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Equity analyst social interactions and geographic information transmission

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Abstract

We find that earnings forecasts by analysts with more local peers, defined as analysts working in the same brokerage office who cover different firms headquartered in the same area, are more accurate. These heightened accuracy effects are concentrated in settings where local peers are particularly valuable, such as when analysts have less access to corporate management, when earnings are harder to forecast, and when analysts have stronger incentives to work hard. In examining the nature of the information transmitted by local peers, we find that earnings forecasts by analysts with more local peers better reflect negative geographic shocks in firm earnings. In addition, geographic momentum in stock returns is attenuated for firms that are followed by more local peers, especially when area returns are negative. These findings suggest that social interactions among local peer analysts facilitate the transmission of complex, soft information about geographic factors to investors.

Keywords Social interaction · Financial analyst · Geographic information · Geographic momentum

JEL codes G10 · G11 · G12 · G14 · G23 · G24

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1 Introduction

A growing body of evidence in accounting and finance indicates that social networks facilitate information sharing in a variety of settings.[1] Less is known about the nature of information shared in social interactions. In this paper, we contribute to this literature by examining whether social interactions among local peer analysts help transmit geographic information. We define local peers as analysts working in the same brokerage office who cover different firms headquartered in the same geographic area. Geographic information pertains to information about firm fundamentals that comes from the geographic region surrounding firm headquarters and is orthogonal to industry information.

Evidence on whether local peers help transmit geographic information can shed light on both the analysts' role in transmitting geographic information to the market and the nature of information transmitted via social networks. Regarding the first issue, a large urban economics literature establishes that geography-specific factors exist and can significantly affect firms in the same local area, even firms in unrelated industries.[2] A recent example is Dougal et al. (2015), who find that firms' investments, earnings, and sales exhibit a significant geographic component that is orthogonal to the industry component. At the same time, Parsons et al. (2020) find that equity prices exhibit geographic momentum, suggesting that investors are slow to understand geographic information. Furthermore, Parsons et al. (2020) conjecture that analysts may be to blame for this lack of investor understanding because brokerages are designed to specialize by industry, not by geographic region. Evidence on whether local peers help analysts impound geographic information into their forecasts can shed light on this conjecture.

Regarding the second issue, which pertains to the nature of information transmitted in social networks, prior literature finds that analysts benefit from direct ties with firm insiders, consistent with the idea that analysts receive first-hand firm-specific information from these social networks. The social network we consider pertains to the dissemination of second-hand information where communication frictions are more pronounced. Information about geographic factors is complex, especially when it comes to assessing how a given firm will be affected by any particular geographic factor.[3] Based on findings, from prior literature, that people in close proximity are more likely to engage in social interactions and discuss things they share in common (Allen 1984; Burt 1995), we posit that analysts with more local peers have more opportunities to exchange, absorb, and internalize complex geographic information. These analysts should therefore gain a better understanding of how geographic factors impact their covered firms and transmit this information to the market. Since analysts produce

[1] These include social interactions for investors (Hong et al. 2005; Cohen et al. 2008; Pool et al. 2015; Ivković and Weisbenner 2007), corporate directors and managers (Shue 2013), and analysts (Cohen et al. 2010; Fang and Huang 2017; Bradley et al. 2020; Gu et al. 2019).

[2] See *Handbook of Regional and Urban Economics* (Herderson and Thisse 2004) for a comprehensive review of the related literature.

[3] The complexity of geography information is recognized in the existing literature. For example, while Dougal et al. (2015) outline a number of spillovers and externalities at the local level, they acknowledge that "for the most part, we cannot distinguish between the various people-based explanations" (page 169). Instead, they broadly refer to these geographic spillovers as "urban vibrancy."

publicly available forecasts, we can overcome the difficulty of not directly observing what is communicated between analysts by examining whether earnings forecast accuracy is higher in the presence of local peers.

Whether we will ultimately observe higher forecast accuracy is unclear ex ante. On the one hand, information about geographic factors should be useful for analysts, given research establishing that earnings has a geographic component (Dougal et al. 2015). On the other hand, since complex information degrades analyst earnings forecast accuracy (Lehavy et al. 2011), the complex nature of geographic spillovers and externalities on firm earnings may increase processing costs to such a level that analysts ultimately forgo analysis of geographic information. In such a case, we would observe no association between analyst forecast accuracy and local peers.

To illustrate local peer sharing of geographic information, consider two otherwise identical analysts, Jack and Jane, who cover grocery stores at two different brokerage firms. They are both located in the New York offices of their respective brokerages, and both cover Whole Foods in Austin, Texas, and Kroger in Cincinnati, Ohio.[4] Jack happens to have local peers for Austin (i.e., officemates covering other firms in Austin such as Dell Computer and Cirrus Logic) but not for Cincinnati, whereas the opposite is true for Jane (who has local peers for Cincinnati but not Austin). Both Jack and Jane have the same access to public information about changes in the local Austin economy. However, because Jack has more opportunities to exchange, absorb, and internalize such information with his Austin peers, he may reach a higher-quality assessment about how the changes in Austin affect Whole Foods, relative to Jane.[5]

We begin our analysis by compiling a comprehensive, hand-collected panel dataset on analysts' physical office locations from *Nelson's Directory of Investment Research*. This enables us to determine the number of local peers for each of the firms an analyst follows. Our final sample consists of 234,340 analyst annual forecasts over the period 1993 to 2005, covering 5469 unique analysts and 6527 unique firms. This granular dataset allows us to use high dimensional fixed effects to effectively rule out many confounding factors. The main fixed effect is the firm-year interactive fixed effect, which flexibly absorbs any firm-year information that is potentially available to all analysts following the same firm and allows us to assess the accuracy of an analyst's forecast relative to the average accuracy of all forecasts for the same firm-year. We also include analyst-specific and office-specific fixed effects, which identify the effect of local peers from within-analyst variation. In our example above, controlling for firm-year fixed effects allows us to show that Jack's forecast accuracy for Whole Foods is higher than Jane's forecast accuracy for Whole Foods; the addition of brokerage office and analyst fixed effects allows us to assess whether Jack's forecast accuracy is higher for Whole Foods than for Kroger due to his differential exposure to Austin-based local peers and Cincinnati-based local peers.

Our main result is a significantly positive association between analyst forecast accuracy and local peers. This relation holds after controlling for known factors affecting forecast accuracy such as forecast horizon, an analyst's general and firm-

[4] It is well established that analysts tend to specialize by industry (Piotroski and Roulstone 2004; Brown et al. 2015; Parsons et al. 2020).

[5] Similarly, Jack's forecasts for Whole Foods may also be of a different quality from his forecasts for Kroger, especially when compared with those by Jane.

specific experience, and distance between an analyst's office and the covered firm (Malloy 2005). We also control for the number of other analysts working for the same brokerage firm covering the same geographic area but located in a different office (brokerage peers). This control ensures that local peers are not simply proxying for unobserved broker-wide connective technologies (e.g., direct messaging, emails, phone calls, video-conferencing, or subscription to local news) that can vary over time and enable analysts to obtain geographic information in ways that do not require in-office social interactions.

To better understand the nature of a local peer benefit, we conduct several subsample analyses to ascertain whether the effect we document varies predictably. We find the effect of local peers is stronger in subsamples where (1) direct access to management is relatively lacking, (2) earnings are harder to forecast, and (3) analysts have stronger internal competition incentives to achieve all-star status. These findings support the idea that analysts benefit from interactions with local peers, which allow them to internalize geographic factors when forecasting firm earnings.

Our findings are based on the premise that social interactions with local peers help analysts absorb and internalize geography-specific information. Since we cannot test this premise by directly observing what is discussed among local peer analysts, we do so indirectly by examining whether analysts with more local peers are more attuned to area-specific shocks. We proxy for geographic shocks using the signed forecast errors common to all analysts covering firms in a geographic area. We use signed, as opposed to absolute, forecast errors so that we can capture both positive and negative shocks. We do this for two reasons. First, in our sample, analyst forecasts are optimistically biased on average, implying that optimism reduction could play an important role in reducing absolute forecast errors. Second, experimental work shows that negative news is more likely to survive transmission in social networks relative to positive news (Bebbington et al. 2017). Studying absolute forecast errors does not allow for such transmission asymmetry to play a role. We find that the signed forecast error by an analyst with more local peers is less correlated with positive area-specific common errors (i.e., negative area shocks), suggesting that analysts are more attuned to negative shocks when they have more opportunities to engage in social interactions with local peers.

Our findings that analyst forecasts are more accurate as a result of local peers and that forecast errors are more sensitive to negative area shocks together imply that we should observe less geographic momentum in stock prices (Parsons et al. 2020) when local peers are present. Furthermore, the momentum reduction should result more from negative prior returns than positive prior returns. Consistent with these implications, we find that geographic momentum effects attenuate as the number of local peers increases, and this effect is concentrated among area returns that are negative. These findings suggest that local peer analysts facilitate more efficient pricing of geographic information.

Collectively, our results suggest that local peers enhance earnings forecast quality by helping analysts internalize information about geographic factors in their forecasts. Two important caveats are in order, however. First, we are unable to provide direct evidence on the particular types of spillovers or externalities, at the local level, that local peers help analysts internalize. This is partly the result of limited guidance in the literature with respect to determining how (and which) externalities affect a given firm, and partly the result of data constraints. For example, if employee sentiment in a geographic area around headquarters is particularly favorable and indicative of improved fundamentals (Green et al. 2019), perhaps having local peers helps make an analyst more aware and appreciative of such effects. Due to the lack of crowdsourced employer reviews, we have no means to directly assess this possibility during our sample period. Second, our sample period ends in 2005, which limits the generalizability of our findings. While our evidence suggests that what is relevant is the in-person interaction among local peers and not brokeragewide connective technologies or brokerage peers, advances in communication technology since 2005, such as social media applications, may nonetheless enable analysts to generate networks that render in-person office interaction less relevant.

Our paper belongs to the broad literature on information transmission in social networks among market participants (e.g., Hong et al. 2004; Hong et al. 2005; Ivković and Weisbenner 2007; Brown et al. 2008; Cohen et al. 2008; Shue 2013; Pool et al. 2015). Among this literature, our study is closely related to those documenting the value of analyst social networks stemming from alumni ties (Cohen et al. 2010; Fang and Huang 2017), workplace and hometown connections (Gu et al. 2018), and professional connections (Bradley et al. 2020). These studies find that analysts perform better when they have first-hand, firm-specific information from their connections with insiders of their covered firm. Our study differs in that the social interactions we study are likely more frequent, occurring weekly or even daily. More importantly, they are among analysts covering firms in different industries but headquartered in the same geographic areas. Their communication is therefore less likely to be about sharing firm-specific information and more likely to be about understanding how geographic factors may (or may not) map into the earnings of their own covered firms.[6] Our findings indicate that second-hand information-sharing via social interactions is a distinct information collection and processing channel. Our investigation suggests that officemates within the brokerage represent an additional network for analysts to utilize. Overall, our evidence helps answer the call for research on deepening the understanding of how analysts obtain and process information (Schipper 1991; Ramnath et al. 2006; Brown et al. 2015; Kothari et al. 2016).

We also add to the growing literature, in finance, on how the dissemination of geography-related information relates to stock price efficiency.[7] Our finding that local peers moderate geographic momentum builds on the findings of Parsons et al. (2020) to better understand the geographic momentum phenomenon, and provides evidence suggesting that a link exists between stock price momentum and information transmission via social interactions (Jegadeesh and Titman 2001). To the extent that information about geographic factors tends to be subjective and that its communication relies on personal social interactions, our findings also shed light on the longstanding puzzle of local bias and the slow diffusion of geography-related news.

[6] Our paper is also related to, but distinct from, the strand of the analyst literature showing that geographic distance to an information source facilitates *first-hand* information acquisition, which in turn matters for analyst forecast quality (Malloy 2005; Bae et al. 2008; Jennings et al. 2017) and coverage decisions (O'Brien and Tan 2015; Engelberg et al. 2018).

[7] See, for example, Dougal et al. (2015), Addoum et al. (2016), Core et al. (2016), and Matsumoto et al. (2022). For asset pricing specifically, see Pirinsky and Wang (2006), Feng and Seasholes (2004), Loughran and Schultz (2005), Loughran (2007), Hong (2008), and Garcia and Norli (2012).

The rest of the paper proceeds as follows. Section 2 develops our main hypotheses and discusses empirical design. Section 3 describes the sample construction procedure and provides summary statistics. Sections 4 and 5 present empirical results, and Section 6 concludes.

2 Hypothesis development

We examine whether the social interactions among local peers help analysts better incorporate and internalize geography information in their earnings forecasts. The idea relies on two premises. The first is that individuals in close proximity are more likely to communicate and share information about common experiences during social interactions. It is well established in communication theory that proximity promotes social interaction and information flow, i.e., the "Allen Curve" in Allen (1984). Applying this idea, a large literature in accounting and finance uses the physical distance between agents as a proxy for word-of-mouth communication to study the effect of social interactions on activities in the financial markets (e.g., Hong et al. 2004; Hong et al. 2005; Ivković and Weisbenner 2007).

People tend to share information related to their common experiences during social interactions (Burt 1995). For example, immigrants from the same country are more likely to share updates about their home country with each other, and moviegoers are more likely to discuss their views of a movie with those who have seen the same movie. We examine a specific type of commonality shared by local peers: the overlap in geographic areas of their covered firms. We posit that analysts with more local peers for a given area have more opportunities to exchange and communicate information related to the area. Beyond the direct transfer of information, such interactions can also reduce analysts' costs in processing complex information and help them better internalize such information in their forecasts (Blankespoor et al. 2020). Since we cannot observe either the content of these social interactions or how analysts incorporate them into their forecasts, we indirectly assess whether such interactions help analysts by examining the relationship between earnings forecast accuracy and the number of local peers.

The second premise underlying our hypothesis is that geographic factors exist that affect the performance of local firms in unrelated industries. We take as given, based on findings from the large literature on urban agglomeration economies, the importance of geography for various aspects of local economies, as summarized in the *Handbooks of Regional and Urban Economics* (Herderson and Thisse 2004). Recent research also documents that information about geography-related factors is value-relevant (Pirinsky and Wang 2006; Addoum et al. 2016), with Dougal et al. (2015) establishing that investment, earnings, and sales of firms have both an industry component and a nontrivial geographic component (equivalent to one-third of the industry component). These findings suggest that analysts can potentially improve their forecast accuracy through social interactions with local peers, which allow them to aggregate individual information about geographic factors (Ellison and Fudenberg 1995; Stein 2008).

However, whether social interactions with local peers translate to higher forecast accuracy is unclear for three reasons. First, geographic information is soft and complex to analyze. While academic research has provided large sample evidence suggesting several non-mutually exclusive channels of how geographic factors affect local firms in unrelated industries, including knowledge spillovers (Jaffe et al. 1993), human capital externalities (Moretti 2004), consumption externalities (Glaeser et al. 2001), and collateral values (Chaney et al. 2012), it is difficult to pinpoint a priori how a given location or a given firm may be affected by any particular channel. Indeed, although Dougal et al. (2015) document a strong geographic component to firm performance, they cannot identify the specific channels and can only refer to them collectively as "urban vibrancy." The complexity of geographic information implies that the communication frictions can be so high as to prevent analysts from successfully decoding the impact of geographic factors on their covered firms, even if they can discuss them with local peers.

Second, the investment analyst business is organized by industry rather than geographic region (Gilson et al. 2001; Boni and Womack 2006; Kadan et al. 2012; Bradley et al. 2017), and analyst recognition such as Institutional Investor all-star status and sell-side analyst compensation is based heavily on whether the analyst is superior in a given industry, not in a geographic area (Brown et al. 2015; Stickel 1992). This means that if an officemate covers different firms in the *same* industry, that officemate might not share information, given that analysts are differentially compensated on industry expertise. Officemates covering the same firm are likely to be team members who certainly share experiences and information (Fang and Hope 2021). However, they also may share team-specific, broker-supplied resources, making it difficult to isolate social interaction effects.

Lastly, unlike information directly acquired by analysts first-hand from being in close physical proximity to the covered firm (Malloy 2005; Bae et al. 2008) or from firm insiders (Cohen 2010; Fang and Huang 2017; Bradley et al. 2020; Gu et al. 2019), information communicated from local peers is second-hand and therefore more prone to distortion. Indeed, a large literature, starting from Bartlett (1932), has characterized how information can degrade as it travels via social interaction in a social network (Kalish et al. 2007; Bebbington et al. 2017). This tendency of information to degrade, together with the fact that the geographic information itself is "soft," makes it difficult to identify precisely how and which factors in a geographic area affect the local economy and, in turn, the firm.

The above arguments suggest that it is ultimately an empirical question whether social interaction among local peer analysts meaningfully communicates geography-related information. Therefore, we present our main hypothesis in the null form.

> *Hypothesis: Analyst earnings forecast accuracy is not associated with exposure to local peers.*

3 Sample construction and descriptive statistics

In Table 1, we describe the sample selection process. We start with all analyst-firm-year combinations that ever appeared in the IBES US Detail History file between November 1992 and October 2005. We identify 10,008 unique analysts and 9722 unique firms. We then retain all observations where we can identify the analyst's name and brokerage from the IBES recommendation detail file, which results in 8302 unique analysts covering 9637 unique firms. Then we follow the methodology described in Malloy (2005) to manually

Table 1 Sample Selection

Sample selection criteria	Observations	Unique analysts	Unique firms
Analyst-firm-year combinations from IBES Detail File between Nov 1992 and Oct 2005	457,793	10,008	9722
Retain analyst names identified in IBES Recommendation File	424,013	8302	9637
Retain analyst location identified in Nelson's Directory (excluding teams and research department)	322,127	5602	9325
Retain observations with geography and industry information and firms/analysts located within mainland US.	282,272	5568	8466
Retain observations with control variables for accuracy test	234,340	5469	6527

This table reports how the sample is derived for the forecast accuracy analysis

identify the location of each analyst from the annually produced *Nelson's Directory of Investment Research* from 1993 to 2005.[8] The *Nelson's Directory* contains each analyst's name, phone number, brokerage firm, and the city of the brokerage office.[9] Each volume is published in December of year *t* using data as of November of year *t*. Therefore, we classify each analyst's location starting in November of year *t-1* and lasting until October of year *t* according to the information in *Nelson's Directory* of year *t*. We exclude analyst teams and research departments because we cannot uniquely identify their locations. We also exclude firms and analysts located in non-contiguous US states and territories (Hawaii, Alaska, and Puerto Rico) and outside of the US.

We define an analyst's industry and the firms that fall into a given industry using a firm's six-digit Global Industry Classification Standard (GICS) code. Prior literature finds that the GICS code is most consistent with the industry definition used by the analyst community (Bhojraj et al. 2003; Hrazdil et al. 2013). We define a firm's location as the location of its headquarters, which is the research design choice typical in geography-related studies (Dougal et al. 2015; Parsons et al. 2020). Headquarter location is particularly relevant in the analyst coverage decision setting because corporate headquarters is the center of decision making (Giroud 2013; Dougal et al. 2015) and information exchange between the firm and its suppliers, service providers, and investors (Davis and Henderson 2008). We use the zip code of the corporate headquarters extracted from firms' regulatory 10-K filings with the Securities and Exchange Commission to assign each firm to a unique economic area (EA). Economic areas are defined by the U.S. Bureau of Economic Analysis (BEA) as "the relevant regional markets surrounding metropolitan or micropolitan statistical areas," and are "mainly determined by labor commuting patterns that delineate local labor markets and that also serve as proxies for local markets where businesses in the areas sell their

[8] The Nelson's Directory of Investment Research continued until 2008, but only the 1993–2005 volumes are available at our institution. The lack of data beyond 2005 limits the generalizability of our findings to more recent time periods when advances in technology such as social media applications potentially minimize the benefits of in-person social interaction.

[9] The IBES recommendation detail file only contains each analyst's last name and first name initial. We rely on the match in brokerage names to make sure the link is correct. If there is still ambiguity, we exclude these observations from the sample.

products." Examples of economic areas are New York-Newark-Bridgeport, Los Angeles-Long Beach-Riverside, and Philadelphia-Camden-Vineland. In robustness tests, we define geographic area using the metropolitan statistical area (MSA) and obtain qualitatively similar results (Appendix B Table 8).

All firm-specific variables are obtained from the CRSP/COMPUSTAT Merged Database (CCM) and are winsorized at the 1% level to mitigate the influence of outliers. Stock returns and trading volume are obtained from CRSP. Institutional ownership is obtained from CDA/Spectrum 13F Holdings. The latitude and longitude of cities are obtained from the 2010 U.S. Census Bureau's Gazetteer file.[10] The distance between cities is computed using the great circle distance formula. After conditioning into the IBES sample where we can identify the location of the analyst and obtain control variables required for subsequent analyses, we are left with a sample of 234,340 analyst-firm-year observations for our test of earnings forecast accuracy, which represents 5469 unique analysts covering 6527 unique firms.

Table 2 presents descriptive statistics, and formal definitions of all variables are provided in Appendix A Table 7. The average analyst in the sample has 6.93 years of forecasting experience and follows 16.84 firms from 3.61 industries located in 9.51 economic areas during a year. In terms of officemates, the average analyst has 7.31 local peers (*OEA*) and 1.73 industry (*OIND*) peers, consistent with the organization of the analyst business being industry-oriented rather than geography-oriented. The average price-scaled earnings forecast errors (*FE*) in our sample are 0.36%, consistent with the well-known optimism in annual analyst forecasts (Kothari et al. 2016).

4 Main results

4.1 Local peers and earnings forecast accuracy

To test our hypothesis, we estimate the following OLS regression with standard errors clustered at the firm and analyst level:

$$AFE_{i,j(a),k,t} = \beta_0 + \beta_1 LN\_OEA_{-i,j(a),k,t} + Controls + \epsilon_{i,j(a),k,t} \tag{1}$$

where $AFE_{i,j(a),k,t}$ is the absolute forecast error of analyst i's (working at office k) most recent annual earnings forecast for firm j located in area a for fiscal year t, scaled by the firm's stock price at the end of fiscal year t-1, multiplied by 100 (i.e., in percentage terms).[11] We require the analyst to have provided at least one one-year-ahead annual earnings forecast for firm j to be considered for this analysis. Following Clement (1999), we restrict the sample to forecasts supplied during the first 11 months of the fiscal year. We focus on annual forecasts to facilitate benchmarking our results with prior literature, the majority of which uses annual forecasts when *AFE* is the dependent variable. The focus on annual forecasts maximizes external validity because substantially more analysts provide annual forecasts than quarterly forecasts. During our

[10] https://www.census.gov/geo/maps-data/data/gazetteer2010.html. If the city name was not included in the Gazetteer file, we manually searched for the latitude and longitude on http://www.latlong.net/.

[11] The results (not tabulated) are qualitatively unchanged if we instead use *AFE* without scaling by price.

Table 2 Descriptive Statistics

Variable	N	Mean	Median	Std Dev
AFE	234,340	1.04	0.16	3.13
FE	234,340	0.36	−0.01	2.76
OEA	234,340	7.31	3	9.76
OIND	234,340	1.73	1	2.16
BEA	234,340	2.48	0	5.52
BIND	234,340	0.54	0	1.40
age	234,340	153.32	112	83.79
gexp	234,340	6.93	5.45	5.38
fexp	234,340	3.23	1.86	3.69
allstar	234,340	0.15	0	0.36
bsize	234,340	54.82	44	44.23
dist	234,340	1462.68	1087.94	1377.15
nfirm	234,340	16.84	14	12.53
nind	234,340	3.61	3	2.92
nea	234,340	9.51	8	6.01
nfirm_ea	234,340	1.57	0	2.65
nfirm_ind_ea	234,340	0.93	0	1.99

This table reports descriptive statistics for the sample derived in Table 1. Variable definitions are provided in Appendix A Table 7

sample period (1993–2005), the average number of unique analysts each year that provide annual (quarterly) forecasts is 4136 (3287).

$OEA_{-i, j(a), k, t}$ is the number of analyst i's officemates (working at office k) who cover at least one firm in the same area a during year t. To facilitate interpretation relative to other covariates in our model, we use the natural logarithm of *OEA* in the regression; i.e., $LN\_OEA_{-i, j(a), k, t} = \ln(1 + OEA_{-i, j(a), k, t})$. The results (untabulated) are qualitatively the same if we use *OEA* directly. If social interactions with local peers help analysts to better incorporate value-relevant geographic information in their earnings forecasts, we should observe a negative association between the absolute forecast error and the number of local peers (i.e., $\beta_1 < 0$).

Firm-year fixed effects, $\gamma_{j, t}$,represent our most important control variable, as they flexibly absorb any time-varying, firm-specific factors that can affect analysts' forecast accuracy and allow us to compare accuracy across forecasts for the same firm-year. This way, β_1 is identified from variation in analyst accuracy among all forecasts made for the same firm-year.[12]

To guard against the possibility that there are analyst and brokerage characteristics that are positively correlated with both *OEA* and forecast accuracy, we also include brokerage office fixed effects, η_k, and analyst fixed effects, φ_i, to control for any time-

[12] Following prior literature (Clement 1999; Malloy 2005; Bradley et al. 2017), we de-mean both the dependent variable and the independent variables in alternative specifications and obtain qualitatively similar results (not tabulated). We opt to use the firm-year fixed effect specification, as Gormley and Matsa (2014) show that the de-mean approach can generate biased results.

invariant unobserved office or analyst factors. In addition, we include a set of time-varying variables that are commonly associated with forecast accuracy (*Controls*). At the analyst level, we include general experience, as measured by the natural logarithm of the number of years working as a sell-side analyst (*ln_gexp*); an indicator for all-star status (*allstar*); the size of the brokerage, measured by the natural logarithm of the number of analysts in the brokerage firm (*ln_bsize*); and the natural logarithm of the number of covered firms (*ln_nfirm*), industries (*ln_nind*), and economic areas (*ln_nea*).

We also include analyst-firm-pair-level controls, such as the natural logarithm of the number of analysts working in the same office k who also cover a firm in the same industry as firm j during year t (*LN_OIND*, *local industry peers*); the age of the forecast, calculated as the natural logarithm of the number of days between the forecast date and the earnings announcement date (*ln_age*); and the analyst's firm-specific experience, measured by the natural logarithm of the number of years forecasting the firm (*ln_fexp*). As noted in O'Brien and Tan (2015), being in close physical proximity to covered firms facilitates access to first-hand information and enables analysts to produce higher-quality forecasts (Malloy 2005; Bae et al. 2008). We therefore control for the physical distance (measured as the natural logarithm of the distance in kilometers) between the analyst and the firm (*ln_dist*). Finally, to ensure that *OEA* is not capturing analysts who are simply gathering geography-related information themselves, we also include, as control variables, the number (in logarithm) of other firms that analyst i follows that are in the same area (and in the same industry) as firm j during year t (*ln_nfirm_ea* and *ln_nfirm_ind_ea*).

Brokerages may supply broker-wide connective technologies (e.g., direct messaging systems) and resources (e.g., subscriptions to local news) that enable analysts to obtain geographic information in ways that do not require in-office social interaction. The number of analysts within the brokerage utilizing such technologies and resources can vary over time, so we include, as a covariate, *LN_BEA*, defined as the natural logarithm of the number of analysts in the same brokerage firm as analyst i but located in a different office, who also cover firms in the same geographic area as firm j. We do the same for industry peers in the brokerage (*LN_BIND*).

As a collection, in the presence of firm-year, brokerage office, and analyst fixed effects, our identification relies on variation in the number of local colleagues within the same target firm-year, after controlling for time-invariant characteristics specific to the brokerage office and the analyst and a wide range of time-varying factors known to be associated with forecast accuracy. Returning to our introductory example of the hypothetical analysts Jack and Jane, who are otherwise identical analysts in different New York brokerages covering Austin-based Whole Foods. Now suppose that Jack has five colleagues covering the Austin area, while Jane only has two. If Jack's forecast for Whole Foods has a different accuracy than Jane's, then the difference can be attributed to the difference in the number of local peers (five vs. two), which proxies for the information shared among local peers. $\beta_1 < 0$ would be consistent with local peers improving forecast accuracy.

Table 3 presents the results from estimating Eq. (1). Since our main specification relies on a large number of fixed-effects, we build up to the full specification following the suggestion in Jennings et al. (2021) to ascertain the role of fixed effects in drawing inferences. Specifically, Jennings et al. (2021) note that when control variables are measured with error, high dimensional fixed effects can distort the estimate on the main variable of interest and over-reject the null. Thus, by comparing the results with and

Table 3 Local Peers and Annual Forecast Accuracy

	(1)	(2)	(3)	(4)
LN_OEA	−0.009	−0.033***	−0.029***	−0.021***
	(−0.55)	(−7.46)	(−4.56)	(−3.39)
LN_OIND				−0.006
				(−0.69)
LN_BEA				−0.002
				(−0.24)
LN_BIND				−0.022*
				(−1.76)
ln_age				0.715***
				(30.39)
ln_gexp				−0.030
				(−1.53)
ln_fexp				−0.040***
				(−6.85)
allstar				0.008
				(0.49)
ln_bsize				−0.028
				(−1.26)
ln_dist				−0.002
				(−0.68)
ln_nfirm				−0.083***
				(−4.15)
ln_nind				0.006
				(0.49)
ln_nea				0.010
				(0.57)
ln_nfirm_ea				−0.021*
				(−1.88)
ln_nfirm_ind_ea				0.016
				(1.23)
firm-year FE	NO	YES	YES	YES
office FE	NO	NO	YES	YES
analyst FE	NO	NO	YES	YES
observations	234,340	234,340	234,129	234,129
adj. R-sq	0.000	0.817	0.823	0.833

This table reports the results of estimating variants of Eq. (1) via OLS regressions with absolute forecast error (*AFE*) as the dependent variable to examine the relation between local peers and annual forecast accuracy during our sample period from 1993 to 2005. Variable definitions are provided in Appendix A Table 7. Standard errors are double clustered by firm and analyst. T-statistics are in parentheses. ***, **, and * denote two-sided tests of significance levels of less than 0.01, 0.05, and 0.1, respectively

without the control variables, we can gauge whether the fixed-effects erroneously overestimate the coefficient of interest.

In column (1), we find a negative coefficient on *LN_OEA* equal to −0.009 that is statistically insignificant (t-stat = −0.55), indicating that local peers do not have a first-order effect on forecast accuracy across firms. This is not surprising, as a firm's business model and the time period are expected to be the primary factors for how predictable the firm's reported earnings are. Consistent with this interpretation, in column (2), we add firm-year fixed-effects and find that they explain the vast majority (81.7%) of variation in forecast accuracy. The coefficient for *LN_OEA* is −0.033 and significant at less than the 1% level, indicating that among annual earnings forecasts for the same firm-year, those by analysts with more local peers are more accurate. In column (3), we control for time-invariant brokerage office and analyst-fixed effects and find only a marginal increase in the regression R-squared to 82.3%. The coefficient estimate on *LN_OEA* is slightly smaller at −0.029, but nonetheless remains highly significant.

Finally, we present the fully specified model in column (4). We find that the coefficient estimate on *LN_OEA* is further reduced to −0.021, but remains significant at less than the 1% level, consistent with social interactions among local peers improving analyst forecast accuracy. The fact that the estimated coefficient on our main variable of interest is lowered by the inclusion of additional control variables indicates that the problem of over-rejecting-null with high dimensional fixed effects (as noted in Jennings et al. (2021)) is unlikely to be driving our result.

It is worth noting that the coefficient on *LN_BEA* is statistically insignificant, implying that brokerage peers (analysts in the same brokerage firm as analyst *i* but located in a different office who also cover firms in the same geographic area as firm *j*) are not important for forecasting accuracy, consistent with in-office personal interaction facilitating information transmission. The coefficient for brokerage industry peers (*LN_BIND*) is negatively (at −0.022) and marginally significant at less than 10%, indicating that forecasts are more accurate in brokerage firms with more analysts covering the same industry. To the extent that *LN_BIND* is correlated with brokerage firms' industry specialization, this is consistent with the fact that brokerage firms are organized by industries, with different brokerages specializing in different industries. At the same time, the coefficient on *LN_OIND* is not statistically different from zero, implying that forecast quality is not impacted by local industry peers in the office. This is arguably consistent with analysts within an office competing by industry, which either results in a reluctance to share information or the non-existence of a local industry peer in the first place.

The explanatory power of the regression estimated in column (4) is high, with an adjusted R-squared of 83.3%. This implies that the local peer effect we observe is incremental to an exhaustive set of controls. In economic terms, the estimated coefficient of −0.021 implies that a 33% increase in local peers (representing one more local peer relative to the median number of local peers of three) is associated with a reduction in absolute forecast error by 0.007%, an improvement of 4.4% relative to the 0.16% sample median value. To put this result in context within our sample, analysts with more firm-specific experience forecasting the firm have higher forecast accuracy, as indicated by the coefficient on *ln_fexp* of −0.04 (t-stat = −6.85). This effect implies that, relative to the median firm experience of 1.86 years, an additional seven months (33% of 1.86 years) of experience would reduce absolute forecast error by 0.007% (=0.04%* 0.33/1.86).

To further calibrate the magnitude of the local peer effect, we compare the 4.4% improvement in forecast accuracy from adding one more local peer to the median number with the network effects documented in the extant literature. Bradley et al. (2020) show that analysts with a past professional connection to corporate management provide earnings forecasts that are 2.0% to 4.4% more accurate than analysts without such a connection. Similarly, Fang and Huang (2017) show that when an analyst is connected to a company's board via being an alumnus of the same university, forecast accuracy increases by 2.0% to 6.0%, depending on the gender of the analyst. Overall, local peers appear to provide a unique type of network that results in similar improvements to other networks studied in the literature. Finally, we note that column (4) shows that the effects of local peers are robust after controlling for the geographic distance (*ln_dist*) between analysts and their covered firms, indicating that the effect of social interactions with local peers is distinct from that of geographic proximity documented in prior literature (Malloy 2005; Bae et al. 2008).[13]

4.2 Subsample analysis

To shed further light on information sharing among local peers, we examine whether the results differ in subsamples partitioned by proxies for when local peers are particularly valuable to analysts who are forecasting earnings. We consider three different analyses. Our first subsample analysis considers the period before and after the passage of Regulation FD, which is a time series partition of the data. Research suggests that Regulation FD severed direct access to management as an information source (Wang 2007; Agrawal et al. 2006; Cohen et al. 2010). As such, we expect that the benefits of local peers will increase after Regulation FD, as the availability of alternative information sources, including local peers, plays a relatively more important role in forecasting earnings.

Our second subsample analysis pertains to whether the firm exhibits high or low earnings volatility, which partitions the data based on a firm characteristic. Low volatility earnings are easier to predict based on past information, relative to high volatility earnings (Dichev and Tang 2009). We therefore expect that other sources of information, such as that provided via social interactions with local peers, will be more helpful when earnings are more difficult to predict.

Our final subsample analysis considers an exogenous shock to analysts' motivation to work hard and to utilize more of the resources available to them, including local peers. Specifically, Li et al. (2020) show that when all-star analysts leave the brokerage, their departure triggers more intense competition among non-all-star analysts, which results in the generation of more accurate forecasts. We therefore posit that the utilization of local peers will increase when all-stars leave the brokerage. To test this, we partition the data on a brokerage characteristic, i.e., whether the brokerage experienced a departure by an all-star analyst during the prior year.

Table 4 presents the results of these subsample analyses. Column (1) presents the estimation of Eq. (1) in the period after the passage of Regulation FD, and column (2)

[13] Column (4) shows an insignificant coefficient for *ln_dist* because we include more controls than Malloy (2005). We can uncover the Malloy (2005) finding when we only include firm-year fixed effects and do not control for the number of local peers.

Table 4 Subsample Analyses

	(1)	(2)	(3)	(4)	(5)	(6)
	After FD	Before FD	High Earn Vol	Low Earn Vol	Star Depart=1	Star Depart=0
LN_OEA	−0.025**	−0.009	−0.027**	−0.002	−0.043***	−0.012
	(−2.35)	(−1.21)	(−2.37)	(−0.29)	(−3.52)	(−1.54)
LN_OIND	−0.015	0.000	−0.024	0.019**	−0.035**	0.000
	(−0.95)	(0.01)	(−1.57)	(2.29)	(−2.09)	(0.01)
LN_BEA	−0.013	0.007	−0.002	−0.008	−0.008	−0.012
	(−1.36)	(0.67)	(−0.13)	(−1.41)	(−0.64)	(−1.24)
LN_BIND	−0.018	−0.012	−0.031	−0.002	−0.037*	−0.024
	(−1.02)	(−0.64)	(−1.48)	(−0.16)	(−1.68)	(−1.28)
firm-year FE	YES	YES	YES	YES	YES	YES
office FE	YES	YES	YES	YES	YES	YES
analyst FE	YES	YES	YES	YES	YES	YES
observations	109,307	123,603	101,716	101,524	83,364	141,321
adj. R-sq	0.823	0.854	0.833	0.824	0.811	0.839

This table reports the results of estimating the analysis in Column 4 of Table 3, which spans 1993–2005, in various subsamples. Each column is an OLS regression of Eq. (1) with absolute forecast error (*AFE*) as the dependent variable. Column (1) presents the estimation of Eq. (1) in the period after the passage of Regulation FD, and column (2) presents results of the same estimation in the period prior to Regulation FD passage. Column (3) presents the estimation of Eq. (1) in the subsample with high earnings volatility, and column (4) presents the same estimation in the subsample with low earnings volatility, where high volatility is assessed as being above the sample median in a given year. In columns (5) and (6), we consider analysts at brokerages where all-star analysts have and have not departed during the prior year, respectively. Variable definitions are provided in Appendix A Table 7. Standard errors are double clustered by firm and analyst. T-statistics are in parentheses. ***, **, and * denote two-sided tests of significance levels of less than 0.01, 0.05, and 0.1, respectively

presents results of the same estimation in the period prior to its passage. We observe a statistically significant association between local peers and forecast accuracy in the post-FD period (coefficient = −0.025, t-stat = −2.35) but not in the pre-FD period (coefficient = −0.009, t-stat = −1.21). In columns (3) and (4), we consider the high and low earnings volatility subsamples, respectively, where earnings volatility is measured as the standard deviation of earnings scaled by average total assets over the past five years, and high (low) volatility is assessed as being above (below) the sample median in a given year. We observe statistically significant effects for local peers in the high earnings volatility subsample (coefficient = −0.027, t-stat = −2.37) but not in the low earnings volatility subsample (coefficient = −0.002, t-stat = −0.29). Finally, in columns (5) and (6) we consider analysts at brokerages where all-star analysts have and have not departed during the prior year, respectively. We find that in the subsample where all-star analysts have departed, local peer effects are statistically significant (coefficient = −0.043, t-stat = −3.52); where there has been no all-star departure, local peer effects are not statistically significant (coefficient = −0.012, t-stat = −1.54).

Overall, the findings from subsample analyses suggest that local peer effects are statistically detectable when analysts have less access to corporate management, when earnings are harder to forecast, and when analysts have incentives to work particularly hard. That we do not observe statistically significant results in the subsamples where we expect the local peer effect to be weaker further helps rule out the possibility that our effects are driven by a combination of measurement error in control variables and the usage of high dimensional fixed effects.

The subsample analyses provide two additional insights that help validate our inference. The first is that the analysis based on Regulation FD provides some comfort that our findings are not completely devoid of external validity. Communication technologies arguably increased during our sample period, with smart phones and social media beginning to emerge towards the end. If communication technologies substitute for in-person social interaction, arguably we would find smaller peer effects later in our sample than earlier in our sample. However, this is not the case empirically, as Regulation FD occurred just beyond the midpoint of our sample.

The second pertains to all-star departures. When interpreting our findings, we take local peers to be exogenous. One could argue that the number of local peers in a brokerage office is not completely exogenous, as brokerages choose the cities in which they set up offices, and analysts select into offices and ultimately pair with individual firms (Liang et al. 2008). In our private discussions with sell-side analysts, we learned that the number of geographic peers in an office is not a statistic that is tabulated when recruiting analysts, so, at least anecdotally, we have no reason to suspect that analysts choose to work at brokerages based upon potential access to local peers. Rather, our private discussions suggest that, on the margin, an analyst would certainly consult a local peer colleague in the same office if they were available. We have no ability, however, to empirically estimate the determinants of the level of local peers at a brokerage. By using all-star turnover as an exogenous shock to analyst effort, we have some comfort that the results we document cannot be fully accounted for by endogenous matching between analysts and brokerages.

4.3 Alternative measure of geographic area and the effect of geographic dispersion

The definition of a geographic area is a key design choice in our empirical estimations. We therefore investigate whether our results are manifestations of our research design choice to define economic areas following the U.S. Bureau of Economic Analysis. In Appendix B Table 8, we instead define geographic areas using metropolitan statistical areas (MSAs) and construct local peers (and all variables related to geography) in accordance with this definition. We obtain a coefficient estimate of −0.022 (t-stat = −3.50) on the new local peer measure *LN_OMSA* with the fully specified model in column (4), which is virtually identical to what we document in the same column of Table 3. This suggests that our results are not sensitive to how we define geographic areas.

We define local peers as officemates who cover firms headquartered in the same area. The premise is that firm headquarters are the information hub as well as the center of decision-making. For example, Dougal et al. (2015) find strong evidence of geographic spillovers at firms' headquarters, and the effect is stronger for larger relative to smaller firms. Green et al. (2019) find that the sentiments of employees who are nearer to firm headquarters provide more fundamental information (including on sales growth, earnings growth, and earnings surprises) about the firm than employee ratings from workers who are not near headquarters. However, it is reasonable to ask whether our results are sensitive to this assumption, i.e., whether headquarter locations play an important role for firms whose operation locations are widely dispersed geographically. To examine this issue, we consider a subsample analysis utilizing geographic dispersion. Garcia and Norli (2012) count the number of states mentioned on a firm's Form 10-K, with more states implying higher geographic dispersion. If geographic dispersion at least partially mitigates the role of area-specific information (which is around firm headquarters), then we would expect stronger local peer effects in less dispersed firms. We therefore use an above (below) the sample median number of states to proxy for high (low) dispersion. In untabulated results, we find the coefficient on local peers to be negative and significant in both subsamples, and of similar magnitudes (−0.022, t-stat = −2.51 in the high dispersion subsample and − 0.026, t-stat = −2.47 in the low dispersion subsample). We interpret this result as consistent with prior literature that indicates the importance of headquarter location.

5 Additional analyses

5.1 Nature of information transmitted by local peers

The results thus far suggest that local peer information sharing improves forecast accuracy, but they do not speak to what information is being shared and whether the shared information has area-specific elements. Since we cannot directly observe whether or what area-specific issues are discussed among local peers, we take an indirect approach to shed light on this issue.

Specifically, we examine whether analysts with more local peers are more or less likely to pick up area-specific shocks to local firm earnings. We do so in two steps. We first obtain a proxy for area-specific shock to by estimating Eq. (2) below:

$$CFE_{j,a,t} = X_{a,t} + Z_{ind,t} + \epsilon_{j,a,t} \tag{2}$$

where $CFE_{j,\,a,\,t}$ is the consensus signed forecast error for firm j located in area a during year t (calculated as the average of all the analysts' forecasts of firm j minus actual earnings), $X_{a,\,t}$ is the area-year fixed effects, and $Z_{ind,\,t}$ is the industry-year fixed effects. Eq. (2) essentially decomposes shocks to firm earnings (as captured by the consensus forecast errors) into an industry-year component ($Z_{ind,\,t}$), an area-year component ($X_{a,\,t}$), and a firm-specific component ($\epsilon_{j,\,a,\,t}$), with $X_{a,\,t}$ capturing shocks common to all firms in a given area-year that are orthogonal to the industry shock.

We then examine whether local peers help analysts become more attuned to the area shock by estimating Eq. (3) below:

$$\begin{aligned} FE_{i,j(a),k,t} = {} & \beta_0 + \beta_1 LN\_OEA_{i,j(a),k,t} + \beta_2 LN\_OEA * \widehat{X}_{a,t} + Controls + \gamma_{j(a),t} \\ & + \eta_k + \varphi_i + \epsilon_{i,j(a),k,t} \end{aligned} \tag{3}$$

where $FE_{i,\,j(a),\,k,\,t}$ is the signed forecast error of analyst i's (working at office k) forecast of firm j located in area a during year t. $\widehat{X}_{a,t}$ is the estimated area-year fixed effect from

Eq. (2) above. The remaining variables in Eq. (3) are the same as in Eq. (1). $\widehat{X}_{a,t}$ is not estimable in the presence of firm-year fixed effects and therefore is not separately included.[14] Our main coefficient of interest is β_2. If analysts with more local peers are better able to internalize and understand the impact of geographic factors on firm earnings, their forecast errors are expected to be less affected by local shocks. If so, we should expect to find $\widehat{\beta}_2 < 0$.

We use the signed forecast error instead of the absolute value of the forecast error so that we can ascertain whether forecast error improvements are differentially more likely for positive versus negative area-specific shocks. Considering the possibility of asymmetry is important for two reasons. First, experimental work shows that negative news is more likely to survive transmission in social networks than positive news is (Bebbington et al. 2017; Baumeister et al. 2001; Rozin and Royzman 2001).[15] Second, in our sample, overall signed annual analyst forecast errors exhibit the commonly observed optimistic bias (Kothari et al. 2016). Together, the forecast accuracy effects of local peers would therefore plausibly stem from optimism reductions. The consideration of signed forecast errors in the analysis allows us to examine this possibility.

Table 5 presents the results from estimating Eq. (3). In column (1), we present the estimation of Eq. (3) for the area-specific component of the consensus forecast error, and in column (2) we allow $\widehat{X}_{a,t}$ to vary in a piecewise linear fashion based on whether $\widehat{X}_{a,t} > 0$ (i.e., positive common errors in the consensus forecast and hence negative area-related news) or $\widehat{X}_{a,t} < 0$ (i.e., positive area-related news) to examine if there exists asymmetry in local peer information transmission. In column (1), we find a negative but statistically insignificant coefficient on the interaction between *LN_OEA* and $\widehat{X}_{a,t}$ (coefficient = −0.037, t-stat = −1.27). However, when we allow the effect to vary based on whether area-related news is positive or negative, we find a negative coefficient − 0.066 (t-stat = −2.02) for $LN\_OEA * \widehat{X}_{a,t}$ when $\widehat{X}_{a,t} > 0$ (i.e., negative area-related news) and an insignificant coefficient when $\widehat{X}_{a,t} < 0$ (i.e., positive area-related news).[16]

In both columns, we also observe a negative and statistically significant coefficient estimate on *LN_OEA*. Given that the average annual forecast error in the sample is optimistically biased (at 0.36% per Table 2), this result implies that analysts with local peers generate more accurate forecasts as a result of exhibiting less optimistic bias. In

[14] To be consistent with our earlier analysis, we estimate Eqn. (3) after scaling both $FE_{i,j(a),k,t}$ and $\widehat{X}_{a,t}$ with the firm's equity price at the beginning of year *t*. In a sensitivity test (not reported), we obtain qualitatively similar results when we use unscaled versions of $FE_{i,j(a),k,t}$ and $\widehat{X}_{a,t}$.

[15] This effect is hypothesized to be evolutionary in nature, as human survival is maximized by knowing when negative situations exist.

[16] One concern for this analysis is that the dependent variable in Eqn. (3) is part of the sample used in estimating the area-specific shock; thus, the coefficient estimate on the interaction term of *LN_OEA* and $\widehat{X}_{a,t}$ may simply capture the mechanical correlation between the dependent variable and $\widehat{X}_{a,t}$. We note that the average number of firms headquartered in an area in our sample is 140, and the consensus for an average firm is based on 7.3 analyst forecasts. This means the dependent variable in Eqn. (3) is only one of the 1022 (=7.3*140) forecast errors used in constructing area-specific shock, suggesting that there are small mechanical correlation effects. Nonetheless, in an untabulated robustness check, we re-estimate Eqn. (3) by estimating an area-year specific shock without including the consensus forecast for the firm followed by analyst *j*. We find that our main results remain qualitatively similar.

Table 5 Nature of Information Shared Among Local Peers

	(1)	(2)
LN_OEA	−0.036***	−0.031***
	(−4.53)	(−3.02)
$LN\_OEA*\widehat{X_{a,t}}$	−0.037	
	(−1.27)	
$LN\_OEA*\widehat{X_{a,t}}*Dummy(\widehat{X_{a,t}}>0)$		−0.066**
		(−2.02)
$LN\_OEA*\widehat{X_{a,t}}*Dummy(\widehat{X_{a,t}}<0)$		−0.020
		(−0.42)
LN_OIND	−0.000	−0.000
	(−0.04)	(−0.03)
LN_BEA	−0.007	−0.007
	(−0.81)	(−0.80)
LN_BIND	0.021	0.021
	(1.34)	(1.34)
controls	YES	YES
firm-year FE	YES	YES
office FE	YES	YES
analyst FE	YES	YES
observations	232,034	232,034
adj. R-sq	0.695	0.695

This table reports the results from estimating Eq. (3) below:

$$FE_{i,j(a),k,t} = \beta_0 + \beta_1 LN\_OEA_{i,j(a),k,t} + \beta_2 LN_{OEA} * \widehat{X_{a,t}} + Controls + \gamma_{j(a),t} + \eta_k + \varphi_i + \epsilon_{i,j(a),k,t} \quad (3)$$

where $FE_{i,j(a),k,t}$ is the signed forecast error (forecast minus realization) by analyst i working for brokerage k following firm j headquartered in area a for year t, scaled by firm i's share price at the beginning of year t. $\widehat{X_{a,t}}$ is the area-year fixed-effects estimated (scaled by firm i's share price at the beginning of year t) in Eq. (2) below:

$$CFE_{j,a,t} = X_{a,t} + Z_{ind,t} + \epsilon_{j,a,t} \quad (2)$$

where $CFE_{j,a,t}$ is the consensus signed forecast error for firm j located in area a during year t (calculated as the average of all the analysts' forecasts of firm j minus the actual earnings), $X_{a,t}$ is the area-year fixed effects, and $Z_{ind,t}$ is the industry-year fixed effects

We use the same sample from 1993 to 2005 as in Table 3. All control variables from Table 3, firm-year fixed effects, office fixed-effects, and analyst fixed effects are included in the regressions. In column (2) we allow $\widehat{X_{a,t}}$ to vary in a piecewise linear fashion based on whether $\widehat{X_{a,t}} > 0$ or $\widehat{X_{a,t}} < 0$ to examine if there exists asymmetry in how local peers transmit information. Standard errors are double clustered by firm and analyst. T-statistics are in parentheses. ***, **, and * denote two-sided tests of significance levels of less than 0.01, 0.05, and 0.1, respectively

addition, the coefficient in column (2) slightly attenuates (at −0.031) relative to the coefficient in column (1) (at −0.036), where area-specific news is not allowed to vary asymmetrically. This suggests that part of the main effect of local peers on forecast bias is due to their helping analysts become more attuned to negative area-specific shocks.

As a collection, these findings suggest that the benefits of local peers accrue at least partially via local peers better accommodating negative shocks in their earnings forecasts. In other words, social interaction among local peers appears to be more effective in diffusing negative news, consistent with research noting that negative information survives transmission more than positive information (Baumeister et al. 2001; Rozin and Royzman 2001; Bebbington et al. 2017).

5.2 Effects of local peers on the pricing of geographic information

The picture that emerges from the results thus far is that information shared among local peers can help analysts improve forecast quality. Analysts with more local peers produce more accurate forecasts, and the informational benefit appears to occur, at least in part, from local analysts being attuned to negative area-specific information. If local peers facilitate a better understanding of negative area information, implications exist for the geographic momentum findings in Parsons et al. (2020). Parsons et al. (2020) document geographic momentum and suggest that it exists because analysts do not scrutinize local area information due to the industry (not geographic) focus of the brokerage business.

Our findings imply that analysts could play a role in diminishing geographic momentum. Specifically, we expect that the geographic momentum in stock returns documented in Parsons et al. (2020) will be attenuated when firms are followed by analysts with local peers, and that the attenuation will be concentrated most among negative returns. Put differently, negative area returns should exhibit less momentum due to local peers facilitating a quicker impounding of negative area-specific information into equity prices. To assess whether this is the case, we begin by estimating the panel regression model in Parsons et al. (2020)[17]:

$$\begin{aligned} ret_{j,t} = {} & \beta_0 + \beta_1 ret_{ind,t-1} + \beta_2 ret_{a,t-1} + \beta_3 ret_{a,t-1} {*} LN\_OEA + \beta_4 LN\_OEA \\ & + Control_{j,t-1} + \gamma_t + \epsilon_{j,t} \end{aligned} \tag{4}$$

where $ret_{j,\,t}$ is the return of firm j during month t; $ret_{ind,\,t-1}$ is the average return of all firms in the same industry as firm j during month t-1; $ret_{a,\,t-1}$ is the average return of all firms in the same area as firm j (excluding firms in the same industry as j) during month t-1; $control_{j,\,t-1}$ is a set of control variables for firm j, including returns, size, market to book ratio, institutional ownership, trading volume, return on assets (ROA), and sales growth, all as of month t-1; and γ_t is the month fixed effects. $\beta_1 > 0$ indicates industry momentum, and $\beta_2 > 0$ indicates geographic momentum. β_3 captures the extent to which local peers moderate geographic momentum. If local peers assist in the diffusion of geography-related information, we expect geographic momentum to be attenuated, which would be evidenced by $\beta_3 < 0$.

Table 6 presents the results from estimating Eq. (4). In column (1), we first replicate the basic specification in Parsons et al. (2020) without considering the effect of local peers. We find a point estimate of 0.206 and 0.116 for industry and geographic momentum, respectively. These findings are similar in magnitude to Parsons et al. (2020), who also document that the geographic momentum effect is roughly half the magnitude of the industry momentum effect. In column (2), the estimate for β_2 is 0.217

[17] Following Parsons et al. (2020), we only include firms in the 20 largest EAs in the sample.

Table 6 Local Peers and Return Momentum

	(1)	(2)	(3)	(4)
Sample	*Full Sample*	*Full Sample*	*Positive Returns Subsample*	*Negative Returns Subsample*
$ret_{ind, t-1}$	0.206**	0.209**	0.178	0.269**
	(1.98)	(2.01)	(1.37)	(2.72)
$ret_{a, t-1}$	0.116**	0.217***	0.098	0.303***
	(2.60)	(3.81)	(1.27)	(4.53)
$ret_{a, t-1}*LN\_OEA$		−0.060***	−0.033	−0.091***
		(−3.73)	(−1.37)	(−2.93)
LN_OEA		0.001	0.001	−0.004**
		(0.80)	(0.057)	(2.07)
controls	Y	Y	Y	Y
month FE	Y	Y	Y	Y
observations	437,759	437,759	267,604	170,154
adj. R-sq	0.111	0.112	0.081	0.149

Column (1) of this table reports the estimates from the following panel regression (Eq. (4)) for a sample from 1993 to 2005:

$$ret_{j,t} = \beta_0 + \beta_1 ret_{ind,t-1} + \beta_2 ret_{a,t-1} + \beta_3 ret_{a,t-1} * LN\_OEA + \beta_4 LN\_OEA + Control_{j,t-1} + \gamma_t + \epsilon_{j,t}$$

where $ret_{j,t}$ is the return of firm j during month t, $ret_{ind,t-1}$ is the average return of all firms in the same industry as firm j during month t-1, and $ret_{a,t-1}$ is the average return of all firms in the same area as firm j (excluding firms in the same industry as j) during month t-1. $Control_{J,t-1}$ is a set of control variables for firm j, including returns, size, market to book ratio, institutional ownership, trading volume, ROA, and sales growth, all as of month t-1. γ_t is the month fixed effects. Column (2) allows the coefficient on $ret_{a,t-1}$, the geographic momentum estimate, to vary based on the average number of local peers of the analysts that follow the firm. Columns (3) and (4) allow the coefficients estimated in column (2) to vary based on whether the return was *positive* (an indicator variable that equals 1 if $ret_{a,t-1} > 0$ and zero otherwise) or *negative* (an indicator variable that equals 1-*positive*), respectively. T-statistics are in parentheses. ***, **, and * denote two-sided tests of significance levels of less than 0.01, 0.05, and 0.1, respectively

(t-stat. = 3.81), suggesting that, for firms followed by analysts with no local peers (in which case $LN\_OEA = 0$), the geographic momentum effect is of a similar magnitude than the industry momentum effect. We also observe a negative and significant estimate for β_3, −0.060 (t-stat = −3.73), suggesting that local peers attenuate the geographic momentum effect.

In columns (3) and (4), we allow for asymmetric momentum effects by separately estimating Eq. (4) in subsamples where area returns are positive and negative, respectively.[18] This subsample analysis serves two purposes. First, an established finding in the momentum literature is that bad news travels more slowly than good news (Hong et al. 2000), driven in part by managers pushing good news to the market more quickly than bad news (Kothari et al. 2009). Second, given our earlier findings, we expect otherwise slow-moving negative information to move somewhat faster as a result of local peers, implying an attenuating effect of local peers when area returns are negative. Consistent with momentum effects in general existing more when returns are negative,

[18] Returns are never exactly equal to zero in our sample.

we find that momentum effects are statistically detectable in the negative area-return subsample (column 4) but not in positive area-return subsample (column 3).

In the negative area-return subsample, industry momentum effects are of similar economic magnitude (coefficient = 0.269, t-stat = 2.72) to geographic momentum (coefficient = 0.303, t-stat = 4.53). More importantly, local peers statistically attenuate geographic momentum when area returns are negative (coefficient = −0.091, t-stat = 2.93) but not when area returns are positive (coefficient = −0.033, t-stat = 1.37). This differential local peer effect is consistent with stock prices responding more efficiently to negative news (as manifested by a lower geographic momentum effect) for firms that are followed by more local peers.

6 Conclusion

We provide evidence that social interactions among local peers (analysts in the same brokerage office who cover firms in the same geographic areas) share information that improves analysts' forecast quality. Based on a comprehensive, hand-collected dataset on equity analysts' office locations, we find that forecasts by analysts with more local peers are more accurate, with the effect being stronger (1) after Regulation FD (which reduces analysts' direct access to firm management), (2) for firms with more volatile earnings, and (3) when analysts have more incentives to pay attention to various information sources. Furthermore, we find that social interactions among local peers help analysts better incorporate negative area-specific news, consistent with research suggesting that within social networks, negative news is more likely to survive transmission. In addition, we find that the geographic momentum effect is less pronounced for firms covered by analysts with more local peers, and that this effect is concentrated among observations where area returns are negative. Thus, one implication of local peers for the capital market is that prices impound geography-related information more efficiently.

While our findings suggest that social interactions among analysts are useful for transmitting area-specific information, we cannot directly ascertain the particular conversations that facilitate such transmission. Future research might consider the contents of analyst reports that interpret geographic-specific issues. Additionally, our sample period ends in 2005, and numerous technological changes have occurred since then. The extent to which in-person social interactions continue to provide value is an empirical question.

Appendix A

Table 7 Variable Definitions

Variable	Definition
$FE_{i, j, t}$	Signed forecast error of the most recent annual earnings issued during the first 11 months of the fiscal year (Clement 1999) by analyst *i* on firm *j* during fiscal year *t*, scaled by the firm's stock price at the end of fiscal year *t*-1, multiplied by 100 (i.e., in percentage terms).
$AFE_{i, j, t}$	Absolute value of $FE_{i, j, t}$ (in percentage terms).
$OEA_{-i, j(a), k, t}$	Number of analysts in office *k* (excluding analyst *i*) who cover at least one firm located in firm *j*'s area (*a*) during year *t*. The natural logarithm of this variable plus one (*LN_OEA*) is used in the regressions.
$OIND_{-i, j, k, t}$	Number of analysts in office *k* (excluding analyst *i*) who cover at least one firm in firm *j*'s industry during year *t*. The natural logarithm of this variable plus one (*LN_OIND*) is used in the regressions.
$BEA_{-i, j(a), t}$	Number of analysts working for the same brokerage firm as analyst *i* but located in a different office who cover at least one firm located in firm *j*'s area (*a*) during year *t*. The natural logarithm of this variable plus one (*LN_BEA*) is used in the regressions.
$BIND_{-i, j, t}$	Number of analysts working for the same brokerage firm as analyst *i* but located in a different office who cover at least one firm in firm *j*'s industry during year *t*. The natural logarithm of this variable plus one (*LN_BIND*) is used in the regressions.
$age_{i, j, t}$	Number of days between the forecast date of analyst *i* and the earnings announcement date of firm *j* for year *t*. The natural logarithm of this variable is used in the regressions.
$gexp_{i, t}$	General experience of analyst *i* during year *t*, measured as the number of years since analyst *i* first appeared in IBES. The natural logarithm of this variable plus one is used in the regressions.
$fexp_{i, j, t}$	Firm-specific experience of analyst *i* on firm *j* as of the forecast date *t*, measured as the years since analyst *i* first initiated coverage of firm *j*. The natural logarithm of this variable plus one is used in the regressions.
$allstar_{i, t}$	Dummy variable that equals 1 if analyst *i* is an all-star analyst during year *t*.
$bsize_{i, t}$	Number of analysts in analyst *i*'s affiliated brokerage firm during year *t*. The natural logarithm of this variable is used in the regressions.
$dist_{i, j, t}$	The distance between analyst *i*'s office and firm *j*'s headquarters in year *t* (in *km*). The natural logarithm of this variable plus one is used in the regressions.
$nfirm_{i, t}$	Number of firms for which analyst *i* issues an earnings forecast during year *t*. The natural logarithm of this variable is used in the regressions.
$nind_{i, t}$	Number of industries (six-digit GICS code) followed by analyst *i* during year *t*. The natural logarithm of this variable is used in the regressions.
$nea_{i, t}$	Number of economic areas (defined by the Bureau of Economic Analysis) followed by analyst *i* during year *t*. The natural logarithm of this variable is used in the regressions.
$nfirm\_ea_{i, j, t}$	Number of firms (excluding firm *j*) that analyst *i* follows in the same area as firm *j* during year *t*. The natural logarithm of this variable plus one is used in the regressions.
$nfirm\_ind\_ea_{i, j, t}$	Number of other firms (excluding firm *j*) that analyst *i* follows in the same area and industry as firm *j* during year *t*. The natural logarithm of this variable plus one is used in the regressions.
$CFE_{j, a, t}$	Consensus forecast error of firm *j* located in area *a* for year *t*, calculated as the consensus forecast minus the actual earnings.
$ret_{j, t}$	Return of firm *j* during month *t*.
$ret_{j, ind, t-1}$	Average return of all firms in the same industry as firm *j* (excluding firms in the 20 largest EAs) during month *t*-1.
$ret_{j, a, t-1}$	Average return of all firms in the same area as firm *j* (excluding firms in the same industry as *j*) during month *t*-1.

Appendix B

In this appendix, we report the results of estimating Eq. (5), where geographic area is defined by metropolitan statistical area (MSA) instead of economic area (EA):

$$AFE_{i,j(a),k,t} = \beta_0 + \beta_1 LN\_OMSA_{-i,j(a),k,t} + Controls + \gamma_{j(a),t} + \eta_k + \varphi_i + \epsilon_{i,j(a),k,t} \quad (5)$$

Here *LN_OEA* in Eq. (5) is replaced by *LN_OMSA*. All control variables from Table 3 are the same except for *LN_BEA*, which is replaced by *LN_BMSA*. Standard errors are double clustered by firm and analyst. T-statistics are in parentheses. ***, **, and * denote two-sided tests of significance levels of less than 0.01, 0.05, and 0.1, respectively.

Table 8 Alternative Definition of Geographic Area

	(1)	(2)	(3)	(4)
LN_OMSA	−0.021	−0.032***	−0.025***	−0.022***
	(−1.19)	(−7.04)	(−3.93)	(−3.50)
LN_OIND				−0.006
				(−0.67)
LN_BMSA				−0.000
				(−0.02)
LN_BIND				−0.023*
				(−1.78)
Controls	NO	NO	NO	YES
firm-year FE	NO	YES	YES	YES
office FE	NO	NO	YES	YES
analyst FE	NO	NO	YES	YES
Observations	234,340	234,340	234,129	234,129
adj. R-sq	0.000	0.817	0.823	0.833

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