Panic Herding: Analysts' COVID-19 Experiences and the Interpretation of Earnings News

Matteo Vacca*†‡§

August 2024

Abstract

This paper examines how local experiences of the COVID-19 pandemic affect sell-side analysts' interpretation of earnings news. By exploiting the variation in the intensity and timing of local outbreaks, I show that analysts who are more exposed to the virus tend to herd more closely with the consensus forecast. However, I find no evidence of increases in forecast pessimism. The data are consistent with the intensity of exposure to the pandemic having a first-order effect on analysts' risk attitudes, rather than on the bias of their stated expectations.

DOI: https://doi.org/10.1016/j.jeconbus.2024.106206

*Department of Finance, Aalto University School of Business, 02150 Espoo, Finland.

†Department of Finance and Economics, Hanken School of Economics, 65101 Vaasa, Finland.

‡Email: matteo.vacca@aalto.fi.

§Acknowledgements: I am grateful for comments received from Teodor Duevski, Matti Keloharju, Michael Ungeheuer, Kam-Ming Wan (discussant), and seminar participants at Aalto University and at the 2020 GSF Winter Workshop. I thank the OP Group Research Foundation (grant no. 20190075) and the Foundation for the Advancement of the Finnish Securities Markets (grant no. 201900018) for financial support. Finally, I am especially grateful for the detailed comments of two anonymous referees, and the handling Editor, Moritz Ritter. All errors are my own.

1 Introduction

Sell-side analysts play a critical role during times of heightened uncertainty, when investors face challenges in assessing firm prospects (Loh and Stulz, 2018). For example, within a few weeks of the eruption of the COVID-19 crisis, the length of time clients spent on one-on-one phone calls with Citibank's analysts increased by 47%.[1] However, producers of financial information were themselves hit by the pandemic. In this paper, I investigate how these personal experiences shape analysts' interpretation of earnings news.

My work is motivated by the intuition that a major shock can affect the behavior of economic agents, for example through an emotional channel (Loewenstein, 2000). Accordingly, a number of recent papers examine how personal experiences affect the individual behavior of financial agents in the wake of adverse events.[2] However, little is still known about how these experiences shape the behavior of finance professionals during a major global crisis.

Exploiting variation in the intensity and timing of local outbreaks, I investigate the impact of COVID-19 experiences on sell-side analysts' forecast revisions following earnings announcements. This focus on the interpretation of earnings news ensures that analysts have similar non-subjective information sets, which is particularly important given the rapid development of the pandemic. Effectively, my identification strategy allows me to compare how analysts in *different* locations respond to the *same* earnings news.

I find that analysts who experience more severe local pandemic outbreaks tend to

[1] Kothari (2001) and Ramnath et al. (2008), among others, discuss the importance of analysts in financial markets. Clarke (2020) reports on the short-term effects of the pandemic on analyst activity.

[2] See, for example, Dessaint and Matray (2017); Andersen et al. (2019); Cuculiza et al. (2021); and Gao et al. (2020).

deviate less from the consensus forecast. Importantly, I can rule out analyst pessimism as a driver of herding behavior. Figure 1 shows that, during the COVID-19 crisis, pessimistic forecasts tend to be much bolder than optimistic forecasts. This makes it possible to clearly differentiate between the two effects. If anything, I find that more severe COVID-19 outbreaks are associated with greater forecast optimism, although this relationship is generally not significant. In the context of interpreting the same objective information after an earnings announcement, COVID-19 outbreaks seem to have a first-order effect on analyst herding, and only a second-order effect on forecast pessimism.

The increase in analyst herding is consistent with previous research showing that past experiences affect subsequent risk taking (Greenwood and Nagel, 2009; Malmendier and Nagel, 2011). Indeed, reputational models posit that herding may be driven by the risk of deviating (Scharfstein and Stein, 1990; Zwiebel, 1995), and empirical evidence shows that departing from the consensus forecast is inherently risky for a sell-side analyst. For example, forecast boldness has been shown to impact both the credibility and the career prospects of an analyst, and its potential negative consequences outweigh the positive consequences (Hong et al., 2000; Kadous et al., 2009).

I use various combinations of fixed effects to ensure that my results are driven by local experiences of the COVID-19 pandemic. First, earnings announcement fixed effects remove all firm and earnings news characteristics at the time of the earnings announcement, allowing me to compare how analysts in different locations respond to the same earnings news. Second, analyst fixed effects eliminate the characteristics of the individual analyst (e.g., education, experience, all-star status) and of her location (e.g., labor market conditions, estimated population). Third, analyst-firm fixed effects control for an analyst's average boldness and bias on a given company.

Later in my sample (i.e., during the summer of 2020), when the US financial markets had already largely recovered, I find that the effect of COVID-19 experiences becomes noisier and decreases in magnitude. Moreover, I document that decreases in boldness are only observed in pessimistic forecasts. Furthermore, I show that differences in analyst effort due to the pandemic are unlikely to account for my findings, as local pandemic outbreaks do not affect the decision to issue a forecast after the earnings news. To further address concerns that changes in analyst capacity could be influencing my results, I show that herding forecasts are not concentrated among stocks characterized by relatively lower levels of analyst effort (Harford et al., 2019).

I conduct a set of robustness checks to ensure that my findings are arising after the earnings news, rather than before. I use placebo tests to exclude seasonal patterns as a potential explanation and show that the results remain robust across various subsamples. Notably, my results are not driven by analysts located in the state of New York. This latter finding is particularly important as approximately half of all US-based sell-side analysts work within 100 km of New York City (Malloy, 2005), a major financial center that was also an early epicenter of the COVID-19 pandemic in the United States. Finally, I present various alternative measures of both dependent and independent variables, confirming the robustness of the main results.

This paper relates to various strands of literature. First, I contribute to the literature on the impact of recent experiences on the behavior of sell-side analysts. A growing body of work shows that recent experiences shape the behavior of investors and analysts.[3] Previous research primarily focuses on how analysts' behaviors are influenced by their experiences of local crises, such as hurricanes and terrorist attacks, which lead

[3]See, e.g., deHaan et al. (2017); Andersen et al. (2019); Dong et al. (2021); Li et al. (2021); and Laudenbach et al. (2021).

to more pessimistic forecasts due to cognitive biases such as the availability heuristic (Bourveau and Law, 2021; Cuculiza et al., 2021). However, these studies generally compare analyst behavior before and after such events, leaving a gap in our understanding of analyst behavior during ongoing global crises. Addressing this gap, my work examines the effects of the COVID-19 pandemic, a major global crisis that persists over an extended period and significantly impacts the overall economic outlook. I explore how the intensity of personal experiences during this pandemic shapes sell-side analysts' behavior, particularly in their interpretation of earnings news. I find that personal experiences shape the behavior of sell-side analysts even in times of high economic uncertainty, when investors find it harder to assess firm prospects and thus value analyst output more (Loh and Stulz, 2018; Baker et al., 2020).[4]

Second, I add to the ongoing debate on how herding behavior changes during periods of crisis. The literature can be divided into two broad camps: studies that find increases in herding during economic downturns, and those that do not.[5] This paper falls into the first camp: sell-side analysts who are more exposed to the pandemic tend to herd more closely with the consensus forecast. Contrary to previous work, I focus on the intensive margin of a crisis. My empirical strategy exploits granular data on analysts' locations and allows me to identify the degree of exposure to the COVID-19 pandemic as an important driver of heterogeneity in the herding of analyst forecasts. This result is consistent with a standard prediction in the psychology literature: the strength of a stimulus affects the sensation created by the stimulus (Stevens, 1957).

[4]The results in Arand and Kerl (2012) and Kretzmann et al. (2015) also support the conclusion that investors tend to pay more attention to sell-side analysts during bad times.

[5]For example, Huang et al. (2017) and Palmer et al. (2018) find that analysts herd more in recessions, whereas Kretzmann et al. (2015) and Lin et al. (2011) conclude that there is no increase in analyst herding during economic crises. Finally, Patton and Timmermann (2010) show the dispersion in economic predictions by professional forecasters increases during times of greater uncertainty.

Third, I add to the literature exploring the impact on risk-taking behavior of sudden changes to one's environment. My results indicate that the risk-taking tendencies of sell-side analysts, and consequently the informativeness of their outputs, may fluctuate rapidly over time and be influenced by individual experiences. While the effects of negative shocks on risk preferences among the general population remain a topic of debate, my results align with prior research by Jakiela and Ozier (2019) and Said et al. (2015), which suggests that exposure to violence and floods can increase risk aversion. Additionally, studies of individuals and professional investors who have experienced natural disasters have revealed similar patterns (Cameron and Shah, 2015; Cassar et al., 2017; Bernile et al., 2021).[6]

Fourth, I add to the literature that examines the impact of COVID-19 on both individuals in general and sell-side analysts in particular. The COVID-19 crisis has affected individual behavior through various factors, such as partisan bias and direct experiences of the pandemic.[7] Landier and Thesmar (2020) use firm-level data to show that analyst forecasts explain most of the decrease in stock prices due to the COVID-19 outbreak between January 2020 and mid-May 2020. Ramelli and Wagner (2020) and Chen et al. (2022) analyze conference calls and corporate disclosures around the same period. In related and contemporaneous work, Cahill et al. (2022) show that decreases in local mobility dampen price discovery around earnings announcements, and Du (2021) finds that household duties shape forecast timeliness around the school closures that occurred in March 2020. Contrary to other work, this paper leverages earnings announcements as events that trigger analysts to revise their forecasts, a methodology developed by deHaan et al. (2017) that is particularly well-suited to the rapidly

[6]See, e.g., Chuang and Schechter (2015) and Schildberg-Hörisch (2018) for reviews of the literature.

[7]See, e.g., Angrisani et al. (2020); Barrios and Hochberg (2021); Bu et al. (2020); and Cookson et al. (2020).

evolving COVID-19 context.[8] Subsequent work using a less granular methodology to examine Chinese sell-side analysts confirms that exposure to COVID-19 reduces the dispersion of earnings forecasts but finds mixed evidence on the risk aversion channel (Zhang et al., 2022).[9] By exploiting heterogeneity in analysts' exposure to the COVID-19 crisis, I show that first-hand experiences of the pandemic affect the behavior of finance professionals in a real-life setting with important financial stakes.

2 Hypotheses

Since clients heavily rely on analyst forecasts during crises (Loh and Stulz, 2018), and bold forecasts are generally considered more informative than herding forecasts (Clement and Tse, 2005), analysts may have incentives to deviate from consensus during the COVID-19 pandemic. However, individual behavior, and in particular one's attitude towards risk, is also responsive to recent events. Existing research has shown that emotions, particularly fear, play a significant role in driving changes in risk aversion, both among finance professionals and in the general population (Cohn et al., 2015; Guiso et al., 2018; Meier, 2019; Nguyen and Noussair, 2014). Notably, evidence from

[8] For example, Figure 1 of Landier and Thesmar (2020) shows that the average forecasts of annual earnings per share for Ford steadily decreased from mid-March to early May 2020, reflecting the changing information environment. These exceptional dynamics make it difficult, for example, to compare forecasts issued just a week apart and highlight the advantages of the methodology used in this paper.

[9] Specifically, the authors confirm my results on increased herding, but also find mixed evidence in favor of a limited attention channel (as opposed to a shift in risk preferences). It is important to highlight various differences between this paper and Zhang et al. (2022). First, Zhang et al. (2022) exclude the initial wave of COVID-19 and restrict their analysis to a geographic area less hit by the pandemic. Perhaps because of these decisions, they do not find a strong effect at the beginning of their sample period. Second, my empirical approach allows me to exploit earnings announcements as exogenous shocks to the probability of issuing forecasts, therefore circumventing the issue that sell-side analysts seldom issue contemporaneous forecasts on a given firm. This is especially important in a rapidly evolving information environment and is not captured using less granular approaches. Third, the results in Table 6 of Zhang et al. (2022) suggest that risk preferences may have played an important role among Chinese analysts. However, the authors do not explore this channel. Fourth, Section 4.5 of this paper shows that limited attention, or lack of effort, is unlikely to be driving my results. More generally, the idea that finance professionals could be inattentive to earnings announcements seems unlikely given the strong incentives analysts have to provide timely forecasts (Brown et al., 2015).

the psychology literature indicates that the COVID-19 pandemic has asymmetric effects on fear, with larger outbreaks being associated with increased fear in affected regions.[10]

Risk aversion in financial agents is associated with herding behavior.[11] Empirical evidence supports the notion that departing from the consensus forecast is risky for a sell-side analyst. First, Hong et al. (2000) and Kadous et al. (2009) show that the potential negative consequences of bold forecasts are greater than the positive consequences, affecting both an analyst's career prospects and her credibility with investors. Second, bold forecasts are used as a risky strategy to win analyst tournaments (Yin and Zhang, 2014). Third, bold forecasts are issued more often by analysts with higher job security, risk tolerance, and masculinity.[12] From a theoretical perspective, reputational models of herding help rationalize these results, suggesting that herding towards a common decision may be driven by the risk of deviating (Scharfstein and Stein, 1990; Zwiebel, 1995).[13]

These insights on fear, risk aversion, and herding lead to my first hypothesis:

H1: Larger COVID-19 outbreaks are associated with lower forecast boldness after earnings announcements.

In my second hypothesis, I focus on the overall effect of the COVID-19 pandemic on forecast bias. Figure 1 shows that, in my sample, pessimistic forecasts tend to be bolder than optimistic forecasts. This difference is particularly large during the first wave of the pandemic (i.e., after Q1-2020 earnings news), likely because analysts' earnings expectations decrease markedly during the initial stages of the COVID-19 crisis

[10] See, among others, Fitzpatrick et al. (2020) and Reznik et al. (2021).

[11] See, e.g., Zwiebel (1995); Sapienza (2010); Hirshleifer et al. (1994).

[12] See, respectively, Nolte et al. (2014); Christoffersen and Stæhr (2019); and Cleary et al. (2020).

[13] A number of papers examine herd behavior (Banerjee, 1992; Bikhchandani et al., 1992; Golec, 1997; Avery and Zemsky, 1998). Welch (2000) and Clement and Tse (2005) analyze herding among security analysts.

(Landier and Thesmar, 2020). Thus, if *H1* holds, one would expect more severe COVID-19 outbreaks to be associated with greater forecast optimism. Such forecast optimism may also be motivated by regulatory and policy interventions. In fact. government stimulus measures and monetary policy adjustments aimed at stabilizing the economy could lead to optimistic forecasts, countering the initial pessimistic bias induced by the pandemic's economic disruptions.

However, recent papers show that forecast bias can be responsive to external events. For example, factors such as bad weather, pollution, hurricanes, and terrorist attacks have been linked to forecast pessimism.[14] Therefore, channels such as mood, sentiment, and overweighting of left tail events may lead to greater forecast pessimism in the context of COVID-19 experiences.

Whether the effect on forecast boldness outweighs other behavioral channels leading to forecast pessimism is unclear ex ante and ultimately an empirical matter. Thus, I state my second hypothesis in null form:

H2: COVID-19 experiences have no effect on forecast bias.

There are many differences between the beginning of the pandemic and its later stages, and these differences generally suggest a weakening of the relationship between COVID-19 experiences and risk-averse behavior. For example, as more information about the virus becomes available, fear is likely to become less important in shaping analyst behavior.[15] Moreover, the relative importance of the pandemic as a driver of the performance of the main US indexes also decreases over time, with the S&P 500 closing at an all-time high right after the end of my sample (on August 18, 2020). Furthermore, there are

[14]See, respectively, deHaan et al. (2017); Dong et al. (2021); Bourveau and Law (2021); and Cuculiza et al. (2021).

[15]Evidence from the COVID-19 literature suggests that—while the pandemic negatively affected subjective well-being in the short run—various psychological effects reverted to the mean over time (see, e.g., Foa et al., 2020; and Brodeur et al., 2021).

important geographical differences in the intensity of local outbreaks over time. Within the United States, the first wave mainly hit blue states, but the gap between blue and red states narrowed over time (Bump, 2020). This staggered timeline of COVID-19 exposure suggests that analysts in regions that were affected later might have reacted with heightened sensitivity to the pandemic when it did strike, potentially exhibiting stronger risk-averse behaviors at a later stage compared to their counterparts in areas hit earlier. However, Cookson et al. (2020) show that the beliefs of partisan Republicans about equities remain relatively unfazed during the COVID-19 pandemic, while other investors become considerably more pessimistic (see also Barrios and Hochberg, 2021). These insights motivate my third conjecture:

H3: The effect of COVID-19 experiences is stronger at the beginning of the pandemic.

3 Data and methods

In this section, I describe the data set and discuss some descriptive statistics. I use several data sources, including Thomson Reuters' Institutional Brokers Estimate System (I/B/E/S), Google's Community Mobility Reports, and various COVID-19 data sets. Throughout the paper, subscript i identifies the analyst, subscript j identifies the firm, and subscript d identifies the date of the earnings announcement.

3.1 COVID-19 and mobility data

I obtain figures for COVID-19 victims and cases at the regional or state level (I use these two terms interchangeably throughout the paper). The main data source is the database of the Johns Hopkins University (JHU), which includes regional or state-level data for various countries, such as the US, Australia, Canada, and China.[16] For analysts located in countries that are only covered at the national level by JHU, I obtain regional data directly from local sources (see Appendix C for more details). If only one measure (i.e., either victims or cases in a certain region) is made available by the local authorities, I infer the other figure from the country's mortality rate on the same day.[17] Figure A1 provides some additional information on the spatial distribution of local COVID-19 victims in the United States.

I also obtain local mobility data from Google's Community Mobility Reports, which I use for some robustness checks. These reports provide a simple measure of local mobility by geographic segment, and have been used to assess the consequences of mobility restrictions (see, e.g., Chen et al., 2021 and Mendolia et al., 2021). Google reports the changes for each calendar day, compared to a baseline value for that day of the week. The baseline is the median value, for the corresponding day of the week, during the five weeks between January 3 and February 6, 2020. These data are only available from mid-February. In my analysis, I focus on the two measures linked to retail and workplace mobility.[18] As the data display a clear daily seasonality, I use a seven-day moving average:

$$Index_{i,c,t} = 100 + \frac{1}{7}\sum_{\tau=t-7}^{t-1} Change_{c,\tau} \tag{1}$$

for analyst i in region or state c on day t. $Index_{i,c,t}$ is either $RetailIndex_{i,c,t}$ or

[16] The data set is available at https://github.com/CSSEGISandData/COVID-19 (see also Dong et al., 2020).

[17] Moreover, if data for a single day in a certain region is not available, I simply use the values from the previous day. However, I exclude from my sample a few observations for which I am unable to locate any daily data at the regional level for two or more consecutive days, such as Ireland before March 15, 2020.

[18] Google defines the former as *"mobility trends for places like restaurants, cafes, shopping centers, theme parks, museums, libraries, and movie theaters"*; and the latter as *"mobility trends for places of work"*.

$WorkIndex_{i,c,t}$, and $Change_{c,t}$ is the daily change in the corresponding measure of mobility reported by Google. I also add 100 to facilitate the interpretation of the levels: a value of 100 means no change versus the (average) baseline.[19]

3.2 Analyst forecasts and earnings announcements

Similar to Landier and Thesmar (2020), I obtain analyst I/B/E/S forecasts, actual Earnings per Share (EPS), and earnings announcement dates for US-listed firms through a Thomson Reuters Refinitiv-Eikon platform (Eikon). I use Eikon to have up-to-date forecasts, stock prices, and earnings announcement dates, which are available on WRDS only with a delay. I include only firms that are traded on the NYSE, Amex, or NASDAQ according to Compustat. In my analysis, I examine forecasts issued from January to mid-August 2020 about Earnings per Share (EPS) for fiscal year 2020. I focus on FY2020 forecasts because Landier and Thesmar (2020) show that the pandemic affected analyst expectations mainly in the short term, and analysts may have weaker incentives to accurately update their longer-term forecasts (as there is more time before earnings are announced).

I exclude missing forecasts, forecasts for stocks with reported EPS in a currency other than US dollars, as well as firms with a stock price below $1 on the day before the earnings announcement (Chen and Jiang, 2006; deHaan et al., 2015; Cuculiza et al., 2021). I exclude forecasts for firms that are covered by less than three analysts (Hilary and Hsu, 2013; Malmendier and Shanthikumar, 2014). Forecasts made by unidentified analysts (i.e., without an analyst identifier) and those for which I am unable to find the analyst's location are also excluded from the empirical analysis. Finally, I eliminate

[19]Note that some analysts are located in regions for which Google data are not available at the same granularity (or not at all).

announcements for which I am unable to assess the location of at least two analysts (deHaan et al., 2017).

My main source for analyst locations is a sell-side analyst data set from Thomson Reuters, containing various biographical and contact information (similar to Jiang et al., 2016). In particular, for the majority of individuals in my sample, I am able to exploit phone numbers to locate analysts at the regional level. I complement this database with hand-collected information from multiple publicly available online sources, such as websites of both listed firms and brokerage houses. Importantly, analyst locations have been collected in late March and April 2020, in order to have an accurate snapshot of likely locations during the pandemic. Analyst locations are measured at the state level, so that I am implicitly assuming that analysts did not move out of state during the COVID-19 crisis.[20]

My final sample consists of 45,054 analyst-announcement pairs, of which 29,022 forecasts are updated within [0,2] trading days of the earnings news. The sample includes 1,564 analysts and 7,695 earnings announcements by 2,745 firms. While analysts in my sample are located in 56 different regions (or states) around the world, Figure A2 shows that analysts based in the state of New York represent approximately half of the sample. This percentage is in line with previous work (e.g., Malloy, 2005). International analysts account for around 5% of the observations. The most important region outside of the US is Greater London, which accounts for more analyst-announcement pairs than, e.g., Maryland and Connecticut. Figure 2 shows the

[20] The majority of finance professionals have been working from home during the COVID-19 crisis (Adams-Prassl et al., 2020; Brynjolfsson et al., 2020). Thus, using more granular data (e.g., county level) would require a much stronger assumption about where analysts lived during the pandemic and—to my knowledge—there is no established way in the literature to obtain the home addresses of all analysts included in my sample. Moreover, there are many anecdotal reports (see, e.g., Bortz, 2020 and Brody, 2020) about individuals and families moving to the suburbs because of the pandemic, for example from New York City to the Hamptons (which also are in the state of New York, approximately 80 miles from Manhattan).

distribution of observations over time: Q4-2019 announcements were mostly held in February, Q1-2020 announcements in late April and early May, and Q2-2020 announcements in late July and early August.

3.3 Variable definitions

Similar to deHaan et al. (2017), my main focus is on analyst activity after earnings announcements. This empirical strategy is particularly well-suited to examine how the intensity and timing of local outbreaks affect earnings forecasts. First, it ensures that analysts have similar non-subjective information sets, which is particularly important given how quickly the situation changed during the beginning of the COVID-19 pandemic. Second, earnings announcements represent a very important source of new information in the stock market (Bassemir et al., 2013; Basu et al., 2013), so that many analysts update their forecasts after these events.

I also make ample use of various fixed effects which allow me to isolate my main effect, i.e., an analyst's exposure to the COVID-19 pandemic. Effectively, the inclusion of earnings announcement fixed effects allows me to compare how analysts with different levels of exposure to the pandemic respond to the same earnings news. Analyst and analyst-firm indicators complete my fixed effects structure to remove additional time-invariant factors. Thus, the remaining variation in my dependent variables is made up of local and date-specific conditions that analyst i experiences while responding to firm j's earnings news.

I measure boldness as the extent to which a forecast deviates—in absolute value—from the consensus (see, e.g., Hong et al., 2000 and Jame et al., 2016):

$$Boldness_{i,j,d} = \frac{|Fcst_{i,j,d} - Consensus_{j,d-1}|}{|Consensus_{j,d-1}|} \tag{2}$$

for analyst i and firm j announcing its earnings on day d. $Fcst_{i,j,d}$ are the EPS forecasted by the analyst within days [0, 2] of firm j's earnings announcement on date d. $Consensus_{j,d-1}$ is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement.

I use two measures of analyst optimism. First, $Bias$ is defined as the relative optimism against consensus forecast, scaled by the price (see, e.g., Hong et al., 2000 and deHaan et al., 2017):

$$Bias_{i,j,d} = 100 * \frac{Fcst_{i,j,d} - Consensus_{j,d-1}}{Price_{j,d-1}} \tag{3}$$

for analyst i and firm j announcing its earnings on day d. $Fcst_{i,j,d}$ are the EPS forecasted by the analyst within days [0, 2] of firm j's earnings announcement on date d. $Consensus_{j,d-1}$ is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement, while $Price_{j,d-1}$ is the closing stock price on that same day.

Second, $Bias\_alt$ represents the deviation from the actual EPS, scaled by the price (see, e.g., deHaan et al., 2017):

$$Bias\_alt_{i,j,d} = 100 * \frac{Fcst_{i,j,d} - EPS_j}{Price_{j,d-1}} \tag{4}$$

where EPS_j are the future realized EPS of firm j for FY2020, and everything else is defined as above.

I observe considerable variation in analysts' excess optimism and boldness. I follow Jame et al. (2016), deHaan et al. (2017), and Dong et al. (2021), and minimize the influence of these extreme observations by winsorizing the top and bottom 1% of observations.

I use two dummy variables to examine analyst activity before and after the earnings news. *Recent* is an indicator that is equal to one if analyst i has updated her forecast on firm j in the month leading to the earnings announcement held on day d, and zero otherwise. $MakeFcst$ is an indicator equal to one if an analyst has issued a forecast within [0, 2] days from the earnings announcement, and zero otherwise.

The main regressor used in the paper is the natural logarithm of one plus the number of local COVID-19 victims in the previous 30 days (*Ln Local Victims*). Other variables used in the analysis are defined in the text. Appendix B contains the definitions of all the variables used in the paper.

3.4 Summary statistics

Table 1 reports summary statistics for the main variables used in the empirical analysis. A forecast is issued within two trading days of the earnings news in 64.4% of the analyst-announcement pairs, and the average *Bias* (*Boldness*) is -0.577 (0.413), consistent with consensus decreasing after the earnings announcements. The average (median) number of recent local victims is 3,443 (521), indicating that the distribution is skewed to the right. The average reduction in local mobility is approximately one-third, but there are large differences across regions and over time. Finally, analysts update their forecasts in the month before the earnings news in 29.9% of the observations.

In Table A1, I show how some of these variables have evolved over time within my sample. Forecast boldness, as well as analyst activity, tends to increase during the first wave (i.e., after Q1-2020 earnings announcements). On the other hand, later in the sample, analyst forecasts stop being pessimistic on average, as witnessed by the pattern in $Bias$. Furthermore, the table shows that there are very few observations in the month of June.

3.5 Fixed effects

This study adopts a robust empirical approach that leverages a comprehensive set of fixed effects, as guided by the methodology of deHaan et al. (2017). I employ two distinct types of fixed effects to enhance the precision of the analysis. The first type, earnings announcement fixed effects, are systematically included to account for each firm and its earnings announcement date. This allows for the exclusion of all intrinsic characteristics of the company, its earnings announcement, and the broader market conditions prevailing at the time of the earnings news. By incorporating earnings announcement fixed effects, I can examine how sell-side analysts across various regions simultaneously react to identical earnings news.[21]

The second type encompasses controls for other time-invariant characteristics through the inclusion of either analyst fixed effects or analyst-firm fixed effects. Analyst fixed effects serve to adjust for the time-invariant characteristics of the individual analyst (e.g., education, all-star status), as well as any constant characteristics related to the analyst's geographical location. Analyst-firm fixed effects are more granular and

[21] The accounting literature presents a divided stance on how information events, particularly earnings announcements, influence analyst disagreement. On one hand, recent studies (Wang, 2020; Berger et al., 2019) indicate a trend towards analyst herding post-announcement. Conversely, foundational theories by Kim and Verrecchia (1994, 1997)—also supported by empirical evidence (e.g., Barron et al., 2002)—argue that accounting disclosures catalyze the generation of unique insights among analysts. Neilson (2022) adds that uncertainties can remain even after announcements, especially in complex information environments.

additionally capture an analyst's historical tendencies and biases towards a specific company.

Overall, the fixed effect structure allows me to eliminate a lot of variation and ensures that the remaining effect arises because of local, date-specific conditions relating to analyst i while he/she responds to firm j's earnings news on day d. Given the rapid evolution of expectations during the period of the analysis (Landier and Thesmar, 2020), this approach controls for the information set available to analysts at the time of their revisions. By emphasizing the interpretation of earnings news, I ensure that analyst forecasts are evaluated based on a consistent and objective set of information.

4 Results

In this Section, I present the main results of the paper. In my analysis, I use various sets of fixed effects. In particular, earnings announcement fixed effects are always included. This approach allows me allows me to eliminate all characteristics of the earnings news and compare how analysts in different locations respond to the same earnings announcement. Earnings announcement fixed effects also ensure that my results are not driven by the bulk of Q4-2019 announcements.[22] Including analyst or analyst-firm fixed effects allows me to examine the effect of local COVID-19 outbreaks within these dimensions. All standard errors are two-way clustered at the announcement date and at the analyst region level.

4.1 Forecast boldness

I use the following model to study whether local COVID-19 experiences affect the tendency of sell-side analysts to herd their forecasts issued after earnings news towards

[22] Q4-2019 announcements were held in late January and February 2020, while the first COVID-19 death was reported in March 2020 for almost all analyst locations.

the consensus forecast:

$$Boldness_{i,j,d} = \alpha + \beta LnLocalVictims_{i,d-1} + \gamma Controls_{i,j,d} + \epsilon_{i,j,d} \tag{5}$$

for analyst i and firm j announcing its earnings on day d. $Boldness_{i,j,d}$ is a measure of forecast boldness, $LnLocalVictims_{i,d-1}$ measures the intensity of the local outbreaks on the day before the earnings news, and $Controls_{i,j,d}$ mainly include various sets of fixed effects.

Results in Table 2 show that there is a negative relationship between COVID-19 experiences and forecast boldness. Importantly, the estimated coefficients are stable across specifications including various sets of fixed effects. In particular, results in Columns (2) and (3) show that the effect is not driven exclusively by differences in forecast boldness across regions, but rather that the relationship also exists within analyst and even within analyst-firm.

The effect reported in Column (2) implies that a 10% increase in the number of local victims witnessed by an analyst is associated with a relative decrease in forecast boldness of approximately 0.3% ($-0.0130 * 0.1/0.413$). While the economic magnitude may seem small, the exponential growth of the pandemic during the outbreaks implies that most locations suffered from very large increases in the number of local victims. For example, in April 2020, the number of total COVID-19 victims in the state of New York increased by over 600%. Figure 3 shows how the number of recent victims in my sample evolved over time. The chart shows the exponential growth of the pandemic, as well as large cross-sectional differences across locations.

To shed further light on the economic magnitude of this effect, in Table A2 I repeat the analysis using $Victims\_rate$ as the main regressor. $Victims\_rate$ is simply the number of recent COVID-19 victims in a given region per million of inhabitants. Reported coefficients are multiplied by 1,000. The results in Table A2 show that an increase of 1,000 monthly COVID-19 victims per million of inhabitants is associated with a relative decrease in forecast boldness of approximately 10% ($-0.0412 * 1/0.413$).[23]

The empirical evidence presented so far is in line with *H1*: analysts who are more affected by the pandemic tend to deviate less from the consensus forecast. Deviating from consensus is inherently riskier than herding towards the consensus forecast, as bold forecasts affect both the credibility and the career prospects of an analyst, and their potential negative consequences are greater than the positive consequences (Hong et al., 2000; Kadous et al., 2009). Thus, these results are consistent with local COVID-19 outbreaks being associated with more risk-averse behavior among sell-side analysts. These results also suggest that exposure to the pandemic may negatively affect analysts' ability to provide relevant information to investors, as bold forecasts are usually more accurate than herding forecasts (Clement and Tse, 2005).

In terms of economic significance, my results on the relationship between exposure to the pandemic and forecast boldness can substantially influence market dynamics, primarily by altering how investors interpret earnings news. To the extent that heterogeneous COVID-19 experiences can generate higher disagreement among analysts, there exist potential spillovers to market volatility due to investors finding it challenging to assess the true economic impact of the pandemic without the coherent guidance usually provided by the consensus forecast (Lobo et al., 2017). Such an argument is consistent with reports of post-earnings announcement drift during the pandemic (e.g., Fang, 2024).

[23]Such an increase in the intensity of the pandemic is similar to going from a situation in which the virus is not present in a community to a large local outbreak, comparable in magnitude to the ones observed in the state of New York and in Lombardy (Italy) during the spring of 2020.

4.2 Forecast bias

I now examine the relationship between local COVID-19 outbreaks and forecast bias. As discussed in Section 2, the results of this analysis are uncertain ex ante. On the one hand, pessimistic forecasts tend to be bolder than optimistic forecasts, especially during the first wave of the pandemic. Thus, in light of the results presented so far, one would expect more severe COVID-19 outbreaks to be associated with greater forecast optimism. On the other hand, various behavioral channels suggest that COVID-19 experiences are associated with greater forecast pessimism. To test *H2*, I use the following model:

$$Y_{i,j,d} = \alpha + \beta LnLocalVictims_{i,d-1} + \gamma Controls_{i,j,d} + \epsilon_{i,j,d} \tag{6}$$

for analyst i and firm j announcing its earnings on day d. $Y_{i,j,d}$ is either $Bias$ or $Bias\_alt$, $LnLocalVictims_{i,d-1}$ measures the intensity of the local outbreaks on the day before the earnings news, and $Controls_{i,j,d}$ include various sets of fixed effects.

Table 3 reports the results of my analysis. In Panel A of Table 3, the dependent variable is $Bias$ and I examine relative forecast optimism (i.e., deviation from previous consensus). The results of this analysis show a positive but insignificant association between recent pandemic experiences and forecast optimism. In Panel B of Table 3, I confirm my results using $Bias\_alt$ (i.e., a measure of forecast accuracy that is directly relevant to investors). As in Panel A, local COVID-19 outbreaks are associated with a positive forecast bias. However, this association is statistically significant (at the 10% level) only in Column (3), which includes both earnings announcement and analyst-firm fixed effects.[24] Taken together, the results reported in Table 3 show that the effect on

[24]The R-squared figures reported in Panel B of Table 3 are generally higher than in Panel A. However, the figures are consistent with the R-squared of 0.966 reported in Column (2) of Table 3 in deHaan et al. (2017), which is closely comparable in terms of empirical strategy, dependent variable, and fixed effects.

forecast boldness outweighs behavioral channels leading to forecast pessimism, such as mood, sentiment, and the availability heuristic.

Overall, the results discussed so far suggest that local and recent experiences of the pandemic affect analysts' interpretation of earnings news, having a first-order effect on forecast boldness and only a marginal effect on forecast optimism. The empirical evidence is consistent with local COVID-19 experiences being associated with more risk-averse behavior among sell-side analysts. On the contrary, the results are unlikely to be driven by forecast pessimism arising, for example, from extrapolative behavior.

4.3 First wave vs. second wave

Consistent with *H3*, Table 4 shows that the effect of the pandemic is noisier in the second part of my sample. In this analysis, I split the sample by calendar month,[25] and repeat the analysis described in Equation 5. I include only earnings announcement fixed effects, so that I can run exactly the same analysis in each subsample. During the second wave, standard errors increase and the magnitude of the effect decreases. Therefore, the effect is strongest when overall uncertainty and the dispersion of yearly earnings forecasts are higher (Altig et al., 2020; Landier and Thesmar, 2020).

Furthermore, in Table 5 I repeat the analyses presented in Table 2 and Table 3, but including only announcements held between January and May 2020. That is, I exclude the latter part of my sample. In Panel A, the results show that the relationship between local pandemic experiences and forecast boldness is slightly larger in terms of magnitude and significance than the one observed in the whole sample. Panel B and Panel C of Table 5 show that, in this subsample, the association between COVID-19 experiences and forecast

[25] I exclude the months before the beginning of the first wave of the pandemic in most Western countries, as well as June 2020, for which the number of observations is relatively low.

optimism is actually positive and significant when including analyst or analyst-firm fixed effects.

4.4 Lack of optimism or lack of pessimism?

Next, I examine whether the lack of forecast boldness associated with COVID-19 experiences manifests itself through lack of optimism, lack of pessimism, or both. To investigate this question, I separately analyze the boldness of optimistic and pessimistic forecasts including various sets of fixed effects.

The results reported in Table 6 show that sell-side analysts mainly refrain from issuing bold pessimistic forecasts. In the cross section, this finding may in part be driven by pessimistic forecasts being more frequent in the first part of my sample period (see Table A1), when the relationship between local outbreaks and forecast boldness is stronger (see Table 4). However, I obtain similar results when including analyst and analyst-firm fixed effects. In particular, the latter allow me to separately compare pessimistic and optimistic forecasts issued by the same analyst for a given firm, but at different times.

These results are consistent with Hong and Kubik (2003) who find that the labor market rewards extreme relative optimism but not extreme relative pessimism. Moreover, previous research shows that analysts tend to herd more when issuing pessimistic forecasts and downgrades.[26] Chen and Jiang (2006) suggest this behavior is partly driven by banks' incentives to build up investment banking relations with the firms covered by their analysts (see also, e.g., Chan et al., 2007; Kolasinski and Kothari, 2008). Nolte et al. (2014) also find that career concerns have a strong impact on herding for negative (but not positive) deviations from the consensus.

[26] See, respectively, Chen and Jiang (2006); and Jegadeesh and Kim (2010).

4.5 COVID-19 experiences and analyst effort

The results presented so far are consistent with the *panic herding* hypothesis discussed in Section 2. A natural alternative explanation is that the effect of COVID-19 outbreaks on forecast boldness could be driven by sell-side analysts' lack of effort. While this story would run counter to other evidence—Loh and Stulz (2018) show that analysts work harder in times of heightened economic uncertainty, a pattern that is consistent with industry accounts of the effects of the pandemic (Clarke, 2020; Whyte, 2020)[27]—I nevertheless scrutinize it empirically.

I cannot directly measure analyst effort *after earnings news*, but I can test two central implications of the low-effort story. First, I examine whether analysts experiencing larger COVID-19 outbreaks become less likely to respond to earnings announcements. Second, I assess whether herding forecasts are more frequent among stocks that are relatively less important for a given analyst. I find that the results of these tests are not consistent with lack of effort driving the relationship between COVID-19 experiences and analyst herding, neither cross-sectionally nor when including analyst fixed effects.

In the first test, the dependent variable is an indicator that is equal to one if an analyst issues a forecast within [0,2] days of the earnings news, and zero otherwise. deHaan et al. (2017) use a similar specification to show that analysts become less active after earnings news when experiencing unpleasant weather. The results, reported in Table 7, suggest that local outbreaks are not associated with significant decreases in analyst activity following earnings announcements. The estimated coefficients of interest are small, not statistically significant at conventional levels, and positive.

In the second test, I focus on the interaction between COVID-19 experiences and a

[27] Analysts significantly differ in their effort levels (Klettke et al., 2015), and client accessibility and responsiveness are important drivers of analyst compensation (Brown et al., 2015).

firm's importance within an analyst's portfolio. Sell-side analysts face strong incentives to give the appearance of being active during the pandemic, and could decide to issue herding forecasts without actually doing much work. As analysts allocate effort rationally (see Harford et al., 2019), this behavior should be more frequent among stocks that are less important for analyst careers. I estimate the following model:

$$Boldness_{i,j,d} = \alpha + \beta_1 LnLocalVictims_{i,d-1} + \beta_2 Importance_{i,j,d-1} + \\ + \beta_3 LnLocalVictims_{i,d-1} * Importance_{i,j,d-1} + \gamma Controls_{i,j,d} + \epsilon_{i,j,d} \quad (7)$$

for analyst i and firm j announcing its earnings on day d. $Boldness_{i,j,d}$ is a measure of forecast boldness, $LnLocalVictims_{i,d-1}$ measures the intensity of the local outbreaks on the day before the earnings news, $Importance_{i,j,d-1}$ is one of two measures of a firm's importance within an analyst's portfolio, and $Controls_{i,j,d}$ include various sets of fixed effects.

I use two different proxies for a firm's relative importance. In Table 8, I exploit forecast revisions made in the month leading up to the earnings news to identify analyst-announcement pairs characterized by a high level of observable effort. Columns (1) and (2) show that my proxy captures elements of analyst effort, being positively associated with *MakeFcst*. Columns (3) and (4), show that low levels of recent effort are unlikely to be driving the relationship between local COVID-19 experiences and forecast boldness, as the point estimates of β_3 are small and not significant. In Table 9, I obtain similar results using a firm's relative rank in an analyst's portfolio based on its market capitalization as a measure of *Relative Importance*.[28] In particular, the estimates of β_3 reported in Columns

[28] *Relative Importance* is defined as one minus a firm's relative rank in an analyst's portfolio (scaled by the total number of firms within the portfolio), based on its market capitalization as of the end of 2019. It takes on values between [0,1), with higher values indicating larger firms within an analyst's portfolio.

(3) and (4) are negative and not significant, suggesting that herding is not driven by firms that are relatively less important for analysts' careers.

Taken together, these results speak against the story where lack of effort would be driving the relationship between sell-side analysts' COVID-19 experiences and forecast boldness. On the one hand, local pandemic outbreaks do not seem to affect the decision to issue a forecast after the earnings news (deHaan et al., 2017; Driskill et al., 2020). On the other hand, herding forecasts are not concentrated among stocks that are relatively less important for the analysts (Harford et al., 2019).

5 Robustness

In this Section, I discuss the robustness of my results. First, I make sure that my results are not driven by macroeconomics announcements or by forecasts made before the earnings news. Then, I show that my results are robust across various subsamples, and using alternative definitions of the dependent and independent variables. Finally, I briefly discuss some methodological aspects.

5.1 Federal Reserve announcements

The results presented in Table 4 indicate the existence of a time-varying effect of experiencing the COVID-19 pandemic. One possible driver of this variation is that announcements from the Federal Reserve may be contributing to differences in forecast boldness. For this reason, in Table A3, I include an indicator variable that is equal to one if there was a recent meeting of the Federal Open Market Committee (and zero otherwise), as well as an interaction term.

Similar to Harford et al. (2019), I exclude analysts who cover less than four firms within my sample.

The results of this analysis in the full sample, reported in Panel A of Table A3, show that macroeconomic announcements are not driving my results. Nevertheless, this does not mean that action by the Federal Reserve have no effect on market expectations. In fact, Panel B of Table A3 shows that—when restricting the sample to the early stages of the pandemic—Federal Open Market Committee announcements decrease uncertainty and sell-side analysts become less inclined to issue bold (i.e., very pessimistic) forecasts.

5.2 Broader sample analysis

The focus of this paper is on the effect of local COVID-19 experiences on the interpretation of earnings news. This choice is motivated by the fact that expectations were evolving rapidly during the pandemic (Landier and Thesmar, 2020), making it very important to control for the information set of analysts when they are issuing their forecast. In fact, as sell-side analysts seldom issue contemporaneous forecasts on a given firm away from information events, focusing on the interpretation of earnings news ensures that analysts have similar non-subjective information sets.

Nevertheless, I verify the robustness of my results in a broader sample, using the following specification:

$$\begin{aligned} Y_{i,j,t} = \alpha + \beta_1 LnLocalVictims_{i,t-1} + \beta_2 EA_{i,j,t-1} + \\ + \beta_3 LnLocalVictims_{i,t-1} * EA_{i,j,t-1} + \gamma Controls_{i,j,t} + \epsilon_{i,j,t} \quad (8) \end{aligned}$$

for analyst i issuing a forecast on firm j in day t. The dependent variable is either $Bias$ or $Boldness$, $LnLocalVictims_{i,t-1}$ measures the intensity of the local outbreaks on the day before the forecast is issues, $EA_{i,j,t-1}$ is an indicator equal to one if there was a recent earnings announcement, and $Controls_{i,j,d}$ include various sets of fixed effects.[29]

The results of this analysis are reported in Table A4. Reassuringly, the main results of the paper also hold with this alternative specification: analysts with more intense COVID-19 experiences tend to herd more. More specifically, the effect after earnings news is negative and significant in all specification, whereas the effect during other time period is negative but loses significance when including more granular fixed effects.

5.3 Analysis of forecasts before the earnings news

I consider the possibility that my results are driven by something unrelated to earnings news. I do so by analyzing the outstanding forecasts before the earnings announcements. Specifically, in my placebo tests, I examine forecasts issued before the earnings announcement—these forecasts are generally stale (Table 1 shows over 70% are issued more than one month before the earnings news)—on the number of local victims in the 30 days before the earnings announcements. In Table A5, I repeat the analyses in Tables 2, and 3, using three simple placebo measures as the dependent variable.

In Panel A, I use:

$$BoldnessPlacebo_{i,j,d} = \frac{|Fcst_{i,j,d-1} - Consensus_{j,d-1}|}{|Consensus_{j,d-1}|} \tag{9}$$

for analyst i and firm j announcing its earnings on day d.

[29]Note that using this specification, the set of fixed effects must slightly depart from those used so far in the paper. There are two small differences. First, earnings announcement fixed effects are not meaningful if the sample includes forecasts made far away from information events, and are therefore substituted by day and firm fixed effects. Second, these new firm fixed effects are not included at the same time as analyst-firm fixed effects because the latter completely subsume the former.

In Panel B, I use:

$$BiasPlacebo_{i,j,d} = 100 * \frac{Fcst_{i,j,d-1} - Consensus_{j,d-1}}{Price_{j,d-1}} \tag{10}$$

for analyst i and firm j announcing its earnings on day d.

Similarly, in Panel C, I use:

$$Bias\_altPlacebo_{i,j,d} = 100 * \frac{Fcst_{i,j,d-1} - EPS_j}{Price_{j,d-1}} \tag{11}$$

for analyst i and firm j announcing its earnings on day d.

The only difference from the original measures is that $BoldnessPlacebo$, $BiasPlacebo$, and $Bias\_altPlacebo$ use an analyst's outstanding forecast as of the day before the earnings news.

Table A5 reports the results for the over 44,000 analyst-announcement pairs for which I have a forecast on the day before the earnings news. The estimated parameters for the coefficient of interest are all insignificant. Moreover, when either analyst or analyst-firm fixed effects are included, the effect sizes reported in Table 2 are outside of the 95% confidence intervals of the corresponding placebo tests.[30] Overall, these results suggest that forecast boldness is not driven by views already expressed before the earnings announcements.

[30]For example, when including both earnings announcement and analyst fixed effects, the 95% confidence interval of the placebo test reported in Panel A of Table A5 is [-0.0095, 0.0127], while the estimated coefficient reported in Column (2) of Table 2 is -0.0130.

5.4 Forecast boldness across various subsamples

In Table A6, I analyze the robustness of my conclusions on forecast boldness by repeating the analysis presented in Equation 5 across various subsamples. My analyses control for the size of the local population; exclude analysts based in the State of New York or outside of the US; and exclude firms covered by less than 10 analysts, firms with price below \$3, or firms with consensus earnings (in absolute value) below \$0.10. Overall, the magnitude of the estimate is relatively stable across the various subsamples, and the estimated coefficients are significant at conventional levels.

5.5 Alternative definitions of dependent variables

Next, I address potential concerns related to how I measure forecast boldness and optimism. Market conditions change rapidly within the sample, so that the consensus forecast used in the main analysis may be based on stale views.[31] To mitigate this concern, I build dependent variables for which the consensus is computed using only earnings forecasts issued in the month before the earnings announcement. The results, reported in Table A7, show that the estimated coefficients are relatively similar to those discussed in Section 4.

Moreover, when defining *Boldness* and *Bias*, I measure both consensus and closing stock price on the day before the earnings news. Thus, my results could potentially be driven by systematic differences in the timing of forecasts, together with some pattern in the consensus forecast or in the stock price.[32] This does not seem to be the case: in Table A8, I verify that the magnitude and significance of the estimates reported in Table 2 and

[31] The *Bias_alt* variable is immune from these concerns because it does not depend on how the consensus earnings are measured.

[32] For example, my results could be partially explained by analysts experiencing more severe COVID-19 outbreaks reacting immediately to the earnings announcements, and more delayed forecasts systematically being more bold and pessimistic.

in Table 3 are similar when computing modified version of *Boldness* and *Bias*, using consensus and closing stock price as of the day before the analyst makes her forecast.

Finally, to show that the result is not driven by my choice of the dependent variables, in Table A9 I present the results of my analysis using alternative definitions of boldness and optimism based on other continuous measures adapted from the literature. In Panel A, I use *Boldness_unit*. I define this variable following Clement and Tse (2005) and Yin and Zhang (2014), but focus on forecasts issued shortly after the earnings news. For analyst i following firm j and issuing a forecast within [0,2] trading days of the earnings announcement held on day d, *Boldness_unit* is computed as the absolute distance between the forecast and consensus forecast on $d - 1$ minus the minimum absolute distance for all analysts issuing a forecast, with this difference scaled by the range in absolute distances for all analysts who issue a forecast. The main advantage of this measure is that it is standardized: its values range between 0 and 1, with 1 indicating that the forecast is the boldest, and 0 indicating that the forecast is the least bold among all analysts issuing a forecast after the firm's earnings announcement. The results of this robustness check are similar, albeit smaller in magnitude, to those presented in Table 2.

In Panel B of Table A9, I use *BiasStdDev*, which is defined as the difference between forecast and consensus, scaled by the standard deviation of outstanding forecasts on the day before the earnings announcement (similar to Cowen et al., 2006). The results of this analysis suggest that my results on forecast optimism are not driven by the procedure used to measure forecast bias. In Panel C, I confirm these findings using *Bias_altStdDev* as my dependent variable.

5.6 Alternative measures of local COVID-19 outbreaks

Table A10 repeats the analyses reported in Column (2) of Table 2 and Column (2) of Table 3, using other measures of local exposure to the pandemic, such as the number of local cases, and measures of local mobility (which are available only from mid-February, see Figure A3). In Panel A, the point estimates are of the expected signs, and the lack of significance in Column (1) suggests that local victims may be more salient for residents than the number of cases, possibly because the case fatality rate varies both cross-sectionally and over time. None of the estimates reported in Panel B is significant at conventional levels.

5.7 Other analyses

I also address potential concerns about the number of clusters and the fact that they are unbalanced, so that asymptotic approximations may not be valid. Roodman et al. (2019) provide a discussion of this issue, and describe the procedure of the test I use, as well as its technical implementation. Specifically, to verify the robustness of my main results, I use a wild bootstrap procedure (with 1,999 replications) for the specification reported in Column (2) of Table 2. The bootstrapped t-statistic (p-value) is -2.16 (0.03), thus alleviating these concerns.

Moreover, the significance of the coefficients in Table 2 is basically unchanged when dropping singletons (Correia, 2015). Even in specifications that include both earnings announcement and analyst-firm fixed effects (in the full sample), the minimum number of clusters does not drop below the conventional level of 42, recommended by Angrist and Pischke (2008).

6 Conclusion

I examine whether analysts' personal experiences affect the processing of earnings news during a period of extremely high uncertainty. Exploiting variation in the intensity and timing of COVID-19 outbreaks, I find that an increase in the number of local victims is associated with an increase in analysts' tendency to herd their forecasts towards the consensus forecast. However, there is no evidence that more severe pandemic outbreaks induce forecast pessimism in sell-side analysts.

These results challenge explanations based on mood, sentiment, and the availability heuristic. Moreover, I find no evidence that limited attention or lack of effort shape analyst behavior during the pandemic: local COVID-19 outbreaks do not seem to affect the decision to issue a forecast after the earnings news, and herding forecasts are not concentrated among stocks that are relatively less important for the analysts.

My results suggest that recent changes in analysts' environments may shape their risk attitudes through a fear channel. This mechanism could explain why more severe COVID-19 outbreaks have a first-order effect on forecast boldness, rather than on forecast pessimism. However, there also exist alternative explanations for the role of COVID-19 experiences, including stress, cognitive overload, and time-varying asymmetries in technological adaptations and remote working conditions.

My main contributions to the literature are as follows. First, I show that extreme events shape analyst behavior in the midst of a crisis, when their output is likely to matter the most. Second, I provide evidence that COVID-19 experiences directly affect forecast boldness, a crucial aspect of analyst output. Third, I show that local COVID-19 outbreaks influence the behavior of financial information providers, thus improving the understanding of how pandemics affect the financial markets. Finally, my results underscore the importance of analysts' diversity of experiences, in line with recent work emphasizing the positive effects of heterogeneity in the analyst pool on a firm's information environment (Gerken and Painter, 2020; Merkley et al., 2020).

References

Adams-Prassl, A., Boneva, T., Golin, M. and Rauh, C. (2020), 'Work tasks that can be done from home: Evidence on variation within & across occupations and industries', CEPR Discussion Paper No. 14901.

Altig, D., Baker, S., Barrero, J. M., Bloom, N., Bunn, P., Chen, S., Davis, S. J., Leather, J., Meyer, B., Mihaylov, E. et al. (2020), 'Economic uncertainty before and during the COVID-19 pandemic', *Journal of Public Economics* **191**, 104274.

Andersen, S., Hanspal, T. and Nielsen, K. M. (2019), 'Once bitten, twice shy: The power of personal experiences in risk taking', *Journal of Financial Economics* **132**(3), 97–117.

Angrisani, M., Cipriani, M., Guarino, A., Kendall, R. and Ortiz de Zarate, J. (2020), 'Risk preferences at the time of COVID-19: An experiment with professional traders and students', FRB of New York Staff Report No. 927.

Angrist, J. D. and Pischke, J. S. (2008), *Mostly Harmless Econometrics: An Empiricist's Companion*, Princeton University Press.

Arand, D. and Kerl, A. G. (2012), 'Analyst research and investor reactions: Evidence from the 2008 financial crisis', University of Giessen Working Paper.

Avery, C. and Zemsky, P. (1998), 'Multidimensional uncertainty and herd behavior in financial markets', *American Economic Review* **88**(4), 724–748.

Baker, S. R., Bloom, N., Davis, S. J. and Terry, S. J. (2020), 'COVID-induced economic uncertainty', NBER Working Paper No. 26983.

Banerjee, A. V. (1992), 'A simple model of herd behavior', *Quarterly Journal of Economics* **107**(3), 797–817.

Barrios, J. M. and Hochberg, Y. V. (2021), 'Risk perceptions and politics: Evidence from the COVID-19 pandemic', *Journal of Financial Economics* **142**(2), 862–879.

Barron, O. E., Byard, D. and Kim, O. (2002), 'Changes in analysts' information around earnings announcements', *Accounting Review* **77**(4), 821–846.

Bassemir, M., Novotny-Farkas, Z. and Pachta, J. (2013), 'The effect of conference calls on analysts' forecasts - German evidence', *European Accounting Review* **22**(1), 151–183.

Basu, S., Duong, T. X., Markov, S. and Tan, E.-J. (2013), 'How important are earnings announcements as an information source?', *European Accounting Review* **22**(2), 221–256.

Berger, P. G., Ham, C. G. and Kaplan, Z. R. (2019), 'Do analysts say anything about earnings without revising their earnings forecasts?', *Accounting Review* **94**(2), 29–52.

Bernile, G., Bhagwat, V., Kecskés, A. and Nguyen, P.-A. (2021), 'Are the risk attitudes of professional investors affected by personal catastrophic experiences?', *Financial Management* **50**(2), 455–486.

Bikhchandani, S., Hirshleifer, D. and Welch, I. (1992), 'A theory of fads, fashion, custom, and cultural change as informational cascades', *Journal of Political Economy* **100**(5), 992–1026.

Bortz, D. (2020), 'How the pandemic is persuading Millennials to leave the city and make living in the suburbs... Cool?', *Money.com* (August 18, 2020).
URL: *https://money.com/coronavirus-milliennials-homebuying-suburbs/*

Bourveau, T. and Law, K. K. (2021), 'Do disruptive life events affect how analysts assess risk? Evidence from deadly hurricanes', *Accounting Review* **96**(3), 121–140.

Brodeur, A., Clark, A. E., Fleche, S. and Powdthavee, N. (2021), 'COVID-19, lockdowns and well-being: Evidence from Google Trends', *Journal of Public Economics* **193**, 104346.

Brody, L. (2020), 'New Yorkers who fled to Hamptons amid coronavirus look at enrolling', *Wall Street Journal* (May 20, 2020).
URL: *https://www.wsj.com/articles/new-yorkers-who-fled-to-hamptons-amid-coronavirus-look-at-enrolling-11589972255*

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

Brynjolfsson, E., Horton, J. J., Ozimek, A., Rock, D., Sharma, G. and TuYe, H. Y. (2020), 'COVID-19 and remote work: An early look at US data', NBER Working Paper No. 27344.

Bu, D., Hanspal, T., Liao, Y. and Liu, Y. (2020), 'Risk taking during a global crisis: Evidence from Wuhan', *COVID Economics* **5**, 106–146.

Bump, P. (2020), 'Seven in 10 new coronavirus cases are emerging in red states', *Washington Post* (September 3, 2020).
URL: *https://www.washingtonpost.com/politics/2020/09/03/seven-in-10-new-coronavirus-cases-are-emerging-red-states/*

Cahill, D., Ho, C. Y. C. and Yang, J. W. (2022), 'The COVID-19 pandemic: How important is face-to-face interaction for information dissemination?', *Global Finance Journal* **54**, 100674.

Cameron, L. and Shah, M. (2015), 'Risk-taking behavior in the wake of natural disasters', *Journal of Human Resources* **50**(2), 484–515.

Cassar, A., Healy, A. and Von Kessler, C. (2017), 'Trust, risk, and time preferences after a natural disaster: experimental evidence from Thailand', *World Development* **94**, 90–105.

Chan, L. K., Karceski, J. and Lakonishok, J. (2007), 'Analysts' conflicts of interest and biases in earnings forecasts', *Journal of Financial and Quantitative Analysis* **42**(4), 893–913.

Chen, C., Frey, C. B. and Presidente, G. (2021), 'Culture and contagion: Individualism and compliance with covid-19 policy', *Journal of Economic Behavior & Organization* **190**, 191–200.

Chen, C., Stratopoulos, T. C., Wang, V. X. and Xing, B. B. (2022), 'Do firms provide informative disclosures in an environment of extreme uncertainty? Evidence from the COVID-19 pandemic', University of Waterloo Working Paper.

Chen, Q. and Jiang, W. (2006), 'Analysts' weighting of private and public information', *Review of Financial Studies* **19**(1), 319–355.

Christoffersen, J. and Stæhr, S. (2019), 'Individual risk tolerance and herding behaviors in financial forecasts', *European Financial Management* **25**(5), 1348–1377.

Chuang, Y. and Schechter, L. (2015), 'Stability of experimental and survey measures of risk, time, and social preferences: A review and some new results', *Journal of Development Economics* **117**, 151–170.

Clarke, P. (2020), 'Nervous clients flock to banks for virus research in boost to battered analysts', *Financial News* (April 22, 2020).
URL: *https://www.fnlondon.com/articles/nervous-clients-flock-to-banks-for-virus-research-in-boost-to-battered-analysts-20200422*

Cleary, S., Jona, J., Lee, G. and Shemesh, J. (2020), 'Underlying risk preferences and analyst risk-taking behavior', *Journal of Business Finance & Accounting* **48**(7-8), 949–981.

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

Cohn, A., Engelmann, J., Fehr, E. and Maréchal, M. A. (2015), 'Evidence for countercyclical risk aversion: An experiment with financial professionals', *American Economic Review* **105**(2), 860–85.

Cookson, J. A., Engelberg, J. E. and Mullins, W. (2020), 'Does partisanship shape investor beliefs? Evidence from the COVID-19 pandemic', *Review of Asset Pricing Studies* **10**(4), 863–893.

Correia, S. (2015), 'Singletons, cluster-robust standard errors and fixed effects: A bad mix', Duke University Working Paper.

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

Cuculiza, C., Antoniou, C., Kumar, A. and Maligkris, A. (2021), 'Terrorist attacks, analyst sentiment, and earnings forecasts', *Management Science* **67**(4), 2579–2608.

deHaan, E., Madsen, J. and Piotroski, J. D. (2017), 'Do weather-induced moods affect the processing of earnings news?', *Journal of Accounting Research* **55**(3), 509–550.

deHaan, E., Shevlin, T. and Thornock, J. (2015), 'Market (in) attention and the strategic scheduling and timing of earnings announcements', *Journal of Accounting and Economics* **60**(1), 36–55.

Dessaint, O. and Matray, A. (2017), 'Do managers overreact to salient risks? Evidence from hurricane strikes', *Journal of Financial Economics* **126**(1), 97–121.

Dong, E., Du, H. and Gardner, L. (2020), 'An interactive web-based dashboard to track COVID-19 in real time', *Lancet Infectious Diseases* **20**(5), 533–534.

Dong, R., Fisman, R., Wang, Y. and Xu, N. (2021), 'Air pollution, affect, and forecasting bias: Evidence from Chinese financial analysts', *Journal of Financial Economics* **139**(3), 971–984.

Driskill, M., Kirk, M. P. and Tucker, J. W. (2020), 'Concurrent earnings announcements and analysts' information production', *Accounting Review* **95**(1), 165–189.

Du, M. (2021), 'Locked-in at home: The impact of COVID-19 school closures on female analysts' attention at work', University of Mannheim Working Paper.

Fang, D.-J. (2024), 'Post-earnings announcement drift, systemic shock, and limited attention: Evidence from COVID-19 pandemic', NCCU Working Paper.

Fitzpatrick, K. M., Harris, C. and Drawve, G. (2020), 'Fear of COVID-19 and the mental health consequences in America.', *Psychological Trauma: Theory, Research, Practice, and Policy* **12**(1), 17–21.

Foa, R., Gilbert, S. and Fabian, M. O. (2020), 'COVID-19 and subjective well-being: Separating the effects of lockdowns from the pandemic', University of Cambridge Working Paper.

Gao, M., Liu, Y.-J. and Shi, Y. (2020), 'Do people feel less at risk? Evidence from disaster experience', *Journal of Financial Economics* **138**(3), 866–888.

Gerken, W. C. and Painter, M. (2020), 'The value of differing points of view: Evidence from financial analysts' geographic diversity', University of Kentucky Working Paper.

Golec, J. (1997), 'Herding on noise: The case of Johnson Redbook's weekly retail sales data', *Journal of Financial and Quantitative Analysis* **32**(3), 367–381.

Greenwood, R. and Nagel, S. (2009), 'Inexperienced investors and bubbles', *Journal of Financial Economics* **93**(2), 239–258.

Guiso, L., Sapienza, P. and Zingales, L. (2018), 'Time varying risk aversion', *Journal of Financial Economics* **128**(3), 403–421.

Harford, J., Jiang, F., Wang, R. and Xie, F. (2019), 'Analyst career concerns, effort allocation, and firms' information environment', *Review of Financial Studies* **32**(6), 2179–2224.

Hilary, G. and Hsu, C. (2013), 'Analyst forecast consistency', *Journal of Finance* **68**(1), 271–297.

Hirshleifer, D., Subrahmanyam, A. and Titman, S. (1994), 'Security analysis and trading patterns when some investors receive information before others', *Journal of finance* **49**(5), 1665–1698.

Hong, H. and Kubik, J. D. (2003), 'Analyzing the analysts: Career concerns and biased earnings forecasts', *Journal of Finance* **58**(1), 313–351.

Hong, H., Kubik, J. D. and Solomon, A. (2000), 'Security analysts' career concerns and herding of earnings forecasts', *Rand Journal of Economics* **31**(1), 121–144.

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

Jakiela, P. and Ozier, O. (2019), 'The impact of violence on individual risk preferences: Evidence from a natural experiment', *Review of Economics and Statistics* **101**(3), 547–559.

Jame, R., Johnston, R., Markov, S. and Wolfe, M. C. (2016), 'The value of crowdsourced earnings forecasts', *Journal of Accounting Research* **54**(4), 1077–1110.

Jegadeesh, N. and Kim, W. (2010), 'Do analysts herd? An analysis of recommendations and market reactions', *Review of Financial Studies* **23**(2), 901–937.

Jiang, D., Kumar, A. and Law, K. K. (2016), 'Political contributions and analyst behavior', *Review of Accounting Studies* **21**(1), 37–88.

Kadous, K., Mercer, M. and Thayer, J. (2009), 'Is there safety in numbers? The effects of forecast accuracy and forecast boldness on financial analysts' credibility with investors', *Contemporary Accounting Research* **26**(3), 933–968.

Kim, O. and Verrecchia, R. E. (1994), 'Market liquidity and volume around earnings announcements', *Journal of Accounting and Economics* **17**(1-2), 41–67.

Kim, O. and Verrecchia, R. E. (1997), 'Pre-announcement and event-period private information', *Journal of Accounting and Economics* **24**(3), 395–419.

Klettke, T., Homburg, C. and Gell, S. (2015), 'How to measure analyst forecast effort', *European Accounting Review* **24**(1), 129–146.

Kolasinski, A. C. and Kothari, S. (2008), 'Investment banking and analyst objectivity: Evidence from analysts affiliated with mergers and acquisitions advisors', *Journal of Financial and Quantitative Analysis* **43**(4), 817–842.

Kothari, S. (2001), 'Capital markets research in accounting', *Journal of Accounting and Economics* **31**(1-3), 105–231.

Kretzmann, C. W., Maaz, C. M. and Pucker, O. (2015), 'Sell side recommendations during booms and busts', *Risk Governance and Control: Financial Markets and Institutions* **5**, 49–67.

Landier, A. and Thesmar, D. (2020), 'Earnings expectations during the COVID crisis', *Review of Asset Pricing Studies* **10**(4), 598–617.

Laudenbach, C., Loos, B., Pirschel, J. and Wohlfart, J. (2021), 'The trading response of individual investors to local bankruptcies', *Journal of Financial Economics* **142**(2), 928–953.

Li, J. J., Massa, M., Zhang, H. and Zhang, J. (2021), 'Air pollution, behavioral bias, and the disposition effect in China', *Journal of Financial Economics* **142**(2), 641–673.

Lin, W.-Y., Chen, P.-J. and Chen, S.-S. (2011), 'Stock characteristics and herding in financial analyst recommendations', *Applied Financial Economics* **21**(5), 317–331.

Lobo, G. J., Song, M. and Stanford, M. H. (2017), 'The effect of analyst forecasts during earnings announcements on investor responses to reported earnings', *The Accounting Review* **92**(3), 239–263.

Loewenstein, G. (2000), 'Emotions in economic theory and economic behavior', *American Economic Review* **90**(2), 426–432.

Loh, R. K. and Stulz, R. M. (2018), 'Is sell-side research more valuable in bad times?', *Journal of Finance* **73**(3), 959–1013.

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

Malmendier, U. and Nagel, S. (2011), 'Depression babies: do macroeconomic experiences affect risk taking?', *Quarterly Journal of Economics* **126**(1), 373–416.

Malmendier, U. and Shanthikumar, D. (2014), 'Do security analysts speak in two tongues?', *Review of Financial Studies* **27**(5), 1287–1322.

Meier, A. N. (2019), 'Emotions, risk attitudes, and patience', SOEP Paper on Multidisciplinary Panel Data Research No. 1041.

Mendolia, S., Stavrunova, O. and Yerokhin, O. (2021), 'Determinants of the community mobility during the covid-19 epidemic: The role of government regulations and information', *Journal of Economic Behavior & Organization* **184**, 199–231.

Merkley, K. J., Michaely, R. and Pacelli, J. (2020), 'Cultural diversity on Wall Street: Evidence from sell-side analysts' forecasts', *Journal of Accounting and Economics* **10**(1), 101330.

Neilson, J. J. (2022), 'Investor information gathering and the resolution of uncertainty', *Journal of Accounting and Economics* **74**(1), 101513.

Nguyen, Y. and Noussair, C. N. (2014), 'Risk aversion and emotions', *Pacific Economic Review* **19**(3), 296–312.

Nolte, I., Nolte, S. and Vasios, M. (2014), 'Sell-side analysts' career concerns during banking stresses', *Journal of Banking & Finance* **49**, 424–441.

Palmer, M., Eickhoff, M. and Muntermann, J. (2018), 'Detecting herding behavior using topic mining: The case of financial analysts', *Research Papers* **97**.

Patton, A. J. and Timmermann, A. (2010), 'Why do forecasters disagree? Lessons from the term structure of cross-sectional dispersion', *Journal of Monetary Economics* **57**(7), 803–820.

Ramelli, S. and Wagner, A. F. (2020), 'Feverish stock price reactions to COVID-19', *Review of Corporate Finance Studies* **9**(3), 622–655.

Ramnath, S., Rock, S. and Shane, P. (2008), 'The financial analyst forecasting literature: A taxonomy with suggestions for further research', *International Journal of Forecasting* **24**(1), 34–75.

Reznik, A., Gritsenko, V., Konstantinov, V., Khamenka, N. and Isralowitz, R. (2021), 'COVID-19 fear in Eastern Europe: Validation of the fear of COVID-19 scale', *International Journal of Mental Health and Addiction* **19**(5), 1903–1908.

Roodman, D., Nielsen, M. Ø., MacKinnon, J. G. and Webb, M. D. (2019), 'Fast and wild: Bootstrap inference in Stata using boottest', *Stata Journal* **19**(1), 4–60.

Said, F., Afzal, U. and Turner, G. (2015), 'Risk taking and risk learning after a rare event: Evidence from a field experiment in pakistan', *Journal of Economic Behavior & Organization* **118**, 167–183.

Sapienza, P. (2010), 'Discussion of self-selection and the forecasting abilities of female equity analysts', *Journal of Accounting Research* **48**(2), 437–443.

Scharfstein, D. S. and Stein, J. C. (1990), 'Herd behavior and investment', *American Economic Review* **80**(3), 465–479.

Schildberg-Hörisch, H. (2018), 'Are risk preferences stable?', *Journal of Economic Perspectives* **32**(2), 135–154.

Stevens, S. S. (1957), 'On the psychophysical law', *Psychological Review* **64**(3), 153–181.

Wang, J. (2020), 'Does analyst forecast dispersion represent investors' perceived uncertainty toward earnings?', *Review of Accounting and Finance* **19**(3), 289–312.

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

Whyte, A. (2020), 'How the coronavirus crisis changed investment research, according to the all-America research team'.
URL: *https://www.institutionalinvestor.com/article/b1nv1zts9361bl/How-the-Coronavirus-Crisis-Changed-Investment-Research-According-to-the-All-America-Research-Team*

Yin, H. and Zhang, H. (2014), 'Tournaments of financial analysts', *Review of Accounting Studies* **19**(2), 573–605.

Zhang, J., Wu, J., Luo, Y., Huang, Z. and He, R. (2022), 'Covid-19 pandemic, limited attention, and analyst forecast dispersion', *Finance Research Letters* **50**, 103322.

Zwiebel, J. (1995), 'Corporate conservatism and relative compensation', *Journal of Political Economy* **103**(1), 1–25.

Figure 1: Upward and downward forecast boldness by calendar month

This figure shows the average boldness of optimistic and pessimistic forecasts made after earnings announcements held between January 1 and August 15, 2020. Boldness is measured based on the absolute deviation of the forecast from the consensus (variable definitions are reported in Appendix B).

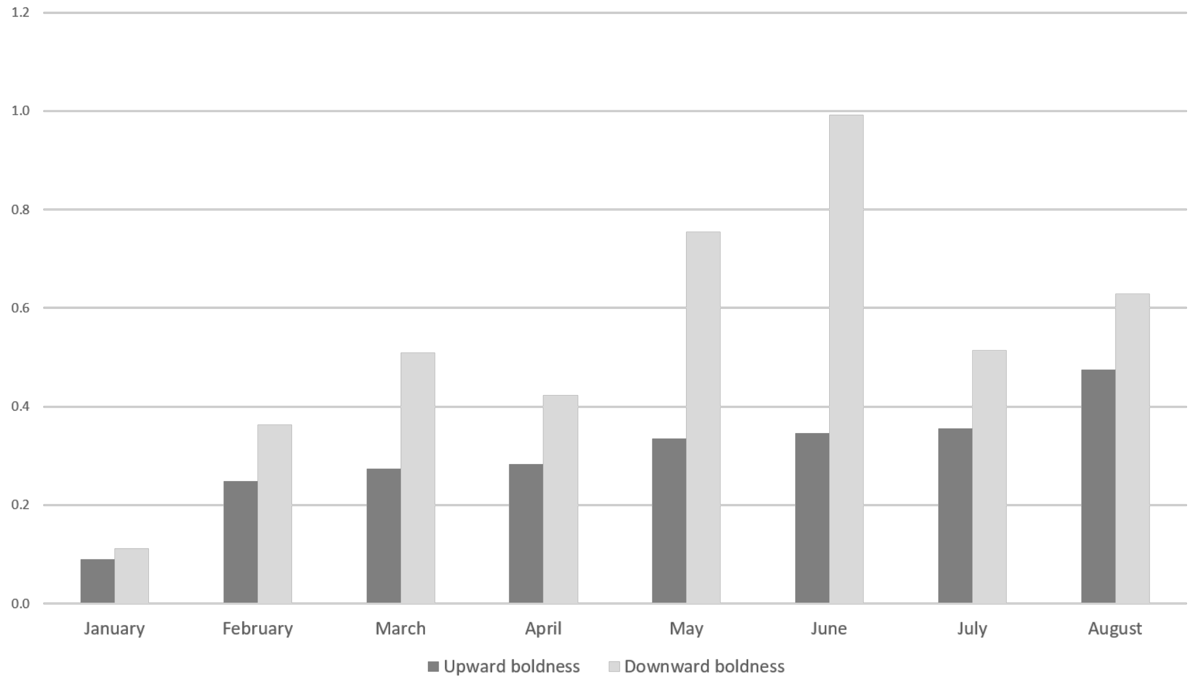

Figure 2: Analyst-announcement pairs over time
This figure shows the distribution of analyst-announcement pairs in my sample, from January 1 to August 15, 2020.

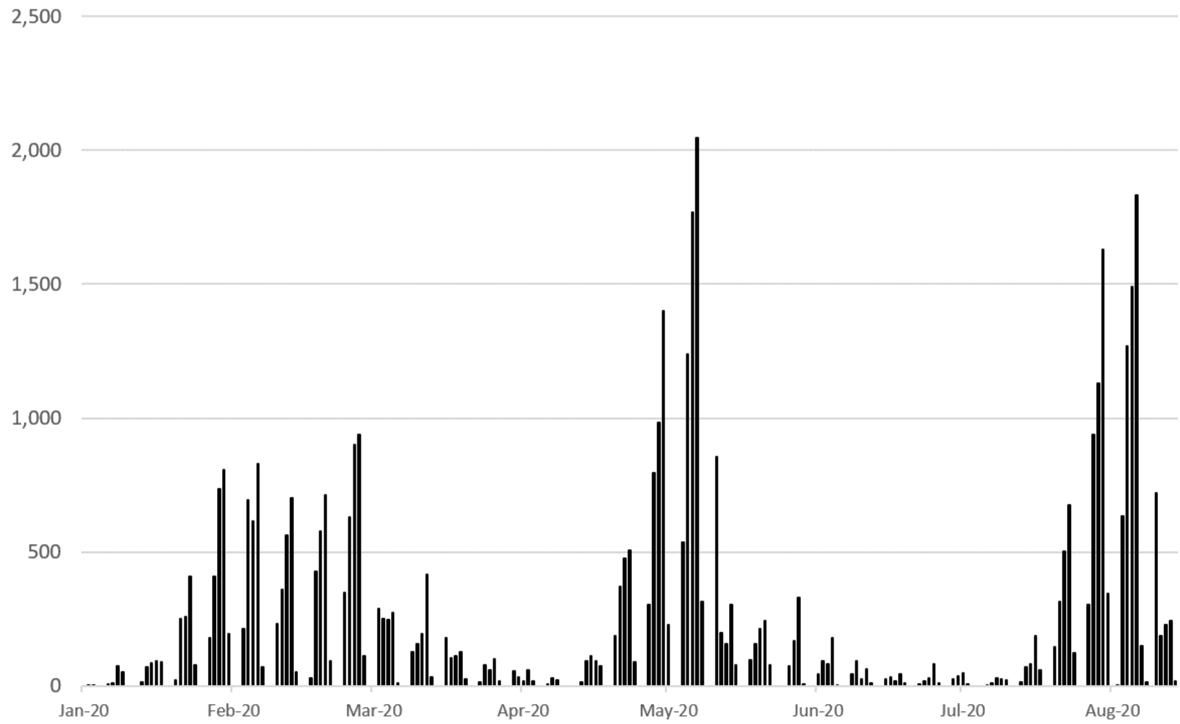

Figure 3: The development of the COVID-19 pandemic
This figure shows the average, 10^{th} and 90^{th} percentiles of the number of local COVID-19 monthly victims, by earnings announcement date.

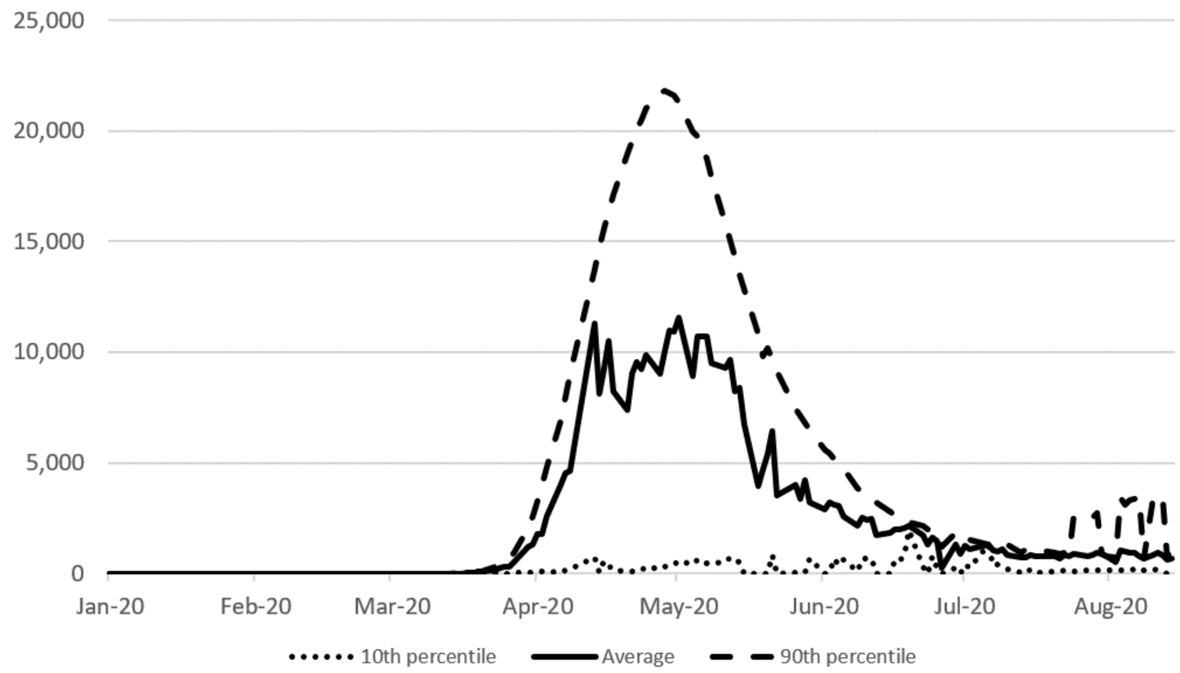

Table 1: Summary statistics

This table presents summary statistics for the main variables used in the empirical analysis. The full sample includes forecasts made following earnings announcements by US-listed firms between January 1 and August 15, 2020. Variables related to local mobility and to local outbreaks are measured on the day before the earnings news. Variable definitions are reported in Appendix B.

Variable	N	Mean	SD	P25	P50	P75
Boldness	28,984	0.413	1.060	0.034	0.105	0.304
Bias	29,022	-0.577	4.487	-0.747	-0.026	0.375
Bias_alt	28,556	1.111	13.571	-1.570	-0.249	0.939
Local Victims (in 000's)	45,054	3.443	6.646	0.000	0.521	2.089
WorkIndex	34,549	65.488	16.820	53.714	64.286	69.143
RetailIndex	34,549	72.354	20.501	54.429	76.714	88.000
Recent	45,054	0.299	0.458	0.000	0.000	1.000
MakeFcst	45,054	0.644	0.479	0.000	1.000	1.000

Table 2: Local outbreaks and forecast boldness

This table analyzes the relationship between local outbreaks and forecast boldness. In all specifications, the dependent variable is *Boldness* and the main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	Dependent variable: *Boldness*		
	(1)	(2)	(3)
Ln Local Victims	-0.0123*	-0.0130**	-0.0110***
	(-1.87)	(-2.55)	(-2.70)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,984	28,984	28,984
R^2	0.824	0.849	0.932

Table 3: Local outbreaks, forecast optimism and forecast bias

The table presents estimates from OLS regressions of *Bias* and *Bias_alt* on the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. In both panels, Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent variable: *Bias*			
	(1)	(2)	(3)
Ln Local Victims	0.0367	0.0326	0.0331
	(1.09)	(1.04)	(1.12)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	29,022	29,022	29,022
R^2	0.695	0.745	0.901

Panel B - Dependent Variable: *Bias_alt*			
	(1)	(2)	(3)
Ln Local Victims	0.0332	0.0357	0.0602*
	(0.86)	(1.01)	(1.78)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,556	28,556	28,556
R^2	0.948	0.959	0.984

Table 4: Local outbreaks and forecast boldness - Analysis by calendar month

The table examines the relationship between local outbreaks and forecast boldness, by calendar month. In all specifications, the dependent variable is *Boldness* and the main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level. Earnings announcement fixed effects are included. More granular fixed effects are not included due to the limited temporal window in each regression.

	Dependent variable: *Boldness*				
	March	April	May	July	August
	(1)	(2)	(3)	(4)	(5)
Ln Local Victims	-0.0192	-0.0139**	-0.0131**	-0.0152	-0.0038
	(-1.47)	(-2.25)	(-2.30)	(-0.84)	(-0.17)
Earnings Announcement FE	Yes	Yes	Yes	Yes	Yes
Observations	1,709	4,028	5,669	4,352	4,276
R^2	0.807	0.762	0.830	0.861	0.797

Table 5: Forecast boldness and optimism during the first wave

The table presents estimates from OLS regressions of *Boldness*, *Bias* and *Bias_alt* on the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Only earnings announcement held between January and May 2020 are included. In both Panels, Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Boldness*			
	(1)	(2)	(3)
Ln Local Victims	-0.0138***	-0.0156***	-0.0159***
	(-3.24)	(-3.08)	(-3.17)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	20,052	20,052	20,052
R^2	0.822	0.851	0.951

Panel B - Dependent Variable: *Bias*			
	(1)	(2)	(3)
Ln Local Victims	0.0364	0.0508*	0.0549**
	(1.19)	(1.85)	(2.14)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	20,085	20,085	20,085
R^2	0.709	0.764	0.933

Panel C - Dependent Variable: *Bias_alt*			
	(1)	(2)	(3)
Ln Local Victims	0.0274	0.0593*	0.0913***
	(0.85)	(1.92)	(3.01)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	19,729	19,729	19,729
R^2	0.953	0.963	0.990

Table 6: Upward and downward forecast boldness

The table presents additional analyses on the relationship between local outbreaks and forecast boldness. In both Panels, the dependent variable is *Boldness* and the main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Panel A includes forecasts with *Bias* >= 0, while Panel B includes forecasts with *Bias* < 0. In both Panels, Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Upward Boldness*			
	(1)	(2)	(3)
Ln Local Victims	0.0034	0.0027	0.0042
	(0.96)	(0.74)	(0.91)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	13,934	13,934	13,934
R^2	0.895	0.917	0.977

Panel B - Dependent Variable: *Downward Boldness*			
	(1)	(2)	(3)
Ln Local Victims	-0.0180***	-0.0173***	-0.0145**
	(-3.13)	(-2.69)	(-2.29)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	15,050	15,050	15,050
R^2	0.870	0.896	0.970

Table 7: Local outbreaks and the processing of earnings news
This table analyzes the relationship between local outbreaks and analyst activity after earnings announcements. In all specifications, the dependent variable is an indicator equal to one if the analyst has issued a new forecast within [0,2] days of the earnings news (and zero otherwise), and the main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	Dep. variable: $MakeFcst$		
	(1)	(2)	(3)
Ln Local Victims	0.0034	0.0025	0.0019
	(0.62)	(0.85)	(0.72)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	45,054	45,054	45,054
R^2	0.380	0.602	0.765

Table 8: Recent activity and its interaction with local outbreaks

This table analyzes whether the relationship between local outbreaks and forecast boldness is modulated by recent activity. In Columns (1) and (2), the dependent variable is $MakeFcst$. In Columns (3) and (4), the dependent variable is $Boldness$. The regressors are the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement; an indicator that the analyst has updated the forecast in the 21 trading days leading to the earnings announcements; and the interaction term. Columns (1) and (3) include earnings announcement fixed effects, Columns (2) and (4) include earnings announcement and analyst fixed effects, Columns (3) and (6) include earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	$MakeFcst$			$Boldness$		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln Local Victims				-0.0129**	-0.0133***	-0.0095**
				(-2.02)	(-2.76)	(-2.61)
Recent	0.0609***	0.0121**	0.0013	-0.0169*	-0.0132	0.0013
	(3.69)	(2.65)	(0.27)	(-1.70)	(-1.54)	(0.08)
Ln Local Victims * Recent				0.0016	0.0007	-0.0028
				(0.87)	(0.29)	(-0.80)
Earnings Announcement FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	Yes	No	No	Yes	No
Analyst-Firm FE	No	No	Yes	No	No	Yes
Observations	45,054	45,054	45,054	28,984	28,984	28,984
R^2	0.382	0.602	0.765	0.824	0.849	0.932

Table 9: Relative importance and its interaction with local outbreaks

This table analyzes whether the relationship between local outbreaks and forecast boldness is modulated by a firm's relative importance. In Columns (1) and (2), the dependent variable is $MakeFcst$. In Columns (3) and (4), the dependent variable is $Boldness$. The regressors are the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement; a continuous measure of the relative importance of firm j for analyst i; and the interaction term. Columns (1) and (3) include earnings announcement fixed effects, Columns (2) and (4) include earnings announcement and analyst fixed effects. Analyst-firm fixed effects are not included as there is no variation in *Relative importance* within an analyst-firm. Only analysts who cover at least four firms in the sample are included. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	$MakeFcst$		$Boldness$	
	(1)	(2)	(3)	(4)
Ln Local Victims			-0.0121*	-0.0117*
			(-1.82)	(-1.87)
Relative importance	0.1800***	0.0335**	-0.0179	0.0430
	(8.26)	(2.05)	(-0.65)	(1.09)
Ln Local Victims * Relative importance			-0.0055	-0.0031
			(-1.26)	(-0.77)
Earnings Announcement FE	Yes	Yes	Yes	Yes
Analyst FE	No	Yes	No	Yes
Observations	37,122	37,122	24,330	24,330
R^2	0.401	0.600	0.835	0.853

Internet Appendix for: "Panic Herding: Analysts' COVID-19 Experiences and the Interpretation of Earnings News"

A Additional results

Figure A1: Geographical distribution of local COVID-19 victims over time (US only)
This figure shows the distribution of local COVID-19 victims across different US states.

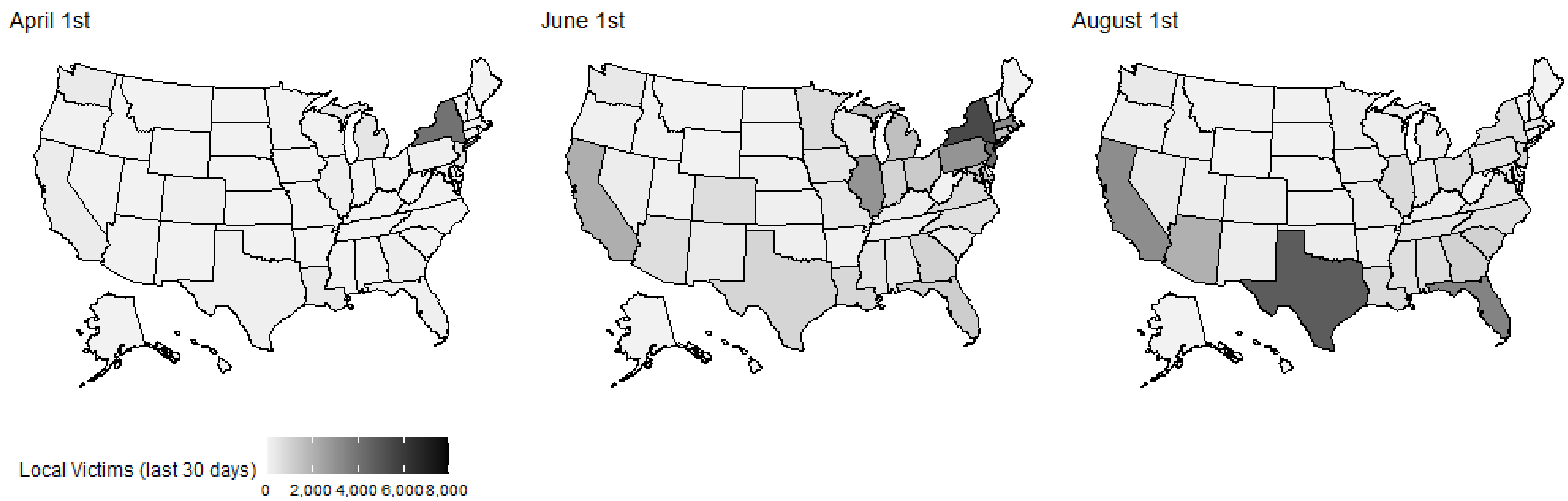

Figure A2: Geographical distribution of analyst-announcement pairs (US only)
This figure shows the distribution of analyst-announcement pairs across different US states. States with no observations are in white.

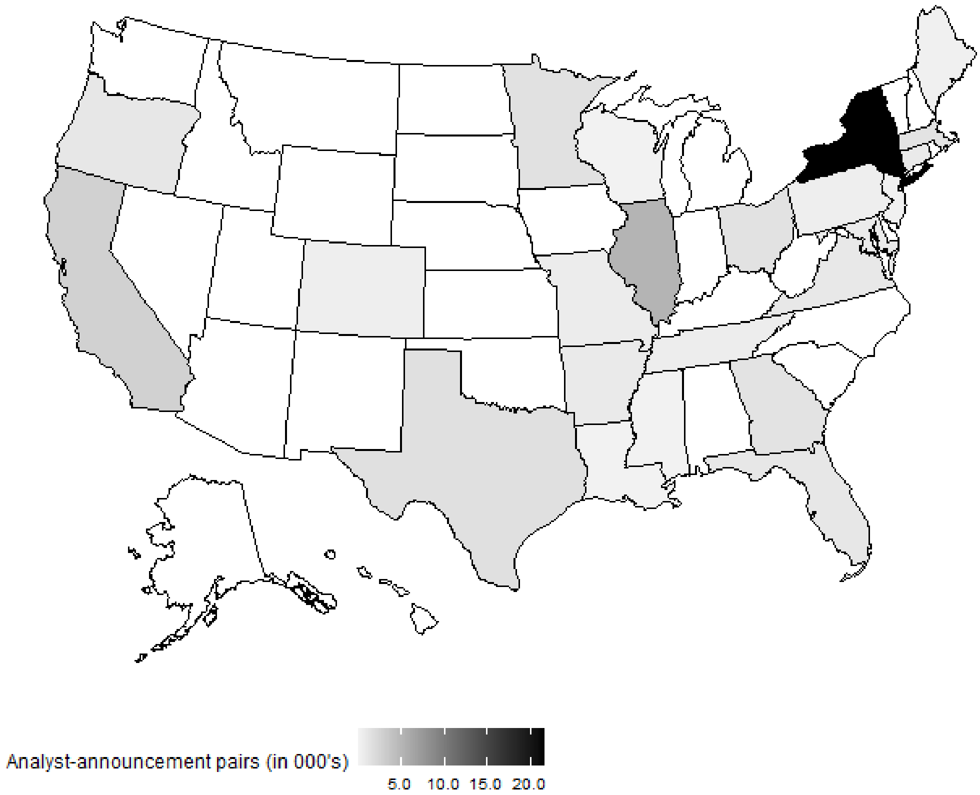

Figure A3: The development of local mobility
This figure shows the average, 10th and 90th percentiles of *RetailIndex*, by earnings announcement date (for earnings announcements held between March and mid-August 2020).

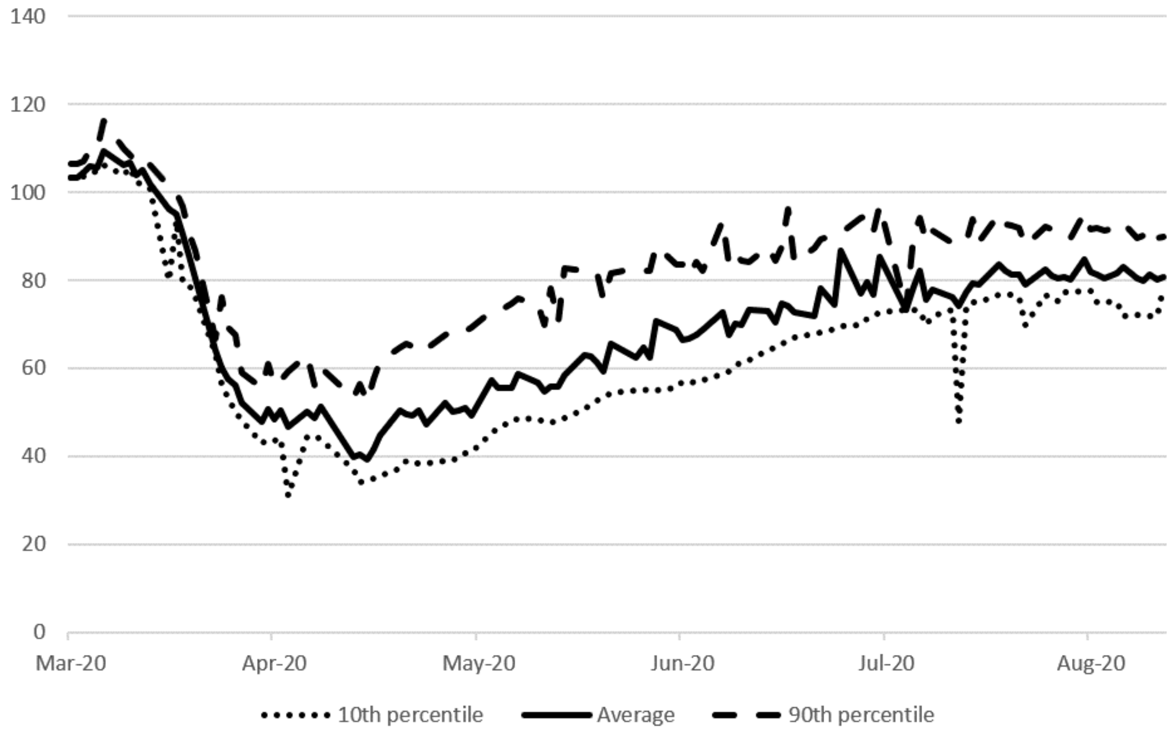

Table A1: Monthly averages of selected variables

This table presents the averages of some of the variables used in the empirical analysis, by calendar month. The full sample includes forecasts made following earnings announcements by US-listed firms between January 1 and August 15, 2020. Variables related to local mobility and to local outbreaks are measured on the day before the earnings news. Variable definitions are reported in Appendix B.

Month	N	Boldness	Upward Boldness	Downward Boldness	Bias	Bias_alt	Recent	MakeFcst
January	3,842	0.101	0.091	0.112	-0.084	1.944	0.278	0.684
February	9,102	0.318	0.249	0.363	-0.408	2.124	0.214	0.663
March	2,912	0.424	0.273	0.509	-1.879	5.506	0.164	0.587
April	5,660	0.380	0.284	0.423	-1.317	-1.471	0.518	0.712
May	9,084	0.590	0.335	0.755	-1.207	0.978	0.345	0.625
June	980	0.641	0.346	0.992	-2.466	3.596	0.185	0.310
July	6,678	0.397	0.355	0.515	0.303	-0.870	0.336	0.652
August	6,796	0.530	0.475	0.629	0.169	1.903	0.221	0.630

Table A2: Local outbreaks and forecast boldness - $Victims\_rate$ as regressor

The table presents estimates from OLS regressions of *Boldness* on the the number of monthly COVID-19 victims in the analyst's region per one million inhabitants, measured on the day before the earnings announcement. Reported coefficients are multiplied by 1,000. Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	Dependent variable: *Boldness*		
	(1)	(2)	(3)
Victims_rate	-0.0482***	-0.0412**	-0.0382**
	(-2.99)	(-2.16)	(-2.41)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,984	28,984	28,984
R^2	0.824	0.849	0.932

Table A3: FOMC announcements

The table presents estimates from OLS regressions of *Boldness* on *Ln Local Victims*, *FOMC*, and their interaction. *FOMC* is an indicator variable equal to one if there was a recent meeting of the Federal Open Market Committee, and zero otherwise. Panel A covers the full sample, whereas Panel B covers only the early pandemic period (until April 2020, included). Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Full sample			
	(1)	(2)	(3)
Ln Local Victims	-0.0125*	-0.0131**	-0.0110**
	(-1.90)	(-2.56)	(-2.66)
FOMC	-0.0024	-0.0001	-0.0083
	(-0.12)	(-0.00)	(-0.49)
Ln Local Victims * FOMC	0.0016	0.0013	-0.0007
	(0.50)	(0.41)	(-0.27)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,984	28,984	28,984
R^2	0.824	0.849	0.932

Panel B - Early pandemic			
	(1)	(2)	(3)
Ln Local Victims	-0.0151**	-0.0165**	-0.0187**
	(-2.38)	(-2.44)	(-2.50)
FOMC	-0.0255	-0.0496***	-0.0860***
	(-1.06)	(-3.09)	(-2.76)
Ln Local Victims * FOMC	0.0043	0.0066*	0.0080
	(1.04)	(1.97)	(1.39)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	14,383	14,383	14,383
R^2	0.810	0.846	0.968

Table A4: Broader sample

The table presents estimates from OLS regressions of *Boldness* and *Bias* on *Ln Local Victims*, *EA*, and their interaction. *EA* is an indicator variable equal to one if there was a recent earnings announcement, and zero otherwise. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the forecast date and at the analyst region level.

Panel A - Dependent Variable: *Boldness*			
	(1)	(2)	(3)
Ln Local Victims	-0.0085**	-0.0053	-0.0059
	(-2.05)	(-1.05)	(-1.20)
EA	-0.0014	0.0049	0.0020
	(-0.11)	(0.36)	(0.12)
Ln Local Victims * EA	-0.0040*	-0.0062***	-0.0050**
	(-1.75)	(-2.95)	(-2.20)
Day FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	60,796	60,796	60,796
R^2	0.318	0.353	0.501

Panel B - Dependent Variable: *Bias*			
	(1)	(2)	(3)
Ln Local Victims	0.0475	0.0437	0.0505
	(1.50)	(1.20)	(1.29)
EA	0.0890	0.0431	0.0454
	(0.93)	(0.54)	(0.49)
Ln Local Victims * EA	0.0118	0.0209*	0.0242**
	(1.05)	(1.88)	(2.26)
Day FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	60,882	60,882	60,882
R^2	0.223	0.302	0.540

Table A5: Placebo tests using forecasts on the day before the announcements

The table presents estimates from OLS regressions of *BoldnessPlacebo*, *BiasPlacebo*, and *Bias_altPlacebo* on the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. In both Panels, Column (1) includes earnings announcement fixed effects, Column (2) includes earnings announcement and analyst fixed effects, Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *BoldnessPlacebo*			
	(1)	(2)	(3)
Ln Local Victims	-0.0046	0.0016	0.0023
	(-0.99)	(0.29)	(0.46)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	44,192	44,192	44,192
R^2	0.639	0.677	0.824

Panel B - Dependent Variable: *BiasPlacebo*			
	(1)	(2)	(3)
Ln Local Victims	-0.0006	-0.0041	-0.0230
	(-0.02)	(-0.19)	(-1.16)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	44,254	44,254	44,254
R^2	0.138	0.281	0.662

Panel C - Dependent Variable: *Bias_altPlacebo*			
	(1)	(2)	(3)
Ln Local Victims	0.2095	0.2838	0.2364
	(1.54)	(1.30)	(1.00)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	43,461	43,461	43,461
R^2	0.982	0.984	0.989

Table A6: Local outbreaks and forecast boldness - Additional analyses

The table presents additional analyses on the relationship between local outbreaks and forecast boldness. In all specifications, the dependent variable is *Boldness* and the main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. Column (1) includes earnings announcement fixed effects and controls for the natural logarithm of the local population. This is due to my estimate for the local population being time-invariant. All other specifications include both earnings announcement and analyst fixed effects. Columns (2) and (3) exclude analysts based in the state of New York and outside of the US, respectively. Column (4) excludes observations for firms covered by less than 10 analysts. Column (5) excludes observations for announcements with $Price_{j,d-1} < \$3$. Finally, Column (6) excludes observations for announcements with $|Consensus_{j,d-1}| < \$0.10$. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

	Dependent variable: *Boldness*					
	Control for population	Exclude NY	Only US-based analysts	Only firms with 10+ analysts	Stock price above \$3	|Consensus| above \$0.10
	(1)	(2)	(3)	(4)	(5)	(6)
Ln Local Victims	-0.0098*	-0.0181*	-0.0112**	-0.0134**	-0.0104**	-0.0103**
	(-1.87)	(-1.78)	(-2.36)	(-2.20)	(-2.10)	(-2.23)
Ln Population	-0.0089					
	(-1.03)					
Earnings Announcement FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	Yes	Yes	Yes	Yes	Yes
Observations	28,984	15,206	27,775	18,420	27,907	28,111
R^2	0.824	0.884	0.854	0.826	0.848	0.797

Table A7: Alternative definitions of dependent variables - Recent consensus

The table examines the relationship between local outbreaks and *Boldness_recent* (Panel A) or *Bias_recent* (Panel B). The main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. In both Panels, Column (1) includes earnings announcement fixed effects; Column (2) includes earnings announcement and analyst fixed effects; Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Boldness_recent*			
	(1)	(2)	(3)
Ln Local Victims	-0.0131*	-0.0114**	-0.0102**
	(-1.97)	(-2.20)	(-2.40)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	24,390	24,390	24,390
R^2	0.820	0.845	0.927

Panel B - Dependent Variable: *Bias_recent*			
	(1)	(2)	(3)
Ln Local Victims	0.0540	0.0559	0.0617*
	(1.59)	(1.68)	(1.83)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	24,404	24,404	24,404
R^2	0.674	0.732	0.900

Table A8: Alternative definitions of dependent variables - Non-fixed reference date

The table examines the relationship between local outbreaks and *Boldness_alt* (Panel A) or *Bias_alt* (Panel B). The main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. In both Panels, Column (1) includes earnings announcement fixed effects; Column (2) includes earnings announcement and analyst fixed effects; Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Boldness_v2*			
	(1)	(2)	(3)
Ln Local Victims	-0.0150**	-0.0172***	-0.0176***
	(-2.54)	(-3.53)	(-4.12)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,981	28,981	28,981
R^2	0.753	0.784	0.895

Panel B - Dependent Variable: *Bias_v2*			
	(1)	(2)	(3)
Ln Local Victims	0.0382	0.0384	0.0482
	(1.13)	(1.16)	(1.61)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	29,022	29,022	29,022
R^2	0.597	0.661	0.864

Table A9: Alternative definitions of dependent variables - Other measures

The table examines the relationship between local outbreaks and *Boldness_unit* (Panel A), *BiasStdDev* (Panel B), or *Bias_altStdDev* (Panel C). The main regressor is the natural logarithm of (one plus) the monthly COVID-19 victims in the analyst's region, measured on the day before the earnings announcement. In both Panels, Column (1) includes earnings announcement fixed effects; Column (2) includes earnings announcement and analyst fixed effects; Column (3) includes earnings announcement and analyst-firm fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Boldness_unit*			
	(1)	(2)	(3)
Ln Local Victims	-0.0062**	-0.0067**	-0.0074***
	(-2.34)	(-2.62)	(-2.74)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,030	28,030	28,030
R^2	0.094	0.180	0.584

Panel B - Dependent Variable: *BiasStdDev*			
	(1)	(2)	(3)
Ln Local Victims	2.1318*	0.4218	0.3580
	(1.90)	(0.42)	(0.35)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	29,017	29,017	29,017
R^2	0.864	0.881	0.952

Panel C - Dependent Variable: *Bias_altStdDev*			
	(1)	(2)	(3)
Ln Local Victims	2.1599*	0.3626	0.3391
	(1.92)	(0.36)	(0.32)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	No	Yes	No
Analyst-Firm FE	No	No	Yes
Observations	28,551	28,551	28,551
R^2	0.998	0.999	0.999

Table A10: Results using alternative measures of COVID-19 outbreaks

The table presents estimates from OLS regressions of either *Boldness*, *Bias*, or *Bias_alt* on alternative measures of local COVID-19 outbreaks and mobility, measured on the day before the earnings announcement. All specifications include earnings announcement and analyst fixed effects. Heteroskedasticity robust t-statistics are in parentheses and are two-way clustered at the announcement date and at the analyst region level.

Panel A - Dependent Variable: *Boldness*			
	(1)	(2)	(3)
Ln Local Cases	-0.0059		
	(-1.40)		
Ln WorkIndex		0.2346**	
		(2.25)	
Ln RetailIndex			0.1303**
			(2.24)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes
Observations	28,984	21,948	21,948
R^2	0.849	0.852	0.852

Panel B - Dependent Variable: *Bias*			
	(1)	(2)	(3)
Ln Local Cases	0.0213		
	(0.71)		
Ln WorkIndex		0.1937	
		(0.38)	
Ln RetailIndex			0.0498
			(0.19)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes
Observations	29,022	21,967	21,967
R^2	0.744	0.755	0.755

Panel C - Dependent Variable: *Bias_alt*			
	(1)	(2)	(3)
Ln Local Cases	0.0268		
	(0.83)		
Ln WorkIndex		0.0437	
		(0.08)	
Ln RetailIndex			0.0508
			(0.19)
Earnings Announcement FE	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes
Observations	28,556	21,614	21,614
R^2	0.958	0.959	0.959

B Variable definitions

Variables related to analyst forecasts

Boldness: The analyst's year-end EPS forecast on day $d + 2$, less the consensus forecast value as of the day prior to the earnings announcement, scaled by the consensus forecast value as of the day prior to the earnings announcement, in absolute value. Consensus forecast is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement, retaining only the most recent forecast per analyst. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if a forecast is made within days [0, 2] of firm j 's quarterly earnings announcement and the consensus forecast is different from zero. Larger values indicate larger deviations from the consensus.

Downward Boldness: *Boldness*, if $Bias < 0$ (missing value otherwise).

Upward Boldness: *Boldness*, if $Bias >= 0$ (missing value otherwise).

Bias: The analyst's year-end EPS forecast on day $d + 2$, less the consensus forecast value as of the day prior to the earnings announcement, scaled by the stock price as of the day prior to the earnings announcement, multiplied by 100. Consensus forecast is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement, retaining only the most recent forecast per analyst. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if a forecast is made within days [0, 2] of firm j's quarterly earnings announcement. A positive (negative) value indicates forecast optimism (pessimism).

Bias_ alt: The analyst's year-end EPS forecast on day $d+2$, less the consensus forecast value as of the day prior to the earnings announcement, scaled by the stock price as of the day prior to the earnings announcement, multiplied by 100. Consensus forecast is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement, retaining only the most recent forecast per analyst. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if a forecast is made within days [0, 2] of firm j's quarterly earnings announcement. A positive (negative) value indicates forecast optimism (pessimism).

MakeFcst: Indicator variable equal to one if the analyst issues an annual EPS forecast within trading days [0, 2] of the earnings announcement, and zero otherwise.

Recent: Indicator variable equal to one if the analyst has issued an annual EPS forecast in the 30 calendar days prior to the earnings announcement, and zero otherwise.

Relative importance: One minus a firm's relative rank in an analyst's portfolio (scaled by the total number of firms within the portfolio), based on its market capitalization as of the end of 2019. The variable is defined only for analysts who cover four or more firms in the sample. It takes on values between [0,1), with higher values indicating larger firms within an analyst's portfolio.

BoldnessPlacebo: The analyst's year-end EPS forecast on day $d-1$, less the consensus forecast value as of the day prior to the earnings announcement, scaled by the consensus forecast value as of the day prior to the earnings announcement, in absolute value. Consensus forecast is calculated as the median of all outstanding EPS forecasts, retaining only the most recent forecast per analyst. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if there is an outstanding forecast on the day before firm j's quarterly earnings announcement and the denominator is different from zero.

BiasPlacebo: The analyst's year-end EPS forecast on day $d-1$, less the consensus forecast value as of the day prior to the earnings announcement, scaled by the stock price as of the day prior to the earnings announcement, multiplied by 100. Consensus forecast is calculated as the median of all outstanding EPS forecasts, retaining only the most recent forecast per analyst. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if there is an outstanding forecast on the day before firm j's quarterly earnings announcement.

Bias_ altPlacebo: The analyst's year-end EPS forecast on day $d-1$, less the actual FY2020 EPS, scaled by the stock price as of the day prior to the earnings announcement, multiplied by 100. The variable is winsorized at the top and bottom 1% of observations. The variable is defined only if a forecast is made within days [0, 2] of firm j's quarterly earnings announcement.

Boldness_ recent: Defined similar to *Boldness*, but consensus is computed using only earnings forecasts issued in the month before the earnings announcement.

Bias_ recent: Defined similar to *Bias*, but consensus is computed using only earnings forecasts issued in the month before the earnings announcement.

Boldness_ v2: Defined similar to *Boldness*, but consensus is computed as of the day before the forecast is issued.

Bias_ v2: Defined similar to *Bias*, but consensus is computed as of the day before the forecast is issued.

Boldness_ unit: For analyst i following firm j and issuing a forecast within [0,2] trading days of the earnings announcement held on day d, *Boldness_unit* is computed as the absolute distance between the forecast and consensus forecast on $d-1$ minus the minimum absolute distance for all analysts issuing a forecast, with this difference scaled by the range in absolute distances for all analysts who issue a forecast. Consensus forecast is calculated as the median value of the latest forecasts issued by analysts following firm j at the trading day before the announcement, retaining only the most recent forecast per analyst. The variable is defined only if a forecast is made within days [0, 2] of firm j 's quarterly earnings announcement and the denominator is different from zero. Its values range between 0 and 1, with 1 indicating that the forecast is the boldest, and 0 indicating that the forecast is the least bold among all analysts issuing a forecast after the firm's earnings announcement.

BiasStdDev: Defined similar to *Bias*, but scaled by the standard deviation of all outstanding EPS forecasts reported on I/B/E/S as of the day prior to the earnings announcement (retaining only the most recent forecast per analyst). A positive (negative) value indicates forecast optimism (pessimism).

Bias_ altStdDev: Defined similar to *Bias_alt*, but scaled by the standard deviation of all outstanding EPS forecasts reported on I/B/E/S as of the day prior to the earnings announcement (retaining only the most recent forecast per analyst). A positive (negative) value indicates positive (negative) forecast bias.

Variables related to COVID-19, local mobility, and other variables

Ln Local Victims: The natural logarithm of the COVID-19 deaths reported by the authorities in the 30 calendar days before the earnings announcement in the region in which the analyst is located, plus one. If only the number of cases is available at the regional level, I infer the number of victims in a region from the number of cases in the region and the country's mortality rate on that day.

Ln Local Cases: The natural logarithm of the COVID-19 cases reported by the authorities in the 30 calendar days before the earnings announcement in the region in which the analyst is located, plus one. If only the number of victims is available at the regional level, I infer the number of cases in a region from the number of victims in the region and the country's mortality rate on that day.

RetailIndex: The average of the change versus a baseline in the mobility trends for places like restaurants, cafes, shopping centers, theme parks, museums, libraries, and movie theaters reported in Google's COVID-19 Community Mobility Reports in the 7 calendar days before the earnings announcement in the region in which the analyst is located, scaled so that a value of 100 represents the baseline. The baseline is the median value, for the corresponding day of the week, during the 5-week period between January 3 and February 6, 2020. Data starts in mid-February.

Ln RetailIndex: The natural logarithm of $RetailIndex$.

WorkIndex: The average of the change versus a baseline in the mobility trends for places of work reported in Google's COVID-19 Community Mobility Reports in the 7 calendar days before the earnings announcement in the region in which the analyst is located, scaled so that a value of 100 represents the baseline. The baseline is the median value, for the corresponding day of the week, during the 5-week period between January 3 and February 6, 2020. Data starts in mid-February.

Ln WorkIndex: The natural logarithm of $WorkIndex$.

Ln Population: The natural logarithm of the estimated population in the region in which the analyst is located.

Victims_ rate: The number of COVID-19 deaths reported by the authorities in the 30 calendar days before the earnings announcement in the region in which the analyst is located, scaled by the local population (in million of inhabitants). If only the number of cases is available at the regional level, I infer the number of victims in a region from the number of cases in the region and the country's mortality rate on that day.

FOMC: An indicator variable equal to one if an earnings forecast was issued in the day immediately after an announcement by the Federal Open Market Committee, and zero otherwise.

EA: An indicator variable equal to one if there was a recent earnings announcement (2-day window), and zero otherwise.

C COVID-19 data sources by geographic segment

Geographic segment	Main data source(s)	Primary URL (last accessed in August 2020)
Argentina	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Argentina_medical_cases
Australia	JHU CSSE COVID-19 Data	https://github.com/CSSEGISandData/COVID-19
Belgium	Sciensano	https://covid-19.sciensano.be/fr/covid-19-situation-epidemiologique
Brazil	Ministry of Health	https://covid.saude.gov.br/
Canada	JHU CSSE COVID-19 Data	https://github.com/CSSEGISandData/COVID-19
Chile	Ministry of Health	https://www.gob.cl/coronavirus/cifrasoficiales/
China	JHU CSSE COVID-19 Data	https://github.com/CSSEGISandData/COVID-19
Colombia	Instituto Nacional de Salud	https://www.ins.gov.co
France	Santé Publique France (see also Regional Health Agencies and Prefectures)	https://www.santepubliquefrance.fr/
Germany	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Germany_medical_cases

India	Ministry of Health and Family Welfare	https://www.mohfw.gov.in/
Ireland	Health Protection Surveillance Centre	https://www.hpsc.ie
Italy	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Italy_medical_cases
Mexico	Gobierno de Mexico	https://coronavirus.gob.mx/datos/
Netherlands	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Netherlands_medical_cases
Norway	Norwegian Institute of Public Health	https://www.fhi.no/en/id/infectious-diseases/coronavirus/daily-reports/daily-reports-COVID19/
Russia	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Russia_medical_cases
South Africa	National Institute for Communicable Diseases	https://www.nicd.ac.za/media/alerts/
South Korea	Local authorities (accessed through Wikipedia)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/South_Korea_medical_cases

Sweden	Local authorities (accessed through Wikipedia, see also www.platz.se)	https://en.wikipedia.org/wiki/Template:COVID-19_pandemic_data/Sweden_medical_cases
UK	UK Government	https://coronavirus.data.gov.uk/
USA	JHU CSSE COVID-19 Data	https://github.com/CSSEGISandData/COVID-19