# The Effect of In-Person Interaction on Analysts' Forecast Accuracy: Evidence from COVID-19 Lockdowns

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**Abstract**

For analysts' information gathering efforts, we investigate the importance of analysts' in-person interactions with covered firm stakeholders. Using Covid-19 lockdowns as an exogenous shock to these in-person interactions and a difference-in-difference design, we find that local analysts no longer produce more accurate earnings forecasts in lockdown periods – a finding which suggests that in-person interactions are an important source of information that cannot be easily substituted with other information sources. Consistent with this information story, we only observe a decline in local analysts' performance in the following situations: when the covered firm has less voluntary disclosure, when local analysts do not have a private information channel with firm management, or when local analysts have greater experience. In supplementary analyses, we also find that local analysts provide less informative forecast revisions and are less likely to be bold during lockdown. Overall, we provide new evidence that broad in-person interactions with firm stakeholders matter to analyst performance.

**Key words:** sell-side analysts, in-person information gathering, geographic proximity, Covid-19

## I. INTRODUCTION

Sell-side analysts play an important role in acquiring and disseminating information in capital markets (Bradshaw et al. 2017). Despite the importance of analysts' information collection and its effects on their earnings forecasts, research in this area is still relatively scarce (e.g., Bradshaw, 2011; Brown et al., 2015).[1] In this paper, we examine one important way that analysts can collect information and ask the following research question: in analysts' information gathering efforts, how important are in-person interactions with covered firm stakeholders?

To provide insight into this question, we use a difference-in-difference (DiD) design where the outcome variable is earnings forecast accuracy and the exogenous shock is Novel Coronavirus Disease-2019 (COVID-19) lockdowns in the United States (US) which reduced analysts' ability to collect information via in-person interactions with covered firm stakeholders.[2] In our DiD design, we categorize local analysts (i.e., those analysts that are geographically close to their covered firm) as the treated group and faraway analysts as the control group. This categorization assumes that local analysts are more affected by the information shock because they rely more on in-person interactions (before the pandemic began) to gather information about nearby firms. Using our DiD design, we examine how the performance of local analysts versus faraway analysts changes around the information shock and shed light on the importance of in-person interactions for analysts' information collection.

The expected performance changes described above are not obvious ex-ante. On the one hand, we may expect the relative performance of local analysts to worsen during lockdown if

[1] Prior research has often focused on how analyst performance relates to analyst attributes (Mikhail et al., 1997; Jacob et al., 1999; Malloy, 2005; Cohen et al., 2010). A recent stream of literature has begun to focus on explicit information gathering activities of analysts – for example, corporate site visits (e.g., Cheng et al., 2016; Han et al., 2018) or Bloomberg terminal usage (Ben-Raphael et al., 2022).

[2] We use lockdown policy and "stay home – work safe" policy interchangeably.

they rely heavily on in-person interactions for their information advantage.[3] On the other hand, we may expect the relative performance of local analysts to remain steady during lockdown if they do not rely heavily on in-person interactions for their information advantage. In this instance, local analysts may rely more heavily on observing local economic conditions for their information advantage or alternatively they may be able to easily substitute in-person information sources for other sources such as telephone conversations, virtual meetings, written firm disclosures, or other online websites.

To investigate our research question empirically, we use US-based analysts' quarterly earnings forecasts for US-based covered firms from January 2016 to June 2020. For each analyst-firm combination, we hand-collect historical analyst location data from LinkedIn and historical covered firm headquarter data from SEC filings. We define an analyst-firm combination as being in lockdown if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement. Using this sample and a DiD design, we generally find that local analysts, on average, produce more accurate forecasts in non-lockdown periods which is consistent with prior research (e.g., Malloy, 2005; Bae et al., 2008). We also discover, however, that local analysts lose their performance advantage in lockdown periods which is consistent with local analysts losing an important information source – namely in-person interactions with firm stakeholders.

We next run three cross-sectional analyses to support our explanation of why local analysts lose their superior performance during lockdowns. First, we predict and find that local analysts suffer a more pronounced decline in relative performance during lockdown when the covered

[3] As discussed in Section 2, prior literature has shown that local analysts generally have more accurate earnings forecasts than faraway analysts and scholars have attributed this superior performance to an information advantage (e.g., Malloy, 2005; Bae et al., 2008).

firm has poorer voluntary disclosure. The reasoning for this prediction is that local analysts' in-person interactions will be especially valuable when the firm has poorer voluntary disclosure because the information in interactions and disclosure could substitute for each other. Second, we predict and find that local analysts suffer a more pronounced decline in relative performance during lockdown when the analyst does *not* have a private information channel with firm management (as proxied by attending the same university). This prediction is based on the idea that local analysts with a private information channel may not need to rely on in-person interactions but instead can communicate through these other channels. Lastly, we argue that local analysts with more experience gather more of their information from sources outside their offices (Ben-Rephael et al., 2022) and so may be more reliant on in-person interactions for their superior performance. Consistent with this argument, we find that these experienced local analysts suffer a larger decline in performance during lockdown. Overall, these three cross-sectional tests support our explanation that in-person interactions with firm stakeholders are an important source of information for analysts.

In supplementary tests, we also use informativeness of forecast revisions as an alternative performance measure and find that local analysts provide less informative revisions (especially for upward revisions) during lockdowns. We also provide some evidence that local analysts are less likely to be bold in their forecasts during lockdown which suggests that local analysts have less private information in this period. In a test of analyst decisions around coverage, we discover that the average distance between an analyst and her covered companies increased during the lockdown which suggests that analysts were more willing to cover more distant firms; a possible explanation of finding is that analysts judged geographical proximity and in-person interactions as less important during lockdown. As a final more exploratory analysis, we investigate how local analysts performed in the post-lockdown period. Using pre-lockdown as the comparison period, we find weak evidence that the performance differential between local and faraway analysts is weaker in the post-lockdown period.

Our paper contributes to the literature on information gathering by analysts, which Bradshaw (2011) and Brown et al. (2015) call for more research on. The primary hurdles in this research area have been the lack of data on such activities and the difficulty in isolating the effect of such activities, but an emerging literature is developing around information gathering. For example, Soltes (2014) obtains data on 75 private interactions (largely phone calls) between analysts and top executives of one large U.S. firm but finds that these private interactions do not improve analysts' forecast accuracy (at least in the case of the one firm in their sample). Green et al. (2014) show that access to management at brokerage-hosted investor conferences leads to more informative research. Kirk and Markov (2016) study the analyst / investor day as another type of private interaction and find a significant stock market reaction around these events. Two recent papers provide evidence consistent with analysts acquiring information through corporate site visits in China (e.g., Cheng et al., 2016; Han et al., 2018). In a concurrent working paper, Ben-Rephael et al. (2022) find that analysts who before the pandemic spent less time on their Bloomberg terminal – which is presumed to mean more time collecting information from outside the office – experience a significant reduction in forecast accuracy during the lockdown.[4] Using an exogenous shock to in-person information gathering activities, we complement the above papers by providing new evidence that broad in-person interactions with firm stakeholders matter to analyst performance.

[4] It is worth emphasizing, however, Ben-Rephael et al. (2022)'s above finding is not the main focus of their paper (i.e., it is one of many findings that explore the determinants and consequences of analyst work habits as measured by their Bloomberg terminal usage).

Relatedly, we contribute to the literature on geographic proximity and analyst performance. Prior literature finds that local analysts often issue more accurate earnings forecasts or more informative forecast revisions (Malloy, 2005; Bae et al., 2008). In this paper, we provide more evidence on how and why local analysts perform better. In prior research, it is difficult to disentangle the source of local analysts' information advantage: it may be due to analysts having a better understanding of the local economic conditions or due to in-person interactions with firm stakeholders. Our research design allows us to isolate the role of in-person interactions and to show that an important factor for gaining an information edge is physical in-person interactions. We also answer Malloy (2005)'s call that "Broader questions, such as… which knowledge is portable are also intriguing. These and other issues are left to future research" (p.753). Specifically, our results suggest that in-person information sources cannot be easily substituted by other information sources such as telephone conversations, virtual meetings, written firm disclosures, or other online websites. This latter finding is similar to Bai and Massa (2021)'s conclusion in the investment fund setting.

Our results also complement those from another paper published in the geography literature. Bratton and Wójcik (2022) conduct semi-structured interviews with 70 capital market participants (of which 45 are sell-side analysts) from Asia during 2021. Relevant to our paper, the authors ask interviewees about the importance of in-person interactions in their information gathering efforts and the effect of lockdowns on these efforts. The authors conclude that their interviews provide evidence "in favour of the continued need for physical proximity and face-to-face interaction in the origination and collection of financial information. The COVID restrictions on physical interactions resulted in a deterioration in the quality of information analysts could access…" (p.136). This qualitative finding provides further supports the explanation of our findings. We add to the work of Bratton and Wojcik by providing quantitative evidence using a much larger sample.

The rest of the paper is structured as follows. In the next section, we review the related literature and state our research hypotheses. Section 3 describes the construction of the sample, while section 4 describes the research design and presents the main results. In section 5, we provide supplementary analyses and in section 6 we offer concluding remarks.

## II. LITERATURE AND HYPOTHESIS DEVELOPMENT

### *2.1 Information Acquisition of Analysts*

Financial analysts play an important role in the capital market as an information intermediary. Analysts collect and interpret financial information as a service provided to their investor clients (Chen et al., 2010; Livnat and Zhang, 2012). The information they collect could from various sources, such as corporate earnings calls (Mayew et al., 2013), management guidance (Merkley et al., 2013), corporate site visiting (Cheng et al., 2016; Han et al., 2018), private connections (Cohen et al., 2010; Soltes, 2014), broker-hosted conference (Bushee et al., 2011; Green et al., 2014), and analyst / investor days (Kirk and Markov, 2016).

The cost of information collection is not always trivial. A long geographic distance between an analyst and her covered firm could be one of the most significant costs of information acquisition and may negatively affect an analyst's forecasting performance for that firm. For example, Jennings et al. (2017) find that analysts' forecast accuracy decreases when a company is located farther away from other companies within the same industry. The authors argue that companies co-located close to each other could reduce analysts' travelling costs of covering multiple firms in the same geographic area.

### *2.2 The Local Advantage*

Related to the idea of non-trivial traveling costs, prior literature provides evidence that local US-based analysts produce more accurate and informative earnings forecasts than faraway analysts (Malloy, 2005). Bae et al. (2008) provide international evidence on the local advantage: analysts resident in the same country as their covered firms make more precise earnings forecasts than non-resident analysts.

Previous literature also finds a local advantage among investors (e.g., Coval and Moskowitz, 1999, 2001). In Coval and Moskowitz (2001), they explain that "Investors located near a firm can visit the firm's operations, talk to suppliers and employees, as well as assess the local market conditions in which the firm operates" (p. 839). This local advantage has also been found among the hedge funds (Sialm et al., 2020), and individual investors (Ivkovic and Weisbenner, 2005a).

Local advantages may stem from the following sources. On the one hand, local analysts may understand the local economy better which helps their forecasting performance. On the other hand, local analysts have greater opportunities to interact with firm stakeholders in-person for first-hand information – this interaction could involve formal meetings in the office or informal meetings in restaurants, golf courses, fitness centers, social clubs, etc. It is difficult, however, to empirically disentangle what source is the main driver of the local advantage.

### *2.3 The Impact of COVID-19 on Local Analysts*

COVID-19 hit the US in Washington State on 21 January 2020. On 26 February 2020 the first coronavirus case of unknown exposure to the virus was confirmed by the Centers for Disease Control and Prevention (CDC) in northern California, marking the beginning of community spread of the disease. From 19 March to 12 April 2020, the infection cases increased exponentially. To prevent the transmission of the disease, the federal government established the White House Coronavirus Task Force on 29 January 2020 and announced federal guidelines for social distancing for a 15-day period in March 2020. From March, state governments issued various directives regarding the lockdown of non-essential businesses and schools and "stay home-work safe".

Some concurrent working papers have documented the impact of the Covid-19 lockdown on capital market participants. Bai and Massa (2021) find that asset managers reduce their investment in proximate stocks and rebalance towards more distant stocks. Cahill et al. (2022) find that lower face-to-face interactions during the Covid-19 lockdown dampen stock price discovery. Ben-Rephael et al. (2022) study the work habits of analysts by recording their intraday usage of their Bloomberg account terminals. They find that analysts who before the pandemic spent less time on their Bloomberg terminal – which is presumed to mean more time collecting information from outside the office – experience a significant reduction in forecast accuracy during the lockdown. Du (2022) and Li and Wang (2021) find that during lockdowns female analysts' performance decreased more than their male counterparts. They explain that female analysts had more child-rearing responsibilities than male analysts during lockdowns (as schools were often closed) and this had a detrimental effect on their job performance.

In our research setting, the Covid-19 lockdown provides a unique setting to disentangle the two potential sources of analysts' local advantage (as described in section 2.1). The Covid-19 lockdowns imposed by state governments reduced analysts' ability to collect information via in-person interactions with covered firm stakeholders. We argue that this information shock negatively affects local analysts much more than faraway analysts given that the former could interact in-person with firm stakeholders at a much lower cost before the pandemic began. At the same time, we argue that this information shock does not affect local analysts' ability to observe and understand the local economy better. Given these arguments, we can infer the importance of in-person interactions for analysts' information collection from performance changes of local analysts versus faraway analysts around the information shock can shed light on.

Specifically, on the one hand, we may expect the relative performance of local analysts to worsen during lockdown if they rely heavily on in-person interactions for their information advantage. On the other hand, we may expect the relative performance of local analysts to remain steady during lockdown if they do not rely heavily on in-person interactions for their information advantage. In this instance, local analysts may rely more heavily on observing local economic conditions for their information advantage or alternatively they may be able to easily substitute in-person information sources for other sources such as telephone conversations, virtual meetings, written firm disclosures, or other online websites. Given these competing arguments, we state our first hypothesis in the null form:

***H1:*** *The local analysts' forecast accuracy does not change during the lockdown relative to that of faraway analysts.*

### *2.4 Cross-sectional Hypotheses*

In this section, we propose three cross-sectional hypotheses based on the quality of the covered firm's voluntary disclosure, the presence of a private information channel, and the level of the analyst's experience. Our cross-sectional arguments below implicitly assume that local analysts rely on in-person interactions for their local advantage.

Prior literature shows that companies' information environment and information disclosure have an impact on analysts' forecast accuracy. For instance, Barron et al. (2002) find that analysts' forecast accuracy increases with companies' earnings announcements. Byard et al. (2011) find that analysts' forecast errors and dispersion decrease more for covered firms that have stronger incentives for transparent financial reporting. In addition, Feng and McVay (2010) show that analysts incorporate the information from management guidance and make more accurate forecasts. Building on this prior literature, we argue that local analysts' in-person interactions will be especially valuable when the firm has poorer voluntary disclosure because the information in interactions and disclosure could substitute for each other. Hence, in Hypothesis 2 we predict that:

***H2:*** *Local analysts will suffer a more pronounced decline in relative performance during lockdown when the covered firm has lower quality voluntary disclosure.*

Turning to our second cross-sectional hypothesis, prior work shows that analysts' may obtain an information advantage through private channels of communication with firm management. Specifically, Cohen et al. (2010) argue that analysts who went to the same university as the covered firm's CEO would have a special bond or connection that facilitates a private channel of communication. Cohen et al. (2010) find that analysts with such a connection produce more accurate earnings forecast for the respective covered firm. Extending this line of reasoning, we expect that local analysts with a private information channel may not need to rely on in-person interactions but instead can communicate through a private channel.[5] Hence, in Hypothesis 3 we expect that:

***H3:*** *Local analysts will suffer a more pronounced decline in relative performance during lockdown when the analyst does not have a private information channel with firm management.*

[5] Given that our sample period is after the enactment of Regulation Fair Disclosure (Reg FD), we do not expect managers to violate Reg FD but rather they can help connected analysts through the mosaic approach to information acquisition.

For our final cross-sectional hypothesis, we argue that local analysts with more experience rely more on gathering information from sources outside their offices (Ben-Rephael et al., 2022) and so may be more reliant on in-person interactions for their superior performance. Thus, our final hypothesis predicts that:

***H4:*** *Local analysts will suffer a more pronounced decline in relative performance during lockdown when the analyst has more experience.*

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## III. DATA AND SAMPLE SELECTION

### *3.1 Sample Selection*

Table 1 shows the sample selection criterion for our main forecast accuracy test. Using Institutional Brokers' Estimate System (I/B/E/S) data, we begin with all quarterly earnings per share (EPS) forecasts for US-based firms from sell-side financial analysts issued between the start of 2016 and the end of the second quarter of 2020. We choose 2016 as the start of our sample period because 2016 provides a long enough pre-COVID period while keeping the costs of hand-collecting analyst location data to a manageable level; we choose the second quarter of 2020 as the end of our sample period because after this period it is less clear which analysts and covered firm stakeholders that are deciding to voluntarily avoid in-person interactions which creates noise in our lockdown variable. Furthermore, we only keep those EPS forecasts that are the latest forecast issued by the analyst before the respective covered firm's earnings announcement date (Malloy, 2005); and those forecasts that can be merged with Compustat data.

[Insert Table 1 Here]

We also require historical location data about an analyst's place of employment and further restrict our sample of those analysts whose workplace is in the US – the rationale for the US only restriction is to avoid other confounding country related factors that may affect forecast accuracy. We also drop observations with missing Compustat data that are used in constructing control variables. Finally, we require covered firms to be followed by at least two analysts and have at a least three EPS forecasts by analysts for a given fiscal quarter (Malloy, 2005). After applying the above sample selection criteria, the sample for our main empirical test consists of 212,343 analyst forecast observations on 3,624 unique covered firms from 1,871 unique analysts.

### *3.2 Location Data*

An important measure in our empirical analysis is the distance between an analyst and covered firm. To measure distance, we first require data on the analyst's historical workplace location (i.e., historical brokerage branch location) and hand-collect this location data as follows. Firstly, we download the entire I/B/E/S recommendation file for the sample period 2016 to 2020. The I/B/E/S recommendation file contains analysts' first name initials, full surnames, and their brokerage houses. Using this analyst information and a list of their covered firms, we use a Bloomberg terminal to find analysts' full names. We next manually search for these full names on LinkedIn and upon finding the analyst's LinkedIn profile page we record their historical workplace locations (Bradley et al., 2017). Using the above searching process, we successfully identify the locations of 1,871 unique analysts. In Panel A of Table 2, we show the top 10 locations of analysts and unsurprisingly observe the most frequent locations in the financial hubs of New York City (~57% of analysts), San Francisco (~7%), Boston (~4%), and Houston (~3%).

[Insert Table 2 Here]

We next require the historical headquarter locations of covered firms which we obtain from SEC filings.[6] Panel B of Table 1shows that we have 3,624 unique firms and the top headquarter locations are New York City (~6% of firms) and Houston (~4%). Otherwise, the covered firm headquarters are widely dispersed throughout the US.

The distance between a specific analyst and covered firm is calculated using the ZIP codes of the analyst's and firm's locations.[7] We use the *zipcitydistance* SAS code to calculate the geographical distance between the two ZIP codes.[8] As shown in the DISTANCE (in miles) row of Table 3, the mean (median) distance between an analyst and covered firm is 1,062 (777) miles which is a similar average distance to that in Malloy (2005).

[Insert Table 3 Here]

### *3.3. Other Data*

Another key measure in our analysis is whether an analyst and firm pair is severely restricted from interacting with each other in-person at the issuance time of the analyst's EPS forecast. We assume that the two parties cannot interact with each other in-person if either the analyst's state or the firm's state has a state-wide "stay home – work safe" policy in place at the forecast issuance time. For each US state, we collect the start and end dates of the "stay home – work safe" policy from CUSP (2020) and Skinner-Dorkenoo et al. (2022).

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[6] Compustat only gives the current location of the firm's headquarters so we use historical headquarter location from SEC filings. We thank Ahmet Kurt from Bentley University for kindly sharing this data with us.

[7] Often the analyst's location is only given at the city level, so we assume that the analyst's workplace ZIP code is the one given by Google Maps when searching that city (i.e., a central city ZIP code).

[8] For reference, the help file for *zipcitydistance* SAS code is available at https://documentation.sas.com/?docsetId=lefunctionsref&docsetTarget=n1r333fdkrofhxn10vmhu9bq5m85.htm&docsetVersion=9.4&locale=en. The underlying math is the same as that used in Coval and Moskowitz (1999), which is $d_{i,j} = arc\ cos\{cos(latitude_i)cos(longitude_i)cos(latitude_j)cos(longitude_j) + cos(latitude_i)sin(longitude_i)cos(latitude_j)sin(longitude_j) + sin(latitude_i)sin(latitude_j)\}2\pi r/360$

We also use several other variables in our analysis (as described in section 4) and the sources of these data are from Compustat, CRSP, and I/B/E/S (see Appendix I for more details).

## IV. EMPIRICAL RESULTS

### *4.1 Main results: Forecast Accuracy*

To test hypothesis 1, we run an ordinary least squares (OLS) regression of the following form:

$$\begin{aligned} DAFE_{i,j,t} = {} & \beta_1 DLOCAL_{i,j,t}\left(or\ DGEOPROXIMITY_{i,j,t},\ DLOGDIST_{i,j,t}\right) + \beta_2 LOCKDOWN_{i,j,t} \\ & + \beta_3 DLOCAL_{i,j,t}\left(or\ DGEOPROXIMITY_{i,j,t},\ DLOGDIST_{i,j,t}\right) \times LOCKDOWN_{i,j,t} \\ & + \beta_4 DBROKERSIZE_{i,j,t} + \beta_5 DAGE_{i,j,t} + \beta_6 DFIRMEXP_{i,j,t} + \beta_7 DGENEXP_{i,j,t} \\ & + \beta_8 DNUMFIRM_{i,j,t} + \gamma\, Analyst\ FE + \delta YearMonth\ FE + \varepsilon_{i,j,t} \end{aligned} \tag{1}$$

where all variables, except the $LOCKDOWN_{i,j,t}$, are firm-quarter mean adjusted (the "D" preceding each variable stands for demeaned). Starting with our dependent variable, we use forecast accuracy as our main analyst performance metric because it is one of the most important dimensions along which financial analysts are assessed and is the most frequently studied performance metric of analysts in the accounting literature. Following Malloy (2005), we calculate the absolute forecast error ($AFE_{i,j,t}$) as the absolute value of analyst *i*'s last quarterly forecast before the earnings announcement minus the actual firm *j*'s earnings, all divided by firm *j*'s stock price measured 12 months prior to the beginning of the fiscal quarter *t*.

Turning to the independent variables, $LOCKDOWN_{i,j,t}$ is an indicator variable which equals one if either the state of analyst *i*'s location or the state of covered firm *j*'s location is in a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst *i*'s location and the state of the covered firm *j*'s location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. $LOCAL_{i,j,t}$ is an indicator variable which equals one if the geographic distance between the analyst *i* and her covered firm *j* is less than 62.14 miles (100 km), and zero otherwise. $GEOPROXIMITY_{i,j,t}$ is calculated as one divided by the geographic distance between analyst *i* and her covered firm *j*. $LOGDIST_{i,j,t}$ is calculated as the natural logarithm of one plus the geographic distance between analyst i and her covered firm j. Note that we should expect the $LOGDIST_{i,j,t}$ coefficient to have the opposite sign from the $LOCAL_{i,j,}$ and $GEOPROXIMITY_{i,j,t}$ coefficients. The interaction of $LOCKDOWN_{i,j,t}$ multiplied by one of our distance variables is our variable of interest.

We also control for several factors that previous research has identified as important to forecast accuracy (Clement, 1999). We measure the analyst's resources by calculating the size of the analyst's brokerage firm ($BROKERSIZE_{i,j,t}$), which is equal to the number of analysts working for the respective I/B/E/S broker in year *t*. We measure the forecast age by calculating the number of days ($AGE_{i,j,t}$) between the forecast date and the corresponding announcement of the actual quarterly earnings. We also control for the analyst's firm-specific experience by calculating the number of years the analyst has followed the respective firm up until quarter *t* ($FIRMEXP_{i,j,t}$); and we control for the analyst's general experience by calculating the number of total years that the analyst appears in the I/B/E/S database up until quarter *t* ($GENEXP_{i,j,t}$). Following Clement and Tse (2005), we also control for the number of firms covered by analyst *i* in quarter *t* ($NUMFIRM_{i,j,t}$). Finally, to control for the time-invariant factors related to analysts and time, we control for analyst fixed effects (Analyst FE) and year-month fixed effects (YearMonth FE).

Table 4 reports the output of regression equation (1). In columns (1), (3) and (5), we do not add control variables, while we add them in columns (2), (4) and (6). Since we control for analyst fixed effects, in each column, we are comparing the forecast accuracy of analyst *i*'s local firms with that of her faraway firms.

[Insert Table 4 Here]

In columns (1) to (4), we find that the coefficients on the distance variables are negative and statistically which suggests that local analysts produce more accurate forecasts in pre-lockdown times – a finding which is consistent to prior work (e.g., Malloy 2005; Bae et al., 2008). Turning to our coefficient of interest on the interaction term, we report positive and statistically significant results. We find similar results in columns (5) and (6) for the interaction term – the difference in sign is as expected when using the DLOGDIST variable. Overall, these results suggest that we should reject Hypothesis 1 as local analysts experience a decline in relative performance during lockdown. These results suggest that in-person interactions are important for analysts' information collection.

We next run three cross-sectional analyses to support our explanation of why local analysts lose their superior performance during lockdowns – namely that they lose an important information source which is in-person interactions with firm stakeholders.

### *4.2 Cross-sectional Result: Quality of Firm Voluntary Disclosure*

We first test our second hypothesis which predicts that the impact of lockdowns on the analysts' local advantage is more pronounced when the company has poorer voluntary disclosures. We use the presence of management guidance to proxy for high quality voluntary disclosures. In Table 5, we partition our sample into covered firms with and without any type of management guidance in the respective quarter (columns (1) and (2)) and with and without EPS guidance (columns (3) and (4)) and then run regression equation (1) on each of these subsamples (using LOCAL as our distance measure).

In Table 5, we discover that the LOCAL and interaction variable are only statistically significant in columns (2) and (4) (i.e., when the firm has poor voluntary disclosure). These results are in line with Hypothesis 2 and suggest that local analysts' in-person interactions are especially valuable when the firm has low quality voluntary disclosure because the information in interactions and disclosure substitute for each other.

[Insert Table 5 Here]

### *4.3 Cross-sectional Result: Presence of a Private Information Channel*

We next test Hypothesis 3 which predicts that local analysts suffer a more pronounced decline in relative performance during lockdown when the analyst does *not* have a private information channel with firm management. Similar to Cohen et al. (2010), we assume that analysts and firm management are more likely to have a private information channel if they went to the same university. In Table 6, we partition our sample into analyst-firm observations with and without a university connection for analysts and firm executive directors (columns (1) and (2)) and with and without a university connection for analysts and any type of firm directors (columns (3) and (4)). We then run regression equation (1) on each of these subsamples (using LOCAL as our distance measure) and report the results in Table 6.

In Table 6, we show that the LOCAL and interaction variables are only statistically significant in columns (2) and (4) (i.e., when the analyst and firm are less likely to have a private information channel). These results are consistent with Hypothesis 3 and suggest that local analysts with a private information channel do not need to rely on in-person interactions but instead can communicate through their private channel.

[Insert Table 6 Here]

### *4.4 Cross-sectional Result: Level of Analyst Experience*

In Hypothesis 4, we expect that more experienced local analysts will suffer a larger decline in performance during lockdown. To test this hypothesis, we partition our sample into analysts with above and below median firm-specific experience ($FIRMEXP_{i,j,t}$) in columns (1) and (2) and with above and below median general experience ($GENEXP_{i,j,t}$) in columns (3) and (4). We then run regression equation (1) on each of these subsamples (using LOCAL as our distance measure) and report the results in Table 7.

In Table 7, we find that the LOCAL and interaction variables are only statistically significant in columns (1) and (3) (i.e., when the analyst has more firm-specific and general experience). These results are consistent with Hypothesis 4 and suggest that experienced local analysts who are more reliant on in-person interactions for their local advantage suffer the most under lockdown.

[Insert Table 7 Here]

## V. SUPPLEMENTARY ANALYSES

### *5.1 Informativeness*

As an alternative way to assess analyst performance, we use the informativeness of the analysts' forecast revisions. Following Loh and Stulz (2018), we measure informativeness as the three-day cumulative abnormal returns (CARs) measured over the three days surrounding the forecast revision announcement:

$$CAR\left[-1.+1\right]=\sum_{t=-1}^{+1}\left(R_{j,t}-R_{m,t}\right) \tag{2}$$

where $R_{j,t}$ is the firm $j$'s stock return on day $t$ and $R_{m,t}$ is the CRSP-value-weighted market return for day $t$. We also align the CARs of upgrade and downgrade forecast revisions by multiplying the CARs for downgrades by minus one. A higher CAR indicates greater informativeness of a forecast revision.

We then estimate the following model to examine how the local advantage changes during lockdown:

$$\begin{aligned} CAR\left[-1.+1\right]_{i,j,t} = {} & \beta_1 LOCAL_{i,j,t}\left(or\ GEOPROXIMITY_{i,j,t},\ LOGDIST_{i,j,t}\right)+\beta_2 LOCKDOWN_{i,j,t} \\ & +\beta_3 LOCAL_{i,j,t}\left(or\ GEOPROXIMITY_{i,j,t},\ LOGDIST_{i,j,t}\right)\times LOCKDOWN_{i,j,t} \\ & +\beta_4 SIZE_{i,j,t}+\beta_5 BROKERSIZE_{i,j,t}+\beta_6 FIRMEXP_{i,j,t}+\beta_7 GENEXP_{i,j,t} \\ & +\beta_8 NUMFIRM_{i,j,t}+\beta_9 ACCURACY_{i,j,t}+\gamma\ Analyst\ FE+\delta YearMonth\ FE+\varepsilon_{i,j,t} \end{aligned} \tag{3}$$

where all the control variables are defined in Appendix I.

Table 8 displays the results from regression equation (3). Columns (1) to (3) report the results when the regression is run on all forecast revisions, while columns (4) to (6) (columns (7) to (9)) report the results from an upward (downward) revision only subsample. Focusing on the interaction coefficients, Table 8 shows that local analysts had less informative revisions in general during lockdown and this finding is especially pronounced for their upward revisions. These results support the conclusions from earlier in the paper when we used analyst forecast accuracy as the performance metric.

[Insert Table 8 Here]

### *5.2 Boldness*

We also examine another characteristic of analyst's forecasts – their boldness. Clement and Tse (2005) find that bold analyst forecasts often incorporate private information. So another way to test Hypothesis 1 is to check whether the relative boldness of local analysts reduces during lockdown. Such a finding would be consistent with local analysts possessing less private information during lockdown due to their large reduction of in-person interactions with firm stakeholders. To test this idea, we use the following model from Clement and Tse (2005):

$$\begin{aligned} BOLD_{i,j,t} = {} & \beta_1 LOCAL_{i,j,t} \left( or\ GEOPROXIMITY_{i,j,t},\ LOGDIST_{i,j,t} \right) + \beta_2 LOCKDOWN_{i,j,t} \\ & + \beta_3 LOCAL_{i,j,t} \left( or\ GEOPROXIMITY_{i,j,t},\ LOGDIST_{i,j,t} \right) \times LOCKDOWN_{i,j,t} \\ & + \beta_4 ACCURACY_{i,j,t} + \beta_5 BROKERSIZE_{i,j,t} + \beta_6 FIRMEXP_{i,j,t} + \beta_7 GENEXP_{i,j,t} \\ & + \beta_8 NUMFIRM_{i,j,t} + \beta_9 HORIZON_{i,j,t} + \gamma\, Analyst\ FE + \delta YearMonth\ FE + \varepsilon_{i,j,t} \end{aligned} \tag{4}$$

where $BOLD_{i,j,t}$ is an indicator variable that equals one if analyst *i*'s forecast is above both (or below both) the analyst's prior forecast and the mean forecast immediately before the forecast revision, and zero otherwise. We further control for prior year forecast accuracy ($ACCURACY_{i,j,t}$), and forecast horizon ($HORIZON_{i,j,t}$) which is the number of days between the forecast date and the fiscal quarter end. Other variables are defined in the same way as earlier but we do not demean the variables in this model.

The estimation results are reported in Table 9. Column (1) shows that local analysts are bolder in their forecast revisions before lockdown (see LOCAL coefficient), but are less bold during lockdown (see interaction variable coefficient). In column (2), we report similar results albeit the GEOPROXIMITY variable by itself is not statistically significant. In column (3), the LOGDIST and interaction variable have the expected signs but are not statistically significant. Overall, we find some evidence that is consistent with the relative private information of local analysts reducing during lockdown.

[Insert Table 9 Here]

### *5.3 Average Coverage Distance*

In this section, we analyse another consequence of lockdowns for analysts' local advantage - namely analysts' choice to cover nearby or faraway companies. We argue that analysts may be more willing to cover faraway firms if they judge that the benefits of geographical proximity and in-person interactions are less important during lockdown. To test whether this is the case, we estimate the following model:

$$\begin{aligned} COVDIS_{i,t}\left(LOGCOVDIS_{i,t}\right) = {} & \beta_1 POST\_2020_{i,t} + \beta_2 AVGPASTACC_{i,t} \\ & + \beta_3 AVGBROKERSIZE_{i,t} + + \beta_4 AVGNUMFIRM_{i,t} \\ & + \gamma\, Analyst\ FE + \varepsilon_{i,t} \end{aligned} \tag{5}$$

where $COVDIS_{i,t}$ is the average distance (in miles) between analyst i and all her covered firms in year *t*.[9] $LOGCOVDIS_{i,t}$ is the natural logarithm of one plus $COVDIS_{i,t}$. $POST\_2020_{i,t}$ is an indicator variable that equals one for the year of 2020, and zero for the years before 2020. $AVGPASTACC_{i,t}$ is the average forecast accuracy for analyst *i* in the one year before year *t*, where the forecast accuracy is defined in the same way as in model (4). $AVGBROKERSIZE_{i,t}$ is the average size of the brokers in which the analyst *i* has been working for in year *t*, where the size of brokers is calculated in the same way as in model (1). $AVGNUMFIRM_{i,t}$ is the average number of firms covered by analyst *i* during year *t*, where the number of firms covered by analyst is calculated in the same way as in model (1). We also control for analyst invariant factors by adding analyst fixed effects in the model. We predict that if analysts add more distant firms to her coverage portfolio during lockdowns, then the coefficient on $POST\_2020_{i,t}$ will be positive.

[9] If we measure average coverage distance on a per month basis instead of yearly, the results are qualitative similar.

The estimation results are reported in Table 10. The coefficients on $POST\_2020_{i,t}$ are positive and significant at the 5% level in all columns, indicating that analysts are willing to cover more distant firms in the lockdown year of 2020 – which is consistent with our argument that analysts perceive the benefits of geographical proximity and in-person interactions as less important during lockdown.

[Insert Table 10 Here]

### *5.4 Post Lockdown Forecast Accuracy*

As a final more exploratory analysis, we investigate how local analysts performed in the post-lockdown period compared with the pre-lockdown period. The rationale for this analysis is that we are interested in whether the local advantage in the post-lockdown period has returned to normal (i.e., that in the pre-lockdown period).

To investigate this issue, we re-run regression equation (1) on a sample period starting in the first quarter of 2016 and ending in the fourth quarter of 2021, but excluding any times when either the state of the analyst or the states of her covered firms are in lockdown. We also use two different treatment variables: $POST\_LOCKDOWN_{i,j,t}$ is an indicator variable that equals one when both the state of the analyst and the state of the firm have lifted up their lockdown policy at the time of the analyst forecast announcement, and zero if the analyst forecast announcement is announced pre-lockdown for both states; and $POST\_VAC_{i,j,t}$ is an indicator variable that equals one when the forecast is announced after December 14, 2020 which is when vaccination becomes widely available in the US, and zero if the analyst forecast is announced pre-lockdown for both states. We use $POST\_VAC_{i,j,t}$ in addition to $POST\_LOCKDOWN_{i,j,t}$ because some analysts and firm stakeholders may have decided to avoid in-person interactions until they were vaccinated even though they could legally interact in-person beforehand.

Our estimation results are reported in Table 11. In columns (1) and (2), the interaction coefficients are positive and statistically significant at the 10% level. These positive coefficients provide some weak evidence that the local advantage is weaker in post-COVID times. We leave the explanation of this finding to further research.

[Insert Table 11 Here]

## VI. CONCLUSION

In this paper, we investigate the following research question: in analysts' information gathering efforts, how important are in-person interactions with covered firm stakeholders? To provide insight into this question, we use a DiD design where the outcome variable is forecast accuracy and the exogenous shock is COVID-19 lockdowns in the US which reduced analysts' ability to collect information via in-person interactions with covered firm stakeholders. By comparing local analysts (the treated group) with faraway analysts (the control group) in our DiD setup, we generally find that local analysts, on average, produce more accurate forecasts in pre-lockdown periods. We also discover, however, that local analysts lose their performance advantage in lockdown periods which is consistent with local analysts losing an important information source – namely in-person interactions with firm stakeholders. Several cross-sectional and supplementary tests support this explanation of our results. Overall, we provide novel evidence that in-person interactions are important in gaining an information edge.

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## Appendix I: Definition of Variables

| Variable | Definition | Data Source |
|---|---|---|
| | **Dependent Variables** | |
| $COVDIS_{i,t}$ | The average geographic distance between analyst *i* and all her covered firms over the year *t*. $COVDIS_{i,t} = \sum_{j=1}^{n} DISTANCE_{i,j,t} / N$ | LinkedIn, EDGAR, I/B/E/S, Compustat |
| $LOGCOVDIS_{i,t}$ | The natural logarithm of one plus $COVDIS_{i,t}$. | |
| $DAFE_{i,j,t}$ | The absolute forecast error for analyst *i*'s forecast of firm *j* for the fiscal quarter *t* minus the mean absolute forecast error for firm *j* for the fiscal quarter, where absolute forecast error is equal to the absolute value of analyst *i*'s last quarterly forecast before the earnings announcement minus the actual firm *j*'s earnings, all divided by the firm *j*'s stock price measured 12 months prior to the beginning of the fiscal quarter *t*. This measure is from Malloy (2005). | I/B/E/S, Compustat |
| $CAR\ [-1,+1]_{i,j,t}$ | The 3-day cumulative abnormal return, where day 0 is when analyst *i* makes a forecast revision for firm *j* on the earnings for fiscal quarter *t*. To align the share price reactions for upgrades and downgrades, we multiply the *CAR*s for downgrades with minus one and leave the *CAR*s for upgrades unchanged. | I/B/E/S, CRSP |
| $BOLD_{i,j,t}$ | An indicator variable for the boldness of analyst *i*'s forecast for firm *j* in quarter *t*, which equals to one if analyst *i*'s forecast is above both the analyst's prior forecast and the mean forecast immediately before the forecast revision, or else below both, and zero otherwise. To be considered in the mean forecast calculation, a forecast must be issued in the 90 days prior to analyst i's forecast revision. This measure is taken from Clement and Tse (2005). | I/B/E/S |
| | **Independent Variables** | |
| $LOCKDOWN_{i,j,t}$ | An indicator variable which equals to one if either the state of the analyst *i*'s location or the state of the covered firm *j*'s location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst *i*'s location and the state of the covered firm *j*'s location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. | CUSP (2020); Skinner-Dorkenoo et al. (2022) |
| POST_LOCKDOWN | An indicator variable that equals to one if both the state of the analyst and the state of the firm has lifted up its state-wide lockdown policy at the time of the analyst forecast announcement, and zero if the analyst forecast announcement is announced pre-lockdown for both the state of the analyst and the state of the firm. | |
| POST_2020 | An indicator variable that equals to one when the forecast is announced after January 1, 2020. | |
| POST_VAC | An indicator variable that equals to one when the forecast is announced after December 14, 2020 which is when vaccination is widely available in the US, and zero if the analyst forecast announcement is announced pre-lockdown for both the state of the analyst and the state of the firm. | CDC Website[10] |
| $LOCAL_{i,j,t}$ ($DLOCAL_{i,j,t}$) | An indicator variable which equals to one if the geographic distance between the analyst *i* and her covered firm *j* is less than 62.14 miles (100 km), zero otherwise. $DLOCAL_{i,j,t}$ is the firm-quarter mean adjusted $LOCAL_{i,j,t}$ variable. This de-meaning approach follows Malloy (2005). | LinkedIn, EDGAR, Compustat |

[10] Source is available at: https://data.cdc.gov/Vaccinations/COVID-19-Vaccinations-in-the-United-States-Jurisdi/unsk-b7fc.

| | | |
|---|---|---|
| $DISTANCE_{i,j,t}$ | The geographic distance between analyst *i* and her covered firm *j* by using the five-digit ZIP codes of analyst *i*'s location and the five-digit ZIP codes of her covered firm *j*'s location. The distance is calculated in miles using the *zipcitydistance* SAS code and the underlying function is $d_{ij}=$ $\arccos\{\cos(latitude_i)\cos(longtitude_i)\cos(latitude_j)\cos(longitude_j)+$ $\cos(latitude_i)\sin(longtitude_i)\cos(latitude_j)\sin(longitude_j)+$ $\sin(latitude_i)\sin(latitude_j)\}2\pi r/360$ | |
| $GEOPROXIMITY_{i,j,t}$ ($DGEOPROXIMITY_{i,j,t}$) | One over $DISTANCE_{i,j,t}$. $DGEOPROXIMITY_{i,j,t}$ is the firm-quarter mean adjusted $GEOPROXIMITY_{i,j,t}$ variable. | |
| $LOGDIST_{i,j,t}$ ($DLOGDIST_{i,j,t}$) | The natural logarithm of one plus $DISTANCE_{i,j,t}$. $DLOGDIST_{i,j,t}$ is the firm-quarter mean adjusted $LOGDIST_{i,j,t}$ variable. | |
| $AVGPASTACC_{i,t}$ | The forecast accuracy of analyst *i* in the past one year of year *t*. Where the forecast accuracy is calculated as the difference between the maximum absolute forecast error for firm *j* in year *t-1* and the analyst *i*'s absolute forecast error for firm *j* in year *t-1*, divided by the difference between the maximum absolute forecast error for firm *j* in year *t -1* and the minimum absolute forecast error for firm *j* in year *t -1*. This measure is taken from Clement and Tse (2003). | I/B/E/S |
| $AVGBROKERSIZE_{i,t}$ | The average number of broker size in the past one year of year *t*, where the broker size is calculated as the number of analysts working for the IBES broker firm that analyst *j* is associated with in year *t -1*. | I/B/E/S |
| $AVGNUMFIRM_{i,t}$ | The average number of firms covered by analyst *i* in the past one year of year *t*. | I/B/E/S |
| $BROKERSIZE_{i,j,t}$ ($DBROKERSIZE_{i,j,t}$) | The number of analysts working for the IBES broker firm that analyst *i* is associated with in year *t*. $DBROKERSIZE_{i,j,t}$ is the firm-quarter mean adjusted $BROKERSIZE_{i,j,t}$ variable. | I/B/E/S |
| $AGE_{i,j,t}$ ($DAGE_{i,j,t}$) | The number of days between the forecast date and the corresponding I/B/E/S report date of the actual quarterly earnings. $DAGE_{i,j,t}$ is the firm-quarter mean adjusted $AGE_{i,j,t}$ variable. | I/B/E/S |
| $HORIZON_{i,j,t}$ | The number of days between the forecast date and the corresponding fiscal year end. | I/B/E/S |
| $FIRMEXP_{i,j,t}$ ($DFIRMEXP_{i,j,t}$) | The number of years of firm-specific experience for analyst *i* following firm *j* in quarter *t*. $DFIRMEXP_{i,j,t}$ is the firm-quarter mean adjusted $FIRMEXP_{i,j,t}$ variable. | I/B/E/S |
| $GENEXP_{i,t}$ ($DGENEXP_{i,t}$) | The number of years of total experience for analyst *i* at quarter *t* (i.e., the number of years that analyst *i* appears in the IBES database measured at quarter *t*). $DGENEXP_{i,t}$ is the firm-quarter mean adjusted $GENEXP_{i,t}$ variable. | I/B/E/S |
| $NUMFIRM_{i,t}$ ($DNUMFIRM_{i,t}$) | The number of firms covered by analyst *i* in quarter *t*. $DNUMFIRM_{i,t}$ is the firm-quarter mean adjusted $NUMFIRM_{i,t}$ variable. (Malloy, 2005) | I/B/E/S |

**Table 1: Sample Selection Procedure**

This table presents the sample selection procedures for the test of forecast accuracy.

| Sample Selection Procedure | Observations remained |
|---|---|
| I/B/E/S US Detail History:<br>(1) Quarterly EPS forecast for US firms,<br>(2) The forecast is announced after 2015,<br>(3) The forecast is the latest forecast issued by the analyst before the earnings announcement date. | 1,061,354 |
| Merge with Compustat / Edgar header, drop if:<br>(1) Missing identifier variables. | 629,821 |
| Merge with Analyst's location data, drop if:<br>(1) Missing analyst's start date or end date of her employment position.<br>(2) When analyst's position is present, the end date is replaced with December 31, 2022. | 349,858 |
| Drop if:<br>(1) Missing Compustat control variables,<br>(2) The firm is followed by fewer than two analysts,<br>(3) The total number of forecasts for the firm in the year is less than three,<br>(4) Sample period starts from the first calendar quarter of 2016 to the end of the second calendar quarter of 2020. | 212,343 |

**Table 2: The Top Ten Locations of Analysts and Firms**

The table presents the top ten locations of analysts and covered firms. The historical analyst locations are from the employment history section of their LinkedIn. The historical firm headquarter data is from historical SEC filings. The sample period starts in the first quarter of 2016 and ends in the second quarter of 2020.

Panel A: The Top Ten Locations of Analysts

| City | Unique Number of Analyst-Location Combinations | Frequency |
|---|---|---|
| New York City | 1,068 | 57.08% |
| San Francisco | 126 | 6.73% |
| Boston | 72 | 3.79% |
| Houston | 63 | 3.37% |
| Chicago | 59 | 3.15% |
| Minneapolis | 36 | 1.92% |
| Portland | 32 | 1.71% |
| Los Angeles | 26 | 1.39% |
| Washington | 26 | 1.39% |
| Atlanta | 24 | 1.28% |
| Other Locations | 339 | 18.12% |
| Total | 1,871 | 100% |

Panel B: The Top Ten Locations of Firms

| City | Unique Number of Firm-Location Combinations | Frequency |
|---|---|---|
| New York City | 231 | 6.37% |
| Houston | 148 | 4.08% |
| San Diego | 66 | 1.82% |
| Cambridge | 63 | 1.74% |
| Chicago | 62 | 1.71% |
| Dallas | 59 | 1.63% |
| San Francisco | 59 | 1.63% |
| San Jose | 58 | 1.6% |
| Denver | 55 | 1.52% |
| Atlanta | 50 | 1.38% |
| Other Locations | 2,773 | 76.52% |
| Total | 3,624 | 100% |

**Table 3: Summary Statistics**

The table presents the summary statistics of the main variables related to the forecast accuracy test. Absolute forecast error (*AFE*) is calculated as the absolute value of analyst's last quarterly forecast before the earnings announcement minus the actual firm's earnings, all divided by the firm's stock price measured 12 months prior to the beginning of the fiscal quarter. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *DISTANCE* is the geographic distance (in miles or in kilometers, where 1 mile equals to 1.609344 kilometers) between analyst's location and her covered firm's location. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. *GEOPROXIMITY* is calculated as one over the geographic distance between analyst's location and her covered firm's location. *LOGDIST* is calculated as the natural logarithm of one plus the geographic distance. Control variables include broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), and the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). All continuous variables are winsorized at the 1 and 99 percent levels. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| | N | Mean | StDev. | p25 | Median | p75 |
|---|---|---|---|---|---|---|
| DAFE | 212,192 | -.015 | .522 | -.313 | -.007 | .212 |
| LOCKDOWN | 182,566 | .068 | .252 | 0 | 0 | 0 |
| DISTANCE | 182,566 | 1049.182 | 896.632 | 215.6 | 773.6 | 1632.9 |
| LOCAL | 182,566 | .131 | .337 | 0 | 0 | 0 |
| GEOPROXIMITY | 182,378 | .03 | .138 | .001 | .001 | .005 |
| LOGDIST | 182,566 | 6.178 | 1.741 | 5.378 | 6.652 | 7.399 |
| DLOCAL | 182,566 | 0 | .231 | 0 | 0 | 0 |
| DGEOPROXIMITY | 182,378 | 0 | .099 | -.002 | 0 | 0 |
| DLOGDIST | 182,566 | .001 | 1.156 | -.259 | .004 | .415 |
| BROKERSIZE | 212,343 | 56.284 | 49.079 | 17 | 41 | 87 |
| FIRMEXP | 212,343 | 4.928 | 3.86 | 2 | 4 | 7 |
| GENEXP | 212,343 | 10.202 | 6.647 | 5 | 9 | 15 |
| NUMFIRM | 212,343 | 18.226 | 7.578 | 13 | 18 | 22 |
| AGE | 212,343 | 61.722 | 34.44 | 26 | 75 | 90 |
| DBROKERSIZE | 212,343 | -.075 | 41.153 | -28.8 | -5.4 | 19.667 |
| DFIRMEXP | 212,343 | -.03 | 3.114 | -2 | -.091 | 1.333 |
| DGENEXP | 212,343 | -.047 | 5.707 | -4.25 | -.533 | 3.5 |
| DNUMFIRM | 212,343 | -.052 | 6.189 | -3.938 | 0 | 3.643 |
| DAGE | 212,343 | .379 | 23.768 | -11.25 | .667 | 14.778 |

**Table 4: Forecast Accuracy and Geographic Distance**

The table presents the results from estimating OLS regressions of demeaned absolute forecast error (*DAFE*) on the demeaned measurements of the geographic proximity (*DLOCAL*, *DGEOPROXIMITY*, *DLOGDIST*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurements of geographic proximity and lockdown variables, demeaned analyst or broker level control variables, and analyst and calendar year month fixed effects. Absolute forecast error (*AFE*) is calculated as the absolute value of analyst's last quarterly forecast before the earnings announcement minus the actual firm's earnings, all divided by the firm's stock price measured 12 months prior to the beginning of the fiscal quarter. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. *GEOPROXIMITY* is calculated as one over the geographic distance between analyst's location and her covered firm's location. *LOGDIST* is calculated as the natural logarithm of one plus the geographic distance. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| DV = DAFE | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| DLOCAL | -0.014* | -0.015* | | | | |
| | (-1.84) | (-1.92) | | | | |
| DGEOPROXIMITY | | | -0.045*** | -0.045*** | | |
| | | | (-3.03) | (-3.04) | | |
| DLOGDIST | | | | | 0.002 | 0.002 |
| | | | | | (1.20) | (1.20) |
| LOCKDOWN | -0.017 | -0.016 | -0.017 | -0.016 | -0.017 | -0.016 |
| | (-1.15) | (-1.09) | (-1.13) | (-1.07) | (-1.14) | (-1.08) |
| LOCKDOWN × DLOCAL | 0.063*** | 0.064*** | | | | |
| | (2.93) | (2.96) | | | | |
| LOCKDOWN × DGEOPROXIMITY | | | 0.081** | 0.080** | | |
| | | | (2.04) | (2.02) | | |
| LOCKDOWN × DLOGDIST | | | | | -0.009** | -0.009** |
| | | | | | (-2.07) | (-2.10) |
| DBROKERSIZE | | 0.000 | | 0.000 | | 0.000 |
| | | (1.59) | | (1.54) | | (1.57) |
| DAGE | | 0.001*** | | 0.001*** | | 0.001*** |
| | | (13.10) | | (13.04) | | (13.09) |
| DFIRMEXP | | -0.001 | | -0.001 | | -0.001 |
| | | (-1.57) | | (-1.55) | | (-1.56) |
| DGENEXP | | -0.000 | | -0.000 | | -0.000 |
| | | (-0.28) | | (-0.31) | | (-0.29) |
| DNUMFIRM | | -0.001* | | -0.001* | | -0.001* |
| | | (-1.77) | | (-1.75) | | (-1.77) |
| Analyst FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 182,396 | 182,396 | 182,208 | 182,208 | 182,396 | 182,396 |
| Adj. R-square | 0.019 | 0.021 | 0.019 | 0.021 | 0.019 | 0.021 |

**Table 5: Forecast Accuracy with Management Guidance Partition**

The table presents results from estimating OLS regressions of demeaned absolute forecast error (*DAFE*) on the demeaned measurement of the geographic proximity (*DLOCAL*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurement of geographic proximity and the lockdown variables, demeaned analyst or broker level control variables, and analyst and calendar year month fixed effects. The regressions are run on subsamples that are separated based on whether the management of the covered firm issues a quarterly earnings guidance or not. Absolute forecast error (*AFE*) is calculated as the absolute value of analyst's last quarterly forecast before the earnings announcement minus the actual firm's earnings, all divided by the firm's stock price measured 12 months prior to the beginning of the fiscal quarter. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), and the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| DV = DAFE | Having at Least One Type of Management Guidance | Not Having Any Types of Management Guidance | Having EPS Guidance | Not Having EPS Guidance |
| DLOCAL | 0.001 | -0.022** | -0.002 | -0.019** |
| | (0.05) | (-2.30) | (-0.10) | (-2.15) |
| LOCKDOWN | 0.036 | -0.036** | 0.023 | -0.025 |
| | (1.19) | (-2.16) | (0.46) | (-1.63) |
| LOCKDOWN × DLOCAL | 0.053 | 0.078** | 0.048 | 0.069*** |
| | (1.56) | (2.53) | (1.06) | (2.63) |
| DBROKERSIZE | 0.000 | 0.000* | 0.000 | 0.000 |
| | (0.18) | (1.72) | (0.71) | (1.22) |
| DAGE | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| | (5.08) | (12.67) | (4.44) | (12.71) |
| DFIRMEXP | 0.000 | -0.002* | 0.001 | -0.002* |
| | (0.19) | (-1.77) | (0.37) | (-1.87) |
| DGENEXP | -0.001 | 0.000 | -0.000 | -0.000 |
| | (-0.89) | (0.20) | (-0.01) | (-0.20) |
| DNUMFIRM | -0.001* | -0.000 | -0.002** | -0.000 |
| | (-1.93) | (-0.97) | (-2.55) | (-1.00) |
| Analyst FE | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes |
| N | 55,549 | 126,695 | 34,207 | 148,061 |
| Adj. R-square | 0.037 | 0.020 | 0.047 | 0.020 |

**Table 6: Forecast Accuracy with Analyst's Alumni Connection Partition**

The table presents results from estimating OLS regressions of demeaned absolute forecast error (*DAFE*) on the demeaned measurement of the geographic proximity (*DLOCAL*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurement of geographic proximity and the lockdown variables, demeaned analyst or broker level control variables, and analyst and calendar year month fixed effects. The regressions are run on subsamples that are separated based on whether the analyst does or does not have alumni connection to either executive directors or any type of directors (including supervisory directors, executive directors, and senior managers). Absolute forecast error (*AFE*) is calculated as the absolute value of analyst's last quarterly forecast before the earnings announcement minus the actual firm's earnings, all divided by the firm's stock price measured 12 months prior to the beginning of the fiscal quarter. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| DV = DAFE | (1) With Alumni Connection to Executive Directors | (2) Without Alumni Connection to Executive Directors | (3) With Alumni Connection to at least one type of Directors | (4) Without Alumni Connection to all types of Directors |
|---|---|---|---|---|
| DLOCAL | -0.024 | -0.017* | -0.011 | -0.020* |
| | (-0.56) | (-1.93) | (-0.68) | (-1.90) |
| LOCKDOWN | -0.097 | -0.017 | -0.017 | -0.021 |
| | (-1.64) | (-1.08) | (-0.58) | (-1.11) |
| LOCKDOWN × DLOCAL | -0.102 | 0.063*** | 0.017 | 0.079*** |
| | (-0.79) | (2.71) | (0.41) | (2.80) |
| DBROKERSIZE | 0.001 | 0.000 | 0.000 | 0.000 |
| | (1.43) | (1.58) | (0.96) | (1.61) |
| DAGE | 0.000 | 0.001*** | 0.001*** | 0.001*** |
| | (1.15) | (11.26) | (6.81) | (10.49) |
| DFIRMEXP | -0.008 | -0.002** | -0.002 | -0.002 |
| | (-1.41) | (-2.19) | (-1.35) | (-1.60) |
| DGENEXP | 0.001 | -0.000 | -0.001 | 0.000 |
| | (0.13) | (-0.16) | (-0.72) | (0.17) |
| DNUMFIRM | -0.004 | -0.001 | -0.000 | -0.001 |
| | (-1.33) | (-1.36) | (-0.61) | (-1.63) |
| Analyst FE | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes |
| N | 4,506 | 149,828 | 55,170 | 110,125 |
| Adj. R-square | 0.044 | 0.026 | 0.027 | 0.022 |

**Table 7: Forecast Accuracy with Analyst Experience Partition**

The table presents results from estimating OLS regressions of demeaned absolute forecast error (*DAFE*) on the demeaned measurement of the geographic proximity (*DLOCAL*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurement of geographic proximity and the lockdown variables, demeaned analyst or broker level control variables, and analyst and calendar year month fixed effects. The regressions are run on subsamples that are separated based on whether the analyst has more or less firm or general experience (based on whether the analyst's experience is greater or less than the median experience of all the analysts who cover the firm at quarter *t*). Absolute forecast error (*AFE*) is calculated as the absolute value of analyst's last quarterly forecast before the earnings announcement minus the actual firm's earnings, all divided by the firm's stock price measured 12 months prior to the beginning of the fiscal quarter. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| DV = DAFE | More Firm Experience | Less Firm Experience | More General Experience | Less General Experience |
| DLOCAL | -0.025* | -0.004 | -0.022** | -0.009 |
| | (-2.42) | (-0.35) | (-2.01) | (-0.79) |
| LOCKDOWN | -0.040* | -0.004 | -0.024 | -0.001 |
| | (-1.73) | (-0.16) | (-1.14) | (-0.05) |
| LOCKDOWN × DLOCAL | 0.098*** | 0.030 | 0.085*** | 0.038 |
| | (3.08) | (0.87) | (2.84) | (1.19) |
| DBROKERSIZE | 0.000 | 0.000 | 0.000 | 0.000 |
| | (1.23) | (0.85) | (0.80) | (1.28) |
| DAGE | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| | (8.13) | (10.75) | (8.19) | (10.71) |
| DFIRMEXP | -0.001 | -0.000 | -0.001 | -0.000 |
| | (-0.49) | (-0.22) | (-1.48) | (-0.32) |
| DGENEXP | 0.001 | -0.001 | 0.001 | -0.002** |
| | (0.67) | (-1.18) | (0.55) | (-2.24) |
| DNUMFIRM | -0.001 | -0.000 | -0.001 | -0.001 |
| | (-1.34) | (-0.47) | (-1.09) | (-1.29) |
| Analyst FE | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes |
| N | 76,337 | 90,000 | 79,048 | 96,073 |
| Adj. R-square | 0.025 | 0.026 | 0.022 | 0.024 |

**Table 8: Informativeness of Forecast Revisions and Geographic Distance**

The table presents results from estimating OLS regressions of three-day cumulative abnormal return around forecast revision dates (*CAR [-1,+1]*) on the measurements of the geographic proximity (*LOCAL*, *GEOPROXIMITY*, *LOGDIST*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurement of geographic proximity and the lockdown variables, analyst or broker level control variables, and analyst and calendar year month fixed effects. *CAR[-1,+1]* is the three-day cumulative abnormal return (adjusted by the value weighted market return) around the forecast revision dates. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. *GEOPROXIMITY* is calculated as one over the geographic distance between analyst's location and her covered firm's location. *LOGDIST* is calculated as the natural logarithm of one plus the geographic distance. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (NUMFIRM), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), analyst's forecast accuracy from the past one year (*ACCURACY*), and the natural logarithm of the covered firm's market capitalization (*SIZE*). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| DV = CAR [-1,+1] | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | All Forecast Revisions | | | Upward Forecast Revisions | | | Downward Forecast Revisions | | |
| LOCAL | -0.003 | | | -0.006 | | | 0.001 | | |
| | (-1.05) | | | (-1.61) | | | (0.24) | | |
| GEOPROXIMITY | | 0.003 | | | 0.000 | | | 0.005 | |
| | | (0.39) | | | (0.03) | | | (0.50) | |
| LOGD | | | 0.000 | | | 0.001 | | | -0.000 |
| | | | (0.42) | | | (0.83) | | | (-0.05) |
| LOCKDOWN | -0.003 | -0.004 | -0.026 | 0.018 | 0.016 | -0.038 | -0.013 | -0.013 | -0.022 |
| | (-0.17) | (-0.23) | (-1.50) | (0.50) | (0.44) | (-1.15) | (-0.62) | (-0.62) | (-0.81) |
| LOCKDOWN × LOCAL | -0.018** | | | -0.032*** | | | -0.012 | | |
| | (-2.10) | | | (-2.66) | | | (-0.96) | | |
| LOCKDOWN × GEOPROXIMITY | | -0.052** | | | -0.080** | | | -0.046 | |
| | | (-2.00) | | | (-2.24) | | | (-1.17) | |
| LOCKDOWN × LOGD | | | 0.003** | | | 0.009*** | | | 0.001 |
| | | | (2.13) | | | (3.47) | | | (0.47) |
| SIZE | -0.001 | -0.001 | -0.001 | 0.001 | 0.001 | 0.001 | -0.002* | -0.002* | -0.002* |
| | (-1.15) | (-1.18) | (-1.18) | (0.64) | (0.64) | (0.63) | (-1.78) | (-1.79) | (-1.80) |
| BROKERSIZE | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (-1.30) | (-1.29) | (-1.30) | (-0.48) | (-0.45) | (-0.48) | (-1.14) | (-1.15) | (-1.14) |
| FIRMEXP | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.001 | -0.000 | -0.000 | -0.000 |
| | (-0.76) | (-0.70) | (-0.73) | (-1.13) | (-0.98) | (-1.15) | (-1.04) | (-1.06) | (-1.03) |
| GENEXP | 0.002 | 0.002 | 0.002 | -0.005 | -0.005 | -0.005 | 0.008 | 0.008 | 0.008 |
| | (0.40) | (0.38) | (0.39) | (-1.42) | (-1.42) | (-1.42) | (0.81) | (0.80) | (0.81) |
| NUMFIRM | -0.000 | -0.000 | -0.000 | -0.001 | -0.001 | -0.001 | 0.000 | 0.000 | 0.000 |
| | (-0.22) | (-0.22) | (-0.14) | (-1.19) | (-1.19) | (-1.05) | (0.70) | (0.70) | (0.71) |
| ACCURACY | 0.004 | 0.004 | 0.004 | 0.008 | 0.009* | 0.008 | 0.000 | 0.000 | -0.000 |
| | (1.35) | (1.40) | (1.36) | (1.50) | (1.67) | (1.54) | (0.01) | (0.01) | (-0.01) |
| Analyst FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 14,849 | 14,841 | 14,849 | 5,932 | 5,925 | 5,932 | 8,638 | 8,637 | 8,638 |
| Adj. R-square | 0.053 | 0.053 | 0.053 | 0.121 | 0.121 | 0.123 | 0.087 | 0.087 | 0.086 |

**Table 9: Boldness of Forecast Revisions and Geographic Distance**

The table presents results from estimating OLS regressions of whether the forecast revision is bold (*BOLD*) on the measurements of the geographic proximity (*LOCAL*, *GEOPROXIMITY*, *LOGDIST*), the lockdown indicator variable (*LOCKDOWN*), the interaction between the measurement of geographic proximity and the lockdown variables, analyst or broker level control variables, and analyst and calendar year month fixed effects. *BOLD* is an indicator variable for the boldness of analyst *i*'s forecast for firm *j* in quarter *t*, which equals to one if analyst's forecast is above both the analyst's prior forecast and the mean forecast immediately before the forecast revision, or else below both, and zero otherwise. To be considered in the mean forecast calculation, a forecast must be issued in the 90 days prior to analyst *i*'s forecast revision. *LOCKDOWN* is an indicator variable which equals to one if either the state of the analyst's location or the state of the covered firm's location issued a state-wide lockdown policy at the time of the analyst forecast announcement, and zero if both the state of the analyst's location and the state of the covered firm's location is not in a state-wide lockdown policy at the time of the analyst forecast announcement. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. *GEOPROXIMITY* is calculated as one over the geographic distance between analyst's location and her covered firm's location. *LOGDIST* is calculated as the natural logarithm of one plus the geographic distance. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), analyst's forecast accuracy from the past one year (*ACCURACY*), and the day difference between analyst's forecast revision date and the fiscal year end date (*HORIZON*). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the second quarter of 2020.

| DV = BOLD | (1) | (2) | (3) |
|---|---|---|---|
| LOCAL | 0.017** | | |
| | (2.53) | | |
| GEOPROXIMITY | | 0.042 | |
| | | (1.61) | |
| LOGDIST | | | -0.002 |
| | | | (-1.45) |
| LOCKDOWN | 0.034** | 0.034** | -0.003 |
| | (2.41) | (2.44) | (-0.11) |
| LOCKDOWN × LOCAL | -0.030* | | |
| | (-1.81) | | |
| LOCKDOWN × GEOPROXIMITY | | -0.115** | |
| | | (-1.98) | |
| LOCKDOWN × LOGDIST | | | 0.005 |
| | | | (1.64) |
| ACCURACY | 0.007 | 0.007 | 0.007 |
| | (1.00) | (1.04) | (1.01) |
| BROKERSIZE | -0.000 | -0.000 | -0.000 |
| | (-0.26) | (-0.28) | (-0.26) |
| FIRMEXP | 0.001 | 0.001 | 0.000 |
| | (0.76) | (0.78) | (0.71) |
| GENEXP | 0.003 | 0.003 | 0.003 |
| | (0.22) | (0.22) | (0.23) |
| NUMFIRM | 0.001 | 0.001 | 0.001 |
| | (1.64) | (1.63) | (1.64) |
| HORIZON | 0.000** | 0.000** | 0.000** |
| | (2.50) | (2.51) | (2.49) |
| Analyst FE | Yes | Yes | Yes |
| Year-Month FE | Yes | Yes | Yes |
| N | 74,406 | 74,360 | 74,406 |
| Adj. R-square | 0.034 | 0.034 | 0.034 |

**Table 10: Average Coverage Distance and Lockdowns**

The table presents results from estimating OLS regressions of the annual average distance between the analyst and her covered firms (*COVDIS*) and the natural logarithm of one plus the annual average distance (*LOGCOVDIS*) on the post 2020 year variable (*POST_2020*), analyst level control variables, and analyst fixed effects. The time unit of analysis is yearly instead of the usual quarterly. *POST_2020* is an indicator variable that equals to one when the forecast is announced after January 1, 2020. Control variables include the analyst's average forecast accuracy for the past one year (*AVGPASTACC*), the average broker size for the past one year (*AVGBROKERSIZE*), and the average number of firms covered by the analyst in the past one year (*AVGNUMFIRM*). Standard errors are clustered by analysts. T-statistics are reported in parentheses. All p-values are two-tailed. *, **, and *** indicate statistical significance at 10%, 5%, and 1% level, respectively. The sample starts in the first quarter of 2016 and ends in the fourth quarter of 2020.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| DV = | COVDIS | COVDIS | LOGCOVDIS | LOGCOVDIS |
| POST_2020 | 17.358** | 20.424** | 0.025** | 0.028** |
| | (2.03) | (2.24) | (2.35) | (2.38) |
| AVGPASTACC | | 29.892 | | 0.037 |
| | | (0.67) | | (0.62) |
| AVGBROKERSIZE | | 0.262 | | 0.000 |
| | | (0.86) | | (1.05) |
| AVGNUMFIRM | | -1.253 | | -0.000 |
| | | (-0.96) | | (-0.13) |
| Analyst FE | Yes | Yes | Yes | Yes |
| N | 5,061 | 5,061 | 5,061 | 5,061 |
| Adj. R-square | 0.842 | 0.842 | 0.806 | 0.806 |

**Table 11: Local Analyst's Advantage Before Vs After Lockdown**

This table presents results from estimating OLS regressions of demeaned absolute forecast error (*DAFE*) on the demeaned measurements of the geographic proximity (*DLOCAL*), the indicator variables of the period after the lockdown policy is lifted up (*POST_VAC* and *POST_LOCKDOWN*), the interaction between the measurements of geographic proximity and the lockdown variables, demeaned analyst or broker level control variables, and analyst and calendar year month fixed effects. Absolute forecast error (*AFE*) is calculated as the absolute value of an analyst's latest forecast, minus actual company earnings, as a percentage of stock price 12 months prior to the beginning of the fiscal year. *POST_VAC* is an indicator variable that equals to one when the forecast is announced after December 14, 2020 which is when vaccination is widely available in the US, and zero if the analyst forecast is announced pre-lockdown for both states. *POST_LOCKDOWN* is an indicator variable that equals to one when both the state of the analyst and the state of the firm have lifted up their lockdown policy at the time of the analyst forecast announcement, and zero if the analyst forecast announcement is announced pre-lockdown for both states. *LOCAL* is an indicator variable that equals to one if analyst's location is less than 62.14 miles (100 km) from the location of her covered firm. Control variables include the broker size (*BROKERSIZE*), the number of firms covered by the analyst (*NUMFIRM*), analyst's firm experience (*FIRMEXP*), analyst's total experience (*GENEXP*), the date difference between the analyst's forecast date and the firm's earnings announcement date (*AGE*). All variables, except the *LOCKDOWN* variable, are firm-quarter mean adjusted (the D preceding each variable stands for demeaned). The sample starts in the first quarter of 2016 and ends in the fourth quarter of 2021, but excludes any times when either the state of the analyst or the states of her covered firms are in lockdown.

| DV = DAFE | (1) | (2) |
|---|---|---|
| DLOCAL | -0.015** | -0.016** |
| | (-2.04) | (-2.09) |
| POST_VAC | 0.004 | |
| | (0.38) | |
| POST_LOCKDOWN | | -0.001 |
| | | (-0.13) |
| POST_VAC × DLOCAL | 0.026* | |
| | (1.85) | |
| POST_LOCKDOWN × DLOCAL | | 0.026* |
| | | (1.88) |
| DBROKERSIZE | 0.000 | 0.000 |
| | (1.59) | (1.58) |
| DAGE | 0.001*** | 0.001*** |
| | (17.38) | (17.41) |
| DFIRMEXP | -0.002*** | -0.002*** |
| | (-2.72) | (-2.72) |
| DGENEXP | -0.000 | -0.000 |
| | (-0.51) | (-0.51) |
| DNUMFIRM | -0.000 | -0.000 |
| | (-1.09) | (-1.09) |
| Analyst FE | Yes | Yes |
| Year-Month FE | Yes | Yes |
| N | 232,041 | 232,041 |
| Adj. R-square | 0.0220 | 0.0220 |