ANALYSTS' PRIVATE CONNECTIONS

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

In Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

Charles Chao Kang

May 2021

ANALYSTS' PRIVATE CONNECTIONS

Charles Chao Kang, Ph. D.
Cornell University 2021

In this dissertation, I examine how analysts' private connections to external peers across brokerages affect the quality of their research. I find that analysts with private connections (i.e., who are connected to external peers via past employment ties and covering the same firm) issue more accurate earnings forecasts than "unconnected" analysts. In addition, the effect of private connections on earnings forecast accuracy is more notable for analyst pairs who are, on average, employed by smaller brokerages, and who are, on average, less skilled, less experienced, and more resource constrained, thereby suggesting that private connections help to mitigate resource constraints and allow less skilled and less experienced analysts to improve their research through generating synergy. Importantly, the market underreacts to forecasts of privately connected analysts, leading to significant stock price drifts following forecast revisions. Using coverage terminations of connected peers as exogenous shocks to private connections, I find decreases in analyst forecast accuracy of the recipient analyst following coverage terminations of mutually covered stocks by connected peers due to permanent employment terminations of connected peers, brokerage terminations of the connected peers' industry sector, and connected peers' brokerage closures.

BIOGRAPHICAL SKETCH

Charles Chao Kang earned his Bachelor of Science in Materials Science and Engineering and Bachelor of Arts in Economics and Asian Studies from Cornell University in 2013. In 2015, he joined the doctoral program in Accounting at the S.C. Johnson Graduate School of Management at Cornell University. His work has been presented at numerous conferences, including the American Accounting Association Annual Meeting, the Financial Accounting and Reporting Section Midyear Meeting, the Trans-Atlantic Doctoral Conference, China International Conference in Finance, and the MIT Asia Conference in Accounting.

Charles Chao Kang's dissertation was supervised by Dr. P. Eric Yeung.

ACKNOWLEDGMENTS

First and foremost, I would like to express my deep and sincere gratitude to my committee chair, Prof. P. Eric Yeung, for his years of dedicated mentoring, constant guidance, and personal attention. I am indebted to him for being so generous in providing me more than my due share of his time in guiding me and supporting me. His guidance, inspiration, and insights throughout my time at Cornell University has been invaluable to me growing as a researcher. I would also like to thank my committee members, Prof. Roni Michaely, Prof. Luo Zuo, Prof. Kenneth Merkley, and Prof. David Ng for their invaluable advice, feedback, and suggestions over the course of this research. I am deeply grateful for the guidance and support from my committee throughout my doctoral studies. Moreover, I would like to thank Prof. Joseph Pacelli for his help and suggestions over the course of this research. I would also like to thank the faculty at the S.C. Johnson School of Business for their comments and suggestions. Additionally, a special thanks to my family, friends, and God for their continuous love and support.

TABLE OF CONTENTS

LIST OF TABLES

CHAPTER 1

ANALYSTS' PRIVATE CONNECTIONS

1.1 Introduction

Social connection is an important source of an economic agent's social capital and is especially critical for information acquisition (Glaeser et al. 2000; Faccio 2006; Karlan et al. 2009; Cohen, Frazzini and Malloy 2008, 2010; Fracassi and Tate 2012; Gu et al. 2018). While the bulk of prior studies on social connection have focused on its role in facilitating information transfer among managers, directors, and mutual funds, little research has examined the role of social connection in facilitating private information acquisition between analysts. Analysts' information environment is distinct from managers, directors, and mutual funds due to the unique feature of the brokerage setting. In particular, analysts in smaller brokerages have limited resources and fewer opportunities to learn from colleagues within the brokerage house. In this study, I examine how analysts' private social connections to external peers across brokerages can augment limited internal resources in improving the quality of their research.

My study is based on the premise that analysts, like any economic agents, have limited time, resources, and energy (Kahneman 1973). Prior studies show that analysts' research quality is positively related to internal brokerage resources and negatively related to the number of industries that they cover (Clement 1999; Jacobs, Lys, and Neale 1999; Cornaggia and Zou 2008). These studies suggest that analysts in smaller brokerages and non-industry specialists are at a disadvantage, as they face greater resource constraints. While analysts employed by smaller brokerages may be internally constrained, analysts will utilize any potential resources to achieve their career advancement, including access to corporate managers and other financial market participants, such as commercial bankers and institutional investors (Malloy 2005;

Bradshaw 2011; Chen and Martin 2011; Cicero, Kalpathy, and Sulaeman 2011; Green et al. 2014; Li, Wong, and Yu 2019).

Prior literature has neglected one important external resource that analysts draw: their external peers. These external peers could be analysts covering the same firm or related firms in the same industry, where they share a complementarity of firm-specific and industry-specific information that, when exchanged, can improve their respective research (Mikhail, Walther, and Willis 1997; Stein 2008; Clement, Koonce, and Lopez 2007; Bradley, Gokkaya, and Liu 2017). In contrast to prior studies that have focused on in-house peer effects, I focus on private connections between analysts across different brokerages to examine how these private channels augment formal institutional channels in improving analysts' research output.

The private connections of an analyst are defined as shared interpersonal ties with other analysts ("related analysts who cover the same firm in the same year"). For instance, two analysts who share past employment ties ("past employment overlap of at least one year") are considered to have interpersonal ties. This type of connection is distinct from formal institutional channels (i.e., brokerages, research departments, teams, conferences) in that private connections are based on shared past experience and formed on a personal level. This induces greater trust and empathy between individuals, which can affect their decision making. Additionally, unlike formal institutional channels, which are constrained by the boundaries of the brokerage house, departments, or teams, private connections are formed on a broader unregulated network, which allows for freer interactions between participants within the shared network through which they could build long-term contractual relationships. These factors can induce greater information sharing among privately connected analysts, the implications of which I elaborate on below.

I develop testable predictions based on this information-sharing angle to investigate whether analysts' private connections can enhance their research quality. On the one hand, under this information-sharing hypothesis, analysts who share interpersonal ties with related analysts are more likely to obtain information that enables them to produce more accurate forecasts. This is consistent with prior studies documenting that social connections between economic agents can facilitate the transfer of information by lowering information-gathering costs (Cohen et al. 2008, 2010; Fang and Huang 2017).

On the other hand, information transfer between privately connected analysts may not necessarily occur for several reasons. First, it is not obvious whether analysts can efficiently transfer knowledge outside the brokerage setting, as learning could be limited by spatial distance (Marshall 1890; Arrow 1962; Lucas 1988; Glaeser et al. 1992; Beckmann 1994; Von Hippel 1994; Audretsch and Feldman 1996). While the brokerage setting facilitates interactions between analysts that makes social learning easier, analysts with private connections may face limits in their physical interactions, which may constrain learnings and knowledge transfer.

Second, analysts' connected peers may not necessarily hold high-quality information. This could occur, for example, if analysts share interpersonal ties with biased peer analysts who are driven by various conflicts of interest, such as the incentive to generate investment banking revenues, increase trading profits, and obtain closer access to management (Lin and McNichols 1998; Michaely and Womack 1999; Cowen et al. 2006; Ke and Yu 2006; Brown et al. 2015). The biased analysts may transfer along biased or low-quality information, thereby potentially undermining the research quality of their connected peers. Analysts may also be in competition with their connected peers, which could also impede information transfer.

I measure private connections between analysts using several different proxies. My main proxy is based on analysts' past employment ties. Using manually collected employment history, I measure analysts' private connections based on whether they share common work experience in the past with at least one year of overlap. I define private connections at the analyst-firm-year level and require the privately connected analysts to cover the same firm in the same year. Approximately 25% of the forecasts in the sample are issued by privately connected analysts defined using this approach.[1]

I test my predictions using data on analysts' earnings forecasts issued during the period from 1992 to 2017. Consistent with information sharing, I find that privately connected analysts, on average, issue more accurate forecasts relative to unconnected analysts. In terms of economic magnitude, the effect of private connections is greater than or comparable with most of the known determinants of earnings forecast accuracy, such as past forecast accuracy, firm-specific experience, general forecasting experience, broker size, All-Star status, in-house team size, colleague quality, and connections to managers. These findings are robust to controlling for various analyst and broker characteristics, and inclusion of industry, year, analyst, and broker fixed effects.[2] The results are also robust to including earnings announcement fixed effects.

Moreover, to control for analysts' past institution-specific experience, I rerun the analyses by restricting the sample to only analysts who have shared past employment ties and include cohort fixed effects to narrow the comparison to among analysts who shared past employment at the same institution in the same year. I compare analysts privately connected to external peers currently covering the same firm with: 1) analysts who share past employment ties with external peers who do not cover the same firm nor

---

[1] Alternatively, I proxy for private connections based on past educational ties. Approximately 26% of the forecasts in the sample are issued by privately connected analysts defined using this approach.

[2] The results are robust to alternative methods of demeaning analyst forecast accuracy and ways of establishing private connections (i.e., educational ties).

industry and 2) analysts who share past employment ties with external peers who do not cover the same firm, yet cover the same industry. In both cases, I find that the effect of private connections on forecast accuracy is greater for analysts who are privately connected to external peers covering the same firm, which suggests that analysts' private connections are associated with greater forecast accuracy through the channel of information complementarity and are not explained by analysts' past institution-specific experiences.

Furthermore, through partitioning the sample by the average broker size of the analysts and their external peers, I find that, while the results hold for analyst pairs employed by both small and large brokerages, the effect of private connections on forecast accuracy is greater for analyst pairs employed by smaller brokerages, who face greater resource constraints. This suggests that analysts' private connections can augment internal brokerage resources in improving analysts' research quality, with the benefits of private connections accruing more to analysts employed by smaller, resource-constrained brokers.

Next, I examine whether the relations between analysts' private connections and forecast accuracy differ based on the analysts' ability, experience, and resource constraints. I proxy for analysts' ability, experience, and resources using the average of the past forecast accuracy, All-Star status, firm-specific forecasting experience, and number of industries of the recipient analyst and her connected peers. I find that analyst pairs with lower average ability, experience, and resources experience greater improvement in their forecast accuracy through private connections, consistent with these analysts having greater information complementarity and thus deriving greater synergies through collaboration.

To mitigate endogeneity concerns, I exploit sources of variation that give rise to plausibly exogenous changes in the number of private connections. I focus on the case of connected peer analysts terminating their coverage for a mutually covered firm due to various exogenous shocks to examine its effect on the forecast accuracy of the recipient analyst. If the effect of private connections on forecast accuracy is indeed due to the support that analysts receive from their connected peers, shocks to connected peers should negatively affect the forecast accuracy of the recipient analyst for the mutually covered stocks. Consistent with this prediction, I document decreases in the forecast accuracy of the recipient analyst following coverage terminations of mutually covered stocks by connected peer analysts due to: 1) permanent departures of the connected peer analysts from equity research, 2) brokerage sector terminations affecting the connected peer analysts, and 3) brokerage closures affecting the connected peer analysts. The inferences remain the same using a differences-in-difference analyses comparing the change in forecast accuracy of analysts affected by the each of the plausibly exogenous shocks with analysts who are not influenced by these shocks.

I also examine whether analysts' private connections have market implications. I find that investors differentially perceive forecasts issued by privately connected analysts and those issued by unconnected analysts, as evidenced by stronger stock price reaction to the forecast revision in the three-day window following the forecast revision, holding the magnitude of the revision constant. However, I document evidence of delayed price reaction during the three- and six-month windows following the forecast revisions. The estimated coefficients are large (0.2842 to 0.5732) compared with the coefficient for the three-day window returns (0.1186), suggesting that investors significantly underreact to forecasts of privately connected analysts. Unlike formal institutional boundaries which are largely visible to investors, private connections are more difficult to detect, and investors are often unaware of the existence of such channels, leading to significant underreactions and post-revision drifts for forecasts of privately connected analysts (Clement and Tse 2003). Moreover, partitioning by the median length of time since an analyst became privately connected, I find strong evidence of investor underreaction only to forecasts issued by privately connected analysts in the below-median subsample, but not for forecasts issued by privately connected analysts in the above-median subsample, thereby suggesting that private connections that are newly formed are the most difficult to detect.

I conduct several additional analyses to gain further insight into the relations between private connections and forecast accuracy. First, I examine whether the effect of private connections on forecast accuracy is more pronounced when the strength of ties between analysts is greater. I proxy for the strength of ties between analysts using the duration of their shared past employment. Through partitioning the sample by the median and the top/bottom quartile, I find that the effect of private connections on forecast accuracy is greater for analysts with longer duration of shared past employment, consistent with a greater effect of private connections on forecast accuracy for analysts sharing stronger ties. In addition, the effect of private connections on forecast accuracy is statistically insignificant for analysts in the bottom quartile who share weak ties, despite sharing past employment at the same firm.[3] This reinforces the notion that the effect of private connections on forecast accuracy is due to information sharing between connected analysts.

Alternatively, I proxy for the strength of ties between analysts based on whether analysts share common expertise in their past employment. I define connected analysts as having common expertise if they were both sell-side or non-sell-side analysts in their shared past employment. Through partitioning the sample by past expertise, I find that the effect of private connections on forecast accuracy is positive and statistically

[3] I also find a statistically insignificant effect of private connections on forecast accuracy employing a placebo test that defines analysts as privately connected if they share past employment duration of less than a year.

significant for the subsample of analysts who share common past expertise but statistically insignificant for the subsample of analysts who differ in their past expertise. Overall, these results are consistent with stronger ties between analysts leading to private connections having a greater effect on forecast accuracy.

Second, I examine the role of herding in the relations between private connections and forecast accuracy. I partition the sample based on whether a forecast is a bold or a herding forecast, and I find a positive and statistically significant effect of private connections on forecast accuracy in both subsamples. In un-tabulated analyses, I also examine the timeliness of forecasts but find no significant differences between forecasts issued by privately connected analysts and those issued by unconnected analysts, which is inconsistent with herding. These results suggest that the association between private connections and forecast accuracy is unlikely to be explained by herding.[4] Interestingly, herding forecasts issued by privately connected analysts are more accurate than herding forecasts issued by unconnected analysts, consistent with herding forecasts issued by privately connected analysts being more likely to be based on private information transfer than being driven by career incentives.[5]

In further analyses, given that success in the sell-side analyst profession depends on more than research quality, I examine whether analysts' private connections yield tangible career benefits. I find that analysts' private connections are positively associated with the likelihood of being voted as future All-Star, after controlling for

[4] Herding to the consensus analyst forecast relies on public information and is a level playing field for all analysts. In contrast, private connections allow analysts to obtain private information, which gives a comparative advantage to their forecast accuracy relative to other analysts. Prior studies have shown that herding results in suboptimal forecasts with no increase in the analyst's *relative* forecast accuracy (i.e., Trueman 1994; Hong, Kubik, and Solomon 2000).

[5] An alternative explanation is that analysts are simply aware of their connected external peers and choose to follow their forecasts (Merton 1987). However, analysts should be more likely to follow more skilled and more experienced external peers, whereas I show that private connections allow two less skilled and less experienced analysts to improve their research by generating synergies. I also do not find evidence of privately connected analysts delaying their forecasts to "follow the leader."

various analyst and broker characteristics, including forecast accuracy and current All-Star status. In addition to All-Star status, I also document that privately connected analysts have greater career mobility and are more likely to switch from small brokers to larger and more prestigious brokers.[6] Overall, these results are consistent with private connections leading to positive career outcomes for analysts.

My study contributes to several streams of research. First, it contributes to the literature on analysts' social connections. Prior studies examining analysts' social connections have focused primarily on analysts' access to management (Malloy 2005; Chen and Matsumoto 2006; Ke and Yu 2006; Cohen et al. 2010; Brochet, Miller, and Srinivasan 2013; Mayew, Sharp, and Venkatachalam 2013; Green et al. 2014; Fang and Huang 2017; Rees, Sharp, and Wong 2017). In particular, Malloy (2005) and Cohen et al. (2010) show that analysts who are geographically closer to managers and analysts who are connected to managers via school ties have more informative research. While management is undeniably an important source of information for analysts, I show that private connection to external peers is also a valuable channel that allows analysts to improve their performance.[7] Li et al. (2019) also focus on analysts' connections to managers yet take the additional step to show that the connected analysts can disseminate information to other analysts through common institutional investors. In contrast to third-party dissemination channels, my study focuses on private connections as a direct channel of information transfer between analysts.[8]

---

[6] Through collaboration and information sharing, privately connected analysts can maintain their social ties and increase their chance of moving to a more prestigious broker.

[7] Employment-based private connection is distinct from other types of social connection in that analysts may share common expertise through their shared employment. The information channel may also be different between differently qualified analysts (i.e., junior/junior interactions versus junior/senior interactions).

[8] Private connections between analysts are important because analysts' connections to managers have been significantly limited by Regulation FD, whereas private connections continue to have a strong effect on analysts' forecast accuracy in the post-Regulation FD period. Between-analyst

Second, my study contributes to the long-standing debate on whether analysts learn from the forecasts of their peers. While the bulk of the evidence is of the view that analysts' herding forecasts are uninformed and driven by career concerns, a few recent studies have suggested that analysts can potentially learn both from each other's forecasts and from stock prices (Hong, Kubik, and Solomon 2000; Surowiecki 2004; Clements and Tse 2005; Bernhardt, Campello, and Kutsoati 2006; Bloomfield and Hales 2009; Guttman 2010; Clement, Hales, and Xue 2011; Cohn and Juergens 2014; Huang et al. 2017). For example, Clement et al. (2011) show that analysts learn from the mean market reactions and forecast revisions by other analysts following earnings announcements. Cohn and Juergens (2014) provide evidence of analysts influencing each other through their forecasts. Cen, Chang, and Dasgupta (2020) suggest that analysts learn from the mean forecasts of other analysts. In contrast to these studies, I provide evidence of analysts learning directly from their peers through informal private channels. I document that privately connected analysts are more accurate in both their herding and bold forecasts, suggesting that analysts are better able to incorporate information from their peers when they are privately connected.

My study extends the nascent literature on peer effects. Prior studies have provided evidence of peer effects for analysts within the same brokerage house (Groysberg and Lee 2008; Groysberg, Lee, and Nanda 2008; Groysberg and Lee 2010; Haushalter and Lowry 2011; Law 2013; Hugon, Kumar, and Lin 2015; Hugon, Lin, and Markov 2016; Hwang, Liberti, and Sturgess 2018; Do and Zhang 2020). In particular, Brown and Hugon (2009) document timelier yet less accurate forecasts for analysts working in teams within the brokerage house. Phua, Tham, and Wei (2018) find that

---

collaboration also entails more than dissemination of information from managers and includes analysts' collaboration on information processing and acquisition of private information such as through site visits and EDGAR (Fischer and Stocken 2010; Cheng et al. 2016; Jennings, Lee, and Matsumoto 2017; Han, Kong, and Liu 2018; Gibbons, Iliev, and Kalodimos 2021).

analysts who are more centrally located within a brokerage house produce more accurate forecasts. These studies show that analysts can improve their performance through access to in-house colleagues under the same institutional setting (e.g., teams and brokerage houses), whereas I document information transfer between analysts across institutional settings. I show that analysts' private connections to external peers have a greater effect on their productivity for analysts in smaller brokerages who lack sufficient internal resources.

Finally, my study contributes to the line of research on the determinants of analysts' performance. Prior studies have attributed the superior performance of analysts to various analyst, brokerage, and firm characteristics (Mikhail et al. 1997; Jacob et al. 1999; Clement et al. 1999, 2007; Sonney 2007; Clement 2009; Kini et al. 2009; Brown and Muhammad 2010; Hilary and Shen 2013). For example, Mikhail et al. (1997) and Clement et al. (2007) show that analysts' firm-specific and task-specific experience helps to improve their forecast accuracy. Jacob et al. (1999) find that analysts' forecast accuracy is a function of brokerage size and industry specialization. I extend this line of research to show that analysts' performance is also influenced by their private connections to external peers.

1.2 Literature Review and Hypotheses Development

1.2.1 Social Connections

The literature on analysts' social connections has focused primarily on their connections to managers using various proxies such as school ties, geographical proximity, and participation in conference calls and broker-hosted conferences (Malloy 2005; Chen and Matsumoto 2006; Ke and Yu 2006; Cohen et al. 2010; Mayew et al. 2013; Green et al. 2014; Soltes 2014; Cheng et al. 2016; Fang and Huang 2017; Rees et al. 2017; Li et al. 2019; Bradley, Gokkaya, and Liu 2020). For example, Cohen et al.

(2010) and Fang and Huang (2017) document that analysts with educational ties to managers have timelier and more accurate forecasts. Similarly, Malloy (2005) shows that analysts who are geographically closer to managers have more informative research. Likewise, Chen and Matsumoto (2006), Ke and Yu (2006), and Rees et al. (2017) find that analysts who are more favorable in their earnings forecasts and conference calls are granted closer access to managers resulting in more informative research. Although these studies suggest that analysts can gain an informational advantage through their social connections to managers, Regulation FD has significantly limited these types of information pathways.

Li et al. (2019) document that firms with closer connections between managers and analysts have more accurate consensus forecasts and lower forecast dispersion. Though still focusing on analysts' connections to managers, they take the additional step to show that analysts who are connected to managers can disseminate information to other unconnected analysts through common institutional clients. While their study suggests that analysts can disseminate information through third-party channels, they do not examine whether analysts can exchange information directly with their peers to improve their respective research.

1.2.2 Peer Effects

The few studies that have examined analysts' connections to peer analysts focus exclusively on the importance of brokerage-level resources on analysts' performance (Groysberg and Lee 2008; Groysberg et al. 2008; Groysberg and Lee 2010; Law 2013; Do and Zhang 2019). For example, Clement (1999) and Jacobs et al. (1999) document a positive link between the resources of the brokerage house and analysts' forecast accuracy. Similarly, Groysberg and Lee (2008, 2010) show that star analysts' performance depends not only on their innate ability, but also on the quality of their colleagues within the brokerage house. The presence of higher quality colleagues within the brokerage house offers analysts the opportunity to observe and learn, allowing them to improve their performance. Consistent with this, Groysberg et al. (2008) find that all-star analysts who switch employers experience an immediate decline in individual performance. Law (2013) provides evidence of career imprinting, in which sell-side analysts who work with more optimistic coworkers early in their career issue more optimistic research outputs throughout their career.

Moreover, other studies provide more direct evidence of analysts collaborating with each other within the brokerage house (Brown and Hugon 2009; Hwang et al. 2018; Phua et al. 2018; Fang and Hope 2020). For example, Hwang et al. (2018) examine analysts who cover multiple mergers and acquisitions (M&As) and find that the earnings forecasts for the merged firm are significantly more accurate when the analyst has an in-house colleague covering the target prior to the M&A. Notably, Brown and Hugon (2009) show that analysts covering the same industry often work in teams when covering larger companies. They document that earnings forecasts issued by analysts working in teams are less accurate yet timelier than those provided by individual analysts. In contrast, Fang and Hope (2020) find that analysts working in teams issue more accurate, frequent, and timely earnings forecasts than individual analysts and that the stock market reacts more strongly to forecasts issued by analyst teams. These studies show that resources within a brokerage house is an important contextual factor influencing analysts' performance, yet little is known about whether analysts can tap into the expertise of their external peers to improve the quality of their research.

Furthermore, prior studies have documented knowledge spillover between in-house peers of different specializations who possess complementary knowledge (Chen and Martin 2011; Haushalter and Lowry 2011; Hugon et al. 2015; Hugon et al. 2016). In particular, Chen and Martin (2011) provide evidence of information flow from commercial bankers to analysts. They show that analysts who are affiliated with the commercial bank experience an improvement in forecast accuracy when their covered firms borrow from the affiliated banks. In a similar vein, Haushalter and Lowry (2011) document information flow between the equity research and investment banking divisions of a firm. More recent studies show that analysts also benefit from interactions with in-house macroeconomists and in-house debt analysts. Analysts who have access to an active in-house macroeconomist are more efficient at incorporating macroeconomic news into their earnings forecast than analysts do not have access to an active in-house macroeconomist (Hugon et al. 2015). Likewise, analysts who have access to higher quality debt research are associated with more frequent and more accurate cash flow forecasts (Hugon et al. 2016).

Overall, these studies suggest that information flow within a brokerage house is an important information-acquisition channel for analysts. While close collaboration between colleagues is natural in the workplace, it is less obvious whether analysts in different brokerage houses can collaborate and learn from each other.

1.2.3 Learning from Public Information

Related to my study is the long-standing debate on whether analysts could learn from the forecasts of their peers. Early studies on herding are generally of the view that analysts are driven by career concerns and issue uninformed herding forecasts following the consensus forecasts of their peers (Trueman 1994; Hong, Kubik, and Solomon 2000; Welch 2000; Surowiecki 2004; Jegadeesh and Kim 2009). These studies suggest that career concerns are so pervasive that they largely negate incentives for analysts' learning from their peers. Accordingly, herding forecasts are based on little private information and are shown to be less accurate and less informative (Clements and Tse 2005).

However, a few recent studies have suggested that analysts can potentially learn both from each other's forecasts and from stock prices (Bloomfield and Hales 2009; Guttman 2010; Clement, Hales, and Xue 2011; Cohn and Juergens 2014; Huang et al. 2017; Cen, Chang, and Dasgupta 2020). In particular, Clement et al. (2011) document that analysts who are more efficient in extracting information from stock prices and other analysts' forecasts have more accurate forecasts. Specifically, they find that when the market reactions following earnings announcement and other analysts' revisions are more informative about future earnings changes, analysts who revise their earnings forecasts more strongly have more accurate forecasts. Similarly, Cen, Chang, and Dasgupta (2020) argue that analysts are likely incorporating idiosyncratic information from their peers' forecasts, as evidenced by more accurate consensus and individual forecasts when analysts' locations are geographically more dispersed. While analysts' learning from public information is a level playing field for all analysts, it remains an open question whether analysts can learn directly and privately from their external peers to achieve comparative advantage in their forecasts.

1.2.4 Hypotheses Development

I extend these streams of literature by examining how analysts' private connections to their external peers affect their performance. On the one hand, prior studies have shown that social connections between economic agents can facilitate learning and transfer of information by lowering information-gathering costs (Cohen et al. 2008, 2010; Christensen et al. 2016; Fang and Huang 2017; Cici, Shane, and Yang 2019; Li et al. 2019). In the analyst context, analysts who share similar past experiences should also be more likely to interact and share useful information for several reasons. First, having common past experiences helps bridge the distance between analysts by building trust and promoting a sense of common identity, which makes it more comfortable for analysts to interact. Second, having a shared social network allows analysts to build long-term contractual relationships through which they can exchange information and learn from each other. Analysts who are socially connected also have greater opportunities to socialize, which may enable voluntary or involuntary exchange or disclosure of information. Hence, I expect analysts who are privately connected to external peers to have an information advantage relative to unconnected analysts.

On the other hand, it is not obvious whether analysts will transfer knowledge outside the brokerage setting. Knowledge is expensive to acquire, reproduce, and transmit over long distances, and learning is often held to be an interactive process requiring face-to-face communication. The brokerage setting enables spatial proximity and ease of interactions, which makes social learning easier between analysts (Glaeser et al. 1992; Beckmann 1994; Von Hippel 1994; Audretsch and Feldman 1996; Cornish 1997; Massa and Rehman 2008). Groysberg et al. (2008) show that All-Star analysts who switch employers experience an immediate decline in performance, with the effect more pronounced for those who move solo without other team members. This suggests that knowledge transfer and social learning may be constrained by the boundaries of the brokerage house.

Furthermore, private connections may not always be constructive (Guan et al. 2016; He et al. 2017). Analysts connected to external peers with whom they are in competition may not necessarily receive useful information. Additionally, analysts who are connected to biased external peers may receive biased information driven by various conflicts of interest that their peers face, such as the incentive to generate investment banking revenues, increase trading profits, and obtain closer access to management. This negates any positive effect of information transfer through private connections and could even lead to an impairment of research quality.

Given the contrary predictions above, I make no directional prediction regarding the effect of private connections on analysts' research quality. Empirically, I measure analysts' research quality using the accuracy of their earnings forecasts. The prediction is formally stated as follows (in null form):

H1*: Analysts who are privately connected to their external peers do not issue more accurate earnings forecasts than those issued by unconnected analysts.*

My second set of predictions focuses on whether the relations between private connections and forecast accuracy differ depending on: 1) analysts' ability and experience and 2) analysts' resource constraints. I focus on whether the relations between private connections and forecast accuracy differ as a function of the average ability, experience, and resources of the analyst and her connected peers. On the one hand, analyst pairs of lower average ability, experience, and resources may derive greater synergy from working together, as these analysts have a greater need for complementary information to improve their respective research and thus are more codependent on each other. On the other hand, analyst pairs of low average ability, experience, and resources may not achieve greater synergy if they are unable to help each other, given their low ability, experience, and resources. Hence, I state my prediction as follows (in null form):

H2*: Analysts who are privately connected to external peers do not have greater earnings forecast accuracy when they are of lower average ability, experience, and resources relative to when they are of higher average ability, experience, and resources.*

1.3 Sample Selection and Method

1.3.1 Sample Selection

I employ data from several sources. I first obtain earnings forecast data from the Institutional Brokers Estimate System (I/B/E/S) for the period 1992 to 2017. I require analysts to be active on I/B/E/S for at least one year and limit earnings forecasts to annual forecasts that are issued 30 days prior to the firm's fiscal year-end date. I match the earnings forecast data with firm characteristics and stock price data from Compustat and the Center for Research in Security Prices (CRSP), as well as institutional holdings data from Thomson Reuters and analysts' All-Star rankings from *Institutional Investor* magazine.

I obtain analysts' employment history and educational background from LinkedIn and the Financial Industry Regulatory Authority (FINRA).[9] For all sell-side analysts employed between 1982 and 2016, I search for the corresponding analyst on the FINRA broker check search engine. I require the matching analysts to hold Series 86 or 87 licenses, which are FINRA-sponsored exams to ensure the competency of entry-level registered representatives to perform their job as research analysts. I then search the corresponding analysts on LinkedIn and merge FINRA employment data with LinkedIn employment and educational background data. Of the original 13,315 analysts, 1,846 matches are found on the FINRA broker check site, of which 1,161 matches are found on LinkedIn. This sample corresponds to 282,495 earnings forecast revisions after the inclusion of control variables.

I focus on FINRA analysts, who, on average, hold higher qualifications than the average I/B/E/S analyst (see Table 1). On average, FINRA analysts are more

[9] I do not use employment history based on the I/B/E/S dataset, as I/B/E/S data consist of employment history only for the most senior analysts and include only the experience of sell-side analysts. LinkedIn and FINRA employment data provide a complete history of an analyst's entire career and includes analysts' experience in non-financial services and non-sell-side financial services.

experienced in both general forecasting experience and firm-specific experience, more likely to be All-Star analysts, and more likely to be employed by larger brokers. They also issue more accurate, frequent, and bold forecasts, and cover a smaller number of industries. I expect my results to be unaffected by differences in these characteristics and generalizable to the overall I/B/E/S sample.

1.3.2 Test of Analysts' Forecast Accuracy

Using the following ordinary least squares (OLS) model, I test whether privately connected analysts have higher quality research than do unconnected analysts. I employ several different measures of analyst performance. The first measure is analysts' forecast accuracy. I predict that privately connected analysts are more likely to interact and obtain information, which enables them to produce more accurate forecasts than analysts who lack such connections.

$$\begin{aligned} Accuracy_{ijt} = {} & \alpha + \beta_1 PrivatelyConnected_{ijt} + \beta_2 InternallyConnected_{ijt} + \beta_3 LagAccuracy_{ijt} \\ & + \beta_4 AbsRevision_{ijt} + \beta_5 Bold_{ijt} + \beta_6 Numindustries_{ijt} + \beta_7 GenExperience_{ijt} \\ & + \beta_8 FirmExperience_{ijt} + \beta_9 Horizon_{ijt} + \beta_{10} DaysElapsed_{ijt} + \beta_{11} ForFrequency_{ijt} \\ & + \beta_{12} BrokerSize_{ijt} + \beta_{13} NumFirm_{ijt} + \beta_{14} Allstar_{ijt} + \epsilon_{ijt} \qquad (1), \end{aligned}$$

where *i* indexes analyst, *j* indexes firm, and *t* indexes day. I follow the approach in Clement and Tse (2005) by transforming each of the variables to a range from 0 to 1. *Accuracy* is calculated by subtracting *AFE* (the absolute difference between the analyst's earnings forecast and actual earnings) by the maximum *AFE* of all other analysts covering the firm in a given year, scaled by the yearly range (maximum value − minimum value).[10] For ease of interpretation, I multiply this by -1, so it can be interpreted as an accuracy measure:

[10] In alternative specifications, I also follow the approach in Clement (1999) by demeaning an analyst's forecast error by the average forecast error.

$$Accuracy = -\frac{AFE - maxAFE}{maxAFE - minAFE}$$

I measure connections between related analysts based on their past employment ties. I classify analysts as privately connected if they share past employment ties of more than one year and cover the same firm. I distinguish between privately connected analysts who share past employment ties and are no longer employed by the same brokerage (*PrivatelyConnected*) and analysts who share connections with other analysts who are currently employed by the same brokerage (*InternallyConnected*). I employ both an indicator variable and a continuous variable for the number of private connections. I expect a greater number of private connections to result in increased forecast accuracy.

I control for the absolute magnitude of the revision, *AbsRevision*, calculated as the absolute value of the difference between an analyst's forecast and her prior forecast, scaled by the stock price two days prior. A larger forecast revision is reflective of greater effort by analysts and is expected to be more accurate and informative. I also control for the boldness of the forecast (*Bold*), as prior studies have shown that bold forecasts are more likely to be based on private information and are associated with greater accuracy and informativeness (Trueman 1994; Hong, Kubik, and Solomon 2000; Welch 2000; Jegadeesh and Kim 2009). I classify earnings forecasts into bold forecasts and herding forecasts following the methodology of Clement and Tse (2005). Forecasts are classified as bold if they are above the analyst's prior forecast and the prerevision consensus forecasts, or below both. All other forecasts are classified as herding forecasts. The prerevision consensus forecast is calculated using the consensus forecasts of other analysts during the 90-day period prior to the forecast revision (robust to using various time periods—180 days and 365 days). I define the analyst's prior forecasts as the previous forecast issued by the same analyst, regardless of the time of issuance.[11]

Following prior literature, I control for various analyst- and broker-level characteristics, such as the analyst's prior-year forecast accuracy (*LagAccuracy*), the analyst's general forecasting experience (*GenExperience*), the analyst's firm-specific experience (*FirmExperience*), the number of days between the analyst's forecast and the actual earnings-per-share announcement (*Horizon*), the number of times the analyst issues forecasts for the firm during the year (*ForFrequency*), the length of time between the analyst's earnings forecast and the previous forecast issued by any analyst (*DaysElapsed*), whether the brokerage firm is a large broker based on whether the number of analysts employed is in the top 10% (*BrokerSize*), the number of firms that the analyst covers (*NumFirm*), whether the analyst is ranked as an All-Star analyst in the previous year (*Allstar*), whether the analyst has an investment banking affiliation with the covered firm (*Affiliate*), the number of in-house colleagues working within the analyst's industry sector (*TeamSize*), the quality of the analyst's in-house colleagues (*ColleagueQuality*), and the analyst's connection to managers (*ConnectedtoManager*). I control for the degree to which the analyst is an industry specialist by including *NumIndustries*—the number of Global Industry Classification Standard (GICS) industry groups that the analyst follows. I include industry and year fixed effects and cluster standard errors by analyst. I winsorize all continuous variables at the 1st and 99th percentiles to alleviate the effects of outliers.

For each of these analyst- and broker-level characteristics, I follow the approach in Clement and Tse (2005) to demean the characteristics relative to those of other

[11] In alternative specifications, I set the analyst's prior forecasts to missing if the forecast is the analyst's first forecast for the fiscal year and find robust results.

analysts covering the same firm in a given year.[12] Each characteristic is subtracted by the minimum value of the characteristic of all other analysts covering the same firm in a given year, scaled by the yearly range (maximum value − minimum value):

$$Characteristic = \frac{RawCharacteristic - MinRawCharacteristic}{MaxRawCharacteristic - MinRawCharacteristic}$$

1.3.3 Test of Variations in Analyst Ability, Experience, and Resources

Next, I examine whether the relations between analysts' private connections and forecast accuracy differ based on: 1) analysts' ability and experience and 2) analysts' resource constraints. I proxy for analysts' ability and experience using their past forecast accuracy, All-Star status, and firm-specific forecasting experience. I proxy for analysts' resource constraints using their broker size and the number of industries they cover. Analysts working for small brokerages and analysts covering a greater number of industries are more resource constrained at the broker level and industry level, respectively. I test my predictions using the following model:

$$\begin{aligned} Accuracy_{ijt} = \alpha &+ \beta_1 PrivatelyConnected_{ijt} + \beta_2 AvgAbility_{ijt} \\ &+ \beta_3 AvgAbility_{ijt} \times PrivatelyConnected_{ijt} + \text{Controls} + \epsilon_{ijt} \qquad (2). \end{aligned}$$

I employ the same set of control variables as model (1) and focus on the interaction term *PrivatelyConnected* × *AvgAbility*, where *AvgAbility* represents the average of the ability, experience, and resources of the recipient analyst and her connected external peers. I expect analyst pairs with low average ability, experience, or resources to realize greater synergy through their private connections with each other.

1.4 Main Results

[12] In alternative specifications, I follow the approach in Clement (1999) by subtracting each of these characteristics by the average of all other analysts covering the same firm in a given year, scaled by the same mean.

1.4.1 Descriptive Statistics

In Table 1 Panel A, I present the descriptive statistics for the full sample of 282,495 analyst-firm-day observations from 1992 to 2017. Consistent with prior studies, bold forecasts consist of 68% of all forecast revisions. The average forecast is issued approximately 211.76 days prior to the earnings announcement, and the average number of days between the forecast revisions of any analyst is 5.71. The average analyst in my sample has approximately 4.30 years of firm-specific experience and 9.92 years of general forecasting experience, covers approximately 18.20 firms, and issues approximately 5.69 forecasts per year. The average covered firm in my sample has an analyst following of 14.16, average market capitalization of $15.95 billion, and institutional holding of 75.52%.

Panel B presents the descriptive statistics for the private connection variables. Using the employment-based private connection measure, I find that approximately 25% of the forecasts in my sample are issued by privately connected analysts and that male analysts are, on average, less likely to have private connections (0.25) than are female analysts (0.28). Around 4% of the forecasts are issued by internally connected analysts who are currently employed by the same brokerage and share common past employment.

For analysts who are privately connected, the average number of private connections per connected analyst is 1.44, with a standard deviation of 0.86.[13] The average analyst has 2.59 years of shared past employment with her connected peers, and the average forecast is issued 6.61 years since the shared past employment. The sample consists of 1,022 analysts covering 5,033 firms, forming 21,729 unique analyst–firm pairs. Panel C aggregates the observations at the analyst, firm, and analyst–firm levels

[13] In addition to first-order private connections, each privately connected analyst is on, average, connected to 2.41 second-order external peers.

Table 1
Descriptive Statistics

This table presents the descriptive statistics. Panel A presents the descriptive statistics for the key variables in the empirical analyses. Panel B presents detailed descriptive statistics for the private connection variables. Panel C presents detailed descriptive statistics for the private connection variables partitioning by broker size. Panel D aggregates private connections at the analyst, firm, and analyst-firm level. Panel E presents descriptive statistics by the types of shared employment firms. All variables are defined in appendix A1. * indicates significant difference at the 5% level between the mean of FINRA and non-FINRA analysts in Panel A and between the mean of small and large brokers in Panel D.

Panel A Key Variables

Variable	Mean	Std. Dev.	25th	Median	75th
Accuracy	0.67*	0.28	0.48	0.74	0.90
LagAccuracy	0.82*	0.39	0.69	0.90	1.00
InternallyConnected	0.04	0.19	0.00	0.00	0.00
AbsRevision	0.00*	0.01	0.00	0.00	0.00
Bold	0.68*	0.47	0.00	0.00	1.00
Numindustries	0.26*	0.32	0.00	0.17	0.50
GenExperience	0.61*	0.29	0.37	0.63	0.88
FirmExperience	0.46*	0.32	0.18	0.42	0.75
Horizon	0.51	0.33	0.00	0.47	0.76
DaysElapsed	0.11	0.22	0.00	0.00	0.10
ForFrequency	0.59*	0.29	0.40	0.60	0.83
BrokerSize	0.72*	0.45	0.00	1.00	1.00
Numfirm	0.44*	0.27	0.24	0.40	0.62
Allstar	0.20*	0.40	0.00	0.00	0.00
Affiliated	0.01*	0.08	0.00	0.00	0.00
TeamSize	0.43*	0.41	0.00	0.32	0.95
ColleagueQuality	0.12*	0.16	0.00	0.00	0.21
ConnectedtoManager	0.04*	0.18	0.00	0.00	0.00
Size	8.19	1.73	6.95	8.10	9.38
Log(BM)	-0.97	0.76	-1.38	-0.87	-0.42
LnAnalyst	2.53	0.63	2.08	2.56	3.00
PriorRet	0.00	0.12	-0.05	0.01	0.06
PriorVol	2.95	5.52	0.34	0.99	2.86
PriorRetVolatility	0.02	0.02	0.01	0.02	0.03
InstOwn	0.76	0.20	0.63	0.79	0.91
BHAR (0, 2)	-0.00	0.07	-0.03	-0.00	0.03
NumInst	352.74	322.95	140.00	246.00	446.00
SwitchBroker	0.15	0.36	0.00	0.00	0.00
ToTopBroker	-0.01	0.35	0.00	0.00	0.00
Timeliness	0.06	0.18	0.00	0.00	0.02

Panel B Private Connections at the Analyst-Firm-Time Level

Variable	Mean	Std. Dev.	25th	Median	75th
PrivatelyConnected	0.25	0.43	0.00	0.00	0.00
PrivatelyConnected (Male)	0.25	0.43	0.00	0.00	0.00
PrivatelyConnected (Female)	0.28	0.45	0.00	0.00	1.00
NumPrivatelyConnected	0.36	0.76	0.00	0.00	0.00
Employment Ties / Connected Analyst	1.44	0.86	1.00	1.00	2.00
Shared Employment Duration	2.59	1.96	1.00	2.00	3.00
Years Since Shared Employment	6.61	3.95	4.00	6.00	9.00
SecondOrderConnected	0.71	1.91	0.00	0.00	1.00
SecondOrder Ties / Connected Analyst	2.80	2.94	1.00	2.00	3.00
SchoolTiesConnected	0.26	0.44	0.00	0.00	1.00
NumSchoolTiesConnected	0.30	0.54	0.00	0.00	1.00
School Ties / Connected Analyst	1.14	0.40	1.00	1.00	1.00

Panel C Mean Private Connections at the Analyst, Firm, and Analyst-Firm Level

Variable	Analyst	Firm	Analyst-Firm
PrivatelyConnected	0.47	0.43	0.30
SchoolTiesConnected	0.47	0.49	0.33

Panel D Private Connections By Broker Size at the Analyst-Firm-Time Level

	Small Broker		Large Broker	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
PrivatelyConnected	0.24*	0.42	0.26	0.44
NumPrivatelyConnected	0.34*	0.72	0.37	0.77
Employment Ties / Connected Analyst	1.43	0.81	1.44	0.88
Shared Employment Duration	2.27*	1.81	2.70	2.00
Years Since Shared Employment	7.69*	4.35	6.23	3.73
SecondOrderConnected	0.61*	1.64	0.75	1.94
SecondOrder Ties / Connected Analyst	2.59*	2.50	2.88	3.08
SchoolTiesConnected	0.21*	0.41	0.28	0.45
NumSchoolTiesConnected	0.24*	0.50	0.32	0.55
School Ties / Connected Analyst	1.15	0.40	1.14	0.41

Panel E Firms of Common Employment

Variable	# of Connections (Analyst-Firm-Year)	# of Connections (Analyst-Firm)	% of Connections (Analyst-Firm-Year)	% of Connections (Analyst-Firm)
Top Broker	22,230	8,353	0.47	0.47
Non-Top Broker	21,208	7,897	0.45	0.44
Non-Financial	4,124	1,517	0.09	0.09

and shows that approximately 47% of the analysts are privately connected, 43% of the firms are covered by privately connected analysts, and 30% of the analyst–firm pairs are covered by privately connected analysts.[14]

In Panel D, I present the descriptive statistics, partitioning by broker size. The asterisk (*) indicates statistically significant differences between the characteristics of large and small brokers at the $p < 0.05$ level. Approximately 24% of the forecasts are issued by privately connected analysts in small brokers, whereas 26% of the forecasts are issued by privately connected analysts in large brokers. For analysts who are privately connected, the number of private connections per connected analyst is similar across the two subsamples at 1.43 and 1.44, respectively. Privately connected analysts employed by large brokers also have a longer duration of shared past employment, less time passed since the last shared employment, and a higher number of second-order connections compared with privately connected analysts employed by small brokers. In Panel E, I present detailed descriptive statistics on the types of shared past employment—47% of the shared past employment is at a top broker, 44% of the shared past employment is at a non-top broker, and 9% of the shared employment is at a non-financial service firm.[15]

Table 2 reports the univariate correlations. Correlations that are statistically significant at the $p < 0.05$ level are flagged with an asterisk (*). Consistent with H1, I observe a positive and statistically significant correlation between *PrivatelyConnected* and *Accuracy*. *PrivatelyConnected* is also positively correlated with *BrokerSize*, *Allstar*,

[14] Using the alternative school-tie-based measure of private connections, I find that 26% of the forecasts in the sample are issued by privately connected analysts. The average number of private connections per connected analyst is 1.14, with a standard deviation of 0.40. Aggregating the observations at the analyst, firm, and analyst–firm levels, approximately 47% of the analysts are privately connected, 49% of the firms are covered by privately connected analysts, and 33% of the analyst–firm pairs are covered by privately connected analysts.

[15] A top broker is defined as belonging to one of following firms: Goldman Sachs, JP Morgan Chase, Barclays, Bank of America, Morgan Stanley, Deutsche Bank, Citigroup, Credit Suisse, UBS, and Wells Fargo.

Table 2 Correlation Matrix

This table reports univariate correlations for the key variables in the analyses. Only select variables are shown to reduce clutter. * denotes statistical significance at the 5% level.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
PrivatelyConnected [1]	1.00										
Accuracy [2]	0.01*	1.00									
Accuracy_lag [3]	-0.01*	0.01*	1.00								
BrokerSize [4]	0.02*	0.01*	0.03*	1.00							
Allstar [5]	0.04*	0.01*	0.01*	0.25*	1.00						
GenExperience [6]	0.03*	0.04*	-0.00	0.02*	0.23*	1.00					
FirmExperience [7]	-0.00*	0.05*	-0.00	0.01*	0.19*	0.39*	1.00				
Numindustries [8]	0.04*	-0.02*	-0.05*	-0.07*	-0.01*	0.04*	0.05*	1.00			
Horizon [9]	0.00	-0.43*	0.00*	-0.01*	-0.02*	-0.08*	-0.11*	-0.00*	1.00		
DaysElapsed [10]	-0.03*	-0.04*	-0.01*	0.01*	0.00	0.00*	0.02*	0.03*	-0.08*	1.00	
ForFrequency [11]	-0.01*	-0.02*	0.01*	0.05*	0.02*	-0.07*	0.03*	0.00*	-0.09*	0.10*	1.00

GenExperience, and *NumIndustries* and negatively correlated with *FirmExperience*, *DaysElapsed*, and *ForFrequency*, highlighting the need to control for these variables in my multivariate analyses. Consistent with prior studies, *Accuracy* is positively correlated with *BrokerSize*, *GenExperience*, *FirmExperience*, and *Allstar*, and negatively correlated with *NumIndustries*, *Horizon*, and *DaysElapsed*.

1.4.2 Analyst Forecast Accuracy: Full Sample Analyses

Table 3 presents the results of the analyses on analyst forecast accuracy. Panel A shows the results for the full sample using Clement and Tse's (2005) measure of relative forecast accuracy.[16] In my main specification, I include controls for time-varying analyst and broker characteristics, as well as industry and year fixed effects to account for time-invariant industry characteristics and temporal variations in factors that affect analyst forecast properties. The standard errors are clustered by analyst to address cross-sectional dependence.[17] My main result in Column (1) indicates a positive and statistically significant coefficient on *PrivatelyConnected*, which suggests that analysts have more accurate forecasts when they are privately connected to external peer analysts covering the same firm. This evidence is consistent with private connections enhancing information transfer and learning between analysts, which leads to more accurate forecasts.

In terms of economic magnitude, the beta coefficients indicate that having private connections is associated with a 0.0139 increase in relative forecast accuracy, which is greater or comparable in magnitude to many of the prior known determinants of forecast accuracy, such as past forecast accuracy (0.0071), number of industries

[16] The results are robust to using the approach in Clement (1999) in which each continuous variable is subtracted by the average of all forecasts covering the same firm in the fiscal year and scaled by the same mean.

[17] The results are robust to alternative two-way clustering by analyst and year, firm and year, or analyst and firm (Petersen 2009; Gow, Ormazabal, and Taylor 2010).

Table 3
Private Connections and Forecast Accuracy

This table reports the association between analysts' private connections and their relative forecast accuracy. Panel A reports the results using the full sample. Panel B reports the results using a subsample of analysts with past employment ties. Columns (1) and (2) of Panel B report the results by restricting the control sample to analysts who have past employment ties with external peers who do not currently cover the same firm or industry sector. Columns (3) and (4) report the results by restricting the control sample to analysts who share past employment ties with external peers who currently cover the same industry sector, yet not the same firm. Panel C reports the results partitioning by the average broker size of the analyst pairs. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A Private Connections and Relative Forecast Accuracy Using the Full Sample

	(1)	(2)	(3)	(4)	(5)
PrivatelyConnected	**0.0091***	**0.0088***	**0.0069**	**0.0075**	**0.0103***
	(2.81)	**(2.73)**	**(2.30)**	**(2.50)**	**(2.76)**
InternallyConnected	-0.0016	-0.0015	0.0035	0.0000	-0.0077
	(-0.31)	(-0.29)	(0.48)	(0.00)	(-1.40)
LagAccuracy	0.0051*	0.0051*	0.0073	0.0045	0.0033
	(1.69)	(1.69)	(1.63)	(1.54)	(1.15)
AbsRevision	0.6771***	0.6837***	0.9200***	0.7056***	0.7947***
	(9.38)	(9.46)	(8.83)	(9.87)	(11.44)
Bold	0.0168***	0.0168***	0.0135***	0.0164***	0.0167***
	(10.83)	(10.82)	(6.17)	(10.69)	(10.93)
Numindustries	-0.0141***	-0.0136***	-0.0165***	-0.0132***	-0.0126***
	(-3.54)	(-3.42)	(-2.96)	(-3.52)	(-2.87)
FirmExperience	0.0017	0.0018	0.0016	-0.0005	-0.0031
	(0.46)	(0.48)	(0.28)	(-0.13)	(-0.83)
GenExperience	0.0033	0.0041	0.0049	-0.0005	0.0174*
	(0.65)	(0.80)	(0.75)	(-0.11)	(1.75)
Horizon	-0.3704***	-0.3702***	-0.3760***	-0.3712***	-0.3713***
	(-93.61)	(-93.57)	(-75.69)	(-94.58)	(-94.43)
DaysElapsed	-0.0758***	-0.0752***	-0.0727***	-0.0727***	-0.0717***
	(-23.87)	(-23.69)	(-14.90)	(-23.95)	(-23.80)
ForFrequency	-0.0544***	-0.0546***	-0.0480***	-0.0544***	-0.0631***
	(-12.91)	(-12.98)	(-8.49)	(-15.31)	(-17.23)
BrokerSize	0.0102***	0.0079**	-0.0007	0.0081*	0.0060*
	(3.58)	(2.37)	(-0.14)	(1.69)	(1.67)
NumFirm	-0.0264***	-0.0257***	-0.0243***	-0.0276***	-0.0346***

	(-5.49)	(-5.37)	(-3.73)	(-6.18)	(-7.00)
Allstar	0.0068**	0.0040	0.0014	0.0064*	0.0045
	(1.99)	(1.11)	(0.33)	(1.91)	(1.12)
Affiliated		-0.0388***	-0.0235		
		(-4.97)	(-1.51)		
TeamSize		0.0073**	0.0015		
		(2.45)	(0.34)		
ColleagueQuality		0.0085*	0.0065		
		(1.68)	(1.29)		
ConnectedtoManagers			0.0068*		
			(1.70)		
FE (Industry)	Yes	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes	Yes
FE (Broker)	No	No	No	Yes	No
FE (Analyst)	No	No	No	No	Yes
Observations	282,495	282,495	113,455	282,495	282,495
R-squared	0.205	0.205	0.219	0.212	0.223

Panel B Private Connections and Relative Forecast Accuracy for the Subsample of Analysts with Past Employment Ties

	(1)	(2)	(3)	(4)
	Past Employment Ties Only		Past Employment Ties and Same Industry Sector Coverage	
PrivatelyConnected	**0.0082****	**0.0075****	**0.0076****	**0.0066****
	(2.53)	**(2.34)**	**(2.28)**	**(2.01)**
InternallyConnected	0.0002	0.0008	-0.0027	-0.0026
	(0.04)	(0.15)	(-0.45)	(-0.44)
LagAccuracy	0.0055*	0.0036	0.0036	0.0027
	(1.68)	(1.16)	(1.00)	(0.76)
AbsRevision	0.6310***	0.6943***	0.6139***	0.6570***
	(8.16)	(9.23)	(6.84)	(7.53)
Bold	0.0160***	0.0162***	0.0142***	0.0145***
	(9.72)	(9.89)	(7.93)	(8.08)
Numindustries	-0.0159***	-0.0161***	-0.0147***	-0.0158***
	(-3.82)	(-4.18)	(-3.27)	(-3.74)
FirmExperience	0.0021	0.0015	-0.0004	0.0001
	(0.52)	(0.39)	(-0.09)	(0.01)
GenExperience	0.0037	0.0045	0.0006	-0.0001
	(0.67)	(0.86)	(0.10)	(-0.02)
Horizon	-0.3674***	-0.3676***	-0.3655***	-0.3658***
	(-87.23)	(-87.20)	(-79.73)	(-79.30)
DaysElapsed	-0.0769***	-0.0751***	-0.0795***	-0.0788***
	(-22.67)	(-22.78)	(-20.77)	(-20.55)
ForFrequency	-0.0537***	-0.0567***	-0.0482***	-0.0520***
	(-11.75)	(-14.08)	(-10.44)	(-11.96)
BrokerSize	0.0100***	0.0075**	0.0076**	0.0055
	(2.81)	(2.18)	(2.07)	(1.43)
NumFirm	-0.0259***	-0.0273***	-0.0300***	-0.0295***
	(-5.09)	(-6.08)	(-5.62)	(-6.07)
Allstar	0.0067*	0.0056	0.0063*	0.0065*
	(1.93)	(1.64)	(1.74)	(1.77)
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
FE (Cohort)	No	Yes	No	Yes
Observations	253,411	253,411	199,696	199,696
R-squared	0.204	0.213	0.204	0.212

Panel C Private Connections and Relative Forecast Accuracy Partitioning by the Average Broker Size of Analysts Pairs

	(1) Small Brokers	(2) Large Brokers
PrivatelyConnected	**0.0166*** **	**0.0068***
	(3.23)	**(1.81)**
InternallyConnected	-0.0075	0.0012
	(-0.51)	(0.23)
LagAccuracy	0.0047	0.0050
	(0.93)	(1.31)
AbsRevision	0.7905***	0.6270***
	(6.79)	(7.47)
Bold	0.0159***	0.0172***
	(6.19)	(9.60)
Numindustries	-0.0104	-0.0158***
	(-1.58)	(-3.44)
FirmExperience	-0.0009	0.0017
	(-0.15)	(0.39)
GenExperience	-0.0002	0.0056
	(-0.02)	(0.94)
Horizon	-0.3692***	-0.3709***
	(-57.03)	(-81.84)
DaysElapsed	-0.0792***	-0.0744***
	(-13.43)	(-20.48)
ForFrequency	-0.0493***	-0.0573***
	(-6.26)	(-13.40)
NumFirm	-0.0347***	-0.0216***
	(-4.00)	(-4.10)
Allstar	0.0049	0.0058*
	(0.62)	(1.65)
FE (Industry)	Yes	Yes
FE (Year)	Yes	Yes
Observations	85,670	196,825
R-squared	0.205	0.207
Chi-square difference in coeff.	**3.12 (p < 0.10)**	

covered (-0.0158), firm-specific experience (0.0019), general forecasting experience (0.0034), broker size (0.0179), and All-Star status (0.0096). The coefficients on the control variables are consistent with prior studies. The positive and statistically significant coefficients on *LagAccuracy*, *Bold*, *GenExperience*, *BrokerSize*, and *Allstar* suggest that analysts with greater past forecast accuracy, analysts who issue bold forecasts, All-Star analysts, analysts with more experience, and analysts employed by large brokers have greater forecast accuracy. The negative and statistically significant coefficients on *NumIndustries*, *Horizon*, *DaysElapsed*, and *Numfirm* suggest that analysts who cover a greater number of industries, issue forecasts with longer horizons, have a greater number of days elapsed since issuing the previous forecast, and cover a greater number of firms have lower forecast accuracy.

In Column (2), I include additional controls related to analysts' access to brokerage-level resources and analysts' bias, such as *TeamSize*—the number of in-house colleagues working in the analyst's industry sectors; *ColleagueQuality*—the quality of analysts' colleagues, defined as the percentage of in-house colleagues ranked as All-Star by *Institutional Investor* magazine in the analyst's industry sector; and *Affiliated*—whether the analyst has affiliation bias for the covered firm (Lin and McNichols 1998; Groysberg and Lee 2008; Hwang and Kim 2009; Hugon et al. 2016; Do and Zhang 2020). Consistent with prior studies, the negative and statistically significant coefficient on *Affiliated* suggests that analysts issue more biased forecasts for firms with investment banking affiliations (-0.0388, *t*-stat -4.97). The positive and statistically significant coefficients on *TeamSize* and *ColleagueQuality* suggest that analysts' in-house colleagues have a positive effect on their performance (0.0073, *t*-stat 2.45; 0.0085, *t*-stat 1.68, respectively). I continue to find a strong positive and statistically significant coefficient on *PrivatelyConnected*, which is 27.10% and 91.25%

larger in economic magnitude compared with the beta coefficients on *TeamSize* and *ColleagueQuality*, respectively.

In Column (3), I further control for analysts' connections to managers of the covered firm (Cohen et al. 2008). The managers are based on the Execucomp sample of the top 1,500 firms. *ConnectedToManager* is defined as 1 if the analyst shares prior educational ties with the CEO, CFO, or chairperson of the firm, and 0 otherwise. The economic magnitude on *PrivatelyConnected* is 37.54% greater than the economic magnitude on *ConnectedToManager* using beta coefficients. Further, un-tabulated results show that, in the post-Regulation FD period, the effect of analysts' connections to managers is insignificant, while private connections continue to have strong effects on analysts' forecast accuracy.

As alternative specifications, in Columns (4) and (5) I further add broker and analyst fixed effects to control for time-invariant broker and analyst characteristics, respectively. While the results indicate that analysts with private connections have more accurate forecasts relative to other analysts within the broker who do not have private connections, and relative to other firms in their portfolio for which they do not have private connections, I note that private connections vary relatively slowly over time within a brokerage and within an analyst and are highly collinear with brokerage and analyst fixed effects.[18] Thus, I do not include broker and analyst fixed effects in my main specification, and instead control for analyst and brokerage characteristics that are only slightly correlated with private connections. Overall, I interpret the results in Panel A as consistent with analysts' private connections enhancing the quality of their research.[19]

[18] Adding analyst-year or broker-year fixed effects to the regression yields insignificant results.
[19] In un-tabulated results, I repeat the analyses in Table 3 Panel A by replacing *PrivatelyConnected* with a continuous measure of the number of private connections (*NumPrivatelyConnected*) and find similar results.

1.4.3 Restricting the Control Sample to Analysts with Shared Past Employment Ties

In Panel B, I rerun the analyses by restricting the sample to only analysts with shared past employment ties. In other words, I eliminate from the sample analysts who do not have common past employment with other analysts outside of the brokerage. In Columns (1) and (2), I compare analysts privately connected to external peers covering the same firm with analysts who have past employment ties with external peers not covering the same firm or industry by restricting the control sample to analysts who do not currently cover the same firm or industry as their external peers. In Columns (3) and (4), I further compare analysts privately connected to external peers covering the same firm with analysts who have past employment ties with external peers covering the same industry, yet not the same firm, by restricting the control sample to analysts who share past employment ties with external peers and cover the same industry sector. In addition, I add cohort fixed effects (institution of past shared employment × beginning overlap year) in Columns (2) and (4) to control for institution-specific career experience among analysts, and narrow the comparison to among analysts with shared past employment at the same institution in the same year. This allows me to test for the effect of private connections, controlling for analysts' past institution-specific experience. In all four columns, the coefficients on *PrivatelyConnected* remain positive and statistically significant, suggesting that the effect of private connections is not explained by analysts' past institution-specific experiences.

1.4.4 Partition by Broker Size

Given that analysts employed by small brokers have limited internal resources compared with analysts employed by large brokers, I examine whether private connection is more important to analysts facing greater resource constraints. I partition the sample by the broker size of the privately connected analysts based on whether the average of the number of analysts employed by the broker of the recipient analyst and her connected external peers is in the top 10%, and rerun model (1) separately for the two subsamples. I expect the improvement in forecast accuracy due to private connections to be greater for analyst pairs employed by small brokers.

Panel C presents the results of this test. Column (1) reports the results for analyst pairs employed by small brokers and Column (2) reports the results for analyst pairs employed by large brokers. While *PrivatelyConnected* is positively associated with forecast accuracy in both columns, the effect of private connections for analyst pairs employed by small brokers is greater than the effect of private connections for analyst pairs employed by large brokers (0.0166, *t*-stat 3.23; 0.0068, *t*-stat 1.81, respectively). A chi-square test of differences on *PrivatelyConnected* across the two columns is statistically significant, consistent with analysts at smaller brokers being better able to utilize private connections to augment limited internal resources. Overall, the evidence in Panel C is consistent with private connections playing a greater role for analyst pairs facing brokerage-level resource constraints.

1.4.5 Variations in Average Ability, Experience, and Resources

Table 4 reports the results of the cross-sectional regressions of analyst pairs' average ability, experience, and resources on forecast accuracy. Columns (1) to (3) focus on the cross-sectional analyses of analyst pairs' average ability and experience, as proxied by the average of the past forecast accuracy, All-Star status, and firm-specific experience of the recipient analyst and her connected external peers. Column (1) shows a negative and statistically significant coefficient on *PrivatelyConnected* × *AvgLagAccuracy* (-0.0136, *t*-stat -2.05), consistent with analyst pairs of lower average past forecast accuracy benefitting more from their private connections than analyst pairs of higher average past forecast accuracy. Column (2) shows a negative and statistically significant coefficient on *PrivatelyConnected* × *AvgAllstar* (-0.0127, *t*-stat -1.93), consistent with analyst pairs who are, on average, non-All-Star benefitting more from their private connections. Similarly, Column (3) reports a negative and statistically significant coefficient on *PrivatelyConnected* × *AvgFirmExperience* (-0.0011, *t*-stat -1.74), consistent with analyst pairs who are, on average, less experienced gaining more through their private connections. In other words, analyst pairs of less ability and experience are able to obtain greater synergy than are analyst pairs of higher ability and experience.

With respect to analysts' resource constraints, Column (4) shows a positive and statistically significant coefficient on *PrivatelyConnected* × *AvgNumIndustries* (0.0069, *t*-stat 3.03), which suggests that analyst pairs who cover, on average, a greater number of industries have greater improvements in forecast accuracy through their private connections with each other.[20] Overall, the results from Table 4 are consistent with analyst pairs of lower skill, experience, and resources benefitting the most from their private connections, as these analysts derive greater synergy from their private connections.[21]

[20] Consistent with the results in Table 3 Panel C, I also find a negative and statistically significant coefficient on *PrivatelyConnected* × *AvgBrokerSize* (-0.0100, *t*-stat -1.72), suggesting that analysts employed by smaller brokers derive greater synergies from their private connections to external peers.

[21] I also find a greater association between private connections and forecast accuracy when the forecasted firm has received lower investor attention, as proxied by the number of institutional owners and past trading volume.

Table 4
Cross-sectional Analyses: Average Ability, Experience, and Resources of Analyst Pairs

This table reports the results of the cross-sectional tests related to the average ability, experience, and resources of the analyst pairs on relative forecast accuracy. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. *AvgAllstar* is the average of the All-Star status of the recipient analyst and her connected peers. *AvgLagAccuracy* is the average of the relative past forecast accuracy of the recipient analyst and her connected peers. *AvgFirmExperience* is the average of the firm-specific experience of the recipient analyst and her connected peers. *AvgNumindustries* is the average of the number of industries covered by the recipient analyst and her connected peers. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)
PrivatelyConnected	0.0198***	0.0119***	0.0138***	-0.0073
	(3.25)	(3.19)	(3.25)	(-1.23)
AvgLagAccuracy	0.0091***	0.0052*	0.0052*	0.0091***
	(2.59)	(1.69)	(1.69)	(2.59)
AvgAllstar	0.0048	0.0084**	0.0046	0.0051
	(1.41)	(2.07)	(1.37)	(1.50)
AvgFirmExperience	0.0018***	0.0018***	0.0021***	0.0018***
	(5.59)	(5.59)	(5.60)	(5.63)
AvgNumindustries	-0.0004	-0.0004	-0.0004	-0.0019
	(-0.35)	(-0.37)	(-0.33)	(-1.64)
PrivatelyConnected x AvgLagAccuracy	**-0.0136****			
	(-2.05)			
PrivatelyConnected x AvgAllstar		**-0.0127***		
		(-1.93)		
PrivatelyConnected x AvgFirmExperience			**-0.0011***	
			(-1.74)	
PrivatelyConnected x AvgNumindustries				**0.0069*****
				(3.03)
Other Controls	Yes	Yes	Yes	Yes
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
Observations	282,495	282,495	282,495	282,495
R-squared	0.205	0.205	0.205	0.205

1.4.6 Exogenous Variations in Private Connections

1.4.6.1 Terminations of Connected Peer Analysts' Coverage

In this section, I focus on the case of connected peer analysts terminating their coverage for mutually covered firms to identify a source of variation that gives rise to plausibly exogenous changes in analysts' private connections. If the effect of private connections on forecast accuracy is indeed due to the support that analysts receive from their connected peers, then stock coverage termination by connected peer analysts should negatively affect the recipient analysts' forecast accuracy for the same firm.[22]

I first identify all stock terminations by the connected peer analysts. I require the connected peer analysts to *permanently* drop coverage of the mutually covered firm to ensure that there is a permanent loss of support from the connected peer analysts to the recipient analysts with regards to the mutually covered firm.[23] I define the event year as the year in which the connected peer analysts permanently drops coverage of the mutually covered firm. This is obtained using the last date that the analyst issues a forecast for a given firm on the I/B/E/S tape. I then examine the changes in forecast accuracy for the recipient analyst before and after the event using various windows around the event year.

Note that I require the recipient analyst to cover the firm in both the pre- and post-period to ensure that the results are not confounded by the recipient analyst dropping coverage of the firm. I also require the recipient analyst to be employed by the same broker before and after the event to ensure that the results are not confounded by analysts switching brokers. In un-tabulated analyses, I find a statistically significant

[22] If, on the other hand, information transfer is one-way rather than two-way between the privately connected analysts, stock coverage termination by connected peer analysts may not necessarily affect the recipient analysts' forecast accuracy for the same firm.
[23] Relaxing this criterion to allow analysts to terminate their stock coverage for a minimum of two years yields similar results.

drop in the forecast accuracy of the recipient analyst following stock coverage terminations of her connected peers using the (-2, +2) year window.

1.4.6.2 Termination of Connected Peer Analysts' Employment

Next, I conduct further analyses to ensure that the stock coverage terminations are exogenous and not driven by firm-specific factors. Given that the departure of an analyst from equity research is less likely to be driven by firm-specific fundamentals, I require coverage terminations to coincide with the simultaneous departure of the connected peer analysts from equity research. I retain only coverage terminations that coincide in the same year as the analyst's *permanent* departures. The analyst's departure year is defined as the year in which the analyst issues her last forecast for any firms on the I/B/E/S tape.

Column (1) of Table 5 Panel A presents the results of this analysis. Consistent with my prediction, Column (1) indicates a statistically significant decrease in the forecast accuracy of the recipient analyst (- 0.0593, *t*-stat - 2.19) following the departure of her connected peers from equity research using the (-2, +2) year window.[24]

1.4.6.3 Termination of Connected Peer Analysts' Coverage Due to Brokerage Dropping Coverage of Industry Sectors

Alternatively, to mitigate the likelihood that the connected peer analysts' coverage termination decisions are not endogenously driven by firm-specific factors, I follow an approach similar to Kelly and Ljungvist (2007) and construct a set of coverage

[24] The drop in forecast accuracy is robust across alternative event windows: (-1, +1) and (-3, +3) years. The drop in forecast accuracy is greater using the longer (-2, +2) or (-3, +3) year windows, suggesting that the effect of the loss of external peer support becomes more pronounced after one year. Connected peer analysts could still provide partial support to the recipient analyst in the short-run even after dropping coverage of the mutually covered stock.

Table 5

Coverage Terminations by Connected Peer Analysts

This table reports the results of the analyses related to exogenous changes in analysts' private connections due to coverage terminations by connected peer analysts. Panel A reports the effect of coverage terminations by connected peer analysts on the relative forecast accuracy of the recipient analyst covering the same firm. Panel B reports the difference-in-differences regression of relative forecast accuracy for treatment analysts affected by coverage terminations of connected peer analysts versus control analysts unaffected by the shocks. Column (1) reports the results using coverage terminations due to the connected peer analysts departing equity research. Column (2) reports the results using coverage terminations due to brokerages dropping coverage of the connected peer analysts' industry sector. Column (3) reports the results using coverage terminations due to brokerage closures. *Post* is an indicator equal to 1 if the forecast is issued in the two years following the connected peer analysts' coverage terminations, and 0 if the forecast is issued in the two years prior to the connected peer analysts' coverage terminations. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A Terminations of Connected Peer Analysts' Coverage on Relative Forecast Accuracy of the Recipient Analyst

	(1)	(2)	(3)
	Analyst Employment Terminations	Broker Sector Terminations	Broker Closures
Post	-0.0593**	-0.0447**	-0.0676**
	(-2.19)	(-2.11)	(-2.01)
Controls	Yes	Yes	Yes
FE (Analyst-Firm)	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes
Observations	10,591	4,602	1,006
R-squared	0.444	0.470	0.493

Panel B Difference-in-differences Regressions of Relative Forecast Accuracy

	(1)	(2)	(3)
	Analyst Employment Terminations	Broker Sector Terminations	Broker Closures
Post	0.0603**	-0.0231	-0.0018
	(2.18)	(-0.76)	(-0.07)
Treatment	-0.0319	-0.0378	0.0111
	(-1.30)	(-0.71)	(0.24)
Post x Treatment	**-0.0800****	**-0.1313*****	**-0.1120*****
	(-2.24)	**(-3.46)**	**(-2.85)**
Controls	Yes	Yes	Yes
FE (Analyst-Firm)	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes
Observations	10,226	6,196	1,382
R-squared	0.514	0.527	0.541

terminations that are the result of brokerage terminations of entire industry sectors. Brokers typically downsize their operations in response to adverse changes in the cost of producing research, which is unlikely to be driven by firm-specific fundamentals.[25] Consistent with this, Kelly and Ljungvist (2007) report that the broker's choice of sector terminations does not reflect prior or future firm performance. Following Kelly and Ljungvist (2007), I define industry sectors using the six-digit GICS industry sector code and obtain the last date on the I/B/E/S tape in which the broker covers any stocks in that specific GICS industry sector. I constrain my sample to coverage terminations due to brokerage terminations of entire industry sectors by retaining only analyst coverage terminations in which the broker simultaneously drops coverage of that industry sector within the same year. The results reported in Column (2) are consistent with the results in Column (1).

1.4.6.4 Termination of Connected Peer Analysts' Coverage Due to Brokerage Closures

To further tighten the criterion and arrive at a set of coverage terminations that can be treated as plausibly exogenous, I also examine coverage terminations due to brokerage closures. The use of brokerage closures as an exogenous shock is common in the prior literature (Kelly and Ljungvist 2007; Hong and Kacperczyk 2010; Kelly and Ljungqvist 2012). For each broker, I identify the last date that the broker appears on the I/B/E/S tape and constrain the connected peer analysts' coverage terminations to correspond to the year of the brokerage closures. The results reported in Column (3) are consistent with those of other shocks.

[25] I note that industry sector termination by a single broker is unlikely to affect the entire market's competitiveness. Further, any changes in market competitiveness affect all analysts equally and should not change analysts' *relative* forecast accuracy.

1.4.6.5 Propensity Score Matching

I use a differences-in-difference regression to compare the change in forecast accuracy of analysts affected by each of the three plausibly exogenous shocks with analysts who are not influenced by these shocks. The control sample comes from the set of observations excluding those analysts who have connected peer analysts terminating coverage of the mutually covered stock due to any of the above shocks. I use propensity score matching to pair each treatment with control using the same set of analyst- and broker-level determinants of forecast accuracy in model (1). I match the treatment and control analysts one-to-one using the pre-event year $t-2$ and require the analysts to cover the same firm during the same year.

I compare the changes in the forecast accuracy of the treatment analysts versus the control analysts two years before and two years after the shocks. The results are presented in Panel B. For all three shocks, I document negative and statistically significant coefficients on *Post* × *Treatment*, implying that analysts who are exposed to the treatment experience greater decrease in their forecast accuracy than do analysts who are not affected by the treatment. Overall, the results in Table 5 provide strong evidence that analysts' private connections positively affect their forecast accuracy, as the loss of such connections leads to decreases in the forecast accuracy of the recipient analysts.

1.4.7 Market Implications of Private Connections: Post-Forecast Revision Drift

Next, I employ a market-based test to examine whether the market behaves as if it views the forecast revisions of privately connected analysts to be of higher quality. I estimate the following model:

$$BHAR_{jt} = \alpha + \beta_1 PrivatelyConnected_{ijt} + \beta_2 Revision_{ijt} + \beta_3 PrivatelyConnected_{ijt} \times Revision_{ijt} + \text{Controls} + \epsilon_{ijt} \quad (3),$$

where *i* indexes analyst, *j* indexes firm, and *t* indexes day. My dependent variable, *BHAR (0,2)*, is calculated as the buy-and-hold returns measured over the three-day period beginning on the forecast announcement date and adjusted using the Carhart four-factor model. I similarly calculate the buy-and-hold abnormal returns three months and six months following the forecast revision. Similar to Gleason and Lee (2003), I include controls for firm size (*LogMV*), book-to-market ratio (*BM*), and momentum in the month ending five days prior to the forecast announcement date (*PriorRet*). I include industry, year, and day-of-the-week fixed effects and cluster standard errors by the forecast date.

Table 6 Panel A presents the results of these analyses. Column (1) reports a positive and significant coefficient on *PrivatelyConnected* × *Revision* (0.1186, *t*-stat 2.57) in regression of three-day cumulative abnormal returns (*BHAR*), consistent with analysts' differentially perceiving forecasts issued by privately connected analyst compared to forecasts issued by unconnected analysts. Columns (2) and (3) report the buy-and-hold abnormal (*BHAR*) returns three months and six months following the forecast revisions, respectively. In both columns, the coefficients on *PrivatelyConnected* × *Revision* are positive and statistically significant, indicating evidence of delayed price reaction during the three- and six-month post-forecast revision window. The estimated coefficients are large (0.2842 to 0.5732) compared with the coefficient in Column (1) (0.1186), suggesting that the market significantly underreacts to forecasts of privately connected analysts.

Since newly formed private connections may be more difficult to detect, I rerun the analyses partitioning the sample by the median length of time since an analyst became privately connected following her departure from the shared employment. Panel B provides the results of this test. Columns (1) and (2) report the results for the below-median subsample, and Columns (3) and (4) report the results for the above-median subsample. I find strong evidence of investor underreaction to forecasts issued by

Table 6
Private Connections and Post-Forecast Revision Drift

This table reports the market reaction to forecasts of privately connected analysts versus unconnected analysts. Panel A reports the results of the analyses using the full sample. Column (1) presents the results for the Carhart 4-factor adjusted buy-and-hold abnormal returns (BHAR) over the three-day period beginning on the forecast revision date. Columns (2) and (3) present the results for the Carhart 4-factor model adjusted buy-and-hold abnormal return (BHAR) for the three- and six-month window following the forecast revisions, respectively. Panel B reports the results of the analyses partitioning by the length of time since an analyst became privately connected following her departure from the shared employment. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. All other variables are defined in Table A1. t-statistics are based on standard errors clustered by forecast date. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A Private Connections and Post-Forecast Revision Drift Using the Full Sample

	(1) BHAR (0, 2)	(2) BHAR 3-month	(3) BHAR 6-month
PrivatelyConnected	-0.0012***	-0.0015	-0.0023
	(-3.59)	(-1.30)	(-1.06)
PrivatelyConnected x Revision	**0.1186****	**0.2842****	**0.5732****
	(2.57)	**(2.00)**	**(2.13)**
Revision	0.7105***	-0.1436	-0.5708
	(9.03)	(-0.57)	(-1.19)
Log(MV)	0.0010***	0.0056***	0.0178***
	(8.32)	(14.16)	(23.63)
Log(MV) x Revision	-0.0238**	-0.1126***	-0.1696**
	(-2.25)	(-3.17)	(-2.51)
Log(BM)	-0.0044***	0.0013	0.0134***
	(-14.13)	(1.31)	(6.59)
Log(BM) x Revision	-0.1844***	0.1648*	0.2113
	(-7.15)	(1.94)	(1.28)
PriorRet	-0.0030	-0.0178**	-0.0395**
	(-1.39)	(-2.24)	(-2.55)
PriorRet x Revision	0.2580**	1.1772***	1.7148**
	(1.99)	(2.89)	(2.16)
FE (Industry)	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes
FE (Day of Week)	Yes	Yes	Yes
Observations	282,495	282,495	282,495
R-squared	0.021	0.021	0.021

Panel B Private Connections and Post-Forecast Revision Drift Partitioning by the Length of Time Since an Analyst Became Privately Connected

	Below Median		Above Median	
	(1)	(2)	(3)	(4)
	BHAR (0, 2)	BHAR 3-month	BHAR (0, 2)	BHAR 3-month
PrivatelyConnected	-0.0013***	-0.0034*	-0.0013***	0.0004
	(-2.99)	(-1.91)	(-2.78)	(0.28)
PrivatelyConnected x Revision	**0.0290**	**0.3850****	**0.2164*****	**0.1911**
	(0.51)	**(2.01)**	**(3.46)**	**(0.98)**
Revision	0.6785***	-0.3024	0.7449***	-0.2895
	(8.52)	(-0.94)	(9.30)	(-0.88)
Log(MV)	0.0009***	0.0059***	0.0009***	0.0056***
	(7.75)	(12.42)	(7.53)	(12.03)
Log(MV) x Revision	-0.0197*	-0.0990**	-0.0275**	-0.0963**
	(-1.84)	(-2.20)	(-2.56)	(-2.14)
Log(BM)	-0.0045***	0.0008	-0.0044***	0.0010
	(-14.17)	(0.66)	(-13.71)	(0.79)
Log(BM) x Revision	-0.1871***	0.2116*	-0.1662***	0.2366**
	(-7.07)	(1.85)	(-6.33)	(2.17)
PriorRet	-0.0038*	-0.0241**	-0.0022	-0.0250**
	(-1.79)	(-2.33)	(-1.04)	(-2.48)
PriorRet x Revision	0.1739	1.3263**	0.2198*	1.6234***
	(1.31)	(2.45)	(1.68)	(3.06)
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
FE (Day of Week)	Yes	Yes	Yes	Yes
Observations	244,724	244,724	249,659	249,659
R-squared	0.020	0.020	0.021	0.021

privately connected analysts for the below-median subsample, but not for the above-median subsample, thereby suggesting that the market underreacts to a greater extent to newly formed private connections that are more difficult to detect. Overall, these results provide strong evidence of a post-forecast revision drift associated with privately connected forecasts, indicating that investors have difficulty processing privately connected forecasts in the short term.

1.5 Additional Analyses

1.5.1 Strength of Ties

As additional analyses, I examine whether the effect of private connections on analysts' forecast accuracy is more pronounced when the strength of ties between analysts is greater. I proxy for the strength of ties between analysts using the duration of their shared past employment, defined as the number of years in which the privately connected analysts are employed by the same firm in the past. For analysts with multiple private connections, *Duration* is calculated as the average number of years in which analysts share employment ties with their connected peers.

Table 7 Panel A presents the results of this test. Columns (1) and (2) partition the sample by the median duration, and Columns (3) and (4) partition the sample by the bottom and top quartile duration. While Column (4) shows a positive and statistically significant effect of private connections on forecast accuracy using the top quartile subsample, Column (3) shows a statistically insignificant effect of private connections on forecast accuracy using the bottom quartile subsample, suggesting that private connections are ineffective at improving forecast accuracy for connected analysts with weak strength of ties. A chi-square test of differences on *PrivatelyConnected* between Columns (3) and (4) is statistically significant, implying that a greater strength of ties between analysts leads to a greater effect of private connections on forecast accuracy.

Table 7
Strength of Ties

This table reports the association between analysts' private connections and their relative forecast accuracy partitioning by the strength of ties between privately connected analysts. Panel A reports the results partitioning by the duration of shared past employment between privately connected analysts. Columns (1) and (2) partition by the median duration, and Columns (3) and (4) partition by the bottom and top quartile of duration. Panel B reports the results partitioning by whether the privately connected analysts share common past expertise in their shared past employment. Column (1) presents the results for privately connected analysts who are both sell-side analysts in their shared past employment. Column (2) presents the results for privately connected analysts who are both non-sell-side analysts in their shared past employment. Column (3) presents the results for privately connected analyst who share common past expertise, defined as privately connected analysts who are either both sell-side or both non-sell-side in their shared past employment. Column (4) presents the results for privately connected analysts who differ in their shared past expertise. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A Duration of Shared Past Employment

	(1) Below Median	(2) Above Median	(3) Bottom Quartile	(4) Top Quartile
PrivatelyConnected	0.0089*	0.0117***	0.0046	0.0104**
	(1.87)	(2.59)	(0.93)	(2.31)
Controls	Yes	Yes	Yes	Yes
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
Observations	236,152	240,329	213,015	228,603
R-squared	0.208	0.209	0.212	0.210

Panel B Common Past Expertise

	(1) Both Sell-side	(2) Both Non-sell-side	(3) Same Expertise	(4) Different Expertise
PrivatelyConnected	0.0091***	0.0146**	0.0093***	0.0062
	(2.71)	(2.06)	(2.83)	(1.21)
Controls	Yes	Yes	Yes	Yes
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
Observations	272,191	216,881	277,223	217,121
R-squared	0.206	0.212	0.206	0.211

Alternatively, I proxy for the strength of ties between analysts based on whether analysts share common expertise in their past employment. Analysts who share common past expertise are more likely to have working relationships in the past, and thereby more likely to collaborate with each other in the future. I partition the sample based on whether the privately connected analysts share common past expertise and rerun model (1) separately for the subsamples. I determine common expertise based on whether both analysts are sell-side analysts or non-sell-side analysts in their shared past employment.

Panel B presents the results of this test. Columns (1) through (3) show a positive and statistically significant effect of private connections on forecast accuracy for the subsamples of privately connected analysts who are both sell-side analysts, both non-sell-side analysts, or either both sell-side analysts or non-side analysts in their shared past employment, respectively. However, Column (4) shows a statistically insignificant effect of private connections on forecast accuracy using the subsample of privately connected analysts who differ in their past expertise. A chi-square test of differences on *PrivatelyConnected* between Columns (1)-(3) and Column (4) is statistically significant. This suggests that private connections are more effective at improving forecast accuracy for connected analysts who share common past expertise than analysts who do not share common past expertise, consistent with a greater strength of ties between analysts leading to a greater effect of private connections on forecast accuracy.

1.5.2 Test of Herding

Next, I test for the alternative explanation that herding explains the relations between private connections and forecast accuracy. I partition the sample based on whether a forecast is a bold or a herding forecast and rerun model (1) separately for the subsamples. Table 8 presents the results of this test. I document a positive and statistically significant effect of private connections on forecast accuracy for both subsamples, which mitigates the likelihood that herding drives the relations between private connections and forecast accuracy. In addition, while prior studies have shown that herding forecasts are less accurate and more likely to be driven by career concerns, I document that herding forecasts issued by privately connected analysts are in fact more accurate than herding forecasts issued by unconnected analysts. This suggests that not all herding forecasts are driven by career incentives, as those issued by privately connected analysts are more likely to be based on information transfer.

1.5.3 Diversity Analyses

While the main analyses suggest that analyst pairs of low average ability, experiences, and resources generate greater synergies through their private connections, it is not clear whether the effect of private connection differs as a function of the diversity within each privately connected pair. I conduct several additional analyses to better understand the dynamics between the privately connected pair.

First, I examine whether the relations between private connections and forecast differ as a function of the diversity of analysts' ability, experience, and resources constraints vis-à-vis their connected external peers. On the one hand, analysts with greater diversity in ability, experience, and resources might complement each other in their respective research and derive greater synergies through cooperation. On the other hand, analysts who have greater diversity in ability, experience, and resources may have less in common and be less likely to work together than analysts who are more homogenous, leading to a dampening of the effect of private connections on their respective research quality. Similar to model (2), I test for the role of diversity in analysts' ability, experience, and resources using the following model:

Table 8
Test of Herding

This table reports the association between analysts' private connections and their relative forecast accuracy partitioning by whether the forecast is a herding forecast. I define a forecast as bold if it is above the analyst's prior forecast and the prerevision consensus forecasts, or below both. All other forecasts are classified as herding forecasts. Columns (1) presents the results for the subsample of herding forecasts, and Columns (2) presents the results for the subsample of bold forecasts. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Herding Forecasts	(2) Bold Forecasts
PrivatelyConnected	0.0076*	0.0095***
	(1.89)	(2.88)
Controls	Yes	Yes
FE (Industry)	Yes	Yes
FE (Year)	Yes	Yes
Observations	89,352	193,143
R-squared	0.189	0.211

$$Accuracy_{ijt} = \alpha + \beta_1 PrivatelyConnected_{ijt} + \beta_2 DivAbility_{ijt}$$
$$+ \beta_3 DivAbility_{ijt} \times PrivatelyConnected_{ijt} + \text{Controls} + \epsilon_{ijt} \quad (4).$$

I focus on the interaction term *PrivatelyConnected* × *DivAbility*, where *DivAbility* represents the absolute difference in the ability, experience, and resources of the recipient analyst and her connected external peers. Table 9 presents the results of the analyses. While Column (1) indicates an insignificant coefficient on *PrivatelyConnected* × *AvgLagAccuracy*, Columns (2) and (3) report negative and statistically significant coefficients on *PrivatelyConnected* × *DivAllstar* and *PrivatelyConnected* × *DivFirmExperience* (-0.0112, *t*-stat 1.68; -0.0011, *t*-stat 1.80, respectively), consistent with a greater effect of private connections on forecast accuracy for analysts who are more homogenous in their ability and experience. Column (4) reports a positive and statistically significant coefficient on *PrivatelyConnected* × *DivNumindustries* (0.0042, *t*-stat 2.30), suggesting that analysts who differ in whether they are industry specialists have a greater effect of private connections on their forecast accuracy. Further analyses show that this is due to analysts who cover multiple industries having limited resources to devote to each industry and thus relying on their private connections with industry specialists to mitigate disadvantages arising from limited industry-level resources. Overall, the results in Table 9 are consistent with a greater effect of private connections on forecast accuracy for analysts with more homogeneity in ability and experience.

Table 9
Cross-sectional Analyses: Diversity

This table reports the results of the cross-sectional analyses of the diversity in ability, experience, and resources of the recipient analyst and her connected peers on relative forecast accuracy. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. *DivLagAccuracy* is the absolute difference in relative past forecast accuracy between the recipient analyst and her connected peers. *DivAllstar* is the absolute difference in All-Star status between the recipient analyst and her connected peers. *DivFirmExperience* is the absolute difference in firm-specific experience between the recipient analyst and her connected peers. *DivNumindustries* is the absolute difference in the number of industries covered by the recipient analyst and her connected peers. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)
PrivatelyConnected	0.0150**	0.0115***	0.0144***	0.0015
	(2.55)	(3.10)	(3.47)	(0.31)
DivLagAccuracy	0.0045			
	(0.78)			
DivAllstar		0.0112		
		(1.59)		
DivFirmExperience			0.0031***	
			(6.70)	
DivNumindustries				0.0010
				(0.75)
PrivatelyConnected x DivLagAccuracy	**-0.0079**			
	(-1.60)			
PrivatelyConnected x DivAllstar		**-0.0112***		
		(-1.68)		
PrivatelyConnected x DivFirmExperience			**-0.0011***	
			(-1.80)	
PrivatelyConnected x DivNumindustries				**0.0042****
				(2.30)
Controls	Yes	Yes	Yes	Yes
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
Observations	282,495	282,495	282,495	282,495
R-squared	0.205	0.205	0.206	0.205

1.5.4 Cross-sectional Analyses of Analysts' Potential for Improvement vis-à-vis Connected External Peers

In addition to diversity, I also examine whether the relations between private connections and forecast accuracy differ as a function of the recipient analysts' ability, experience, and resources relative to that of their connected external peers. From the information sharing angle, less skilled, less experienced, and more resource-constrained analysts are low producers of quality information and are more likely to be information importers than information exporters. Private connections to higher quality external peers would allow these analysts to narrow the information gap and improve their research quality. This implies that analysts who are less skilled, less experienced, and more resource-constrained relative to their connected external peers are likely to derive greater benefits through private connections in terms of their research quality. On the other hand, if higher quality external peers are unable or unwilling to share information with lower quality analysts due to spatial distance or lack of complementarity, then an analysts' level of ability, experience, and resources relative to their connected peers should not affect the relations between private connection and forecast accuracy. Similar to the previous analyses, I employ the following model:

$$Accuracy_{ijt} = \alpha + \beta_1 PrivatelyConnected_{ijt} + \beta_2 DiffAbility_{ijt} + \beta_3 DiffAbility_{ijt} \times PrivatelyConnected_{ijt} + \text{Controls} + \epsilon_{ijt} \quad (5).$$

I focus on the interaction term *PrivatelyConnected* × *DiffAbility*, where *DiffAbility* represents the "signed" gap in ability, experience, and resources between the recipient analyst and her connected external peers (*MeanAbilityConnectedPeer-AbilityRecipientAnalyst*). A positive and statistically significant coefficient on *PrivatelyConnected* × *DiffAbility* would indicate that analysts with greater gaps in ability, experience, and resources relative to their connected external peers experience a greater improvement in forecast accuracy through their private connections.

Table 10 presents the results of the cross-sectional analyses on the "signed" gap in ability, experience, and resources between the recipient analyst and her connected external peers. Columns (1) and (2) report positive and statistically significant coefficients on *PrivatelyConnected* × *DiffLagAccuracy* and *PrivatelyConnected* × *Allstar* (0.0080, *t*-stat 1.66; 0.0098, *t*-stat 1.87, respectively), consistent with a greater effect of private connections on forecast accuracy for analysts of lower ability relative to their connected external peers. Column (3) reports a positive and statistically significant coefficient on *PrivatelyConnected* × *DiffFirmExperience* (0.0019, *t*-stat 3.43), indicating that analysts who are less experienced relative to their connected peers have a greater effect of private connections on forecast accuracy. Lastly, Column (4) reports a negative and statistically significant coefficient on *PrivatelyConnected* × *DiffNumindustries* (-0.0027, *t*-stat -1.73), suggesting that analysts who cover greater number of industries relative to their connected peers have a greater effect of private connections on forecast accuracy. Overall, the results in Table 10 suggest that analysts with greater gaps in ability, experience, and resources relative to their connected peers have greater potential to improve their research quality through their private connections.

Table 10

Cross-sectional Analyses: Potential for Improvement vis-à-vis Connected External Peer

This table reports the cross-sectional analyses of the "signed" gap in relative ability, experience, and resources between the recipient analyst and her connected external peers on relative forecast accuracy. *PrivatelyConnected* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm in year *t*, and 0 otherwise. *DiffAllstar* is the *signed* difference in All-Star status between the average connected peer analyst and the recipient analyst, and is equal to 1 if the recipient analyst is a non-All-Star and her connected peers are on average All-Star, 0 if the recipient analyst and her connected peers are both either All-Star or non-All-Star, and -1 if the recipient analyst is an All-Star and her connected peers are on average non-All-Star. *DiffLagAccuracy* is the *signed* difference in relative past forecast accuracy between the average connected peer analyst and the recipient analyst. *DiffFirmExperience* is the *signed* difference in firm-specific experience between the average connected peer analyst and the recipient analyst. *DiffNumindustries* is the *signed* difference in the number of industries covered by the average connected peer analyst and the recipient analyst. All other variables are defined in Appendix A1. Continuous variables are demeaned and transformed to a range from 0 to 1 using the approach in Clement and Tse (2005). t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)	(3)	(4)
PrivatelyConnected	0.0152***	0.0114***	0.0190***	0.0057
	(2.78)	(3.11)	(5.30)	(1.33)
DiffLagAccuracy	-0.0073			
	(-1.17)			
DiffAllstar		-0.0134		
		(-1.64)		
DiffFirmExperience			-0.0029***	
			(-6.57)	
DiffNumindustries				-0.0010
				(-0.81)
PrivatelyConnected x DiffLagAccuracy	**0.0080***			
	(1.66)			
PrivatelyConnected x DiffAllstar		**0.0098***		
		(1.87)		
PrivatelyConnected x DiffFirmExperience			**0.0019*****	
			(3.43)	
PrivatelyConnected x DiffNumindustries				**-0.0027***
				(-1.73)
Controls	Yes	Yes	Yes	Yes
FE (Industry)	Yes	Yes	Yes	Yes
FE (Year)	Yes	Yes	Yes	Yes
Observations	282,495	282,495	282,495	282,495
R-squared	0.205	0.205	0.206	0.205

1.5.5 Career Outcomes

1.5.5.1 All-Star Status

Given that success in the sell-side analyst profession depends on more than just research quality, I examine whether analysts' private connections yield tangible career benefits. I first focus on the effect of private connections on analysts' All-Star status. Analysts are ranked annually by institutional investors on how much value they provide to the buy-side. These All-Star rankings are associated with better compensation and career advancement and are a good proxy for analysts' career success (Asquith, Mikhail, and Au 2005; Groysberg, Healy, and Maber 2011).

Prior literature has shown that analysts' ties to firm managers increase their likelihood of being voted as All-Star (Cohen et al. 2010; Fang and Huang 2017). I predict that analysts' private connections should also enhance their likelihood of being voted as All-Star by expanding their ties to institutional investors through peer analysts. Institutional investors may also be more willing to judge analysts favorably according to the analysts' social capital (private connections). Using the following model, I test whether private connections are positively associated with All-Star status in the following year, controlling for forecast accuracy and various broker- and analyst-level characteristics related to the probability of favorable career outcomes:

$$\begin{aligned} FutureAllStar = \alpha &+ \beta_1 PrivatelyConnectedAggregated_{it} \\ &+ \beta_2 InternallyConnectedAggregated_{it} + \beta_3 MeanAccuracy_{it} \\ &+ \beta_4 MeanLagAccuracy_{it} + \beta_5 Numindustries_{it} + \beta_6 GenExperience_{it} \\ &+ \beta_7 MeanFirmExperience_{it} + \beta_8 MeanForFrequency_{it} + \beta_9 BrokerSize_{it} \\ &+ \beta_{10} MeanNumFirm_{it} + \beta_{11} Allstar_{it} + \epsilon_{it} \qquad (6), \end{aligned}$$

where *i* indexes analyst and *t* indexes time. All continuous variables are defined at the analyst-year level and calculated as the average of the firms in the analysts' portfolio in year *t*. *PrivatelyConnectedAggregated* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm for any firms in her portfolio in year *t*, and 0 otherwise.[26] Given that All-Star status is likely subject to a high degree of autocorrelation, I control for current All-Star status in the model.

Table 11 presents the results of the analyses. Consistent with my prediction, Column (1) indicates a positive association (0.0512, *t*-stat 2.98) between private connections and future All-Star status after controlling for various analyst- and broker-level characteristics. The results are unchanged in Column (2) after further controlling for analysts' current year All-Star status. These results are consistent with private connections leading to positive career outcomes for analysts.[27]

1.5.5.2 Analyst Turnover

In addition to future All-Star status, I examine how private connections affect analysts' future career outcome by focusing on analysts' future turnovers. I predict that privately connected analysts have greater employment mobility and more favorable career-enhancing turnovers than do unconnected analysts as a result of their connections with related analysts employed by prospective employers.

Following Groysberg and Lee (2008), I distinguish between two types of turnover: analysts switching to a competitor's research department and analysts exiting from equity research. I focus on turnover due to analysts switching to competitors' research departments, which are more likely to relate to favorable career changes when

[26] Alternatively, I measure private connections using the log of the number of firms in the analysts' portfolio for which they have private connections and find similar results.

[27] Through partitioning by the median of the mean number of institutional owners in an analyst's portfolio of covered firms, I find that private connections have a greater effect on future All-Star status for analysts with access to a low number of institutional investors. This suggests that the positive association between private connections and future All-Star status is in part due to analysts using private connections to expand their access to institutional investors and benefitting from the social capital that their connected peers provide.

Table 11
Private Connections and Future All-star Status

This table reports the association between analysts' private connections and their one-year ahead All-star status. *PrivatelyConnectedAggregated* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm for any firms in his portfolio in year *t*, and 0 otherwise. All variables are at the analyst-year level and defined in Appendix A1. t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1)	(2)
PrivatelyConnectedAggregated	**0.0512*** **	**0.0142** **
	(2.98)	**(2.11)**
InternallyConnectedAggregated	-0.0042	-0.0167
	(-0.20)	(-1.37)
MeanAccuracy	0.1032***	0.0659***
	(3.44)	(3.40)
MeanLagAccuracy	0.0037	0.0058
	(0.36)	(0.97)
Numindustries	-0.0752***	-0.0272**
	(-2.71)	(-2.47)
MeanFirmExperience	0.1729***	-0.0090
	(5.20)	(-0.66)
GenExperience	0.1357***	0.0370***
	(4.43)	(3.02)
MeanForFrequency	0.1246***	0.0953***
	(5.85)	(7.87)
BrokerSize	0.1927***	0.0504***
	(15.02)	(9.61)
NumFirm	0.1293***	0.0353***
	(3.83)	(2.73)
AllStar		0.7352***
		(52.70)
FE (Industry)	Yes	Yes
FE (Year)	Yes	Yes
Observations	8,452	8,452
R-squared	0.185	0.617

the turnover is from a smaller broker to a larger broker. Using the following model, I test whether privately connected analysts are more likely to switch from a small broker to a large broker, controlling for various analyst- and broker-level characteristics:

$$
\begin{aligned}
ToTopBroker_{it} = {} & \alpha + \beta_1 PrivatelyConnectedAggregated_{it} \\
& + \beta_2 InternallyConnectedAggregated_{it} + \beta_3 MeanAccuracy_{it} \\
& + \beta_4 MeanLagAccuracy_{it} + \beta_5 Numindustries_{it} + \beta_6 GenExperience_{it} \\
& + \beta_7 MeanFirmExperience_{it} + \beta_8 MeanForFrequency_{it} + \beta_9 BrokerSize_{it} \\
& + \beta_{10} MeanNumFirm_{it} + \beta_{11} Allstar_{it} + \epsilon_{it} \qquad (7),
\end{aligned}
$$

where *i* indexes analyst and *t* indexes time. All continuous variables are defined at the analyst-year level and calculated as the average of the firms in the analyst's portfolio in year *t*. *ToTopBroker* is defined as 1 if the analyst moves to a larger broker in the following year, 0 if the analyst moves to a similar-sized broker in the following year, and -1 if the analyst moves to a smaller broker in the following year.

Table 12 presents the results of the analyses. In Column (1), I first test whether private connection is positively associated with analysts' likelihood of switching brokers by regressing *SwitchBroker* on *PrivatelyConnected*, controlling for various analyst- and broker-level characteristics. *SwitchBroker* is defined as 1 if the analyst switches to a different broker in the following year, and 0 otherwise. Column (1) reports a positive and statistically significant coefficient on *PrivatelyConnected* (0.0172, *t*-stat 1.76), consistent with analysts with private connections having greater career mobility.

In Column (2), I test whether private connections are associated with positive career outcomes by regressing changes in broker size between the prospective employer and past employer on private connections, controlling for various analyst- and broker-level characteristics. Column (2) indicates a positive and statistically significant coefficient on private connections (0.0203, *t*-stat 2.12), consistent with turnovers of privately connected analysts being more likely to involve moving to a more prestigious

Table 12
Private Connections and Analyst Turnover

This table reports the association between analysts' private connections and their future turnovers. Column 1 reports the association between analysts' private connections and the likelihood of analysts switching brokers in the following year. Column 2 reports the association between analysts' private connections and likelihood of analysts moving to a top broker in the following year. *SwitchBroker* is defined as 1 if the analyst switches to a different broker in the following year, and 0 otherwise. *ToTopBroker* is the signed difference in broker size, defined as 1 if the analyst moves to a larger broker in the following year, 0 if the analyst moves to a similar-sized broker in the following year, and -1 if the analyst moves to a smaller broker in the following year. *PrivatelyConnectedAggregated* is an indicator equal to 1 if the analyst shares past employment ties of more than one year with external peers covering the same firm for any firms in his portfolio in year *t*, and 0 otherwise. All variables are at the analyst-year level and defined in Appendix A1. t-statistics are based on standard errors clustered by analyst. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) SwitchBroker	(2) ToTopBroker
PrivatelyConnectedAggregated	**0.0172***	**0.0203****
	(1.76)	**(2.12)**
InternallyConnected	0.0125	0.0266
	(0.68)	(1.61)
Accuracy	-0.2952***	0.0701*
	(-7.27)	(1.75)
LagAccuracy	0.0040	-0.0065
	(0.38)	(-0.64)
Numindustries	-0.0222	-0.0604***
	(-1.38)	(-3.62)
FirmExperience	0.0330	-0.0355
	(1.40)	(-1.52)
GenExperience	-0.0315*	-0.0171
	(-1.85)	(-0.94)
ForFrequency	-0.4208***	0.0923***
	(-20.93)	(5.06)
BrokerSize	-0.0315***	-0.3007***
	(-3.40)	(-27.02)
NumFirm	-0.0891***	0.0863***
	(-4.74)	(4.52)
Allstar	-0.0116	0.0583***
	(-1.08)	(5.99)
FE (Industry)	Yes	Yes
FE (Year)	Yes	Yes
Observations	8,452	8,452
R-squared	0.134	0.160

broker.[28] Overall, as in the All-Star analyses, the evidence in Table 12 suggests that private connections are related to positive career advancement.

1.6 Supplementary Analyses

The evidence thus far is consistent with analysts' private connections being associated with better forecast accuracy and career outcomes. In supplementary analyses, I gauge the robustness of these results and include additional un-tabulated tests for alternative measures of private connections. Section 1.6.1 considers alternative measures of private connections. Section 1.6.2 gauges the robustness of my results. Sections 1.6.3 through 1.6.9 provide further extensions of my results.

1.6.1 Alternative Measures

1.6.1.1 Second-order Connections

I first examine whether analysts' second-order connections affect the accuracy of their forecasts. To capture second-order connections, I create a variable *SecondOrderConnected,* defined as the total number of second-order connections of an analyst. I document a positive association between second-order connections and forecast accuracy, with the effect being more pronounced for analysts with lower past forecast accuracy and non-All-Star analysts.

1.6.1.2 School-ties-based Private Connections

I also employ an alternative measure of private connections based on school ties. I define *SchoolTiesConnected* as 1 if analysts who cover the same firm are connected

[28] For privately connected analysts, 21% of the broker turnovers are from small to large brokers, 60% of the broker turnovers are between similar sized brokers, and 19% of the broker turnovers are from large to small brokers. For unconnected analysts, 16% of the broker turnovers are from small to large brokers, 20% of the broker turnovers are from large to small brokers, and 64% of the broker turnovers are between brokers of similar size.

through common educational ties and are currently employed by different brokers, and 0 otherwise. I find results consistent with my employment-based measure of private connections.

1.6.1.3 Industry- and Analyst-level Private Connections

In addition to defining private connections at the analyst-firm-year level, I examine an alternative measure of private connections defined at the analyst-industry-year level. I classify analysts as privately connected if they share past employment ties and cover the same industry in the same year (*IndPrivatelyConnected*). Compared with the firm-level measure of private connections, almost 71% of the forecasts in the sample are issued by analysts who are privately connected and covering the same industry. Due to the high proportion of analysts who are classified as privately connected, I focus both on a binary measure and a continuous measure of the number of private connections for my analyses (*NumIndPrivatelyConnected*). *NumIndPrivatelyConnected* has a mean of 3.07 and standard deviation of 3.59. I find that the effect of private connections on forecast accuracy is robust to using the industry-level measure of private connections, which is consistent with information transfer being both industry-specific and firm-specific.

I also define private connections broadly at the analyst level. Under this approach, analysts who share past employments are classified as privately connected regardless of whether they cover the same firm or industry (*GenPrivatelyConnected*). Compared with the firm-level private connection measure, in which only around 25% of analysts are privately connected and each analyst is connected to an average of 0.33 peer analysts, at the analyst level, almost 90% of analysts have at least one private connection and each analyst is connected to an average of 28.40 peer analysts. I compare the three alternative measures of the private connection variable (firm, industry, and analyst level) by including them in the same regression and find that only the firm-level measure of private connection continues to be significantly associated with forecast accuracy. Thus, controlling for firm-level private connections, the incremental value of industry- and analyst-level connections is comparatively small.

1.6.2 General Robustness

1.6.2.1 Revision Types and Concurrent Information Events

I conduct several tests to gauge the robustness of my results. First, I explore whether my results are dependent on the types of earnings revisions issued by analysts. I partition earnings revisions into earnings increases and earnings decreases and rerun the analyses separately for the two subsamples. The results are robust for both earnings increases and earnings decreases.

Second, I examine whether the results are dependent on concurrent information events. I partition analysts' forecast revisions into those issued standalone and those issued within the five days following information events such as quarterly and annual earnings announcements (EA) and management earnings guidance (MF). I report positive and statistically significant associations between private connections and forecast accuracy for both the *EA*=0 and *EA*=1 subsamples and for both the *MF*=0 and *MF*=1 subsamples, suggesting that the effect of private connections on forecast accuracy is not dependent on concurrent information events.

1.6.2.2 Shared Employment Types

I also examine whether the types of shared past employment affect the relations between private connections and forecast accuracy. I classify private connections into three types based on the firm of shared past employment- top broker, small broker, and non-financial service firm. I report a positive and statistically significant coefficient on

PrivatelyConnected in regressions of forecast accuracy across all three types of private connections.

1.6.3 Timeliness of Forecasts

I provide several additional extensions of my main analyses. First, I examine whether there is a tradeoff between accuracy and timeliness for forecasts issued by privately connected analysts. Since privately connected analysts are more likely to rely on their private connections for generating forecasts, they may delay their forecasts to wait for the "leader" resulting in less timely forecasts. Following Brown and Hugon (2009), I calculate the timeliness of forecasts (*Timeliness*) as the number of days between the immediately preceding forecast and the forecast of interest divided by the number of days between the immediately succeeding forecast and the forecast of interest, demeaned by all analysts covering firm j in year t. I do not find a statistically significant difference in the timeliness of forecasts between privately connected analysts and unconnected analysts. When controlling for the timeliness of forecasts, I continue to find a positive and statistically significant effect of private connections on forecast accuracy.

1.6.4 Test of Nonlinearity

Next, I test whether the effect of private connections on forecast accuracy is monotonic by including both the number of private connections and its squared term in my regression of forecast accuracy. If a greater number of private connections facilitates even greater information sharing, then the effect of private connections on forecast accuracy may be an increasing function of the number of private connections. I find that the coefficient on the squared term of the number of private connections is statistically insignificant, which is inconsistent with a non-linear relationship between the number of private connections and forecast accuracy. I document similar results for second-order connections. Overall, the evidence is consistent with a linear relationship between the number of private connections and forecast accuracy.

1.6.5 Competition Between Privately Connected Analysts

Given that competition could impede information transfer, I examine whether the effect of private connections on forecast accuracy is less pronounced when the competition between the connected analysts is higher. I proxy for competition between connected analysts using the total number of analysts covering firm *j*, with greater analyst coverage implying lower competition between the connected analysts. I argue that privately connected analysts are more likely to view each other as collaborators than as competitors when they are competing with a greater number of other analysts.[29] Consequently, privately connected analysts will have greater incentive to collaborate to generate synergy, which will allow them to outcompete other analysts.

Through partitioning the sample by the median and the top/bottom quartile levels of competition between privately connected analysts, I document a positive and statistically significant effect of private connections on forecast accuracy for the low competition subsamples but a statistically insignificant effect for the high competition subsamples, implying that high competition between privately connected analysts (with other analysts) impedes (facilitates) information transfer and leads to a dampening effect of private connections on forecast accuracy.[30] This suggests that greater competition

[29] In other words, greater competition with other analysts leads to more collaboration and less competition between the privately connected analysts. Given a greater number of other competing analysts covering the firm, privately connected analysts can optimize their competitive strategy to generate synergy through collaboration, which will allow them to outcompete other analysts. On the other hand, if only two analysts are covering a firm, relations between the privately connected analysts will be characterized solely by competition.

[30] A chi-square test of differences on *PrivatelyConnected* across the two columns is statistically significant.

with other analysts compels privately connected analysts to opt for a strategy of collaboration in order to outcompete other unconnected analysts.

1.6.6 Test of Peer Bias

While my main findings are consistent with a positive effect of information sharing between privately connected analysts, it is also possible for analysts connected to biased external peers to be negatively influenced by their peers due to various conflicts of interest that their peers face such as the incentive to generate investment banking revenues, increase trading profits, and obtain closer access to management. I partition the sample based on whether the connected peer analysts are influenced by investment banking incentives regarding the mutually covered firm. Specifically, *PeerBias* is defined as 1 if the analyst is privately connected to peer analysts whose average time since last participating as a lead underwriter or co-manager for an IPO or SEO for the mutually covered firm is less than 365 days, and 0 otherwise. I rerun model (1) separately for the *PeerBias*=0 and *PeerBias*=1 subsamples and find a positive and statistically significant effect of private connection on forecast accuracy for the *PeerBias*=0 subsample but a negative and statistically significant effect of private connection on forecast accuracy for the *PeerBias* =1 subsample. Thus, while private connection has a positive effect on forecast accuracy for the majority of analysts, for a small subset of analysts connected to biased peers, private connections undermine analysts' independence and negatively affect their research quality.

1.6.7 Limits of Private Connections

In this section, I extend my analyses in section 1.5.4. Given that analysts of less skill, experience, and resources benefit vis-a-vis their connections to analysts of more skill, experience, and resources, I examine whether private connections allow analysts of less skill, experience, and resources to outperform analysts of more skill, experience, and resources. I find that while private connections enable analysts of less skill, experience, and resources to outperform unconnected analysts of less skill, experience, and resources, the former still underperform relative to analysts with more (above median) skill, experience, and resources. The mean relative forecast accuracy of All-Star analysts, more experienced analysts (above median), large broker analysts, and industry specialists are the highest, followed by the mean relative forecast accuracy of privately connected non-All-Star analysts, less experienced analysts (below median), small broker analysts, and non-industry specialists, and followed by the mean relative forecast accuracy of unconnected non-All-Star analysts, less experienced analysts (below median), small broker analysts, and non-industry specialists. In other words, knowledge transfer from analysts of more skill, experience, and resources to analysts of less skill, experience, and resources only helps the latter improve their position relative to unconnected analysts and does not change the former's dominant position with respect to relative forecast accuracy.

1.6.8 Portfolio-level Knowledge Transfer

In addition, knowledge transfer for an analyst's *entire portfolio* of covered firms is not unidirectional between the privately connected pairs as analysts hold varying degrees of forecasting skill, experience, and industry expertise for different firms in their portfolio. For my sample of privately connected analysts, *FirmExperience* is negatively correlated with *Accuracy_lag* (-0.02) and positively correlated with *Numindustries* (0.04), indicating that analysts with more firm-specific experience may cover greater number of industries (more resource-constrained at the industry level) and have lower past forecast accuracy for other firms in their portfolio. This implies that analysts transferring knowledge to their connected external peers for one covered firm may likely be on the receiving end for a different covered firm in their portfolio.

To show that the recipient analyst and her connected external peer both benefit from their private connections, I run a cross-sectional analysis of analysts' firm-specific experience but include an interaction term with analysts' experience for the other firms in their portfolio (*PrivatelyConnected* × *FirmExperience* × *OtherFirmExperience*). *OtherFirmExperience* is defined as the average number of years that the analyst has issued forecasts for other firms in their portfolio other than firm *j* with which they share coverage with their connected external peers.

The coefficient on *PrivatelyConnected* × *FirmExperience* is negative and statistically significant, consistent with less experienced analysts deriving greater benefits from their private connections. However, the coefficient on *PrivatelyConnected* × *FirmExperience* × *OtherFirmExperience* is positive and statistically significant, suggesting that less experienced analysts with more experience in other firms covered by their connected peers derive greater benefits from their private connections. In other words, analysts' level of experience for other firms in their portfolio can help facilitate knowledge transfer for firms that they have less experience in and improve the effectiveness of their private connections. The transfer of knowledge between the privately connected analysts is not necessarily unidirectional as both the recipient analyst and her connected external peers benefit from their connections. Analysts who are receivers of information for certain firms in which they have less experience are also providers of information for other firms in their portfolio in which they have more experience.

1.6.9 Intrafirm Knowledge Transfer

Lastly, given the various benefits that analysts derive from their private connections, I test whether the benefits of private connections can be internalized within the brokerage house. Specifically, I focus on whether privately connected analysts benefit their colleagues within the brokerage house who cover the same industry or firm. I compare analysts who have colleagues with private connections versus analysts who do not have colleagues with private connections and versus other unconnected analysts. I find a statistically insignificant difference in their relative forecast accuracy. These analysts also underperform relative to their privately connected colleagues, which suggests that, on average, privately connected analysts do not engage in intrafirm information transfer and retain their comparative advantage relative to in-house colleagues.

1.7 Conclusion

Analysts are important information intermediaries who have limited time, resources, and energy. Prior studies have shown that analysts' research productivity is influenced by their brokerage-level resources, and those employed by smaller brokers are at a disadvantage since they face greater resource constraints and fewer opportunities to learn from colleagues within the brokerage house. In this study, I examine how private connections to external peers improve analysts' research quality and mitigate internal resource constraints. I form testable hypotheses to investigate whether analysts' private connections enhance their research quality. Using data on analysts' earnings forecasts from 1992 to 2017, I find that privately connected analysts (i.e., who are connected to external peers via past employment ties and cover the same firm in the same year) have more accurate earnings forecasts than those issued by "unconnected" analysts, consistent with greater information sharing between privately connected analysts.

Furthermore, I find that the benefits of private connections are more pronounced for analyst pairs who are, on average, employed by smaller brokerages, and who are, on average, less skilled, less experienced, and more resource-constrained, suggesting that private connections help to mitigate analysts' internal resource constraints and allow less skilled and less experienced analysts to experience greater improvement in their research through generating greater synergy. Moreover, using coverage terminations of connected peers as exogenous shocks to private connections, I find decreases in analyst forecast accuracy of the recipient analyst following coverage terminations of mutually covered stocks by connected peers due to permanent departures of the connected peer analysts from equity research, brokerage sector terminations affecting the connected peer analysts, and brokerage closures affecting the connected peer analysts. Importantly, my results have market implications as investors significantly underreact to forecasts of privately connected analysts, leading to stock price drifts during the three- and six-month window following forecast revisions.

APPENDIX A

A.1 Variable Definitions

Affliated	Equal to one if the analyst issuing the earnings forecast is a lead underwriter or co-manager during the past year in an IPO or SEO for the firm being forecasted, and zero otherwise
AbsRevision	The absolute value of *Revision*
Accuracy	AFE (the absolute difference between the analyst's earnings forecast and actual earnings), subtracted by the maximum value of the AFEs of all other analysts covering firm *j*, and scaled by the yearly range (maximum value- minimum value)
Allstar	Equal to one if the analyst is ranked as an All-Star analyst in the previous year, and zero otherwise
AFE	The absolute value of the earnings forecast minus actual earnings
AvgAllstar	The average of the All-Star status of the recipient analyst and her connected peers
AvgFirmExperience	The average of the firm-specific experience of the recipient analyst and her connected peers
AvgLagAccuracy	The average of the relative past forecast accuracy of the recipient analyst and her connected peers
AvgNumindustries	The average of the number of industries covered by the recipient analyst and her connected peers
Bold	Equal to one if the forecast is above the analyst's prior forecast and the prerevision consensus forecasts, or below both, and zero otherwise
BHAR (0, 2)	The Carhart 4-factor adjusted buy-and-hold returns over the three-day period beginning on the forecast announcement date
BrokerSize	Equal to one if the number of analysts employed by the analyst's brokerage is in the top 10%, and zero otherwise
ColleagueQuality	The percentage of in-house colleagues ranked as an All-Star in the analyst's specific GICS industry sector in year *t*
ConnectedToManager	Equal to one if the analyst shares prior educational ties with the CEO, CFO, or chairperson of firm *j*, and zero otherwise
DaysElapsed	The length of time between the current earnings forecast and the previous earnings forecast issued by any analyst
DiffAllstar	The *signed* difference in All-Star status between the average of analyst *i*'s connected peers and analyst *i*, and

	is equal to 1 if analyst *i* is a non-All-Star and her privately connected peers are on average All-Star, 0 if analyst *i* and her connected peers are both either All-Star or non-All-Star, and -1 analyst *i* is an All-Star and her connected peers are on average non-All-Star
DiffFirmExperience	The *signed* difference in firm-specific experience between the average of analyst *i*'s connected peers and analyst *i*
DiffLagAccuracy	The *signed* difference in relative forecast accuracy between analyst *i*'s connected peers and analyst *i*
DiffNumindustries	The *signed* difference in the number of industries covered by analyst *i*'s connected peers and analyst *i*
DivAllstar	The absolute difference in All-Star status between analyst *i* and the average of analyst *i*'s connected peers
DivFirmExperience	The absolute difference in firm-specific experience between analyst *i* and the average of analyst *i*'s connected peers
DivLagAccuracy	The absolute difference in relative forecast accuracy between analyst *i* and the average of analyst *i*'s connected peers
DivNumindustries	The absolute difference in the number of industries covered by analyst *i* and the average of analyst *i*'s connected peers
Duration	The number of years in which the recipient analyst and her connected peers shares common past employment
EA	Equal to one if the forecast is issued within five days following quarterly earnings announcements, and zero otherwise.
FirmExperience	The number of years the analyst has issued forecasts for firm *j*
ForFrequency	The number of earnings forecasts that the analyst made for firm j in year *t*
GenExperience	The number of years the analyst has issued forecasts on the IBES tape
GenPrivatelyConnected	Equal to one if an analyst shares with past employment ties or more than one year with external peers, and zero otherwise
Herding	Equal to one if the forecast is not a bold forecast, and zero otherwise.
Horizon	The number of days from the analyst's forecast revision to the date of the earnings announcement
IndPrivatelyConnected	Equal to one if the analyst shares past employment ties of more than one year with external peers covering the same industry, and zero otherwise

InstOwn	The percentage of shares held by institutional investors for firm *j* in year *t*
InternallyConnected	Equal to one if the analyst shares past employment ties with in-house colleagues covering the same firm, and zero otherwise
LagAccuracy	The analyst's forecast accuracy for firm *j* in year *t*-1
LnAnalyst	The natural log of the number of analysts following the firm in the month before the announcement of year *t* earnings
Log(BM)	The natural log of the firm's book-to-market ratio in year *t*
MF	Equal to one if the forecast is issued within five days following annual or quarterly management forecasts, and zero otherwise.
NumFirm	The number of firms for which the analyst has issued forecasts in year *t*
NumGenPrivatelyConnected	The number of employment-based connections that an analyst shares with external peers
NumIndPrivatelyConnected	The number of employment-based connections that an analyst shares with external peers covering the same industry
NumIndustries	The number of GICS 24 industries for which the analyst has issued forecasts in year *t*
NumInst	The number of institutional owners of the firm in year *t*
NumNonBrokerConnected	The number of private connections for an analyst based on shared past employment at non-financial service firms
NumPrivatelyConnected	The number of employment-based connections that an analyst shares with external peers covering the same firm
NumSchoolTies	The number of private connections based on school ties that the analyst shares with external peers covering the same firm
NumSmallBrokerConnected	The number of private connections based on shared past employment at a non-top brokerage firm
NumTopBrokerConnected	The number of private connections based on shared past employment at one of the following firms- Goldman Sachs, JP Morgan Chase, Barclays, Bank of America, Morgan Stanley, Deutsche Bank, Citigroup, Credit Suisse, UBS, and Wells Fargo
OtherFirmExperience	The average number of years the analyst has issued forecasts for other firms in their portfolio other than firm *j* with which they share coverage with their connected peers

PeerBias	Equal to one if the analyst is privately connected to peer analysts who are the lead underwriters or co-managers of an IPO or SEO for the mutually covered firm in the past year (if the average of the time since last IPO or SEO for the peer analysts is less than 365 days, and zero otherwise)
PriorRet	The buy-and-hold stock returns over the month ending five days prior to the forecast announcement date
PriorRetVolatility	The return volatility over the month ending five days prior to the forecast announcement date
PriorVol	The average trading volume of the month ending five days prior to the forecast announcement date
PrivatelyConnected	Equal to one if the analyst shares past employment ties of more than one year with external peers covering the same firm, and zero otherwise
PrivatelyConnectedAggregated	Equal to one if the analyst shares past employment ties of more than one year with external peers covering the same firm for any firms in his portfolio in year t, and zero otherwise
PrivatelyNumConnected	The number of private connections of an analyst in which the analyst shares past employment ties of more than one year with an external peer covering the same firm
Revision	The difference between the analyst's current forecast and her prior forecast, scaled by the stock price two days prior
SchoolTiesInternallyConnected	Equal to one if the analyst shares educational ties with in-house colleagues covering the same firm, and zero otherwise
SchoolTiesConnected	Equal to one if the analyst shares educational ties with external peers covering the same firm, and zero otherwise
SecondOrderConnected	The total number of second-order private connections for analyst i covering firm j in year t
Size	The natural log of the firm's market capitalization in year t
SwitchBroker	Equal to one if the analyst switches to a different broker in the following year, and zero otherwise
ToTopBroker	The signed difference in broker size, equal to 1 if the analyst moves to a larger broker in year t+1, 0 if the analyst moves to a similar-sized broker in year t+1, and -1 if the analyst moves to a smaller broker in year t+1
TeamSize	The number of in-house colleagues working in the analyst's industry sector

Timeliness	The number of days between the immediately preceding forecast and the forecast of interest divided by the number of days between the immediately succeeding forecast and the forecast of interest

BIBLIOGRAPHY

Arrow, K. J. (1962). The Economic Implications of Learning by Doing. *The Review of Economic Studies*, 29(3): 155–173.

Asquith, P., Mikhail, M. B., & Au, A. S. (2005). Information content of equity analyst reports. *Journal of Financial Economics*, 75(2), 245-282.

Audretsch, D. B., and Feldman M. P. (1996). R&D Spillovers and the Geography of Innovation and Production. *The American Economic Review*, 86 (3): 630–640.

Beckmann, M. J. (1994). On knowledge networks in science: collaboration among equals. *The Annals of Regional Science*, 28(3), 233-242.

Bernhardt, D., Campello M., and E. Kutsoati. 2006. Who herds? *Journal of Financial Economics* 80: 657-675.

Bloomfield, R., & Hales, J. (2009). An experimental investigation of the positive and negative effects of mutual observation. *The Accounting Review*, 84(2), 331-354.

Bradley, D., Gokkaya, S., & Liu, X. (2017). Before an analyst becomes an analyst: Does industry experience matter?. *The Journal of Finance*, 72(2), 751-792.

Bradley, D., Gokkaya, S., & Liu, X. (2020). Ties that bind: The value of professional connections to sell-side analysts. *Management Science*, Forthcoming.

Bradshaw, M. T. (2011). Analysts' forecasts: what do we know after decades of work?. Working Paper.

Brochet, F., Miller, G. S., & Srinivasan, S. (2013). Do analysts follow managers who switch companies? An analysis of relationships in the capital markets. *The Accounting Review*, 89(2), 451-482.

Brown, L. D., Call, A. C., Clement, M. B., & Sharp, N. Y. (2015). Inside the "black box" of sell-side financial analysts. *Journal of Accounting Research*, 53(1), 1-47.

Brown, L. D., & Hugon, A. (2009). Team earnings forecasting. *Review of Accounting Studies*, 14(4), 587-607.

Brown, L. D., & Mohammad, E. (2010). Is analyst earnings forecast ability only firm specific?. *Contemporary Accounting Research*, 27(3), 727-750.

Cen, L., Chang, Y. Y. C., & Dasgupta, S. (2020). Do Analysts Learn from Each Other? A New Approach to an Unresolved Question. Working Paper.

Chen, S., & Matsumoto, D. A. (2006). Favorable versus unfavorable recommendations: The impact on analyst access to management-provided information. *Journal of Accounting Research*, 44(4), 657-689.

Chen, T., & Martin, X. (2011). Do Bank-Affiliated Analysts Benefit from Lending Relationships?. *Journal of Accounting Research*, 49(3), 633-675.

Cheng, Q., Du, F., Wang, X., & Wang, Y. (2016). Seeing is believing: Analysts' corporate site visits. *Review of Accounting Studies*, 21(4), 1245-1286.

Christensen, D. M., Mikhail, M. B., Walther, B. R., & Wellman, L. A. (2016). From K street to wall street: political connections and stock recommendations. *The Accounting Review*, 92(3), 87-112.

Cicero, D., Kalpathy, S., & Sulaeman, J. (2011). Equity analysts affiliated with corporate lenders. Working Paper.

Cici, G., Shane, P. B., & Yang, Y. S. S. (2019). Do connections with buy-side analysts inform sell-side analyst research?. Working Paper.

Clement, M. B. (1999). Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter?. *Journal of Accounting and Economics*, 27(3), 285-303.

Clement, M. B., & Tse, S. Y. (2003). Do investors respond to analysts' forecast revisions as if forecast accuracy is all that matters?. *The Accounting Review*, 78(1), 227-249.

Clement, M. B., & Tse, S. Y. (2005). Financial analyst characteristics and herding behavior in forecasting. *The Journal of Finance*, 60(1), 307-341.

Clement, M. B., Koonce, L., & Lopez, T. J. (2007). The roles of task-specific forecasting experience and innate ability in understanding analyst forecasting performance. *Journal of Accounting and Economics*, 44(3), 378-398.

Clement, M. B., Hales, J., & Xue, Y. (2011). Understanding analysts' use of stock returns and other analysts' revisions when forecasting earnings. *Journal of Accounting and Economics*, 51(3), 279-299.

Cohen, L., Frazzini, A., & Malloy, C. (2008). The small world of investing: Board connections and mutual fund returns. *Journal of Political Economy*, 116(5), 951-979.

Cohen, L., Frazzini, A., & Malloy, C. (2010). Sell-side school ties. *The Journal of Finance*, 65(4), 1409-1437.

Cohn, J. B., & Juergens, J. L. (2014). How much do analysts influence each other's forecasts? *Quarterly Journal of Finance*, 4(3), 1-35.

Cornaggia, J., & Zou, L. (2008). Does Initial Placement Matter for Equity Analysts' Forecast Accuracy. Working Paper.

Cornish, S. L. (1997). Product innovation and the spatial dynamics of market intelligence: does proximity to markets matter?. *Economic Geography*, 73(2), 143-165.

Cowen, A., Groysberg, B., & Healy, P. (2006). Which types of analyst firms are more optimistic?. *Journal of Accounting and Economics*, 41(1-2), 119-146.

Do, T. P., & Zhang, H. (2020). Peer effects among financial analysts. *Contemporary Accounting Research*, 37(1), 358-391.

Faccio, M. (2006). Politically connected firms. *American Economic Review*, 96(1), 369-386.

Fang, L. H., & Huang, S. (2017). Gender and connections among Wall Street analysts. *The Review of Financial Studies*, 30(9), 3305-3335.

Fang, B., & Hope, O. K. (2020). Analyst Teams. *Review of Accounting Studies*, Forthcoming.

Fischer, P. E., & Stocken, P. C. (2010). Analyst information acquisition and communication. *The Accounting Review*, 85(6), 1985-2009.

Fracassi, C., & Tate, G. A. (2012). External networking and internal firm governance. *Journal of Finance*, 67(1), 153-194.

Gibbons, B., Iliev, P., & Kalodimos, J. (2021). Analyst information acquisition via EDGAR. *Management Science*, 67(2), 769-793.

Glaeser, E. L., H. D. Kallal, J. A. Scheinkman, and A. Shleifer. (1992). Growth in Cities. *Journal of Political Economy,* 1126–1152.

Glaeser, E. L., Laibson, D. I., Scheinkman, J. A., & Soutter, C. L. (2000). Measuring trust. *The Quarterly Journal of Economics*, 115(3), 811-846.

Gleason, C. A., & Lee, C. M. (2003). Analyst forecast revisions and market price discovery. *The Accounting Review*, *78*(1), 193-225.

Gow, I. D., Ormazabal, G., & Taylor, D. J. (2010). Correcting for cross-sectional and time-series dependence in accounting research. *The Accounting Review*, 85(2), 483-512.

Green, T. C., Jame, R., Markov, S., & Subasi, M. (2014). Access to management and the informativeness of analyst research. *Journal of Financial Economics*, 114(2), 239-255.

Groysberg, B., Lee, L. E., & Nanda, A. (2008). Can they take it with them? The portability of star knowledge workers' performance. *Management Science*, 54(7), 1213-1230.

Groysberg, B., & Lee, L. E. (2008). The effect of colleague quality on top performance: The case of security analysts. *Journal of Organizational Behavior*, 29(8), 1123-1144.

Groysberg, B., & Lee, L. E. (2010). Star power: colleague quality and turnover. *Industrial and Corporate Change*, 19(3), 741-765.

Groysberg, B., Healy, P. M., & Maber, D. A. (2011). What drives sell-side analyst compensation at high-status investment banks?. *Journal of Accounting Research*, 49(4), 969-1000.

Groysberg, Boris, and Ingrid Vargas. "Innovation and Collaboration at Merrill Lynch." Harvard Business School Case 406-081, December 2005.

Gu, Z., Li, Z., Yang, Y. G., & Li, G. (2018). Friends in need are friends indeed: An analysis of social ties between financial analysts and mutual fund managers. *The Accounting Review*, 94(1), 153-181.

Guan, Y., Su, L. N., Wu, D., & Yang, Z. (2016). Do school ties between auditors and client executives influence audit outcomes? *Journal of Accounting and Economics*, 61(2-3), 506-525.

Guttman, I. (2010). The timing of analysts' earnings forecasts. *The Accounting Review*, 85(2), 513-545.

Han, B., Kong, D., & Liu, S. (2018). Do analysts gain an informational advantage by visiting listed companies?. Contemporary Accounting Research, 35(4), 1843-1867.

Haushalter, D., & Lowry, M. (2011). When do banks listen to their analysts? Evidence from mergers and acquisitions. *The Review of Financial Studies*, 24(2), 321-357.

He, X., Pittman, J. A., Rui, O. M., & Wu, D. (2017). Do social ties between external auditors and audit committee members affect audit quality? *The Accounting Review*, 92(5), 61–87.

Hilary, G., & Shen, R. (2013). The role of analysts in intra-industry information transfer. *The Accounting Review*, 88(4), 1265-1287.

Hong, H., Kubik, J. D., & Solomon, A. (2000). Security analysts' career concerns and herding of earnings forecasts. *The Rand Journal of Economics*, 121-144.

Hong, H., & Kacperczyk, M. (2010). Competition and bias. *The Quarterly Journal of Economics*, 125(4), 1683-1725.

Horton, J., & Serafeim, G. (2009). Security analyst networks, performance and career outcomes. Working Paper.

Huang, R., Krishnan, M., Shon, J., & Zhou, P. (2017). Who herds? Who doesn't? Estimates of analysts' herding propensity in forecasting earnings. *Contemporary Accounting Research*, 34(1), 374-399.

Hugon, A., Kumar, A., & Lin, A. P. (2015). Analysts, macroeconomic news, and the benefit of active in-house economists. *The Accounting Review*, 91(2), 513-534.

Hugon, A., Lin, A. P., & Markov, S. (2016). Do Equity Analysts Benefit from Access to High Quality Debt Research? Working Paper.

Hwang, B. H., & Kim, S. (2009). It pays to have friends. *Journal of Financial Economics*, 93(1), 138-158.

Hwang, B. H., Liberti, J. M., & Sturgess, J. (2018). Information sharing and spillovers: Evidence from financial analysts. *Management Science*, 65(8), 3624-3636.

Jacob, J., Lys, T. Z., & Neale, M. A. (1999). Expertise in forecasting performance of security analysts. *Journal of Accounting and Economics*, 28(1), 51-82.

Jegadeesh, N., & Kim, W. (2009). Do analysts herd? An analysis of recommendations and market reactions. *The Review of Financial Studies*, 23(2), 901-937.

Jennings, J., Lee, J., & Matsumoto, D. A. (2017). The effect of industry co-location on analysts' information acquisition costs. *The Accounting Review*, 92(6), 103-127.

Kahneman, D. (1973). *Attention and effort* (Vol. 1063, pp. 218-226). Englewood Cliffs, NJ: Prentice-Hall.

Karlan, D., Mobius, M., Rosenblat, T., & Szeidl, A. (2009). Trust and social collateral. *The Quarterly Journal of Economics*, 124(3), 1307-1361.

Ke, B., & Yu, Y. (2006). The effect of issuing biased earnings forecasts on analysts' access to management and survival. *Journal of Accounting Research*, 44(5), 965-999.

Kelly, B. T., & Ljungqvist, A. (2007). The value of research. Working Paper.

Kelly, B., & Ljungqvist, A. (2012). Testing asymmetric-information asset pricing models. *The Review of Financial Studies*, 25(5), 1366-1413.

Kini, O., Mian, S., Rebello, M., & Venkateswaran, A. (2009). On the structure of analyst research portfolios and forecast accuracy. *Journal of Accounting Research*, 47(4), 867-909.

Law, K. (2013). Career imprinting: The influence of coworkers in early career. Working Paper.

Li, Z., Wong, T.J. and Yu, G. (2019). Information Dissemination Through Embedded Financial Analysts: Evidence from China. *The Accounting Review.*

Lin, H.W. and McNichols, M.F. (1998). Underwriting relationships, analysts' earnings forecasts and investment recommendations. *Journal of Accounting and Economics*, 25(1), 101-127.

Lucas, R. E. (1988). On the mechanism of economic development. *Journal of Monetary Economics*, 22(1), 3–42.

Petersen, M. A. (2009). Estimating standard errors in finance panel data sets: Comparing approaches. *The Review of Financial Studies*, 22(1), 435-480.

Phua, J. K., Tham, T. M., & Wei, C. (2018). Do Equity Analysts Learn from Their Colleagues? Working Paper.

Malloy, C. J. (2005). The geography of equity analysis. *The Journal of Finance*, 60(2), 719-755.

Marshall, A. (1890/1966) Principles of Economics (London: MacMillan).

Massa, M., & Rehman, Z. (2008). Information flows within financial conglomerates: Evidence from the banks–mutual funds relation. *Journal of Financial Economics*, 89(2), 288-306.

Mayew, W. J., Sharp, N. Y., & Venkatachalam, M. (2013). Using earnings conference calls to identify analysts with superior private information. *Review of Accounting Studies*, 18(2), 386-413.

Merton, R. C. (1987). A simple model of capital market equilibrium with incomplete information. *Journal of Finance*, 42(3), 483–510.

Michaely, R., & Womack, K. L. (1999). Conflict of interest and the credibility of underwriter analyst recommendations. *The Review of Financial Studies*, 12(4), 653-686.

Mikhail, M. B., Walther, B. R., & Willis, R. H. (1997). Do security analysts improve their performance with experience?. *Journal of Accounting Research*, 35, 131-157.

Mikhail, M. B., Walther, B. R., & Willis, R. H. (1999). Does forecast accuracy matter to security analysts?. *The Accounting Review*, 74(2), 185-200.

Rees, L., Sharp, N. Y., & Wong, P. A. (2017). Working on the weekend: Do analysts strategically time the release of their recommendation revisions?. *Journal of Corporate Finance*, 45, 104-121.

Rogers, E. M., & Kincaid, D. L. (1981). *Communication networks: Towards a new paradigm*. New York: Free Press.

Soltes, E. (2014). Private interaction between firm management and sell-side analysts. *Journal of Accounting Research*, 52(1), 245-272.

Sonney, F. (2007). Financial analysts' performance: Sector versus country specialization. *The Review of Financial Studies*, 22(5), 2087-2131.

Stein, J. C. (2008). Conversations among competitors. *American Economic Review*, 98(5), 2150-62.

Surowiecki, J. (2004). The wisdom of crowds: Why the many are smarter than the few and how collective wisdom shapes business. *Economies, Societies and Nations*, 296.

Trueman, B. (1994). Analyst forecasts and herding behavior. *The Review of Financial Studies*, *7*(1), 97-124.

Von Hippel, E. (1994). "Sticky information" and the locus of problem solving: implications for innovation. *Management Science*, 40(4), 429-439.

Welch, I. (2000). Herding among security analysts. *Journal of Financial Economics*, 58(3), 369-396.