# Do Connections with Buy-Side Analysts Inform Sell-Side Analyst Research?


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**Abstract:** We hypothesize that connections with buy-side analysts provide a sell-side analyst with private information generated by the buy-side that enhances the quality of sell-side research. We proxy for these connections with the number of stocks at the intersection of stocks held in the portfolios of institutional investors and followed by the sell-side analyst. The larger this intersection, the more opportunities the sell-side analyst has to interact with institutional investors. We proxy for the research quality of the sell-side analyst with her earnings forecast accuracy. We find that such connections enhance the accuracy of earnings forecasts, but up to a point of diminishing returns. Additional tests rule out reverse causality and omitted variables as explanations for the association, and strengthen the inference that connections between sell- and buy-side analysts increase the flow of information and improve the quality of sell-side research output.



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## 1. Introduction

This paper investigates whether, through connections with buy-side analysts, sell-side analysts glean information that enhances the quality of their research reports. A vast literature studies the characteristics of the research produced by sell-side analysts and its impact on stock prices, arguably through its impact on institutional investor decisions.[1] This research generally assumes that the flow of information between sell-side and buy-side analysts working for institutional investors is one-directional; i.e., information supplied by the sell-side flows to institutional investors, whose trades move stock prices (e.g., Gu, Li, Li, & Yang 2016; Irvine, Lipson, & Puckett, 2007; Mikhail, Walther, & Willis, 2007). Our paper looks at the flow of information in the other direction; i.e., do insights from the research of buy-side analysts, in support of institutional investor decisions, flow to sell-side analysts and improve the quality of sell-side analyst research reports?

We posit that connections with buy-side analysts provide a sell-side analyst with private information generated by the buy-side that enhances the quality of sell-side research. Our primary measure of connections assumes that a sell-side analyst's opportunity to learn about a given firm's prospects from the buy-side analysts reporting to a given institutional investor holding the firm's stock increases with the number of *other stocks* followed by the analyst and also held in the institution's portfolio. The idea is that the larger this number, the more discussions the sell-side analyst likely has with the institutional investor's buy-side analysts with respect to the given firm. This number of *other stocks* averaged across all institutional investors holding the given firm represents our primary unscaled *CONNECTIONS* variable. To further

[1] Recent papers of this ilk include Bradley, Gokkaya, & Liu (2017), Merkley, Michaely, & Pacelli (2017), Horton, Serafeim, & Wu (2017), Bernhardt, Wan, & Xiao (2016), and Franck & Kerl (2013). Papers providing thorough reviews of research with that perspective include Ramnath, Rock, & Shane (2008a, 2008b), Bradshaw (2011) and, most recently, Bradshaw, Ertimur, & O'Brien (2017).

illustrate the construction of our *CONNECTIONS* measure, figure 1 provides an example. There we see that the stock of interest, *f1,* is held by three institutional investors. Analyst *a*1 is strongly connected, having three connections (beyond *f*1) with each of the three institutional investors holding *f*1. Hence *a1*'s *CONNECTIONS* measure is 3 [(3+3+3)/3]. On the other hand, analyst *a2* has no connections (beyond *f*1) with the institutional investors holding *f*1. Hence *a2*'s *CONNECTIONS* measure is 0 [(0+0+0)/3].

Interactions between buy-side and sell-side analysts create opportunities for the exchange of information about firms of mutual interest. Discussions about a particular firm might include topics such as the firm's strategy, growth and value drivers, risks, management quality, and of course, earnings prospects. For three reasons, we use the sell-side analyst's earnings forecast accuracy relative to other analysts covering the same firm as a proxy for the effectiveness of these discussions. First, prior research shows that information in earnings forecasts affects analysts' stock recommendations (e.g., Ertimur, Sunder, & Sunder 2007) and target price forecasts (e.g., Gleason, Johnson, & Li 2013), making earnings forecast accuracy a reasonable proxy for overall sell-side analyst research quality. Second, earnings forecasts are more prevalent than stock recommendations and target price forecasts. Third, we can measure earnings forecast accuracy more precisely than the accuracy of stock recommendations and target price forecasts. For these reasons, our summary measure of the quality of a research report for a given analyst and a given firm is the absolute difference between the firm's actual earnings and the analyst's forecast of those earnings. We label this measure *ACCURACY*.

For both *ACCURACY* and *CONNECTIONS*, as well as control variables, we hold constant the firm and year and measure the respective variable for a particular analyst relative to all other analysts following the same stock in the same year. We do this by scaling *ACCURACY* and *CONNECTIONS* to each fall between 0 and 1 for all analysts who forecast earnings for the same firm-year. This effectively avoids confounding effects of firm characteristics and time-variant macro effects likely to affect both *ACCURACY* and *CONNECTIONS*. As an example, higher institutional ownership gives rise to more accurate earnings forecasts by sell-side analysts due to a higher demand for more accurate research (see e.g., Frankel, Kothari, & Weber 2006; Ljungqvist, Marston, Starks, Wei, & Yan 2007; and Jung, Wong, & Zhang 2017). Using the extent of connections and accuracy relative to other analysts covering the same firm-year abstracts away the confounding effect of institutional ownership on forecast accuracy. Similarly, for larger firms, which are widely held by institutional investors and extensively covered by analysts, it is typically easier to accurately forecast earnings than for smaller firms. Again, by holding the firm and year constant, our tests effectively control for firm size-related effects on both variables of interest.

We must also establish controls for differences across analysts that affect both *ACCURACY* and *CONNECTIONS* within firm-years. For example, we control for differences in the number of followed firms across analysts, because analysts covering more firms naturally have more opportunities to connect with institutional investors. Since sell-side analysts tend to cover more stocks and learn more about each stock they cover as they mature in their careers (e.g., move up the ladder from junior to senior analyst status), they also have more opportunities for connections with buy-side analysts. Therefore, our examination of how much sell-side analysts learn from their connections with buy-side analysts also controls for analyst experience. Similarly, if sell-side analysts with the same number of connections with the buy-side work for entities that differ in the resources they have to offer, then the analyst with more resources would have an advantage in producing a quality research report. Therefore, we also control for differences in brokerage house size across sell-side analysts. In spite of our best efforts, we can't conceive of all accuracy-enhancing characteristics that differ across sell-side analysts. Therefore, in addition to incorporating control variables, such as coverage, experience, and brokerage house size, section 7 develops an instrumental variable that is correlated with *ACCURACY* only through its correlation with *CONNECTIONS*. Substituting the instrumental variable for *CONNECTIONS* controls for unidentified characteristics that differ among sell-side analysts covering the same firm-year.

We hypothesize and find that *ACCURACY* improves with *CONNECTIONS* until *CONNECTIONS* reaches a point of diminishing returns. This concave pattern is analogous to prior research that finds lower levels of earnings forecast accuracy among sell-side analysts who cover large numbers of firms. It is also consistent with Maber, Groysberg, & Healy 2015, who show that increasing high-touch services with institutional clients comes with opportunity costs limiting the time sell-side analysts spend on other accuracy-enhancing aspects of their research.

Sensitivity tests provide assurance that the above results are robust to two alternative measures of *CONNECTIONS*. The primary *CONNECTIONS* variable captures the breadth *and* depth of an analyst's connections with buy-side analysts regarding a particular stock. Breadth is reflected in the number of institutional investors with which the analyst has stocks in common other than the stock of interest, and depth is reflected in the number of these other stocks. The first alternative measure gives depth *less* importance by simply counting the number of different institutional investors (different perspectives/information sets) with which an analyst is connected through stocks other than the stock of interest. The second alternative measure gives depth *more* importance by only counting connections with an institution where the total market value of all firms followed by the analyst account for more than 5% of the total value of the institution's investment portfolio.

From evidence consistent with the hypothesized non-linear relation between *ACCURACY* and *CONNECTIONS*, we infer that information from connections with buy-side analysts informs sell-side analyst research. However, the relation between *ACCURACY* and *CONNECTIONS* could be endogenous in that buy-side analysts select sell-side analysts who can provide insights that inform the buy-side analysts' research reports, and that selection probably favors sell-side analysts who have already proven themselves in ways that might include forecast accuracy. To support our inferences, we need to address this endogeneity issue.

We address endogeneity issues in five ways. First, we examine whether the relation between *ACCURACY* and *CONNECTIONS* is stronger for buy-side analysts with *more* private information or for buy-side analysts with *less* private information. The former scenario validates the inference that sell-side analysts obtain forecast accuracy-enhancing information from buy-side analysts and the latter supports the inference that buy-side analyst selection of already-accurate sell-side analysts drives the relation. Our proxy for the degree to which buy-side analysts possess private information is inversely related to the degree to which institutional investors rely on sell-side analyst recommendations to support their trading decisions (Kacperczyk & Seru 2007). Our results support the former scenario and mitigate the endogeneity concern.

Second, we examine the implications of an exogenous shock hypothesized to increase sell-side analyst demand for information from buy-side analysts. The shock we exploit appears during 2001-2003 when: (i) Regulation Fair Disclosure (Reg FD) came into effect constraining both sell-side and buy-side analyst ability to obtain private information from management of the firms they follow; and (ii) the Global Analyst Research Settlement (GARS) further constrained sell-side analysts' ability to obtain private information from investment bankers in their own firms. If buy-side selection of accurate sell-side analysts drives the relation between *ACCURACY* and *CONNECTIONS*, we see no reason for the relation to change in the wake of the 2001-2003 regulatory period. In fact, Reg FD potentially reduces sell-side analysts' ability to provide buy-side analysts with access to private information held by management of the covered firm, a key reason for buy-side interest in connecting with sell-side analysts (Brown, Call, Clement, & Sharp 2016), and GARS creates incentives for sell-side analysts to rely more heavily on sources of information outside their own brokerage houses. Both regulations increase the relative importance of buy-side analysts as a source of private information. Consistent with our main hypothesis that connections with buy-side analysts enhance sell-side analyst earnings forecast accuracy, we find an insignificant relation between *ACCURACY* and *CONNECTIONS* in the pre-2001 period and a significantly positive relation in the post-2003 period when sell-side analysts have more reason to seek private information from buy-side analysts.

Third, we examine factors hypothesized to increase buy-side analyst demand for connections with sell-side analysts (i.e., past sell-side forecast accuracy and sell-side analyst prior experience following the covered firm). We find no support for the inference that increased buy-side analyst demand strengthens the relation between *ACCURACY* and *CONNECTIONS*. Fourth, we find that our main finding is robust in a subsample of analysts with less than four years of firm-specific experience. In those cases, the buy-side analyst has very little basis for judging the accuracy track record of the sell-side analyst with whom s/he chooses to work. Finally, as described above, we acknowledge the potential for factors beyond those identified and controlled in our primary analyses to affect both *ACCURACY* and *CONNECTIONS*. Results are robust to our instrumental variables analysis that controls for these other factors.

This paper contributes to the literature in three important ways. First, we open the door to a new avenue of research that can investigate the role of the bilateral flow of information between sell- and buy-side analysts in increasing the quality of information impounded in capital asset prices. Second, by furthering our understanding of the role that buy-side analysts play in financial markets, our paper contributes to the nascent literature that studies buy-side analysts (e.g., Jung, et al. 2017, Brown, et al. 2016, Cici & Rosenfeld 2016, Rebello & Wei 2014). We identify a channel through which buy-side analysts' private information flows to the stock market. Finally, we contribute to the stream of research investigating the factors affecting sell-side analyst earnings forecast accuracy. Prior research essentially identifies three factors: (i) incentives (e.g., Mikhail, Walther, & Willis 1999), (ii) ability (e.g., Jacob, Lys, & Neale 1999; Clement 1999), and (iii) resources (e.g., Jacob, Rock, & Weber 2008). We introduce a fourth factor; i.e., accuracy-enhancing information obtained through interactions with buy-side analysts.

The rest of this paper is organized as follows. The next section describes the institutional setting. Section 3 reviews the literature and presents our hypotheses. Section 4 discusses our research design, and Section 5 describes our sample selection procedure. Section 6 presents the results of our hypotheses tests. Section 7 presents results of additional analyses, and Section 8 concludes.

## 2. Institutional Setting

Buy-side analysts provide advice to portfolio managers working for entities that pool resources of individual investors and invest on their behalf. These entities house investment vehicles such as mutual funds, pension funds, insurance companies, and hedge funds; e.g., Fidelity Investments, General Motors, Progressive Auto Insurance, and Bridgewater Associates. We refer to each of these entities as an institutional investor and each institutional investor employs buy-side analysts, from whom it seeks investment advice. Each buy-side analyst, in turn, interacts with one or more sell-side analysts. The majority of sell-side analysts work for full-service investment banks, such as Goldman Sachs, JP Morgan Chase, and Bank of America Merrill Lynch (Cowen, Groysberg, & Healy 2006).

The literature has extensively documented that sell-side analysts provide information to the buy-side (Ramnath, Rock, & Shane 2008a, 2008b; Bradshaw et al. 2017). For example, Brown, Call, Clement, & Sharp (2017) surveyed investment relations professionals and discovered "that some buy-side analysts privately send questions or comments to sell-side analysts during the Q&A portion of the public earnings conference call (p. 36)." Nonetheless, academic and anecdotal evidence suggests that buy-side analysts generate information incremental to the information developed by sell-side analysts. Direct academic evidence comes from Rebello & Wei (2014), who conclude that "…buy-side analysts produce research that is very different from sell-side research…(p. 777)." They find that the opinions of buy-side analysts, as measured by their stock ratings, differ from the opinions of typical sell side analysts and that trading strategies utilizing information contained in those opinions can generate significant risk-adjusted returns over the next year. Bushee, Jung, & Miller (2017) document that trade sizes around investor-management meeting times increase and abnormal net buys around the meetings are profitable during thirty days subsequent to the private access day. They conclude that the private access to management provides information that changes institutional investors' beliefs and trading. Such beliefs-changing information, which is unlikely to be in the information set of sell-side analysts could be "mosaic" but, nonetheless, valuable in combination with investors' private information and does not violate "Reg FD" (Solomon & Soltes, 2015).

Supporting anecdotal evidence suggests that buy-side analysts often get preferential access to the management of public companies and this provides an advantage in efforts to generate precise information. For example, during a June 22, 2016 conference call announcing the $2.8 billion acquisition of SolarCity, Tesla's CEO, Elon Musk acknowledged that, over the years in private discussions with institutional shareholders, he "bandied about" the idea of combining Tesla Motors with SolarCity (Reuters 2016). The article also suggests that at least one institutional investor, a Fidelity portfolio manager, benefited from trading on foreknowledge of the merger. In another article, David Strasser, a former sell-side analyst at Janney Montgomery Scott LLC, stated that in the meetings he arranged between institutional investors and the companies he followed, he "was sometimes asked to sit outside the room so investors could ask questions without him" (Ng & Gryta 2017).[2]

Although buy-side analysts' research is not publicly available, at least two factors make sell-side analysts privy to some part of this information. First, learning what other buy-side analysts think and sharing that with institutional clients is implicitly expected of sell-side analysts. Brown, et al. (2016) surveyed and interviewed buy-side analysts who indicated that their demand for sell-side analyst services depends, primarily, on: (i) the ability of sell-side analysts to facilitate meaningful one-on-one interaction with CFOs and other knowledgeable executives working for the firms with significant representation in their institution's portfolios; (ii) the quality of sell-side analysts' industry-related research; and (iii) insights sell-side analysts

[2] Holding constant the effects of their interaction with each other, further reason to believe buy-side analysts have information incremental to the information developed by sell-side analysts is provided in: Martin (2005); Abramowitz (2006); Retkwa (2009); Frey & Herbst (2014); Jung, et al. (2017); and Groysberg, Healy, Serafeim & Shanthikumar (2013).

provide into the perspective of buy-side analysts working for other institutional investors. Institutional investors could, and increasingly do, internalize the first two services, but they must outsource the third service, which incentivizes sell-side analysts to discover their buy-side analyst clients' perspectives on the firms the sell-side analysts follow.[3]

Second, sell-side analysts have many opportunities to learn from buy-side analysts. The lion share of a typical sell-side analyst's compensation is driven by broker votes, which are in turn driven by personalized services that sell-side analysts provide for institutional clients including high-touch meetings, phone calls, whitepapers, and concierge services that put buy-side analysts in touch with the management of firms of interest (Maber, et al. 2015). Thus, sell-side analysts have a strong incentive to provide high-touch services, which necessitate regular communication with current or potential institutional investor clients. Based on a sample of sell-side analysts at a mid-size investment bank, Maber et al. (2015) document that the average sell-side analyst holds approximately 750 private calls and 45 one-on-one meetings with client investors in the course of a typical semiannual period. From the perspective of the buy-side, when Brown et al. (2016) asked buy-side analysts how often they have private communication with sell-side analysts, 55% of their respondents said more than 23 times per year and only 4% said "never." These communications provide sell-side analysts with opportunities to uncover and put together various pieces of information produced by institutional investors. For example, Stephen Byrd, a managing director of research at Morgan Stanley told us "when I speak to a variety of institutional clients, I get a strong sense for the investor debates that really matter for a

[3] This differs from information spillovers documented in other studies. For example, Hameed, Morck, Shen, & Yeung (2015) find that sell-side analysts follow stocks whose fundamentals have the greatest correlation with those of other firms in the industry. The information developed about these "bellweather" firm stocks benefits investors in less closely followed stocks. In another study, Muslu, Rebello, & Xu (2014) find that analysts contribute to stock comovement by developing value-relevant information common to the firms in their portfolio of followed firms.

stock. That helps me to understand what catalysts are likely to move a stock. We all have access to the same information, though sometimes our clients track certain catalysts more closely than we do (whereas sometimes we are closer to a particular catalyst).”[4]

We argue that the regular communications with their institutional clients provide sell-side analysts with a window into the private information generated by their institutional clients about companies of common interest. Specifically, as both parties engage in conversations, the questions raised and the requests for clarifications made by the institutional clients tip off sell-side analysts about the private information of their institutional clients. In this regard, Groysberg, Healy, & Chapman (2008) speculate that “sell-side analysts may develop an information advantage through feedback on their ideas from their own institutional clients (p. 33).” That sell-side analysts discern the private information of their institutional clients in the course of such communications is supported by the fact that many buy-side analysts view the knowledge that sell-side analysts have of other buy-side analysts’ opinions as a valuable service provided by the sell-side (Brown et al. 2016). Furthermore, the results of the Brown et al. interviews suggest that buy-side analysts value their relationships with sell-side analysts, because “they are the only portal” into the thinking of buy-side analysts working for other institutions. Quoting one of their interviewees, “The buy side is this whole poker game of, ‘I don't want to show my cards, but I want to see your cards.’ The only people that can actually see everyone's cards is the sell side. When we ask them questions, they can figure out what we're thinking.”

[4] Also, Greg Melich, a partner and senior analyst at MoffettNathanson told us that in the course of a typical interaction with an institutional client he might be alerted of a new piece of public information of which he was not aware. For example, the institutional client might have just learned that a certain company became a supplier of Target Corporation and pass that information along.

Given the information environment described above, the next section develops hypotheses concerning the relation between the quality of a sell-side analyst research report and the degree of connectedness between the sell-side analyst and the buy-side analysts she serves.

## 3. Hypotheses and Literature Review

### ***Main hypothesis***

Section 2 suggests that sell-side analysts have strong incentives to interact with buy-side analysts and refers to previous research and anecdotal evidence that those interactions are indeed highly frequent. Presumably, the more connections an analyst has with institutional investors, the greater the likelihood of communications with these investors and the greater the opportunity for the sell-side analyst to learn and decipher the private information possessed by institutional investors and improve their forecast accuracy. Thus, we expect sell-side analyst forecast *ACCURACY* to be positively correlated with *CONNECTIONS* with institutional investors.

On the other hand, it is costly for analysts to spread themselves too thinly. For example, there appears to be a cost associated with following too many firms (Clement 1999; Jacob, et al. 1999; Myring & Wrege 2011; Pelletier 2015). We expect that for each sell-side analyst there is a cost associated with providing the services associated with too many connections. Too many connections with buy-side analysts are likely to come with an opportunity cost that outweighs the benefit of other sell-side analyst activities, such as independent research, nurturing relationships with the buy-side analysts who matter most, connecting with management of the firms they follow, and writing whitepapers and research reports. This is consistent with Maber, et al. (2015) who show that increases in analysts' time-consuming services for their institutional clients result in less published research output. Thus, we expect the positive impact of *CONNECTIONS* on

*ACCURACY* to exhibit diminishing returns as the number of interactions with different buy-side analysts increases. In light of this reasoning, we hypothesize the following relation:

> **H1:** *ACCURACY* increases with *CONNECTIONS* up to some point where the rate of increase subsides.

***Additional hypotheses***

Given evidence of the relation hypothesized in H1, we test additional hypotheses that identify factors expected to strengthen the relation between *ACCURACY* to *CONNECTIONS*. We develop these additional hypotheses to provide more confidence in the validity of the relation we observe in tests of H1 and to address the endogeneity issue discussed in Section 1.[5] That is, the tests are designed to sort out whether the relation observed in tests of H1 emerges from sell-side analysts obtaining accuracy-enhancing information from buy-side analysts, or from buy-side analysts seeking connections with already-accurate sell-side analysts.

We predict greater sensitivity of *ACCURACY* to *CONNECTIONS* in situations where sell-side analysts have more opportunities to learn from their buy-side analyst counterparts, which would arise when sell-side analysts are connected with certain buy-side analysts who produce relatively large amounts of private information. If, on the other hand, *ACCURACY* drives *CONNECTIONS* because buy-side analysts have more need for information from sell-side analysts, then we expect greater sensitivity of *ACCURACY* to *CONNECTIONS* in situations where sell-side analysts have *less* opportunities to learn from their buy-side analyst counterparts. Such situations arise when sell-side analysts are connected with certain buy-side analysts who produce relatively *small* amounts of private information. This discussion leads to our second hypothesis:

[5] Note that endogenous selection of more accurate sell-side analysts cannot explain a weakened relation between *ACCURACY* and *CONNECTIONS* beyond a certain level of *CONNECTIONS*.

**H2:** The sensitivity of *ACCURACY* to *CONNECTIONS* increases with the opportunity for sell-side analysts to learn from buy-side analysts.

To identify institutional investors that produce more versus less private information, we draw on previous research. In particular, we rely on Kacperczyk & Seru (2007), who document that institutions less reliant on public information produce more private information, which results in superior investment performance relative to institutions that rely more on public information. As Section 4 describes in more detail, the Kacperczyk & Seru (2007) measure of reliance on public information captures the extent to which institutional investor's trading decisions respond to changes in information in the public domain as proxied by changes in consensus sell-side analyst stock recommendations.

We next examine how the relation examined in H1 differs after exogenous regulatory changes strengthen sell-side analyst incentives to obtain private information about the firms they follow. We focus on two related regulatory changes. Since October 2000, Reg. FD has prohibited transmission of private information to sell-side analysts from management of the firms they follow, and since April 2003, the Global Analyst Research Settlement (GARS) between the SEC and the 10 largest investment banks, presumably employing the largest number of sell-side analysts, has prohibited sell-side analysts from discussing the prospects of the firms they cover with the investment bankers within their own firms. For two reasons, we expect stronger sell-side analyst incentives to obtain private information after these regulations. On one hand, sell-side analysts were cut off from private information previously obtained from management of the firms they followed and investment bankers within their own firms. On the other hand, sell-side analysts came under increased pressure to generate commission revenue for their firms. Given that buy-side analysts represent a potential source of private information, if sell-side demand for such information drives sell-side analysts' forecast accuracy, we expect the relation described in H3 below:

> **H3:** The sensitivity of *ACCURACY* to *CONNECTIONS* increases in the wake of Reg FD and GARS.

If the relation we observe in tests of H1 above emanates from buy-side demand for connections with already-accurate sell-side analysts, then we do not expect H3 to hold. Neither Reg FD nor GARS affected buy-side analyst discussions with fund managers within their own firms, but these regulations did affect discussions between sell-side analysts and investment bankers within their firms. Furthermore, Reg FD removed one of the buy side's benefits from interacting with sell-side analysts; i.e., obtaining firm-specific private information from the followed firm's management.

Finally, we turn to the possibility that buy-side analysts' demand for information from sell-side analysts could drive the relation between *ACCURACY* and *CONNECTIONS*. In that respect, we hypothesize that:

> **H4:** The sensitivity of *ACCURACY* to *CONNECTIONS* increases with increases in buy-side analyst demand for connections with sell-side analysts.

We expect higher demand of buy-side analysts for connections with sell-side analysts predicted to be more accurate. We focus on two factors documented by prior research to have strong relations with sell-side analyst forecast accuracy: (i) past accuracy (Brown 2001); and (ii) firm-specific experience (Clement 1999). In addition, in response to the Brown, et al. (2016) survey, buy-side analysts rate the sell-side analyst's firm-specific experience as the most important attribute affecting the decision to use information provided by the sell-side analyst. In fact, this attribute is rated as more important than how often the sell-side analyst speaks with firm management, and whether the sell-side analyst is a member of the Institutional Investor All-

American Research Team. Thus, if buy-side demand drives the relation between *ACCURACY* and *CONNECTIONS*, then we expect to find evidence supporting H4; i.e., we expect the relation to strengthen with sell-side analyst past accuracy and firm-specific experience.

## 4. Research design

### *4.1 Measurement of CONNECTIONS*

As displayed in figure 1, our primary analysis assumes that an analyst (denoted *a* below) learns more about a firm (*f*) from an institutional investor (*i*) in year *t* as the number of *other* stocks followed by *a* and held by *i* ($CONNECTIONS\#_{aft}^{i}$) increases. The larger the number of these *other* stocks held by *i* and covered by *a*, the more opportunities *a* and *i* have to interact and in the process also discuss *f*.[6] Thus, we let each of these *other* stocks held by *i* and followed by *a* proxy for a connection around *f* between *a* and *i* during year *t*. The average number of connections, across all institutions holding *f*, is our variable of interest, $CONNECTIONS_{aft}$, i.e., $\frac{\sum_i CONNECTIONS\#_{aft}^{i}}{INST\_OWNER\#_{ft}}$, where $INST\_OWNER\#_{ft}$ is the number of institutions holding *f*.

We expect that, for each firm followed by *a* in year *t*, the more connections an analyst has with institutional investors, on average, the greater the breadth and depth of dialogue between *a* and institutional investors holding *f* and the greater the opportunity for the sell-side analyst to decipher the private information possessed by institutional investors.

This holdings-based approach to measuring *CONNECTIONS* avoids the confounding effect of institutional ownership on forecast accuracy (Frankel, et al. 2006; Ljungqvist, et al.

[6] Institutional holdings of *f* and other stocks used to construct $CONNECTIONS\#_{aft}^{i}$ are from the calendar quarter preceding analyst *a*'s forecast of firm *f*'s year *t* earnings and analyst coverage of these companies is from the one-year period preceding the calendar quarter end used to measure institutional holdings.

2007). The reason is that our measure of *CONNECTIONS* regarding a stock does not relate to institutional holding of the stock. For analysts that follow the same company *f*, institutional ownership of *f* is the same and thus, the variation in their *CONNECTIONS* with the institutional investors does not relate to institutional ownership. As we describe in detail in Section 4.2, we further scale *CONNECTIONS* within the same firm-year to effectively control for both time-variant and firm-invariant characteristics.

### *4.2 Models for testing H1*

If analysts produce more accurate forecasts due to the private information they collect from their connections with institutional investors, we expect $\beta_1 > 0$ in model (1) below. In addition, if analysts face diminished returns beyond some level of connections with institutional investors, we expect $\beta_2 < 0$.

$$ACCURACY_{aft} = \beta_0 + \beta_1 CONNECTIONS_{aft} + \beta_2 CONNECTIONS_{aft}^2 + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (1)$$

Where:
$F_{aft}$ = analyst *a*'s forecast of firm *f*'s year *t* earnings during the first 90 days following *f*'s year *t*-1 earnings announcement.

$A_{ft}$ = firm *f*'s I/B/E/S-provided actual year *t* earnings.

$FE_{aft} = F_{aft} - A_{ft}$ = error in analyst *a*'s earnings forecast $F_{aft}$.

$\max(|FE_{ft}|)$ = the maximum absolute forecast error among all analysts issuing forecasts of firm *f*'s year *t* earnings during the first 90 days following the firm's year *t*-1 earnings announcement.

$\min(|FE_{ft}|)$ = the minimum absolute forecast error from the distribution generating $\max(|FE_{ft}|)$.

$ACCURACY_{aft}$ = forecast accuracy measured as $\frac{\max(|FE_{ft}|)-|FE_{aft}|}{\max(|FE_{ft}|)-\min(|FE_{ft}|)}$, i.e., absolute error of analyst *a*'s forecast $F_{aft}$ scaled to fall between 0 and 1, with 1 (0) indicating the most (least) accurate forecast.

Variables which control for factors that could affect forecast accuracy include:

$ACCURACY_{af,t-1}$=lagged value of the forecast accuracy measure defined above.

$FIRM\#_{t}^{a}$ = number of firms analyst *a* followed in the year ending with the date of $F_{aft}$.

$INDUSTRY\#_{at}$ = number of industries based on two-digit SIC codes analyst *a* followed in the year ending with the date of $F_{aft}$.

$FIRM\_EXP_{aft}$ = number of years since the first year analyst *a* issued one-year ahead earnings forecasts for firm *f* up to the date of $F_{aft}$.

$BSIZE_{at}$ = number of analysts employed by analyst *a*'s brokerage house or research firm in the year ending with the date of $F_{aft}$.

$DAYS_{aft}$ = number of days between the date of $F_{aft}$ and the most recent one-year ahead forecast of firm *f*'s year *t* earnings preceding $F_{aft}$ by analyst *a*.

$EPS\_FREQ_{aft}$ = frequency of analyst *a*'s one-year ahead earnings forecasts for firm *f* in the one-year period prior to the date of $F_{aft}$.

$HORIZON_{aft}$ = number of days between the date of $F_{aft}$ and the end of fiscal year *t*.

$CONNECTIONS_{aft}$ and all control variables except $ACCURACY_{af,t-1}$ are scaled to fall between 0 and 1 based on the equation below:

$$Scaled\ Variable_{aft} = \frac{|Variable_{aft}| - \min(|Variable_{ft}|)}{\max(|Variable_{ft}|) - \min(|Variable_{ft}|)}$$

By scaling all dependent and independent variables among analysts following the same firm in the same year, we effectively control for all firm characteristics and time-variant macro factors that affect forecast accuracy. Scaling all variables in this manner maintains the relative values of each variable, while allowing comparison across regression coefficients (Clement & Tse, 2005).

To test H1, we also employ a piecewise regression in model (2) below, which allows us to calculate the sensitivity of *ACCURACY* to *CONNECTIONS* in each *CONNECTIONS* tercile.[7]

[7] Tercile cut-off points are derived from the distribution of scaled $CONNECTIONS_{aft}$.

$$ACCURACY_{aft} = \beta_0 + \sum_{k=1}^{3} \beta_k^{Tercile} D_k^{Tercile} CONNECTION_{aft} + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (2)$$

where $D_k^{Tercile}$ is an indicator variable equaling one for the k^th^ $CONNECTIONS$ tercile (1=lowest and 3=highest) and zero otherwise. Under H1, we expect $\beta_1^{Tercile} > \beta_3^{Tercile}$ .

*4.3 Model for testing H2*

If the sensitivity of *ACCURACY* to *CONNECTIONS* increases when buy-side analysts produce greater amounts of private information, we expect $\beta_1 > \beta_5$ in model (3) below. On the other hand, if buy-side analyst selection of already-accurate sell-side analysts drives the relation between *ACCURACY* and *CONNECTIONS* then we expect the sensitivity of *ACCURACY* to *CONNECTIONS* to decrease when buy-side analysts produce more of their own private information, and we expect $\beta_1 < \beta_5$.

$$\begin{aligned} ACCURACY_{aft} = \beta_0 &+ \beta_1 CONNECTIONS_{aft}^{High\ Opp} + \beta_2 (CONNECTIONS_{aft}^{High\ Opp})^2 \\ &+ \beta_3 CONNECTIONS_{aft}^{Med\ Opp} + \beta_4 (CONNECTIONS_{aft}^{Med\ Opp})^2 \\ &+ \beta_5 CONNECTIONS_{aft}^{Low\ Opp} + \beta_6 (CONNECTIONS_{aft}^{Low\ Opp})^2 \\ &+ \sum_m \beta_m Control_m + \varepsilon_{aft} \end{aligned} \quad (3)$$

where $CONNECTIONS_{aft}^{High\ Opp}$ is constructed by:

$$CONNECTIONS_{aft}^{High\ Opp} = \frac{\sum_i CONNECTIONS\#_{aft}^{i} \times D_{High\ Opp}^{i}}{INST\_OWNER\#_{ft}}$$

where $D_{High\ OPP}^{i}$ denotes an institutional investor from which sell-side analysts have more opportunities to acquire useful information. $INST\_OWNER\#_{ft}$ denotes the number of institutional investors holding stock *f* at time *t*. $CONNECTIONS_{aft}^{Med\ Opp}$ and $CONNECTIONS_{aft}^{Low\ Opp}$ are constructed in a similar fashion for medium and low opportunity institutional investors. Note that normalizing the high/medium/low opportunity connections variables by $INST\_OWNER\#_{ft}$ ensures that they add up to $CONNECTIONS_{aft}$. Similar to *CONNECTIONS*, we scale $CONNECTIONS^{High\ Opp}$, $CONNECTIONS^{Med\ Opp}$ and $CONNECTIONS^{Low\ Opp}$ to fall between 0 and 1 among analysts following the same firm in the same year.

As described when Section 3 introduced H3, our approach to identifying high, medium, and low opportunity institutional investors depends on an institution's reliance on public information (RPI). Following Kacperczyk & Seru (2007), we construct RPI for each institution in each calendar quarter, as the $R^2$ from an institution-level regression of changes in the number of shares held in a given stock by a given institution on four lags of changes in mean analyst recommendations. All institutional investors are ranked each quarter by their RPI into terciles. Institutions in the lowest (highest) RPI tercile are considered as relying the least (most) on public information and classified as high- (low-) opportunity institutions.

*4.4 Model for testing H3 and H4*

To test whether the sensitivity of forecast accuracy to connections increases with sell-side analyst demand for information from the buy side or with buy-side analyst demand for information from the sell-side, we employ the following regression model.

$$\begin{aligned} ACCURACY_{aft} \\ &= \beta_0 + \beta_1 CONNECTIONS_{aft} + \beta_2 CONNECTIONS^2_{aft} \\ &+ \beta_3 DEMAND + \beta_4 DEMAND \times CONNECTIONS_{aft} \\ &+ \beta_5 DEMAND \times CONNECTIONS^2_{aft} + \sum_m \beta_m Control_m + \varepsilon_{aft} \qquad (4) \end{aligned}$$

Where *DEMAND* represents one of three variables, *POST_GS*, *FIRM_EXP* and lagged *ACCURACY*. *POST_GS* reflects demand by sell-side analysts for information from the buy-side analysts with whom they are connected, and the other two variables reflect buy-side's information demand. Higher values indicate higher demand. *POST_GS* equals 1 for forecasts issued after 2003, and 0 for forecasts issued before 2001.[8] *FIRM_EXP* indicates firm-specific forecasting experience and lagged *ACCURACY* equals the forecast accuracy measure for the prior year, both of which are as defined in model (1).

If sell-side analyst demand drives the relation between *ACCURACY* and *CONNECTIONS* in model (4) above, then we expect $\beta_4 > 0$ when the demand proxy is *POST_GS*. Similarly, if buy-side demand drives the relation between *ACCURACY* and *CONNECTIONS*, then we expect $\beta_4 > 0$ when the demand proxy is either *FIRM_EXP* or lagged *ACCURACY*.

## 5 Sample selection

Our sample contains 137,835 analyst-firm-year observations from 1995 to 2012, including 3,980 unique firms and 7,615 unique analysts. We employ the following sample construction steps. From I/B/E/S we collect one-year ahead EPS forecasts issued during the first 90 days following the prior year's earnings announcement, and consensus analyst recommendations issued during the year. If an analyst issues more than one forecast for the same firm-year during the 90-day window, we keep only the earliest one. In the latest calendar quarter prior to the 90-day window for each firm-year described above, we collect the number of institutional investors and their holdings for the construction of *CONNECTIONS* and institutional investors' reliance on public information (*RPI*) from the Thomson Reuters 13F database. We exclude analyst-firm-years missing any of the analyst characteristic control

[8] We exclude the regulatory period years, 2001-2003, from this analysis. Regulation Fair Disclosure was promulgated in October of 2000, the Sarbanes Oxley Act took effect in July of 2002, and the Global Analyst Research Settlement occurred in April of 2003.

variables, such as the lagged forecast error. Finally, we require each firm-year be covered by more than one analyst during the 90-day window. Our sample period begins in 1995, the first year when we can construct institutions' reliance on public information (RPI). RPI requires four lags of changes in mean recommendations, I/B/E/S' coverage of which starts in the last quarter of 1993. Our sample period ends in 2012 due to a warning from WRDS documenting severe coverage problems with Thomson Reuters 13F database after 2012. Nonetheless, in unreported supplemental tests, we extend the sample period to 2015 and our main results and inferences remain robust.

## 6 Hypotheses Test Results

### *6.1 Descriptive statistics*

Table 1 Panel A presents descriptive statistics for variables in our models, along with some variable components. For ease of interpretation, with the exception of *ACCURACY*, no variable is scaled among analysts for the same firm-year. Panel A shows that the distribution of absolute analyst-firm-year forecast error, $|FE|$, has a mean (median) of 0.795 (0.146). The *ACCURACY* variable used in our hypotheses tests scales $|FE|$ to fall in a range from 0 to 1. The mean (median) of *ACCURACY* is 0.528 (0.550).

The *CONNECTIONS* variable indicates that, on average, analysts have 6.7 connections per institution holding a given stock followed by the analyst. This means that, on average, an institution holding a given stock followed by an analyst also holds, on average, 6.7 other stocks followed by that analyst, while the average analyst follows 16.7 firms. We attribute this seemingly high overlap between analyst coverage and institutional holding to the relatively broad based and diversified portfolios of stocks held by the average institutional investor. In fact, the mean (median) number of institutional investors holding a particular stock is 293 (213).

Panel A also shows that, in an average analyst-firm-year, a given analyst follows stocks in 3.8 different industries, has about 5 years of experience forecasting earnings of the firm that she covers, works for a brokerage house or research firm employing 67 analysts, issues forecasts 4.6 days after the most recent forecast by any analyst following the same firm, has issued 6 one-year ahead earnings forecasts in the year prior to the current forecast for the same firm, and has a forecast horizon until the end of the fiscal year averaging 310 days. Panel A also shows the averages of the number of connections that the average analyst has with institutions in the three RPI categories proxying for the opportunity to glean useful information from institutional investors.

Table 1 Panel B presents the univariate correlations among the variables used to test our hypotheses, where all variables are scaled among analysts following the same firm-year. Consistent with prior literature, our measure of relative within firm-year *ACCURACY* is significantly positively correlated with the prior year's *ACCURACY*, the analyst's firm-specific experience, and brokerage house size; and *ACCURACY* is negatively correlated with the number of days since the most recent preceding analyst forecast, forecast frequency, number of industries followed, and the horizon between the forecast and the upcoming annual earnings announcement date. *ACCURACY* is not significantly correlated with *CONNECTIONS*, which is consistent with a non-linear relation requiring a quadratic term in our multiple regression analysis. Both *CONNECTIONS* and analyst forecast *ACCURACY* are significantly greater after the global settlement.

*6.2 Test of H1*

Table 2 displays the results of our test of H1, which predicts that the accuracy of an analyst's forecast of a firm's earnings improves, to a point of diminishing returns, with the degree of connectedness between the analyst and institutional investors who hold the firm's stock in their portfolios. For ease of presenting all regression results, the dependent variable (and, thus, each coefficient) is multiplied by 100. Results estimated from both the quadratic forms in columns (1) and (2) and the piecewise regressions in columns (3) and (4), excluding or including control variables, support H1.

In columns (1) and (2), the coefficient on *CONNECTIONS* is significantly positive and the coefficient on the square of *CONNECTIONS* is significantly negative. The p-values being less than 0.01 and the magnitudes of the coefficients suggest that they are statistically, as well as economically, significant. The economic significance is, perhaps, more apparent in column (4) where we perform a piecewise estimation of *CONNECTIONS* by terciles. The coefficient on *CONNECTIONS* in the lowest tercile of the variable is 9.464, which means that a one-standard deviation change in *CONNECTIONS* (0.345, untabulated) is associated with a 3.265% (0.345x9.464) change in the *ACCURACY* dependent variable. That represents 6.2% of mean *ACCURACY* (0.0327/0.528). The less significant coefficient on *CONNECTIONS* in the second tercile and the insignificant coefficient in the highest tercile indicate that, once the analyst's average amount of connections per institution becomes too large, diminishing returns to additional connections become apparent. The coefficient on *CONNECTIONS* in the lowest terciles is significantly larger than that in the highest terciles, with a p-value (untabulated) of less than 0.001.

With reference to the control variables in model (1), the results in Table 2 indicate that *ACCURACY* significantly improves with the number of firms the analyst covers during the year prior to the forecast date and with the prior year's *ACCURACY* (i.e., the *ACCURACY* variable is relatively stable from one year to the next). The coefficient on the number of firms covered is opposite to results in prior research but should be interpreted with caution because of the mechanically very high univariate correlation between *CONNECTIONS* and coverage.[9] *ACCURACY* significantly declines with the number of industries the analyst follows, the number of days between the forecast date and the date of the most recent prior forecast, and the number of days between the forecast date and the end of the firm's fiscal year. These results are consistent with prior literature.

*6.3 Test of H2*

Panel A of Table 3 displays the results of testing H2, which predicts that the benefits to sell-side analysts of connections with buy-side analysts intensify when the buy-side analysts' institutional investment firms have more private information; i.e., when greater opportunities for sell-side analysts to learn from buy-side analysts exist. As described above, our proxy for opportunity is the institutional investor's RPI, measured in the manner described in Kacperczyk & Seru, (2007). We divide the observations into three terciles (high, medium, and low opportunity) and estimate the coefficients of *CONNECTIONS* and its quadratic terms within each tercile. We predict that connections with high opportunity institutions generate greater rewards in terms of improved forecast accuracy than connections with medium and low opportunity institutions.

[9] When excluding *CONNECTIONS* and its squared term, the coefficient on the number of firms covered becomes insignificant.

The results in Panel A of Table 3 are consistent with our prediction; i.e., the strength of the relation between *ACCURACY* and *CONNECTIONS* increases only when analysts are connected with high opportunity institutions. The difference between the coefficient on connections with highest opportunity institutions and that on connections with lowest opportunity institutions is significant at a p-value of 0.052 (untabulated). The results also support our primary hypothesis that connections with buy-side analysts inform sell-side analyst research and improve sell-side analyst forecast accuracy. Furthermore, these results relieve the endogeneity concern that institutions choose to connect with already-accurate analysts. If this was happening, *low opportunity* (i.e., high RPI) institutions would be more likely to have such a preference given that they produce less of their own information and instead rely on public sources of information such as the research output produced by analysts. Such a preference suggests a stronger relation between connections with *low opportunity* institutions and *ACCURACY,* which is opposite to what we find.

In Panel B of Table 3, we provide an assessment of economic significance for our main effect by estimating Models (1) and (2) for the subsample of observations having stronger connections with low *RPI* institutions. For this purpose, we retain only observations whose $CONNECTIONS^{High\ Opp}$ is in the bottom tercile of the entire sample. Because Panel A of Table 3 indicates that the results concentrate in low RPI institutions and the relation between connections and accuracy is nonlinear, we expect larger than average economic significance for this subsample. For this subsample, the untabulated mean, median and standard deviation of *CONNECTIONS* with low *RPI* institutions ($CONNECTIONS^{High\ Opp}$) are 0.517, 0.559, and 0.423, respectively. We evaluate the nonlinear relation between *CONNECTIONS* and *ACCURACY* within this subsample using both the quadratic and piecewise regressions. From Column 2, the coefficient on $CONNECTIONS^{High\ Opp}$ in the lowest tercile of the variable is 38.567. This means that a one-standard deviation change in $CONNECTIONS^{HighOpp}$ is associated with a 16.31% (0.423×38.567) change in the *ACCURACY* dependent variable, representing 30.9% of mean *ACCURACY* (0.1631/0.528).

*6.4 Test of H3*

Table 4 displays the results of our test of H3, which predicts that more demand for information by sell-side analysts increases the impact of *CONNECTIONS* on *ACCURACY*. The sell-side demand proxy is a dummy variable indicating whether the analyst-firm-year observation occurs after 2003; i.e., after Reg. FD and GARS constrained sell-side analysts' private communications with investment bankers in their own firms and with management of the firms the analysts follow. We hypothesize that sell-side analysts' information demand is higher after these regulation changes. Observations during the years of the changing regulatory environment (2001-2003) are omitted from this analysis.

Table 4 shows that the interaction of *POST_GS* and *CONNECTIONS* is positive and significant, supporting increased impact of connections with buy-side analysts on sell-side analysts' forecast accuracy after the regulatory shocks. In fact, the coefficients suggest that the post-regulation observations drive the results discussed in relation to Table 2 above.[10] Overall, this result suggests that increased sell-side demand for information from buy-side analysts intensifies the importance of connections that inform sell-side analyst research and improve forecast accuracy.

[10] In tables 4 and 5, in the interest of brevity we report results only based on the quadratic specification.

*6.5 Test of H4*

Table 5 displays results from tests of H4, which predicts that more demand for information by buy-side analysts strengthens the relation between *CONNECTIONS* and *ACCURACY*. We proxy for this demand with (1) sell-side analyst experience in forecasting earnings of the subject firm and (2) lagged sell-side analyst forecast accuracy for the same firm. We find that neither the firm-specific experience proxy in column (1) nor the past-accuracy proxy in column (2) has a statistically significant interactive effect with the connections variable.

Overall, we believe that our tests of H2, H3, and H4 provide strong evidence that sell-side interest in connecting with buy-side analysts in order to glean information that improves the quality of sell-side research reports drives the relation we find between *ACCURACY* and *CONNECTIONS*. The next section describes the results of additional robustness tests.

## *7 Additional Analyses*

*7.1 Another Test of Reverse Causality*

As described in Section 1, a concern is that rather than sell-side analyst forecast accuracy improving due to connections with information-laden buy-side analysts, less information-laden buy-side analysts may choose to work with sell-side analysts with the best earnings forecast accuracy track records. In other words, the best sell-side analysts may learn nothing from connections with buy-side analysts. Instead, buy-side analysts may connect with the sell-side analysts from whom they can acquire the most information, and the flow of information may be one-directional; i.e., from the sell-side to the buy-side, with accuracy driving connections.

Results described in Sections 5.3 through 5.5 mitigate this concern. To further address this concern, we constrain the sample to sell-side analysts with less than four years of firm-specific experience. We argue that these analysts do not have enough of an accuracy track record to attract the interest of buy-side analysts in the companies they cover, which would then drive connections. In this sample, we expect that our firm-specific experience variable is not significant, while all of the other results still hold. Table 6 displays the results, which mirror the results testing H1 in Table 2, except that, as expected, the firm-specific experience variable is no longer significantly related to forecast accuracy. Thus, our inferences remain unchanged. This suggests that our main result is consistent with connections with buy-side analysts enhancing sell-side analyst forecast accuracy (not the other way around).

### *7.2 Alternative Measures of CONNECTIONS*

Table 7 replicates our test of H1 using models (1) and (2) and two alternative proxies for the degree of connectedness between sell-side and buy-side analysts. The first alternative measures analyst *a*'s *CONNECTIONS* regarding firm *f* as the number of institutions that hold *f* and at least one other stock followed by *a*, divided by the number of institutions that hold *f*. Unlike our original *CONNECTIONS* variable, this proxy treats all institutions with whom analyst *a* is connected equally, thus emphasizing breadth of connections over depth of connections. Like the primary measure, we scale this alternative measure of *CONNECTIONS* among analysts following the same firm-year. The estimation results are presented in columns (1) and (2) of Table 7. The results are essentially the same as those presented in Table 2.

The second alternative is the same as our original method of estimating *CONNECTIONS* but with additional stringency that emphasizes the depth of connections between analysts and institutions holding stocks that the analysts follow. Recall that our original approach measures *CONNECTIONS* as the number of stocks other than *f* that *a* holds in common with each institution holding *f,* averaged across all institutions holding *f*. The additional stringency is that a stock other than *f* is only counted as a stock held in common with an institution, if the total market value of all of the stocks covered by *a* account for at least 5% of the market value of the institution's portfolio. Essentially, this measure views an analyst as being connected with an institution only when the stocks commonly covered by the analyst and held by the institution account for a significant portion of the institution's portfolio. Like the primary measure, we scale this alternative measure of *CONNECTIONS* among analysts following the same firm-year. The estimation results are presented in columns (3) and (4) of Table 7. Again, the results are essentially the same as those presented in Table 2. Overall, the results in Table 7 increase our confidence in the construct validity of our *CONNECTIONS* variable, as a measure of both breadth and depth of connections between sell-side analysts and institutions holding stocks that the analyst follows.

*7.3 Sensitivity of results to controls for potentially unidentified correlated omitted variables*

Table 8 reports the results of an instrumental variable approach designed to further alleviate concerns that unidentified factors affect both *ACCURACY* and *CONNECTIONS* and threaten the validity of the inferences drawn from the results of our primary analyses (Kennedy 2008, p141). Specifically, for connections between analyst *a* who follows firm *f* in year *t* and institutions holding *f*'s stock in year *t*, we construct an instrumental variable as the average of unexpected cash flows to the institutions with connections with *a* (i.e., holding *f* and at least one other stock *a* follows) in the same quarter connections are measured. Unexpected flows prompt the institutions to invest the cash to avoid a drag on performance, which likely involves increasing positions in *f*, adding other stocks covered by *a*, or consulting *a* about the stocks in general. We expect unexpected flows to enhance an institution's connections with *a* while not directly affecting *a*'s forecast of *f*. The influence on forecast accuracy would work through *a*'s connections with the institution.

To estimate unexpected cash flows (*UNEXPECT_FLOW*), we regress current quarter flows on four quarterly lags of flows and four quarterly lags of institution portfolio returns, where flows are divided by institution's assets at the beginning of the quarter.[11] Residuals for each institution-quarter measure the unexpected flows. We first obtain the fitted value of unscaled connection by regressing it on *UNEXPECT_FLOW* and all of the unscaled exogenous variables in equation (1), including year fixed effects. We then scale the fitted value within each firm-year to fall between 0 and 1 and use the scaled measure as an instrument for the scaled *CONNECTIONS* variable. As an instrument for $CONNECTIONS^2$, following Wooldridge (2002, p236), we use the square of the scaled fitted value.

Table 8 shows that the instruments are strong. The instrument's F-statistic, which refers to the "Cragg & Donald (1993) minimum eigenvalue statistic" equals 381.44, way above the critical value of 7.03 for a 10 percent significance level (as constructed by Stock and Yogo 2005). Importantly, results based on the two stage least square procedure continue to support our hypothesized concave relation between analyst connection with institutional investors and analyst earnings forecast accuracy. These results mitigate concerns that our main results are driven by correlated omitted variables.

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[11] This is similar to the approach used in Coval & Stafford (2007). Moreover, flows are computed for each institution in each quarter as the change in the institution's portfolio value over that quarter that is not explained by the return of the portfolio over the same quarter, normalized by the institution's assets at the beginning of the quarter.

## *8 Conclusion*

A plethora of research papers examine the impact of sell-side financial analyst research on the investment community (Ramnath et al. 2008b; Bradshaw et al. 2017), while relatively few papers examine the role of buy-side analysts, working for institutional investors, the most important clients of the investment and boutique research firms that employ sell-side analysts (Brown et al. 2016). Ours is the first large-sample study to examine the impact of private buy-side analyst information on the quality of publicly available sell-side analyst research.

Sell-side analysts have substantial incentives to impress buy-side analysts working for various institutional investors, because buy-side votes for Institutional Investor's all-American team of sell-side analysts largely determine sell-side analyst compensation (Groysberg et al. 2011). Buy-side analysts have substantial incentives to enlist the services of sell-side analysts who add value to the information contained in the research buy-side analysts produce for their fund managers. The value added comes from industry expertise (Bradley, et al. 2017), connections with firm management (Green, Jame, Markov, & Subasi 2014), and information gleaned from sell-side analysts' connections with other buy-side analysts (Brown et al. 2016).

Most prior academic research regarding the interactions between these two sophisticated groups of market participants focuses on the flow of information from sell-side analyst research into stock prices. Some studies focus on the flow of information from stock price changes into sell-side analyst research (e.g., Clement, Hales, & Xue 2011). Our study focuses directly on what sell-side analysts learn from connections with institutional investors. Our evidence of a non-linear relation between connections with institutional investors and sell-side analyst earnings forecast accuracy is consistent with these connections enhancing the quality of sell-side analyst research output and, hence, the quality of information impounded in capital asset prices, although up to a point of diminishing returns.

We recognize that buy-side analysts may invest effort in choosing the sell-side analysts whom they wish to engage, and this choice may depend on the accuracy of sell-side analyst earnings forecasts. At the same time, we hypothesize that the accuracy of sell-side analyst earnings forecasts depends on the intensity of their connections with buy-side analysts. Our tests effectively untangle this endogeneity and focus on the flow of information from the buy-side to the sell-side. To the best of our knowledge, ours is the first study to show that sell-side analysts learn about the stocks they follow from connections with their buy-side counterparts.

The idea of a well-connected sell-side analyst goes beyond just connections with the buy-side. For example, the analyst has connections with industry contacts that enable interactions with suppliers and customers; with management of public and private companies to develop a pipeline of future coverage; with venture capitalists and private equity firms to help her build a pipeline of investment banking deals and future research coverage; and with the business press for general visibility. Our paper only examines connections with buy-side clients, thus leaving room for future research.

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**Figure 1 − Connections Variable for Analysts *a1* and *a2* regarding Firm–Year *f1–t*, where only Institutions *i1*, *i2*, and *i3* Hold *f1***

Analysts following ***f1***

*a*1 follows ***f1***, *f2, f3, f4, f5, f6, f7, f8, f9, f10*

*a*2 follows ***f1***, *f12, f13, f14, f15, f16, f17, f18, f19, f20*

Portfolios of all institutions that invest in ***f1***

*i1* holds ***f1,*** *f*2, *f7*, *f8*

*i2* holds ***f1***, *f3*, *f*5, *f*6

*i3* holds ***f*1**, *f4, f9, f10*

Connections through firms beyond *f*1:

- Analyst *a1* is strongly connected with the three institutions holding *f1* during year *t*. *a1* is connected with *i1* through *f2*, *f7*, and *f8*; with *i2* through *f3*, *f5*, and *f6*; with *i3* through *f4*, *f9*, and *f10*. The unscaled $CONNECTIONS_{f1,t}$ variable takes on a value of (3+3+3)/3 = 3 for analyst *a1*, the maximum among both analysts.
- Analyst *a2* is not connected with the three institutions holding *f1* during year *t*. The $CONNECTIONS_{f1,t}$ variable takes on a value of (0+0+0)/3 = 0 for analyst *a2*, the minimum among both analysts.

Scaled $CONNECTIONS_{f1,t}$ variable = (unscaled CONNECTIONS – minimum Connections) / (maximum CONNECTIONS – minimum Connections):

- For analyst *a1*: (3 – 0) / (3 – 0) = 1
- For analyst *a2*: (0 – 0) / (3 – 0) = 0

**Table 1 — Descriptive Statistics and Correlations**

**Panel A. Descriptive Statistics (137,835 analyst-firm-year observations)**

| Variables | mean | p25 | p50 | p75 | Standard Deviation |
|---|---|---|---|---|---|
| \|FE\| | 0.795 | 0.053 | 0.146 | 0.408 | 5.571 |
| FE | 0.476 | -0.107 | 0.000 | 0.231 | 5.608 |
| *ACCURACY* | 0.528 | 0.208 | 0.550 | 0.864 | 0.357 |
| *CONNECTIONS* | 6.680 | 3.908 | 5.852 | 8.267 | 4.615 |
| INST_OWNER# | 292.827 | 130.000 | 213.000 | 363.000 | 251.839 |
| FIRM# | 16.665 | 11.000 | 15.000 | 20.000 | 9.525 |
| INDUSTRY# | 3.825 | 2.000 | 3.000 | 5.000 | 2.663 |
| FIRM_EXP | 4.953 | 2.000 | 4.000 | 7.000 | 4.137 |
| BSIZE | 67.109 | 22.000 | 52.000 | 101.000 | 58.847 |
| DAYS | 4.589 | 0.000 | 0.000 | 3.000 | 12.525 |
| EPS_FREQ | 6.125 | 4.000 | 6.000 | 7.000 | 2.701 |
| HORIZON | 310.102 | 295.000 | 320.000 | 334.000 | 31.639 |
| *CONNECTIONS* with high RPI institutions | 0.092 | 0.019 | 0.049 | 0.106 | 0.150 |
| *CONNECTIONS* with medium RPI institutions | 0.453 | 0.174 | 0.325 | 0.565 | 0.485 |
| *CONNECTIONS* with low RPI institutions | 5.882 | 3.333 | 5.144 | 7.398 | 4.101 |

## Panel B. Correlations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *CONNECTIONS* | (1) | 1.000 | | | | | | | | | | | |
| *ACCURACY* | (2) | 0.001 | 1.000 | | | | | | | | | | |
| | | *0.811* | | | | | | | | | | | |
| Lag year *ACCURACY* | (3) | **-0.010** | **0.049** | 1.000 | | | | | | | | | |
| | | *0.000* | *0.000* | | | | | | | | | | |
| FIRM# | (4) | **0.822** | 0.004 | **-0.016** | 1.000 | | | | | | | | |
| | | *0.000* | 0.192 | 0.000 | | | | | | | | | |
| INDUSTRY# | (5) | **0.410** | **-0.012** | **-0.015** | **0.474** | 1.000 | | | | | | | |
| | | *0.000* | 0.000 | 0.000 | 0.000 | | | | | | | | |
| FIRM_EXP | (6) | **0.136** | **0.007** | -0.004 | **0.135** | **0.072** | 1.000 | | | | | | |
| | | 0.000 | 0.008 | 0.135 | 0.000 | 0.000 | | | | | | | |
| BSIZE | (7) | **0.123** | **0.006** | **0.007** | **0.055** | **-0.060** | **0.033** | 1.000 | | | | | |
| | | *0.000* | *0.042* | *0.006* | *0.000* | *0.000* | *0.000* | | | | | | |
| DAYS | (8) | **0.077** | **-0.012** | **-0.007** | **0.056** | **0.055** | **0.094** | **0.032** | 1.000 | | | | |
| | | *0.000* | *0.000* | *0.011* | *0.000* | *0.000* | *0.000* | *0.000* | | | | | |
| EPS_FREQ | (9) | **-0.032** | **-0.022** | **-0.070** | **-0.039** | **-0.055** | **-0.014** | **0.080** | **-0.014** | 1.000 | | | |
| | | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | | | | |
| HORIZON | (10) | **-0.062** | **-0.123** | **-0.007** | **-0.063** | **-0.056** | **-0.039** | **-0.024** | **-0.235** | **0.140** | 1.000 | | |
| | | *0.000* | *0.000* | *0.016* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | | | |
| *CONNECTIONS* with low RPI institutions | (11) | **0.994** | 0.002 | **-0.010** | **0.835** | **0.413** | **0.136** | **0.124** | **0.075** | **-0.033** | **-0.065** | 1.000 | |
| | | *0.000* | *0.500* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | | |
| POST_GS | (12) | **0.060** | **0.025** | **0.021** | **0.067** | **0.014** | **-0.069** | **-0.010** | **-0.089** | **-0.019** | **0.064** | **0.061** | 1.00 |
| | | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.000* | *0.001* | *0.000* | *0.000* | *0.000* | *0.000* | |

Panel A reports summary statistics for our sample of 137,835 analyst-firm-year observations from 1995 to 2012, including 3,980 unique firms and 7,615 analysts. Panel B reports correlation coefficients (and associated p-values) among the main variables used in the analysis, where the variables are scaled among analysts for the same firm-year. For ease of interpretation, with the exception of *ACCURACY*, no variable in Panel A is scaled among analysts for the same firm-year.

We choose the absolute forecast errors for the earliest forecast of each analyst forecasting firm *f*'s year *t* earnings, where the forecasts occur during the 90 days post the announcement of firm *f*'s year *t-1* annual earnings. Based on these forecast errors, we compute *ACCURACY* as the difference between the maximum and analyst *a*'s absolute forecast error, scaled by the range between the maximum and minimum. *ACCURACY* falls on a scale between zero (least accurate) and one (most accurate).

$|FE_{aft}|$= absolute error in analyst *a*'s earliest forecast, $F_{aft}$, for firm *f*'s year *t* earnings issued during the 90 days post the announcement of firm *f*'s year *t-1* annual earnings, scaled by the absolute value of actual earnings.

$CONNECTIONS_{aft}$ = analyst *a*'s average number of connections with institutional investors holding *f* as of the date of $F_{aft}$, defined as the number of stocks, other than *f*, covered by analyst *a* and held by institutions that invest in firm *f,* divided by the number of all institutions holding firm *f.* Institutional holdings used to construct this measure are from the calendar quarter preceding the date of $F_{aft}$, and analyst coverage of other companies is from the one year period that precedes the calendar quarter end used for institutional holding measurement.

$FIRM\#_t^a$ = number of firms analyst *a* followed in the year ending with the date of $F_{aft}$.

$INDUSTRY\#_{at}$ = number of industries analyst *a* followed in the year ending with the date of $F_{aft}$.

$FIRM\_EXP_{aft}$ = number of years since the first year analyst *a* issued one-year ahead earnings forecasts for firm *f* up to the date of $F_{aft}$.

$BSIZE_{at}$ = number of analysts employed by analyst *a*'s brokerage house or research firm in the year ending with the date of $F_{aft}$.

$DAYS_{aft}$ = number of days between the date of $F_{aft}$ and the most recent one-year ahead forecast of firm *f*'s year *t* earnings preceding $F_{aft}$ by any analyst.

$EPS\_FREQ_{aft}$ = frequency of analyst *a*'s one-year ahead earnings forecasts for firm *f* in the one-year period prior to the date of $F_{aft}$.

$HORIZON_{aft}$ = number of days between the date of $F_{aft}$ and the end of fiscal year *t*.

$CONNECTIONS_{aft}^{High\ Opp}$, $CONNECTIONS_{aft}^{Med\ Opp}$, and $CONNECTIONS_{aft}^{Low\ Opp}$ are measured the same way as the original *CONNECTIONS* variable except that they are constructed based on connections with subsets of institutions, i.e., high-, medium-, and low-opportunity institutions. To classify institutions into high-, medium-, and low-opportunity institutions, we measure an institution's reliance on public information (RPI). We construct RPI of an institution as the $R^2$ from an institution-level regression of changes in the number of shares held by that institution in a given stock on four lags of changes in mean analyst recommendations. All institutional investors are ranked each quarter by their RPI into terciles. Institutions in the lowest RPI tercile are viewed as having the least reliance on public information and classified as high-opportunity institutions.

## Table 2 — Earnings Forecast Accuracy and Connections

| Variables | (1) Coeff (std. err.) | (2) Coeff (std. err.) | (3) Coeff (std. err.) | (4) Coeff (std. err.) |
|---|---|---|---|---|
| *CONNECTIONS* | 5.679*** | 5.166*** | | |
| | (1.176) | (1.266) | | |
| *CONNECTIONS*$^2$ | -5.723*** | -5.911*** | | |
| | (1.217) | (1.188) | | |
| Break down of *CONNECTIONS* | | | | |
| Bottom *CONNECTIONS* Tercile | | | 9.430*** | 9.464*** |
| | | | (2.430) | (2.458) |
| Middle *CONNECTIONS* Tercile | | | 2.558*** | 2.097** |
| | | | (0.738) | (0.867) |
| Top *CONNECTIONS* Tercile | | | 0.484 | -0.137 |
| | | | (0.407) | (0.577) |
| Lagged *ACCURACY* | | 4.554*** | | 4.565*** |
| | | (0.314) | | (0.315) |
| FIRM# | | 0.942* | | 0.920 |
| | | (0.568) | | (0.568) |
| INDUSTRY# | | -1.648*** | | -1.699*** |
| | | (0.348) | | (0.350) |
| FIRM_EXP | | 0.688** | | 0.704** |
| | | (0.286) | | (0.286) |
| BSIZE | | 0.251 | | 0.269 |
| | | (0.345) | | (0.347) |
| DAYS | | -3.895*** | | -3.947*** |
| | | (0.298) | | (0.297) |
| EPS_FREQ | | -0.010 | | 0.001 |
| | | (0.302) | | (0.302) |
| HORIZON | | -11.796*** | | -11.789*** |
| | | (0.274) | | (0.275) |
| Constant | 52.910*** | 58.341*** | 52.878*** | 58.282*** |
| | (1.012) | (1.031) | (1.016) | (1.035) |
| Year Fixed Effects | YES | YES | YES | YES |
| N | 137,835 | 137,835 | 137,835 | 137,835 |
| Adjusted R-squared | 0.21% | 2.18% | 0.20% | 2.17% |

Table 2 examines the relation between sell-side analysts' forecast accuracy and their connections with institutional investors based on the following regressions:

$$ACCURACY_{aft} = \beta_0 + \beta_1 CONNECTIONS_{aft} + \beta_2 CONNECTIONS_{aft}^2 + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (1)$$

$$ACCURACY_{aft} = \beta_0 + \sum_{k=1}^{3} \beta_k^{Tercile} D_k^{Tercile} CONNECTION_{aft} + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (2)$$

$D_k^{Tercile}$ is an indicator variable equaling one for the k$^{th}$ $CONNECTIONS$ tercile (1=lowest and 3=highest) and zero otherwise. All other variables are defined as in Table 1. In this table, all continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

**Table 3 — Earnings Forecast Accuracy and Connections Stratified by Opportunities for Analysts to Learn**

**Panel A. All observations**

| Variables | coeff (std. err.) |
|---|---|
| *CONNECTIONS* $^{\text{High Opp}}$ | 5.719*** |
| | (1.709) |
| (*CONNECTIONS* $^{\text{High Opp}}$)$^2$ | -3.888*** |
| | (1.462) |
| *CONNECTIONS* $^{\text{Med Opp}}$ | 0.053 |
| | (1.587) |
| (*CONNECTIONS* $^{\text{Med Opp}}$)$^2$ | -1.640 |
| | (1.459) |
| *CONNECTIONS* $^{\text{Low Opp}}$ | 1.470 |
| | (1.202) |
| (*CONNECTIONS* $^{\text{Low Opp}}$)$^2$ | -2.667** |
| | (1.176) |
| Lagged *ACCURACY* | 4.533*** |
| | (0.313) |
| FIRM# | 0.345 |
| | (0.596) |
| INDUSTRY# | -1.550*** |
| | (0.345) |
| FIRM_EXP | 0.729** |
| | (0.286) |
| BSIZE | 0.237 |
| | (0.345) |
| DAYS | -3.812*** |
| | (0.298) |
| EPS_FREQ | -0.019 |
| | (0.302) |
| HORIZON | -11.803*** |
| | (0.275) |
| Constant | 58.473*** |
| | (1.033) |
| N | 137,835 |
| Year Fixed Effects | YES |
| Adjusted R-squared | 2.20% |

**Panel B. Observations where analysts' connections with institutions are in the High Opportunity tercile**

| Variables | (1) coeff (std. err.) | (2) coeff (std. err.) |
|---|---|---|
| *CONNECTIONS*$^{\text{High Opp}}$ | 9.576*** | |
| | (1.950) | |
| (*CONNECTIONS* $^{\text{High Opp}}$)$^2$ | -8.754*** | |
| | (1.896) | |
| Break down of *CONNECTIONS*$^{\text{High Opp}}$ | | |
| Bottom *CONNECTIONS*$^{\text{High Opp}}$ Tercile | | 38.567*** |
| | | (11.676) |
| Middle *CONNECTIONS*$^{\text{High Opp}}$ Tercile | | 3.371*** |
| | | (0.754) |
| Top *CONNECTIONS*$^{\text{High Opp}}$ Tercile | | 0.784 |
| | | (0.525) |
| Lagged *ACCURACY* | 3.598*** | 3.614*** |
| | (0.440) | (0.440) |
| FIRM# | 1.056** | 1.043** |
| | (0.529) | (0.529) |
| INDUSTRY# | -1.074** | -1.077** |
| | (0.468) | (0.468) |
| FIRM_EXP | 1.618*** | 1.622*** |
| | (0.437) | (0.438) |
| BSIZE | 1.280*** | 1.272*** |
| | (0.452) | (0.452) |
| DAYS | -2.616*** | -2.628*** |
| | (0.457) | (0.457) |
| EPS_FREQ | -0.241 | -0.239 |
| | (0.452) | (0.453) |
| HORIZON | -7.033*** | -7.028*** |
| | (0.407) | (0.407) |
| Constant | 52.104*** | 52.211*** |
| | (1.850) | (1.846) |
| N | 37,563 | 37,563 |
| Year Fixed Effects | YES | YES |
| Adjusted R-squared | 1.41% | 1.41% |

Table 3 examines whether the sensitivity of forecast accuracy to connections increases when sell-side analysts have greater opportunities to learn private information from their connections with institutional investors. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Panel A reports results from the following regression model.

$$ACCURACY_{aft} = \beta_0 + \beta_1 CONNECTIONS_{aft}^{High\ Opp} + \beta_2 (CONNECTIONS_{aft}^{High\ Opp})^2$$
$$+\beta_3 CONNECTIONS_{aft}^{Med\ Opp} + \beta_4 (CONNECTIONS_{aft}^{Med\ Opp})^2$$

$$+\beta_5 CONNECTIONS_{aft}^{Low\ Opp} + \beta_5 (CONNECTIONS_{aft}^{Low\ Opp})^2$$
$$+ \sum_m \beta_m Control_m + \varepsilon_{aft} \qquad (3)$$

where $CONNECTIONS_{aft}^{High\ Opp}$ is measured the same way as the original *CONNECTIONS* variable except that it is constructed only based on connections with high-opportunity institutions. $CONNECTIONS_{aft}^{Med\ Opp}$ and $CONNECTIONS_{aft}^{Low\ Opp}$ are constructed in a similar fashion based on connections with medium and low-opportunity institutions, respectively. To classify institutions into high-, medium-, and low-opportunity institutions, we measure an institution's reliance on public information (RPI). We construct RPI of an institution as the $R^2$ from an institution-level regression of changes in the number of shares held in a given stock by a given institution on four lags of changes in mean analyst recommendations. All institutional investors are ranked each quarter by their RPI into terciles. Institutions in the lowest RPI tercile are classified as high-opportunity institutions. All other variables are defined as in Table 1.

Panel B reports results from estimation of Models 1 and 2 from Table 2 on observations where analyst connections with high opportunity institutions, i.e., those in the low RPI group, are in the bottom tercile of the same firm-year group. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

**Table 4 — The Relation between Earnings Forecast Accuracy and Connections Pre- and Post-Exogenous Regulatory Changes**

| Variables | coeff (std. err.) |
|---|---|
| *CONNECTIONS* | -0.462 |
| | (2.504) |
| *CONNECTIONS*$^2$ | -2.358 |
| | (2.463) |
| *CONNECTIONS*× POST_GS | 7.378*** |
| | (2.845) |
| *CONNECTIONS*$^2$× POST_GS | -5.106* |
| | (2.866) |
| POST_GS | 1.079 |
| | (1.119) |
| Lagged *ACCURACY* | 5.029*** |
| | (0.337) |
| FIRM# | 1.159* |
| | (0.616) |
| INDUSTRY# | -1.474*** |
| | (0.371) |
| FIRM_EXP | 0.809*** |
| | (0.306) |
| BSIZE | -0.063 |
| | (0.366) |
| DAYS | -3.959*** |
| | (0.317) |
| Constant | 59.092*** |
| | (1.092) |
| N | 120,983 |
| Year Fixed Effects | YES |
| Adjusted R-squared | 2.09% |

Table 4 examines whether the sensitivity of forecast accuracy to connections increases with sell-side analyst demand for information following the regulatory period that includes Reg. FD and GARS. This table reports coefficient estimates from the following regression.

$$ACCURACY_{aft} = \beta_0 + \beta_1 CONNECTIONS_{aft} + \beta_2 CONNECTIONS^2_{aft} + \beta_3 POST\_GS + \beta_4 POST\_GS \times CONNECTIONS_{aft} + \beta_5 POST\_GS \times CONNECTIONS^2_{aft} + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (4)$$

$POST\_GS$ = 1 for analyst forecasts issued after 2003, and 0 for forecasts issued before 2001.
$FIRM\_EXP_{aft}$ = number of years since the first year analyst *a* issued one-year ahead earnings forecasts for firm *f* up to current year.
$LAGGED\_ACCURACY_{aft}$=one year lagged value of the *ACCURACY* variable.
All other variables are defined as in Table 1. Observations during the years of the changing regulatory environment (2001-2003) are omitted. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

**Table 5 — The Relation between Earnings Forecast Accuracy and Connections by Buy-side Analyst Demand**

| Variables | DEMAND = | (1) coeff (std. err.) FIRM_EXP | (2) coeff (std. err.) Lagged *ACCURACY* |
|---|---|---|---|
| *CONNECTIONS* | | 5.919*** | 5.950*** |
| | | (1.679) | (2.183) |
| $CONNECTIONS^2$ | | -6.287*** | -6.122*** |
| | | (1.642) | (2.231) |
| *CONNECTIONS*× DEMAND | | -2.137 | -1.476 |
| | | (2.826) | (3.183) |
| $CONNECTIONS^2$× DEMAND | | 1.234 | 0.374 |
| | | (2.723) | (3.243) |
| Lagged *ACCURACY* | | 4.553*** | 5.076*** |
| | | (0.314) | (0.593) |
| FIRM# | | 0.941* | 0.942* |
| | | (0.568) | (0.568) |
| INDUSTRY# | | -1.651*** | -1.650*** |
| | | (0.347) | (0.348) |
| FIRM_EXP | | 1.234** | 0.688** |
| | | (0.566) | (0.286) |
| BSIZE | | 0.255 | 0.251 |
| | | (0.345) | (0.345) |
| DAYS | | -3.895*** | -3.894*** |
| | | (0.298) | (0.298) |
| Constant | | 58.147*** | 58.070*** |
| | | (1.045) | (1.065) |
| N | | 137,835 | 137,835 |
| Year Fixed Effects | | YES | YES |
| Adjusted R-squared | | 2.18% | 2.18% |

Table 5 examines whether the sensitivity of forecast accuracy to connections increases with buy-side analyst demand for information. This table reports coefficient estimates from the following regressions.

$$ACCURACY_{aft} = \beta_0 + \beta_1 CONNECTIONS_{aft} + \beta_2 CONNECTIONS^2_{aft} + \beta_3 DEMAND + \beta_4 DEMAND \times CONNECTIONS_{aft} + \beta_5 DEMAND \times CONNECTIONS^2_{aft} + \sum_m \beta_m Control_m + \varepsilon_{aft} \quad (4)$$

We employ the following variables as proxies for buy-side analyst *DEMAND*:
$FIRM\_EXP_{aft}$ = number of years since the first year analyst *a* issued one-year ahead earnings forecasts for firm *f* up to current year.
$LAGGED\_ACCURACY_{aft}$=one year lagged value of the *ACCURACY* variable.
All other variables are defined as in Table 1. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

**Table 6 — Earnings Forecast Accuracy and Connections Based on Only the First Three Years since an Analyst Covered the Same Firm**

| Variables | (1)<br>Coeff (std. err.) | (2)<br>Coeff (std. err.) |
|---|---|---|
| *CONNECTIONS* | 4.665*** | |
| | (1.744) | |
| *CONNECTIONS*$^2$ | -5.204*** | |
| | (1.666) | |
| Break down of *CONNECTIONS* | | |
| Bottom *CONNECTIONS* Tercile | | 7.336** |
| | | (3.428) |
| Middle *CONNECTIONS* Tercile | | 1.853 |
| | | (1.201) |
| Top *CONNECTIONS* Tercile | | -0.046 |
| | | (0.799) |
| Lagged *ACCURACY* | 4.660*** | 4.672*** |
| | (0.436) | (0.437) |
| FIRM# | 1.298 | 1.275 |
| | (0.796) | (0.797) |
| INDUSTRY# | -1.888*** | -1.926*** |
| | (0.472) | (0.473) |
| FIRM_EXP | -0.158 | -0.147 |
| | (0.516) | (0.516) |
| BSIZE | 0.259 | 0.262 |
| | (0.462) | (0.463) |
| DAYS | -4.357*** | -4.399*** |
| | (0.433) | (0.432) |
| EPS_FREQ | -0.131 | -0.124 |
| | (0.415) | (0.416) |
| HORIZON | -11.482*** | -11.472*** |
| | (0.382) | (0.383) |
| Constant | 57.533*** | 57.515*** |
| | (1.526) | (1.531) |
| Year Fixed Effects | YES | YES |
| N | 67,650 | 67,650 |
| Adjusted R-squared | 2.04% | 2.03% |

This table replicates the analysis from specifications (2) and (4) of Table 2 on the subset of our sample observations that include only the first three years since an analyst covered the same firm in the IBES database. All variables are defined as in Table 1. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

## Table 7 — Alternative Measures for Connections

| Variables | (1) Measure 1 Coeff (std. err.) | (2) | (3) Measure 2 Coeff (std. err.) | (4) |
|---|---|---|---|---|
| *CONNECTIONS* | 5.535*** | | 3.757*** | |
| | (1.182) | | (1.127) | |
| *CONNECTIONS*$^2$ | -4.370*** | | -4.929*** | |
| | (1.145) | | (1.176) | |
| Break down of *CONNECTIONS* | | | | |
| Bottom *CONNECTIONS* Tercile | | 2.421** | | 131.865*** |
| | | (0.969) | | (43.737) |
| Middle *CONNECTIONS* Tercile | | 2.127*** | | 3.107*** |
| | | (0.428) | | (1.010) |
| Top *CONNECTIONS* Tercile | | 1.007*** | | -0.752** |
| | | (0.362) | | (0.341) |
| Lagged *ACCURACY* | 4.558*** | 4.558*** | 4.550*** | 4.555*** |
| | (0.315) | (0.315) | (0.314) | (0.315) |
| FIRM# | -0.002 | -0.009 | 0.820** | 0.795** |
| | (0.419) | (0.420) | (0.402) | (0.403) |
| INDUSTRY# | -1.694*** | -1.706*** | -1.606*** | -1.632*** |
| | (0.348) | (0.348) | (0.346) | (0.348) |
| FIRM_EXP | 0.668** | 0.676** | 0.762*** | 0.770*** |
| | (0.286) | (0.286) | (0.286) | (0.286) |
| BSIZE | 0.145 | 0.137 | 0.238 | 0.259 |
| | (0.345) | (0.345) | (0.344) | (0.344) |
| DAYS | -3.944*** | -3.966*** | -3.906*** | -3.923*** |
| | (0.297) | (0.297) | (0.298) | (0.297) |
| EPS_FREQ | 0.004 | 0.005 | -0.010 | -0.009 |
| | (0.302) | (0.302) | (0.302) | (0.302) |
| HORIZON | -11.807*** | -11.799*** | -11.791*** | -11.792*** |
| | (0.275) | (0.275) | (0.274) | (0.275) |
| Constant | 57.987*** | 58.220*** | 58.735*** | 58.672*** |
| | (1.042) | (1.041) | (1.028) | (1.029) |
| N | 137,835 | 137,835 | 137,835 | 137,835 |
| Year Fixed Effects | YES | YES | YES | YES |
| Adjusted R-squared | 2.17% | 2.17% | 2.17% | 2.17% |

This table replicates the analysis from specifications (2) and (4) of Table 2 with two alternative measures of the *CONNECTIONS* variable. The first alternative is measured as the number of institutions that invest in both firm *j* and at least one other company followed by analyst *i*, divided by the number of all institutions holding firm *j*. The second alternative is measured the same way as the original *CONNECTIONS* variable except that it considers only connections with institutions where the value of all stocks covered by an analyst as a percentage of an institution's portfolio value is above 5 percent of the institution's portfolio value. In other words, *CONNECTIONS* is calculated only for analyst-institution pairs where the aforementioned percentage is above 5 percent. All other variables are defined as in Table 1. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.

## Table 8 - Instrumental Variable Analysis

| VARIABLES | (1) First stage for *CONNECTIONS* | (2) First stage for *CONNECTIONS*$^2$ | (3) Second stage for *ACCURACY* |
|---|---|---|---|
| | coeff (std. err.) | coeff (std. err.) | coeff (std. err.) |
| Instrument for *CONNECTIONS* | 25.115*** | −41.008*** | |
| | (1.017) | (1.092) | |
| Instrument for *CONNECTIONS*$^2$ | -0.722 | 62.772*** | |
| | (0.531) | (0.571) | |
| *CONNECTIONS* | | | 45.030*** |
| | | | (8.024) |
| *CONNECTIONS*$^2$ | | | -10.218*** |
| | | | (2.009) |
| Lagged *ACCURACY* | 0.236 | 0.035 | 4.447*** |
| | (0.146) | (0.157) | (0.324) |
| FIRM# | 55.855*** | 57.633*** | -27.433*** |
| | (0.877) | (0.942) | (6.354) |
| INDUSTRY# | 3.166*** | 4.087*** | -2.727*** |
| | (0.160) | (0.172) | (0.447) |
| FIRM_EXP | 2.303*** | 1.685*** | -0.060 |
| | (0.140) | (0.151) | (0.342) |
| BSIZE | 6.782*** | 6.771*** | -2.547*** |
| | (0.155) | (0.167) | (0.744) |
| DAYS | 1.584*** | 2.562*** | -4.709*** |
| | (0.152) | (0.163) | (0.367) |
| EPS_FREQ | -0.093 | -0.273* | 0.138 |
| | (0.149) | (0.160) | (0.314) |
| HORIZON | 0.355*** | 0.028 | -11.814*** |
| | (0.133) | (0.143) | (0.284) |
| Constant | 1.539*** | -2.738*** | 57.204*** |
| | (0.539) | (0.579) | (1.086) |
| N | 137,796 | 137,796 | 137,796 |
| Year Fixed Effects | YES | YES | YES |
| Adjusted R-squared | 68.7% | 66.3% | N.A. |
| IV F-stat | | | 381.44 |

The instrument for unscaled *CONNECTIONS* is based on unexpected cash flows to the institutions with which an analyst is connected with. Each institution-quarter unexpected cash flows are the residuals from regressing current quarter flows on four quarterly lags of flows and four quarterly lags of institution portfolio returns, where flows are divided by institution assets. We first obtain the fitted value of unscaled *CONNECTIONS* by regressing it on unexpected cash flows and all of the unscaled exogenous variables in specification (2) of Table 2, including year fixed effects. We then scale the fitted value within each firm-year to fall between 0 and 1 and use the scaled measure as an instrument for the scaled *CONNECTIONS* variable. As an instrument for *CONNECTIONS*$^2$, following Wooldridge (2002, p236), we use the square of the scaled fitted value. All variables are defined as in Table 1. All continuous variables are scaled to fall between 0 and 1 for the same firm-year and the dependent variable (and, thus, each coefficient) is multiplied by 100. Standard errors are clustered by analyst and are presented in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% significance level, respectively.