0.036%	0.037%	0.037%	0.037%
	(5.67)	(5.73)	(5.81)	(5.67)
Retail OIB x Log (BM)	0.002%	0.002%	0.001%	0.001%
	(0.21)	(0.17)	(0.10)	(0.14)
Retail OIB x Ret (w-1)	-0.005%	-0.005%	-0.005%	-0.005%
	(-0.52)	(-0.52)	(-0.53)	(-0.53)
Retail OIB x Ret (m-1)	-0.010%	-0.010%	-0.010%	-0.010%
	(-1.18)	(-1.17)	(-1.16)	(-1.17)
Retail OIB x Ret (m-7, m-2)	-0.009%	-0.009%	-0.009%	-0.009%
	(-1.30)	(-1.29)	(-1.28)	(-1.29)

Table IA.9: NDRs and the Informativeness of Institutional Trading through the Sponsoring Broker

This table reports estimates from the following panel regression:

$$Ret_{it+x} = \beta_1 SponsorBuy + \beta_2 Non\text{-}SponsorBuy_{it} + \beta_3 Char_{it} + Day_t + \varepsilon_{it.}$$

Ret_{it+x} is the monthly (i.e., 21 trading day) return for firm *i* following the day where institutional trading is measured (i.e., day *t*). *Sponsor Buy* is an indicator equal to one if the *Sponsoring Broker OIB* is greater than zero, and zero if the OIB measure is less than zero, and *Sponsoring Broker OIB* is computed as the total shares of firm *i* bought through the sponsoring broker on day *t* less the total shares of firm *i* sold through the sponsoring broker on day *t*, scaled by total trading volume in firm *i* through the sponsoring broker on day *t* (as reported in Abel Noser). *Non-Sponsor Buy* is computed analogously. *Char* is a vector of firm characteristics taken from Boehmer, Jones, Zhang, and Zhang (2020) and defined in Appendix A. Calendar day fixed effects are included. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate. The last row also reports a test of whether the coefficient on *Sponsor Buy* is significantly different from *Non-Sponsor Buy*. The sample spans from January 2013 to June 2014 and includes all days within 10 trading days of the NDR with non-zero trading volume through the sponsoring broker.

	Month 1	Month 2	Month 3
Sponsor Buy	0.10%	0.29%	-0.35%
	(0.45)	(1.18)	(-1.34)
Non-Sponsor Buy	-0.04%	-0.41%	-0.27%
	(-0.18)	(-1.76)	(-1.01)
Log (Firm Size)	0.20%	-0.09%	-0.10%
	(1.39)	(-0.62)	(-0.66)
Log (Turnover)	-0.30%	-0.58%	-0.81%
	(-0.66)	(-1.28)	(-1.42)
Log (Vol)	0.72%	2.65%	5.03%
	(0.45)	(2.03)	(3.01)
Ret (w-1)	0.11%	0.21%	0.06%
	(0.42)	(0.76)	(0.23)
Ret (m-1)	3.73%	-1.96%	-10.37%
	(0.58)	(-0.33)	(-1.31)
Ret (m-7, m-2)	-4.81%	4.39%	-3.97%
	(-1.23)	(1.29)	(-1.12)
Log (BM)	1.42%	0.37%	0.08%
	(0.93)	(0.24)	(0.03)
Sponsor - Non-Sponsor Buy	0.14%	0.70%	-0.08%
	(0.41)	(1.98)	(-0.21)
Obs. (Firm-Days)	5,471	5,471	5,471

Table IA.10: NDRs and Changes in Analyst Optimism

This table reports estimates from the following panel regression:

$$\Delta Optimism_{jit} = \beta_1 NDR3_{jit} + \beta_2 Conf3_{jit} + \beta_3 Affiliated3_{jit} + \beta_4 Controls + FE + \varepsilon_{jit}.$$

The sample consists of all broker-firm-months from 2013 through 2019 where the broker issues at least one recommendation for the firm in the prior 24 months. The dependent variable is a measure of the change in optimism for analyst *j* for firm *i* in month *t*. The dependent variable is either *Upgrade* (Specifications 1 and 2), an indicator variable equal to one if the analyst's recommendation level is revised upward for a firm in that month, or *Downgrade* (Specifications 3 and 4), an indicator equal to one if the analyst's recommendation level is revised downward for a firm in that month. *NDR3* is an indicator variable equal to one if the broker takes the firm on an NDR over the subsequent three months. *Conf3* and *Affiliated3* are indicator variables equal to one if the broker hosts the firm at a conference or has an investment banking relation with the firm in the subsequent three months. *Controls* is a vector of broker and analyst characteristics. Detailed variable definitions are provided in Appendix A. The regressions include either month fixed effects or firm-month fixed effects. All continuous independent variables are standardized to have mean zero and unit variance. Standard errors are double clustered by firm and month, and *t*-statistics are reported in parentheses below the corresponding coefficient estimate.

	Upgrades		Downgrades	
	[1]	[2]	[3]	[4]
NDR3	0.68%	0.62%	-1.25%	-1.04%
	(10.90)	(9.67)	(-20.01)	(-16.37)
Conf3	0.18%	0.06%	-0.49%	-0.26%
	(3.59)	(1.23)	(-9.42)	(-4.47)
Affiliated3	0.36%	0.13%	-0.52%	-0.39%
	(3.48)	(1.13)	(-5.97)	(-3.76)
Log (Broker Size)	-0.19%	-0.19%	0.00%	0.00%
	(-9.39)	(-8.85)	(0.00)	(-0.02)
Firm Experience	0.01%	0.06%	0.05%	0.07%
	(0.32)	(2.24)	(1.49)	(2.16)
Experience	-0.03%	-0.05%	-0.09%	-0.06%
	(-1.95)	(-3.21)	(-4.55)	(-3.22)
Firms Followed	0.03%	0.09%	0.12%	0.02%
	(1.85)	(5.04)	(5.44)	(1.32)
All-Star	0.00%	-0.05%	0.22%	0.26%
	(0.05)	(-1.13)	(4.03)	(4.90)
Lag (Rec Level)	1.52%	1.72%	-1.16%	-1.42%
	(31.23)	(31.04)	(-25.08)	(-24.76)
Fixed Effects	Month	Firm-Month	Month	Firm-Month
R-squared	1.38%	16.55%	0.83%	19.46%
Observations	1,518,539	1,518,539	1,518,539	1,518,539