The Journal of FINANCE
The Journal of THE AMERICAN FINANCE ASSOCIATION

THE JOURNAL OF FINANCE • VOL. LXXVII, NO. 1 • FEBRUARY 2022

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

DANIEL BRADLEY, RUSSELL JAME, and JARED WILLIAMS

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, together with more "beatable" earnings forecasts, consistent with analysts issuing strategically biased forecasts 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 who organize them.

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 arise 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

Daniel Bradley and Jared Williams are with the Kate Tiedemann School of Business and Finance, Muma College of Business, University of South Florida. Russell Jame is at the Gatton College of Business and Economics, University of Kentucky. We would like to thank Stefan Nagel (the Editor), two anonymous referees, Kevin Crotty (discussant), Steve Dennis, Ying Duan, Clifton Green, Vincent Gregoire (discussant), Lena Larkin (discussant), Kevin Mullaly, Jay Ritter (discussant), Ajai Singh, Alexander Vedrashko, Eric Weisbrod (discussant), and seminar participants at the 2020 American Finance Association, the 2019 Conference on Financial Economics and Accounting, the 2019 TCU Finance Conference, the 2019 Northern Finance Association, the 2020 Midwest Finance Association, Kent State University, Simon Fraser University, University of Central Florida, and University of Kentucky for helpful comments and suggestions. Jame is grateful to the Institution for Free Enterprise at the University of Kentucky for financial support. The authors have read *The Journal of Finance* disclosure policy and have no conflicts of interest to disclose. All errors are their own.

Correspondence: Daniel Bradley, Department of Finance, 4202 E. Fowler Ave., Tampa, FL 33620, 813-974-6358. Kate Tiedemann School of Business and Finance, Muma College of Business, University of South Florida; e-mail: danbradley@usf.edu.

DOI: 10.1111/jofi.13089

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 private "non-deal roadshows (NDRs)," a pervasive activity among 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 also frequently go on roadshows unrelated to securities issuance. The latter roadshows, referred to as NDRs, involve one-on-one meetings between corporate managers and investors that are 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] Moreover, 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 related to both conflicts of interest and information asymmetry. 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 surrounding NDRs makes it more difficult for investors to detect and adjust for this possible bias, which increases the risk that such 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

[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, Liz. (2020) In Boston money managers fire shot at Wall Street brokers. *Wall Street Journal*, 4 March. Available at: https://www.wsj.com/articles/in-boston-money-managers-fire-shot-at-wall-street-brokers-11583323502

[3] For example, the *Wall Street Journal* reports that "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 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).

obtains data on NDRs through a variety of nonpublic 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 in which a firm conducts an NDR (local institutional investors). We find that local institutional investors increase 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 nonlocal institutional investors. Both the intensity and informativeness of institutional trading are significantly greater for local institutions that 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 et al. (2020), we find that retail trading is significantly less informed in the weeks following an NDR. This finding is 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.

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); Goldstein et al. (2009)), which suggests that NDR brokers 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 possible conflicts that they may create for sell-side analysts. The incentives created by NDRs are similar to investment banking conflicts. Specifically, 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 continues to hold 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

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

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 toward 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 together 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 Reg FD. Reg FD prohibits managers from disclosing material, nonpublic information to analysts and institutional investors, but it continues to allow for private meetings between investors and management, provided that material nonpublic information is not disclosed. Although 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 finding that retail investor trading is significantly less informed around NDRs relative to investor conferences suggests that disclosure of NDR activity may 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), p. 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 (Global 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 NDRs also pose serious conflicts of interest that result in optimistic equity research.

Because NDRs do not fall under the Global Settlement or other regulatory purviews, they should be of great interest to policy makers.

Finally, our paper contributes to the literature on the consequences of private meetings in capital markets. Prior research focuses largely on private meetings at widely disclosed events such as analyst investor days (Kirk and Markov (2016)) and investor conferences (Bushee, Jung, and Miller (2011), 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 nonoverlapping three-day flight windows to financial money centers and nonmoney 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 such 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] Finally, 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.

Our paper is structured as follows. We describe NDRs and our data source in Sections I and II. In Section III, we examine the effects of NDRs on the informativeness of institutional and retail trading. We examine the effects of NDRs on the trading commissions that brokerages receive in Section IV and the conflicts of interest that NDRs create for equity research in Section V. Section VI concludes.

I. 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 such as investor conferences, analyst/investor (AI) days, and NDRs.

[5] In particular, Bushee, Gerakos, and Lee (2018) conclude that "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.

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. First, while Reg FD bans the disclosure of material private information, it explicitly permits the disclosure of nonmaterial information that could help an investor complete a "mosaic of information that, taken together, is material."[7] Second, in practice, the demarcation between material and nonmaterial information is subtle and subject to interpretation. Indeed, survey evidence highlights considerable heterogeneity in what both executives and regulators view as appropriate disclosure under Reg FD (Soltes (2018)). Perhaps unsurprisingly, therefore, the lack of clarity regarding "materiality" has made Reg FD difficult to enforce, which has likely undermined its effectiveness in curtailing selective disclosure.[8] Consistent with this view, recent research suggests 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)) and (2017)), Green et al. (2014a and (2014b)) and 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. Although 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 and the firms participating and the executives attending are known. In addition, transcripts of the events are released with little delay, with many brokers and firms webcasting 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 with virtually no disclosure provided.

NDRs also tend to allow for more intimate and in-depth discussions with managers. For example, it is common for hundreds (and occasionally thousands) of investors to attend both AI days and investor conferences. Although these events typically offer time for Q&A and less formal discussions, they generally do not provide an opportunity 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.

[7] See SEC Release Number 33-781: https://www.sec.gov/rules/final/33-7881.htm.

[8] As of 2019, there have been only 13 Reg FD enforcement cases (Soltes (2018)).

[9] See, 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.

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 include many other investors in the room. The questions he asks in these semipublic 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 from asking tough questions or questions that might give a competing firm additional insight, whereas 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 him to dig more deeply into topics of interest.

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 that "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 second most valuable form of investor outreach 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 (2018) 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]

II. Data

A. Data Source and Descriptive Statistics

We collect NDR data from TheFlyOnTheWall.com (FLY), a news aggregator of financial data.[12] FLY relies on a variety of nonpublic sources, including leaks from employees at brokerage firms and buy-side funds, to obtain information

[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 in Section 2 of Barclays Capital Inc., Merrill Lynch, Pierce, Fenner & Smith Incorporated, and

on NDRs. We capture the firm, date(s), location(s), and 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 data include information on the conference date, the names of each of the presenting companies, and the brokerage firm organizing the conference.

Panel A of Table I provides descriptive statistics for 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 3541 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 NDRs in the FLY sample.

Panel B of Table I 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 U.S. 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 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 km 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 the city-firm-quarter level by summing across all local institutions. The variable *Total Trading* provides a lower bound on institutional trading since it does not capture intra-quarter roundtrip trades, short sales, or confidential filings (Agarwal et al. (2013)). We report the average *Total Trading* for all firm-quarters for each city. Not surprisingly, typical financial centers in which institutional investors are concentrated dominate the most visited

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%26q=Barclays%2BCapital,%2BInc.%2Bv.%2BTheflyonthewall.com,%2B700%2BF.%26hl=en%26as_sdt=4000006%26as_vis=1

[13] Table IA.I of the Internet Appendix provides an example of the NDR data for Microsoft in 2013. The Internet Appendix is available in the online version of this article on *The Journal of Finance* website.

[14] The remaining observations include non-U.S. observations (e.g., London), broad U.S. regions (e.g., "Mid Atlantic"), smaller U.S. 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 at https://uknowledge.uky.edu/finance_data/1/. Additional details are available in Dimmock and Gerken (2012) and Dimmock, Farizo, and Gerken (2018).

Table I

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-Months* 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 U.S. 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 United States and reports the percentage of total institutional trading that is driven by institutional investors who are located within 100 km of the city (*Total Trading*), averaged across all stocks.

Panel A: NDRs

	Firm-Day-City (1)	*Firm-Months* (2)	*Firms* (3)	*Brokers* (4)
Full Sample	43,799	24,809	3541	112
I/B/E/S Matched	43,550	24,656	3526	89
Top 30 Cities	32,324	20,100	3433	106

Panel B: Investor Conferences

	Conf. Presentations (1)	*Firm-Months* (2)	*Firms* (3)	*Brokers* (4)
Full Sample	109,486	73,364	4418	368
I/B/E/S Matched	71,095	53,482	4180	93

Panel C: Frequency of NDRs (Top 30 Destinations)

	NDRs (*Firm-Days*) (1)	*Total Trading* (2)	*Firms* (3)	*Brokers* (4)
New York	8881	36.84	2771	92
Boston	5389	11.30	2238	77
San Francisco	2927	3.91	1471	74
Chicago	2774	13.51	1556	72
Los Angeles	1743	3.03	1032	68
Denver	1313	1.28	800	52
Minneapolis	1146	0.95	778	51
Kansas City	1135	0.95	722	47
Milwaukee	1033	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

(*Continued*)

Table I—*Continued*

Panel C: Frequency of NDRs (Top 30 Destinations)

	NDRs (*Firm-Days*) (1)	*Total Trading* (2)	*Firms* (3)	*Brokers* (4)
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, DC	61	5.12	52	20
Cincinnati	58	0.41	36	19
Richmond	57	0.34	39	20
Tampa	52	0.77	49	21

cities. For instance, New York City accounts for more than 20% of all NDRs and 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]

B. Database Representativeness

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

One potential concern is that FLY may redact or disclose more important NDRs ex post. To shed light on this possibility, for each day during the month of August 2020 we record all NDRs that occurred or were scheduled to occur between August 1, 2020 and December 31, 2020. We find zero cases of FLY either redacting or adding NDRs post-event.

A more general concern is that FLY coverage may not be representative of the universe of NDRs. To address this possibility, we collect NDR data from two 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

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

respond told us that they are unwilling to share these data. The lack of response is consistent with the view that NDRs are secretive events 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 next expand 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 combined sample, we examine 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 II of the Internet Appendix.

We find very limited evidence that FLY coverage is correlated with firm characteristics (see Table IA.III of the Internet Appendix). Of the 17 firm characteristics considered, only two are statistically significant at the 5% level: Intangibles (−) and # Institutions (+). We 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 America, Credit Suisse, Morgan Stanley, Goldman Sachs, Barclays, and Citi are reported (see Table IA.IV of the Internet Appendix). This finding is consistent with the view that FLY relies primarily 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 nonbulge-bracket brokerages. Finally, we also directly compare our main findings for NDRs in the hand-collected sample that are reported in FLY with those that are unreported. The results of this analysis provide no evidence that our main results are biased upward due to FLY's incomplete coverage of NDRs (see Tables IA.V, IA.VI, and IA.VII of the Internet Appendix).

C. 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 institutional investors' demand for management access to be greater for firms with more complex information environments that are harder to value. We thus conjecture that firms with a higher level of recognized intangibles (*Intangibles*), higher R&D expenses ((*R&D* + *ADV*)/*OE*), greater growth opportunities as proxied by the market-to-book ratio (*MB*), and higher 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 the benefits of NDRs to be larger for younger firms with lower visibility (*Age*), firms that issue new shares in the next two years (*SEO*), and firms that make an acquisition in the next two years (*M&A—Acquirer*).

We also control for analyst coverage (*Coverage*) to capture demand for published analyst research as well as several 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^2 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 the firm's stock return over the prior month ($Ret_{m\text{-}1}$) or over the prior 2 to 12 months ($Ret_{m\text{-}12,\ m\text{-}2}$). A detailed description of all variables can be found in the Appendix.

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 0 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 II reports the results. Column (1) provides 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 make an acquisition or SEO within the next two years are more likely to conduct an NDR. The results from column (2), which augments column (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 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.

III. 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 in which a firm

Table II

Determinants of Non-Deal Roadshows (NDRs)

The sample includes the universe of CRSP-Compustat firms from 2013 to 2019 with nonmissing data for all of 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 1 if the firm attends at least one non-deal roadshow (NDR) in the given month, and 0 otherwise. All independent variables are defined in the Appendix. 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.

	Broker NDRs—Dummy (1)	Broker NDRs—Dummy (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^2	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 and firm
R^2	4.50%	9.84%
Observations (firm-months)	277,364	277,364
Mean of dependent variable	8.21%	8.21%

conducts an NDR (local institutional investors) and retail investors who are unlikely to be aware that an NDR is taking place.[17]

A. 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 and 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 2017Q1 relative to nonlocal institutional investors? Second, is the net trading of local institutions informed about future returns?

A.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 I), we aggregate $Trading_{fit}$ to a city-level measure ($Total\ Trading_{cit}$) by summing across all local institutions, defined as any institutional investor headquartered within 100 km 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 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.[18]

We split all firm-city-quarters into those in which the firm visited the city in the quarter (Local NDR = 1) and all others (Local NDR = 0). Panel A of Table III 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% vs. 0.25%, respectively).

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

$$Trading_{cit} = \alpha + \beta_1 Local\ NDR_{cit} + \beta_2 NonLocal\ NDR_{cit} + FE + \varepsilon_{cit}. \quad (1)$$

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

[18] 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%.

Table III

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 I. For each firm-city-quarter, we compute *Total Trading* as the total volume traded by institutions located within 100 km 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 panel regression:

$$Trading_{cit} = \alpha + \beta_1 Local\,NDR_{cit} + \beta_2 NonLocal\,NDR_{cit} + FE + \varepsilon_{cit}.$$

The dependent variable is either *Total Trading* or *Total Net Trading*. *Local NDR* is defined as above, and $NonLocal\ NDR_{cit}$ is an indicator variable equal to 1 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 (columns (1) and (4)) or firm-quarter fixed effects (columns (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 to December 2019.

Panel A: Univariate Comparison

	Local NDR = 1 (N = 21,727)			Local NDR = 0 (N = 2,137,826)		
	Mean (1)	Median (2)	*SD* (3)	Mean (4)	Median (5)	*SD* (6)
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%

(*Continued*)

Table III—*Continued*

Panel B: Regression Results

	Total Trading (1)	*Total Trading* (2)	*Log (Total Trading)* (3)	*Total Net Trading* (4)	*Total Net Trading* (5)	*Log (Net Trading)* (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)
NonLocal 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^2	35.14%	36.43%	55.90%	21.88%	23.64%	54.28%
Observations (firm-city-quarters)	2,144,809	2,144,809	2,144,809	2,144,809	2,144,809	2,144,809

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 $\text{Log}\,(1 \times 10^{-6} + \textit{Trading})$. The independent variable of interest is *Local NDR*. *NonLocal NDR* is also included, which equals 1 if a firm participates in an NDR in quarter t but does not visit city c. The fixed effects vector 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 III presents the results. In columns (1) to (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.[19] Columns (4) to (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.

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%, or roughly one-third of our estimated effect of 0.16% in column (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.

A.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 nonlocal institutional trading during the same quarter. We limit the sample to firm-quarters with NDR activity and we require nonzero trading by local and nonlocal institutional investors in the firm-quarter. We then compare the informativeness of local and nonlocal institutional trading during the NDR quarter by examining the extent to which local and nonlocal 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. Nonlocal institutional order imbalance (*NonLocal 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 *NonLocal OIB* terciles, and we report the average return to the strategy of buying stocks in the top tercile of *Local* (or *NonLocal*) *OIB* and selling stocks in the bottom tercile of *Local* (or

[19] For example, the estimate of 0.29% in column (1) is a roughly 85% increase relative the average value of local institutional trading of 0.34%. Similarly, the estimate in column (3) implies an 88% increase ($e^{0.63-1}$).

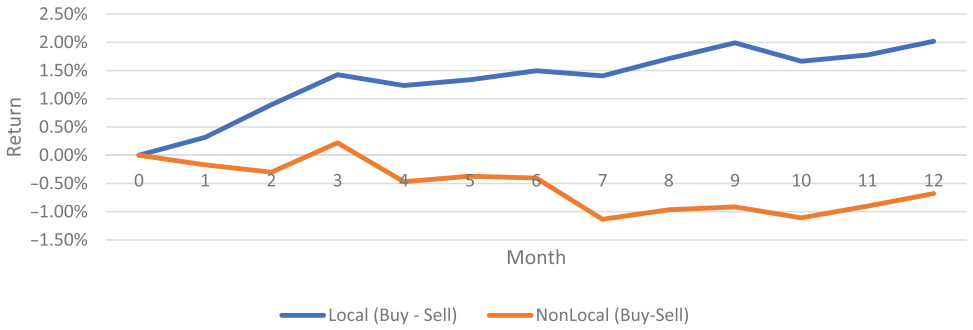

Figure 1. NDRs and the informativeness of institutional trading. At quarter end from March 2013 to December 2019, we sort all NDR firms during the quarter into terciles based on the order imbalances of institutions that are headquartered within 100 km of the NDR location (*Local*) and all other institutions (*NonLocal*). 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. *NonLocal 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 1 to 12 months after the end of the quarter. *NonLocal* reports the returns to the analogous strategy based on *NonLocal OIB*. (Color figure can be viewed at wileyonlinelibrary.com)

NonLocal) *OIB*. Figure 1 plots the returns to this strategy over the subsequent 12 months.[20] 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, with this difference growing to 2.02% over the 12-month holding period. In contrast, the analogous long-short spread based on *NonLocal OIB* is 0.22% over a three-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 nonlocal institutions. Importantly, these findings stand in contrast to the evidence in Bushee, Gerakos, and Lee (2018), who find very weak evidence at best of trading gains around NDRs (p. 374, Table VII). 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:

$$Ret_{it+x} = a + \beta_1 Local\ OIB_{it} + \beta_2 NonLocal\ OIB_{it} + \beta_3 Char_{it} + Qtr_t + \varepsilon_{it}, \quad (2)$$

where Ret_{it+x} is the quarterly return for firm i in quarter $t + x$, with quarter t the NDR quarter. We let x vary from one to four quarters. The independent

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

variables *Local OIB* and *NonLocal OIB* are defined as above, and *Char* is a vector of firm characteristics taken from Boehmer et al. (2020) that includes past one-week returns ($Ret_{w\text{-}1}$), past one-month returns ($Ret_{m\text{-}1}$), returns over the prior two to seven months ($Ret_{m\text{-}7,m\text{-}2}$), 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.

Column (1) of Table IV reports the estimates from equation (2) for the one-quarter-ahead returns. We find that a one-standard-deviation increase in *Local OIB* is associated with a statistically significant 0.66% higher one-quarter-ahead return. In contrast, the coefficient on *NonLocal 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 *NonLocal OIB* (0.56%) is statistically significant.

Columns (2) to (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, with this information subsequently impounded into prices and the majority of the effect occurring within one quarter.

The results from Table IV 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 be more informed about NDR firms than nonlocal institutions even in the absence of an NDR. To address this concern, we next consider an alternative benchmark that compares the informativeness of local institutional trading during the NDR quarter to the informativeness of local (and nonlocal) institutional trading in each of the three quarters prior to the NDR. For example, for quarter (−3) we examine local and nonlocal institutional trading in the NDR firm three quarters prior to the NDR. We exclude firms that 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 IV is due 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 due only to the NDR, the estimated effect should be small in non-NDR quarters.

Figure 2, Panel A, 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 2, Panel A, 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

Table IV

NDRs and the Informativeness of Local Institutional Trading

This table reports estimates from the panel regression:

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

The sample includes all firm-quarters with NDR activity and nonzero trading by local and nonlocal institutional investors in the firm-quarter from January 2013 to December 2019. Ret_{it+x} is the one-quarter return for firm i in quarter $t + x$, where quarter t is the quarter in which 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 *NonLocal OIB* is defined analogously. *Char* is a vector of firm characteristics taken from Boehmer, Jones, and Zhang, and Zhang (2020) and defined in the Appendix. *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 *NonLocal OIB*.

	Qtr. 1 (1)	Qtr. 2 (2)	Qtr. 3 (3)	Qtr. 4 (4)
Local OIB	0.659%	0.020%	0.357%	0.013%
	(3.89)	(0.14)	(1.55)	(0.06)
NonLocal 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 – NonLocal OIB	0.560%	0.210%	0.438%	−0.120%
	(2.34)	(0.93)	(1.24)	(−0.42)
R^2	13.63%	12.36%	12.53%	12.92%
Observations (firm-quarters)	11,240	10,797	10,301	9845

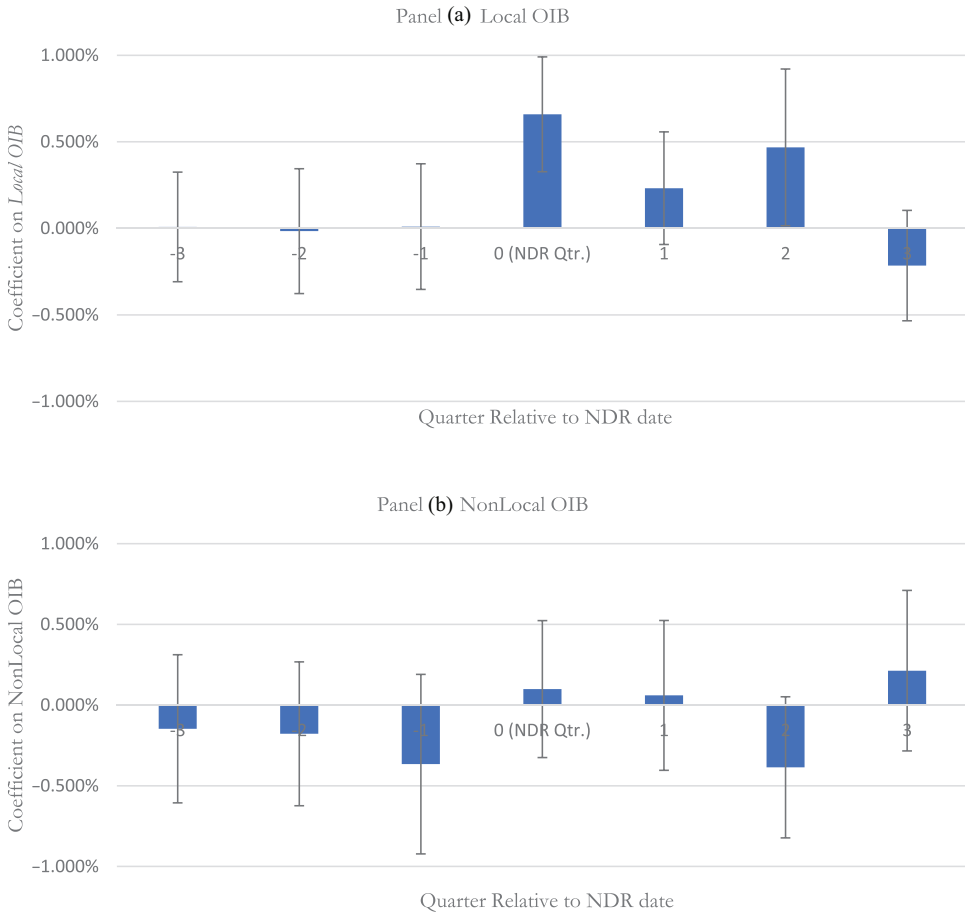

Figure 2. NDRs and the informativeness of institutional trading—pre- and post-NDR. This figure examines the informativeness of local and nonlocal institutional trading in each of the three quarters prior to and after the NDR quarter. We repeat the regression in column (1) of Table IV after shifting the timing of the NDR. For example, for quarter (−3) we examine local and nonlocal institutional trading three quarters prior to the firm conducting the NDR. Panel A (B) plots the estimates and 95% confidence intervals for *Local OIB* (*NonLocal OIB*) for each of the seven regressions over the [−3,3] interval. The confidence intervals are computed from standard errors double-clustered by firm and quarter. (Color figure can be viewed at wileyonlinelibrary.com)

are 0.16%. The estimates across the three quarters are jointly insignificant, although the point estimate for quarter 2 is significant at the 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 *NonLocal OIB*. The results, reported in

Figure 2, Panel B, indicate that nonlocal order imbalances are not significantly related to future returns across any of the quarters.

A.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 the types of investors who firms are more likely to meet with and the types of investors who are more likely to trade following an NDR meeting. Similarly, variation in the informativeness of institutional trading speaks to the type of investors who firms are more likely to meet with and the types of investors who extract the most useful information from NDRs.

We first compare hedge fund trading to the trading of all other institutions (nonhedge funds). Solomon and Soltes (2015) find that hedge funds are more likely to meet privately with management and more likely to trade in informed ways following the meeting. Although 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 that suggests 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 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., Brunnermeier and Nagel (2004), Griffin and Xu (2009)), we classify an institution as a hedge fund if it meets two criteria: (i) at least 50% of its clients are "Other pooled investment vehicles" or "High net worth individuals" and (ii) it charges performance-based fees. We then re-estimate column (3) of Table III after partitioning the sample into hedge funds and other funds. Panel B of Table V reports the results. We find that local nonhedge fund trading increases by 112% ($e^{0.75-1}$) compared to only 45% for local hedge fund trading, with the difference between the two estimates 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 survey evidence suggesting that the majority of firms are reluctant to meet privately with hedge funds (Brown et al. (2019)).

We next compare the informativeness of hedge fund and nonhedge fund trading by repeating column (1) of Table IV for the two groups. The results, reported in columns (2) and (3) of Table V, 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

Table V

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

This table reproduces Tables III and IV after partitioning the sample. Column (1) reports the estimates on *Local NDR* from column (3) of Table III. Columns (2) and (3) report the estimates on *Local OIB* and *NonLocal OIB* from column (1) of Table IV. For reference, Panel A reports the baseline results from the full sample. Panels B through D partition investors into *Hedge Funds* versus *NonHedge 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 median NYSE market capitalization. More detailed variable definitions are provided in the Appendix. 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 (1)	*Local OIB* (2)	*NonLocal OIB* (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)
NonHedge Fund	0.75	0.680%	−0.245%
	(17.91)	(3.44)	(−0.88)
HF – NonHF	−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)

estimate for nonhedge 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 should 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 high-turnover funds to 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 IV suggesting 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*). Column (1) of Panel C shows that the intensity of trading does not vary significantly with fund turnover. However, columns (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 both the intensity and the informativeness of local institutional trading to 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*.[21] Consistent with NDRs being targeted toward the firm's largest investors, in Panel D we report that local trading is significantly larger for *High Ownership* funds.

[21] 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.

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*. We further find that *Low Ownership NonLocal 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 NDRs to 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 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% vs. 0.41%), but both estimates are statistically significant and the estimates are not significantly different from each other. Thus, although NDRs induce substantially more trading for smaller firms, the informational advantages associated with NDRs are not confined to small firms.

B. 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 et al. (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 1 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.

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 1) as retail purchases, while trades with exchange code "D" and prices just above a round penny (fraction of a cent between 0 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.[22]

Our objective is to examine how the relationship between retail order imbalances and future returns changes around NDRs. We measure retail order

[22] 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.

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 $t - 10$.[23]

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

$$\begin{aligned} Ret_{it+x} = a &+ \beta_1 Retail\ OIB_{it} + \beta_2 Retail\ OIB_{it} x NDR_{it,t-10} + \beta_3 NDR_{t,t-10} \\ &+ \beta_4 Retail\ OIB_{it} x Conf_{t,t-10} + \beta_5 Conf_{t,t-10} + \beta_6 Char_{it} \\ &+ \beta_7 Retail\ OIB_{it} x Char_{it-} + Day_t + \varepsilon_{it}, \end{aligned} \tag{3}$$

where Ret_{it+x} is the weekly (i.e., five-day-ahead) return for firm i during week x, and day t is the day for which *Retail OIB* is constructed. We let x vary from one to four weeks. The variable *NDR* is an indicator equal to 1 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. The vector *Char* contains 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 VI 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 nonevent 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.[24] 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 nonevent 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. 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

[23] We focus on a 10-day event window because we expect local institutional trading to persist for a couple of weeks following the NDR. For example, in Table VII, 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.VIII of the Internet Appendix. We find qualitatively similar results.

[24] 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.3 of the Internet Appendix 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.

Table VI

NDRs and the Informativeness of Retail Trading

This table reports estimates from the following panel regression:

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

The sample includes 5,257,844 firm-days from January 2013 to 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. Column (1) reports the one-week ahead return assuming all trades are executed at the closing price on day t, and column (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. Columns (3) to (5) report results for one-week returns for weeks 2 to 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, Jones, and Zhang, and Zhang (2020). *NDR* (*Conf*) is an indicator variable equal to 1 if the firm attended an NDR (Conf) in the previous 10 days, and 0 otherwise. Detailed variable definitions are provided in the Appendix. 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) (1)	Week 1 (Include 0) (2)	Week 2 (3)	Week 3 (4)	Week 4 (5)
Retail OIB	0.042%	0.001%	0.018%	0.014%	0.015%
	(7.66)	(0.19)	(3.95)	(2.96)	(2.99)
Retail OIB × *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 × *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 × *Log* (*Turnover*)	−0.021%	0.002%	0.006%	−0.008%	0.007%
	(−2.54)	(0.20)	(0.73)	(−1.14)	(0.95)
Retail OIB × *Log* (*Vol*)	−0.007%	0.007%	−0.008%	0.004%	0.000%
	(−1.37)	(1.16)	(−1.53)	(0.75)	(−0.05)
Retail OIB × *Log* (*Firm Size*)	0.037%	0.010%	0.020%	0.002%	0.011%
	(5.73)	(1.53)	(3.20)	(0.27)	(1.92)

(*Continued*)

Table VI—*Continued*

	Week 1 (Exclude 0) (1)	Week 1 (Include 0) (2)	Week 2 (3)	Week 3 (4)	Week 4 (5)
Retail OIB × *Log* (*BM*)	0.002%	−0.007%	−0.005%	0.007%	−0.008%
	(0.17)	(−0.67)	(−0.47)	(0.76)	(−0.96)
Retail OIB × *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 × *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 × *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* × *NDR*)	0.001%	−0.034%	0.010%	−0.017%	0.008%
	(0.07)	(−2.37)	(0.79)	(−1.20)	(0.54)

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 et al. (2020) and Akbas and Subasi (2019)).

Following BJZZ (2020), column (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.[25] We next repeat column (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. Column (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

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

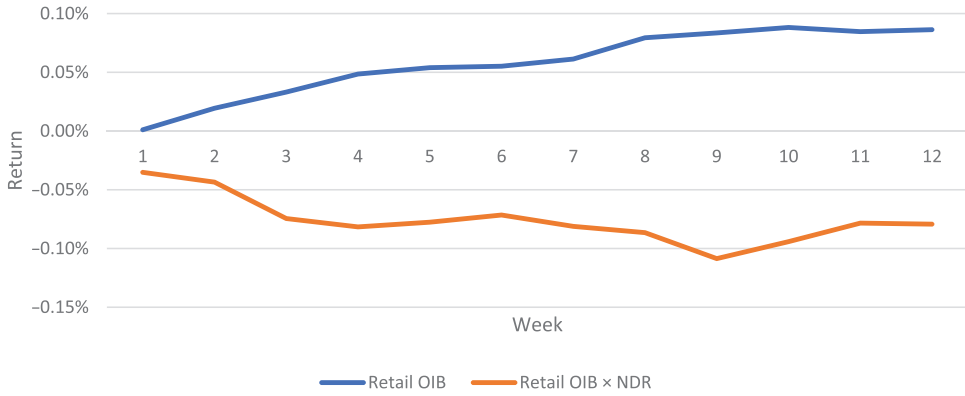

Figure 3. NDRs and the informativeness of retail trading. This figure repeats the regressions reported in Table VI, after replacing the dependent variable with cumulative returns from weeks 1 to 12. Returns are computed assuming that all trades for stocks with positive (negative) retail order imbalances are executed at the retail-volume weighted average purchase (sale) price. The figure plots the estimates on *Retail OIB* and *Retail OIB* × *NDR* from 12 separate regressions (weeks 1 to 12). (Color figure can be viewed at wileyonlinelibrary.com)

retail trade informativeness is concentrated over short horizons but permanent.

IV. NDRs and Trading Commissions

Sections III.A.1 and III.A.2 document that NDRs generate a substantial increase in trading by nearby institutions and that 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 the broker that executed the trade.[26] 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 data set with I/B/E/S by broker name, resulting in a merged sample of 42 I/B/E/S brokers. We drop

[26] 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 (e.g., Jame (2018)). However, more recent vintages that overlap with our NDR sample period are anonymous. See Hu et al. (2018) for additional details on the Abel Noser data set.

broker-firm pairs when there is zero trading volume for the firm through the broker during the sample period. We are able to match 1732 broker-sponsored NDRs and 7452 broker-hosted conferences with the Abel Noser transaction data.

We estimate the panel regression:

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

The dependent variable, *Com*, is a measure of commissions for brokerage firm j in stock i during week t. We consider two measures of commissions: *$Commissions*, the natural log of 1 plus the total dollar commissions for broker j in stock i during week t, and *Commission Share*, the total commissions for broker j in stock i during week t scaled by total Abel Noser commissions across all I/B/E/S-Abel Noser matched brokers for stock i in week t. Thus, *$Commissions* allows us to examine whether NDR brokers generate an increase in commission revenue, due either 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 1 if brokerage firm j organized an NDR for firm i in week t, and *Conf*, an indicator equal to 1 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.[27] Finally, all specifications include broker-firm fixed effects to control for the fact that some brokers tend to have persistently higher commissions in certain stocks.

Columns (1) and (2) of Table VII 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 relative to conferences, and they highlight the perceived value of NDRs to institutional clients. Columns (3) and (4) show 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 is an NDR in the subsequent two weeks (*NDR* [1,2]), subsequent three to four weeks (*NDR*

[27] 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 both including and excluding *Turnover* as a control.

Table VII

Weekly Commissions around NDRs

This table presents estimates from the 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 nonzero trading during the sample period (3,058,195 broker-firm-weeks). In columns (1), (3), and (5), the dependent variable is *$Com*, defined as the log (1 + Commissions) of broker j in firm i during week t. In columns (2), (4), and (6), the dependent variable is *Commission Share*$_{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 1 if broker j took (or will take) firm i on an NDR or a conference during week $t + x$, and 0 otherwise. For example, *NDR* (0) equals 1 if broker j took firm i on an NDR in week t, *NDR* (−1,−2) equals 1 if broker j took firm i on an NDR in week $t - 1$ or $t - 2$, and *NDR* (1,2) equals 1 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. (1)	*CS* (2)	*$ Com.* (3)	*CS* (4)	*$ Com.* (5)	*CS* (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)

(*Continued*)

Table VII—*Continued*

	$ Com. (1)	*CS* (2)	*$ Com.* (3)	*CS* (4)	*$ Com.* (5)	*CS* (6)
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^2	41.61%	16.38%	42.08%	16.38%	42.08%	16.38%
Observations (firm-broker-week)	3,058,195	3,058,195	3,058,195	3,058,195	3,058,195	3,058,195

[3,4]), or subsequent five to eight weeks (*NDR* [5,8]). We further include analogous measures for conferences. Columns (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. These results are 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.[28]

V. NDRs and Analyst Conflicts of Interest

In the previous two sections, we demonstrate 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 whether 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.[29] Because analyst compensation is based on the revenue they generate for the broker firm (Groysberg, Healy, and Maber (2011)), organizing NDRs can be lucrative for 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' 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 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.

[28] A related question is whether the increased trading through the sponsoring broker is informed. In Section III.B of the Internet Appendix, we compare the informativeness of trades executed through the sponsoring and nonsponsoring 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.

[29] 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*).

A. 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 in which 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 nonmissing recommendation (target price) data. We split the sample into firm-months for which a broker will take the firm on an NDR over the subsequent three months (*NDR*3 = 1 or *NDR broker*) versus all other broker-firm-months (*NDR*3 = 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 VIII reports analyst and broker characteristics. Detailed definitions of the analyst and broker characteristics are available in the Appendix. We find that NDR brokers are significantly more likely to host the firm at a conference in the subsequent three months (8.15% vs. 5.01%). Notably, there is no meaningful difference between NDR brokers and non-NDR brokers with respect to banking affiliation status (1.04% vs. 1.11%, respectively).

Panel B provides statistics on three measures of analyst optimism: *Rec Level, Target Return*, and *Target Return Bias*. The first variable, *Rec Level*, is the analyst's current recommendation. We convert this measure to a numeric value using the following scale: 1 = strong buy, 2 = buy, 3 = hold, 4 = sell/underperform, and 5 = strong sell. The second measure, *Target Return*, is the 12-month expected return (excluding dividends) implied by broker *j*'s most recent 12-month price forecast of firm *i* as of month *t*, computed as $(Forecast\ Price_{jit}/Price_{it\text{-}1})-1$. The third measure, *Target Return Bias*, is the difference between *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 4, Panel A, plots *Abnormal Rec Level*

Table VIII
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 to 2019 for which 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 in which broker j takes 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 the Appendix. 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 difference in the means scaled by the standard deviation (column (5)).

	NDR3 = 1 (1)	NDR3 = 0 (2)	Difference (3)	*SD* (4)	Scaled Difference (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%

from months −36 to +36, where 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 level of optimism then declines sharply in the year following the NDR and continues to slowly decline over longer horizons. Figure 3, Panel B, documents a similar pattern for *Target Returns*.[30] 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.[31]

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

[31] 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 who sponsor a firm's NDR only once are far more concentrated over shorter windows around the NDR (see Section III.C of the Internet Appendix).

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 results corresponding to the optimism measure computed using recommendation levels (*Abnormal Rec Level*), where strong buy = 1 and strong sell = 5 (a more negative recommendation level indicates greater optimism). Panel B plots results corresponding to the optimism measure 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). (Color figure can be viewed at wileyonlinelibrary.com)

B. Multivariate Regressions of Analyst Optimism

We next consider a multivariate regression that controls for other factors likely to influence analyst research optimism. The formal model is:

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

where $Optimism_{jit}$ is either *Rec Level* (columns (1) and (2)) or *Target Return* (columns (3) and (4)). The main variable of interest is *NDR*3. We also include

other brokerage activities that have the potential to impact analyst bias. In particular, *Conf*3 (*Affiliated*3) is an indicator variable equal to 1 if the firm will participate in the broker's conference (will become a banking client) in the next three months, and 0 otherwise. Not only are *Conf*3 and *Affiliated*3 important controls, but they also provide a useful benchmark for gauging the magnitude of the bias associated with NDRs.

The remaining variables in equation (5), *Controls*, are common controls for broker and analyst-specific characteristics. Specifically, we include *Log* (*Broker Size*), the natural log of the number of analysts that a broker employs, to control for broker prestige and reputation and *Log* (*Firm experience*) and *Log* (*Experience*), the natural logs of the analyst's firm-specific forecasting experience and overall analyst experience, respectively, to capture expertise and accuracy. We also include *Log* (*Firms Followed*), the natural log of the size of the analyst's coverage portfolio. Analysts with larger coverage portfolios have less time to allocate to each individual firm in their portfolio and therefore may display less accuracy. Finally, we include *All-Star*, an indicator variable equal to 1 if the analyst was chosen for *Institutional Investor's* annual all-star poll, and 0 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 IX reports the estimates. In column (1), *NDR*3 has a coefficient 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 *Conf*3 and *Affiliated*3 are also highly significant although the magnitudes are less than half 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 column (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 and thereby controls for a number of important differences that could 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 *NDR*3 declines but remains economically large at −0.29 and highly statistically significant. The inclusion of firm-month fixed effects has a more pronounced effect on the coefficients *Conf*3 and *Affiliated*3. 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,

Table IX
NDRs and Analyst Optimism

This table reports estimates from the 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 to 2019 for which the broker issues at least one recommendation (columns (1) and (2)) or target price (columns (3) and (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. Columns (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 (a more negative recommendation level indicates greater optimism). In columns (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*$_{it\text{-}1}$)−1. *NDR*3 is an indicator variable equal to 1 if the broker takes the firm on an NDR over the subsequent three months. *Conf*3 and *Affiliated*3 are indicator variables equal to 1 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 the Appendix. 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)
*NDR*3	−0.39	−0.29	7.66%	4.46%
	(−38.88)	(−31.11)	(17.55)	(22.72)
*Conf*3	−0.16	−0.06	7.62%	1.47%
	(−15.59)	(−6.35)	(15.69)	(9.42)
*Affiliated*3	−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^2	2.57%	29.67%	4.57%	71.85%
Observations (broker-firm-month)	1,565,813	1,565,813	1,955,800	1,955,800

and nearly six times as large as the excess optimism for brokers with an investment banking affiliation.

Columns (3) and (4) present analogous results using *Target Return* as the dependent variable. Similar to recommendation levels, NDR brokers issue

significantly more optimistic target prices. For example, in column (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 1 if the analyst revises her recommendation level upward for a given firm-month (e.g., from a buy to a strong buy), or *Downgrade*, defined analogously. The results reported in Table IA.X of the Internet Appendix confirm that *NDR*3 is significantly positively associated with *Upgrade* and significantly negatively associated with *Downgrade*.

C. 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 IV) 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 for NDRs more aggressively (i.e., issue more optimistic research) 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: (i) *Multi-Day NDR*, an indicator equal to 1 if the NDR trip spans multiple days; (ii) *Big Inst. NDR*, an indicator equal to 1 if the firm is visiting a city that has a top five concentration of institutional ownership, and (iii) *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 in turn 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 analysts with a higher reputation, as proxied by all-star status (*All-Star*), experience as an analyst (*Experience*), and the size of the brokerage firm employing the analyst (*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 who 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.[32]

We test these predictions by estimating the following regression:

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

where *Optimism, NDR*3*, Conf*3*, Affiliated*3, and *Controls* are defined as in equation (5), and *CV* is a vector that contains 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 the Appendix. Finally, FE denotes firm-month fixed effects.

Table X, columns (1) to (3), reports the results using *Rec Level* as the dependent variable. Column (1) includes the conditioning variables associated with NDR or firm attributes, column (2) reports the results for analyst-level attributes, and column (3) reports the results for all attributes. Turning to column (3), we find the results are generally consistent with the predicted effects. For example, all three of the proxies for expected trading commissions (*NDR*3 × *MultiDay, NDR*3 × *Big Inst. NDR*, and *NDR*3 × *Turnover*) are at least marginally significant ($p \leq 0.10$) and take the predicted sign. The relation between optimism and valuation difficulty is more mixed, with optimism decreasing in firm size (as predicted) and 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 a 0.08 increase in recommendation optimism. Finally, consistent with optimism declining in analyst reputation, we find that recommendation-level optimism is weaker among analysts with greater experience and among all-star analysts. Columns (4) to (6) report analogous results for target price optimism. Although there are some differences (e.g., the correlation between target price optimism and analyst reputation is 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.

D. NDR Broker Optimism: Strategic or Sincere?

The findings above are consistent with NDR brokers strategically issuing optimistically biased research 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

[32] In a survey of IROs, 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).

Table X

NDRs and Analyst Optimism—Cross-Sectional Patterns

This table reports estimates from the panel regression:

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

The sample consists of all broker-firm-months from 2013 to 2019 for which the broker issues at least one recommendation (columns (1) to (3)) or target price (columns (4) to (6)) for the firm in the prior 24 months. *Optimism* is either *Rec Level* (columns (1) to (3)) or *Target Return* (columns (4) to (6)). *NDR*3 is an indicator variable equal to 1 if the broker takes the firm on an NDR over the subsequent three months. *Conf*3 and *Affiliated*3 are indicator variables equal to 1 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 the Appendix. 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)
*NDR*3	−0.28	−0.30	−0.29	3.89%	4.58%	3.94%
	(−18.72)	(−29.24)	(−18.61)	(14.13)	(21.94)	(14.09)
*NDR*3 × *Multi Day NDR*	−0.03		−0.03	0.56%		0.61%
	(−1.73)		(−1.81)	(1.87)		(2.00)
*NDR*3 × *Big Inst. NDR*	−0.02		−0.02	0.80%		0.87%
	(−1.22)		(−1.65)	(2.40)		(2.60)
*NDR*3 × *Turnover*	−0.02		−0.02	0.50%		0.50%
	(−1.73)		(−1.71)	(1.91)		(1.91)
*NDR*3 × *Firm Size*	0.07		0.07	−0.70%		−0.59%
	(4.91)		(4.26)	(−1.69)		(−1.41)
*NDR*3 × *Volatility*	0.02		0.02	1.96%		1.95%
	(1.84)		(1.98)	(5.82)		(5.80)
*NDR*3 × *Coverage*	−0.08		−0.08	1.65%		1.70%
	(−5.17)		(−5.38)	(4.82)		(4.96)
*NDR*3 × *Broker Size*		0.00	0.01		−0.38%	−0.32%
		(0.17)	(0.86)		(−1.30)	(−1.05)
*NDR*3 × *Experience*		0.05	0.05		−0.37%	−0.18%
		(5.18)	(5.31)		(−1.79)	(−0.88)
*NDR*3 × *All Star*		0.05	0.06		−0.54%	−0.41%
		(1.72)	(2.15)		(−1.07)	(−0.83)
*Conf*3	−0.06	−0.06	−0.06	1.46%	1.47%	1.46%
	(−6.46)	(−6.42)	(−6.45)	(9.46)	(9.50)	(9.47)
*Affiliated*3	(−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)

(*Continued*)

Table X—*Continued*

	Rec Level			Target Return		
	(1)	(2)	(3)	(4)	(5)	(6)
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^2	29.65%	29.65%	29.66%	71.79%	71.78%	71.79%
Observations (broker-firm-month)	1,555,701	1,555,701	1,555,701	1,947,749	1,947,749	1,947,749

optimistic about a firm's prospects, and firms simply select these optimistic analysts to organize their NDRs (hereafter "sincere optimism").[33]

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 and 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. In contrast, 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 but 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 1 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)], 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 a rank of 2, etc., and *Number of Analysts* is the number of analysts issuing a forecast for the firm-quarter. Higher values of *MBE* and *Relative Earnings Pessimism* indicate greater pessimism.

Table XI reports the results. Columns (1) and (2) document a significant positive relation between *NDR3* and *MBE*. Similarly, columns (3) and (4) document

[33] 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.

Table XI

NDRs and Quarterly Earnings Forecast Pessimism

This table reports estimates from the panel regression:

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

The sample consists of all broker-firm-months from 2013 to 2019 for which the broker issues at least one recommendation (columns (1) to (3)) or target price (columns (4) to (6)) for the firm in the prior 24 months. *Optimism* is either *Rec Level* (columns (1) to (3)) or *Target Return* (columns (4) to (6)). *NDR*3 is an indicator variable equal to 1 if the broker takes the firm on an NDR over the subsequent three months. *Conf*3 and *Affiliated*3 are indicator variables equal to 1 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 the Appendix. 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.

	MBE		Relative Earnings Pessimism	
	(1)	(2)	(3)	(4)
*NDR*3	2.55%	1.30%	1.10	1.41
	(5.77)	(5.61)	(6.00)	(6.16)
*Conf*3	1.21%	0.53%	0.24	0.31
	(3.31)	(2.71)	(1.65)	(1.77)
*Affiliated*3	−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^2	0.48%	59.95%	0.04%	0.12%
Observations (broker-firm-month)	1,507,564	1,507,564	1,507,564	1,507,564

a positive relation between *NDR*3 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, together 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 part of the observed correlation between NDR activity and analyst optimism.

VI. Conclusion

We examine the effects of 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 in which a firm attends an NDR substantially increase their trading in the firm, with this trading significantly more informed, while retail investor trading is 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 as a result 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 issue significantly more optimistic recommendations and target price forecasts, with this optimism peaking 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 for two of the most important regulatory reforms pertaining to sell-side analysts in the past several decades: Reg FD and the Global Settlement. In the interest of providing more equal access to information across investors, Reg FD prohibits the selective disclosure of material information. However, it continues to allow for private meetings between investors and management provided that material, nonpublic 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 ties between the corporate finance and research divisions of investment banks, including analyst compensation tied to generating banking business. The Global Settlement also mandates improved disclosure, including disclosure of whether the brokerage house has an investment banking affiliation with the firm. Importantly, NDRs do not fall under the Global Settlement or related regulations, but our evidence suggests that the potential conflicts are just as large economically. Further, in comparison to banking deals for which 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.

Initial submission: March 16, 2020; Accepted: March 4, 2021
Editors: Stefan Nagel, Philip Bond, Amit Seru, and Wei Xiong

Appendix: Variable Definitions

A. 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 advertising are set to zero. Winsorized at the 99th percentile. Source: Compustat.
- *Market-to-Book* (*MB*): Market-to-book ratio computed as the market capitalization at the end of the calendar year scaled by the book value of equity during year $t - 1$. Positive values are winsorized at the 99th percentile. Negative values are set to zero and we include a corresponding negative book value indicator (*Negative Book*, often untabulated). Source: CRSP/Compustat.
- *Idiosyncratic Risk*: Square root of the mean squared residual from an annual regression of a firm's daily returns on market (value-weighted CRSP index) returns. Source: CRSP.
- *Institutional Ownership*: Percentage of the firm's shares held by institutions at year-end. Winsorized at 100%. Source: Thomson Reuters S34.
- *Firm Age*: Number of years since the firm first appeared on CRSP. Source: CRSP.
- *Net Shares*: 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*: Dummy variable equal to 1 if a firm issues a seasoned equity offering in the next two years. Source: SDC.
- *M&A Acquirer*: Dummy variable equal to 1 if a firm announces the acquisition of another firm in the next two years. Source: SDC.
- *Coverage*: Number of analysts issuing earnings forecasts for firm i during quarter t. Source: I/B/E/S.
- # *Institutions*: The number of institutions holding firm shares at year-end. Source: Thomson Reuters S34.
- *Firm Size*: Market capitalization computed as share price times total shares outstanding at the end of June. Source: CRSP.
- *Turnover*: Average daily turnover (i.e., share volume scaled by shares outstanding) over all trading days in the year. Winsorized at the 99th percentile. Source: CRSP.
- *R-squared*: R^2 from an annual regression of a firm's daily returns on market (value-weighted CRSP index) returns. Winsorized at the 99th percentile. Source: CRSP.
- *Ret* $(m - 1)$: The return over the prior month. Source: CRSP.

- *Ret* (m – 12, m – 2): The return over the prior two to 12 months. Source: CRSP.
- *Ret* (m – 7, m – 2): The return over prior two to seven months. Source: CRSP.
- *Ret* (w – 1): The return in the prior week. Source: CRSP.
- *Vol*: Standard deviation of daily returns over the prior calendar year. Source: CRSP.
- *Book-to-Market* (*BM*): 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 99$^{\text{th}}$ percentile. Negative values are set to zero and we include a corresponding negative book value indicator (untabulated). Source: CRSP/Compustat.

B. Institutional Trading Measures and Institutional Investor Characteristics

- *Total Trading*: 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 the city-quarter level by summing *Total Trading* across all institutions local to city c in quarter t. Source: Thomson Reuters S34.
- *Total Net Trading*: (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 the 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* (*NonLocal*) *OIB*: Local (nonlocal) institutional buy volume less local (nonlocal) institutional sell volume, scaled by total institutional volume. Source: Thomson Reuters S34.
  - An institutional investor is classified as local (nonlocal) to a city if the investor is headquartered within (beyond) 100 km of the city's downtown.
- *Local NDR*: Dummy variable equal to 1 if firm i participated in an NDR in city c during the previous quarter, and 0 otherwise.
- *NonLocal NDR*: Dummy variable equal to 1 if firm i participated in an NDR during the previous quarter but not in city c, and 0 otherwise.
- *Hedge Fund* (HF): Indicator equal to 1 if (i) at least 50% of the fund's clients are "Other pooled investment vehicles" or "High net worth individuals" and (ii) the fund charges performance-based fees. Source: Thomson Reuters S34 and Form ADV.
  - *NonHedge Fund* (*NonHF*): Any 13F filing institution not classified as a *Hedge Fund*.
- *Fund Turnover*: 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 1 if the fund is in the top (bottom) half of *Fund Turnover*.

- *Fund Ownership*: Total shares of the stock owned by a fund scaled by total shares outstanding. Source: Thomson Reuters S34.
  - ○ *High* (*Low*) *Ownership*: Indicators equal to 1 if the fund is in the top quintile (bottom four quintiles) of *Fund Ownership*.
- *Sponsor OIB*: Total shares of the firm bought through the sponsoring broker on the date minus 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*: Indicator equal to 1 (0) if *Sponsor OIB* is greater (less) than zero.
- *NonSponsor OIB*: Total shares of the firm bought through the nonsponsoring broker on the date minus total shares of the firm sold through the nonsponsoring broker on the date, scaled by total trading volume in the firm through the nonsponsoring broker on the date. Source: Abel Noser.
  - ○ *NonSponsor Buy*: Indicator equal to 1 (0) if *NonSponsor OIB* is greater (less) than zero.

C. 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, and Zhang, and Zhang (2020). Source: TAQ.

D. Commission Measures

- *$Commissions* (*$Com.*): Natural log of 1 plus the total dollar commissions for broker j in stock i during week t. Source: Abel Noser.
- *Commission Share* (*CS*): 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.

E. Analyst and Broker Characteristics

- *NDR*3: Indicator variable equal to 1 if broker j takes firm i on an NDR in the subsequent three months (i.e., t through $t + 2$), and 0 otherwise. Source: *FLY*.
- *Conf*3: Indicator variable equal to 1 if broker j hosts firm i at one of its investor conferences over the next three months (i.e., t through $t + 2$), and 0 otherwise. Source: Bloomberg Corporate Events Database.
- *Affiliated*3: A dummy variable equal to 1 if broker j is a lead underwriter for firm i for an equity (i.e., SEO) offering or debt offering, or is lead advisor on an M&A, in the next three months, and 0 otherwise. Source: SDC.

- *Broker Size*: Total number of analysts issuing earnings forecasts for brokerage firm j during year t. Source: I/B/E/S.
- *Firm Experience*: Number of years since analyst j first issued earnings forecasts for firm i. Source: I/B/E/S.
- *Experience*: Number of years since analyst j first issued earnings forecasts for any firm. Source: I/B/E/S.
- *Firms Followed*: Number of firms followed by analyst j in year t. Source: I/B/E/S.
- *All-Star*: Dummy variable equal to 1 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*: Indicator equal to 1 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).

F. Research Characteristics

- *Rec Level*: Most recent outstanding recommendation of broker j for firm i in month t. If 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 = strong buy, 2 = buy, 3 = hold, 4 = sell/underperform, and 5 = strong sell. Source: I/B/E/S.
  - *Abnormal Rec Level*: *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*): *Rec Level* of broker j for firm i in month $t - 1$.
- *Upgrade*: Indicator variable equal to 1 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*: Indicator variable equal to 1 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 $(Forecast\ Price_{jit} / Price_{it\text{-}1}) - 1$. The sample is limited to 12-month-ahead forecasts. If 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 percentiles. 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 percentiles. Source: I/B/E/S.
- *Meet or Beat Earnings* (*MBE*): Dummy variable equal to 1 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)], where *Rank* is the rank of the analyst's forecasted earnings estimate, with the highest estimate value takes a ranking of 1, the second-highest estimate 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.

G. NDR Characteristics

- *Multi Day NDR*: NDR trip that spans more than one day. Source: FLY.
- *Big Inst. NDR*: Indicator equal to 1 if the firm is visiting a city that has a top five concentration of institutional ownership. Source: FLY.
- *Hand*: Indicator equal to 1 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*: Indicator equal to 1 if the NDR we obtained from the hand-collected sample (described above) was not reported by FLY. Source: FLY.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix
Replication Code.