What Drives Beliefs about Climate Risks? Evidence from Financial Analysts

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Abstract

This paper examines how exposure to weather events affects earnings forecasts of equity analysts. It uses a unique dataset that matches natural disasters with the location of analysts across the US over 2000-2020. I find that analysts' earnings forecasts become more accurate after experiencing an extreme weather event. The post-exposure effect on forecasting accuracy is long-lasting (up to 1.5 years) and is more pronounced for: (i) firms with high climate risks; (ii) when there is a higher degree of asymmetric information; and (iii) for experienced analysts. These results indicate that the observed enhancement in forecast accuracy is driven by information acquisition rather than behavioral biases and support the hypothesis that weather events prompt analysts to rationally acquire and incorporate more information into their forecasts. This information acquisition process does not spill over to analysts distant from the event or to investors trading the forecasted firms. However, I observe that brokerage firms capitalize on this increased analyst accuracy by hiring more analysts with expertise in stocks which are particularly sensitive to climate risks.

Keywords: Belief Formation, Climate Risks, Physical Risks, Analysts Forecasts.

JEL Codes: G1, G2, G3, Q5, Q54

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1 Introduction

Natural disasters in the U.S. have caused approximately $2.8 trillion in damages over 1980-2023, and this number is expected to rise as global warming intensifies (NOAA, 2023).[1] Studies show that personal experience with extreme weather leads to greater climate awareness, influencing various decisions. For example, support for environmental policies increases after such events (Hoffmann et al., 2022), investors reduce their holdings of high-emission stocks (Choi et al., 2020), and households shift to greener investments (Anderson and Robinson, 2019). However, the underlying mechanisms driving these changes in behavior remain largely unexplored.

This paper aims to uncover these mechanisms by studying how equity analysts adjust their earnings forecasts after experiencing weather events. Equity analysts are key information providers (Mikhail et al., 2007), regularly issuing detailed and timely earnings forecasts. My identification strategy relies on a staggered difference-in-differences approach, comparing the earnings forecasts made by analysts exposed to the event with those made by analysts not exposed. These forecasts are specifically for firms that were not affected by the event. I study unaffected companies because any changes in forecasts are more likely to reflect shifts in analysts' climate beliefs—expectations about the impact of climate change on firm performance—rather than efforts to estimate the event's direct costs.

I propose two mechanisms for why analysts might adjust their earnings forecasts following weather events. The *information hypothesis* suggests that experiencing such events prompts agents to rationally seek out and incorporate more information about climate change, thereby improving their ability to assess climate risks. These risks include physical risks, such as natural disasters, and transition risks associated with carbon reduction policies. In contrast, the *behavioral hypothesis* posits that extreme weather events may have an emotional impact, leading analysts to overestimate climate risks without any clear and long-term effect on their forecasting abilities.[2]

[1]This estimate is a lower bound, as it does not fully capture indirect economic losses. For instance, higher temperatures increase workplace injuries (Park et al., 2021), reduce firm sales, and raise operating costs (Hugon and Law, 2019). When weather shocks disrupt supply chains, companies experience a decrease in operating performance (Pankratz and Schiller, 2021) and lose sales (Barrot and Sauvagnat, 2016 and Custodio et al., 2021). Firms in high-risk areas for weather events face more volatile earnings and cash flows (Huang et al., 2018). For this reason, both scholars and practitioners regard physical risks as the most significant source of long-term climate risk (Stroebel and Wurgler, 2021).

[2]Determining which mechanism predominates is critical for informing public policy (Deryugina, 2013). Policies like the SEC's climate disclosure rule (March 2024) in the US and the EU Taxonomy, which require companies to disclose

I find that exposure to extreme weather events leads to more accurate earnings forecasts, consistent with the information hypothesis. The effect is more pronounced for firms facing high physical climate risk and in settings with greater asymmetric information. This supports the idea that exposure to weather events prompts a reassessment of climate-related beliefs. Moreover, while brokerage firms seem to capitalize on analysts' increased accuracy by hiring more analysts, this effect appears limited to those with firsthand experience of the events, with no spillover to other analysts.

My results contribute to the growing literature on how people form and update beliefs about climate risks (e.g., Deryugina, 2013; Krueger et al., 2020; Ceccarelli and Ramelli, 2024; Bauer et al., 2024) by showing how experiencing weather events can raise climate awareness. They also enhance our understanding of the impact of climate events on financial markets (e.g., Choi et al., 2020; Anderson and Robinson, 2019; Alekseev et al., 2021) by providing insights from information providers. Finally, this study builds on the literature regarding analysts' responses to climate events (e.g., Cuculiza et al., 2021; Addoum et al., 2020; Bourveau and Law, 2021) by examining the underlying mechanisms and their spillover effects on other analysts and brokerage firms.

To study the effect of exposure to weather events, I build a comprehensive dataset that matches the working location of equity analysts with the occurrence of natural hazards across 24 US states over 20 years. The selected events are natural disasters that resulted in at least 100 injuries, 10 fatalities, or $1 billion in economic damages. I document that these events serve as salient shocks that influence individuals' beliefs. Specifically, the affected states show an increase in Google searches related to climate change (as in Alekseev et al., 2021) and greater concern about climate change among their population, as measured by the Yale Climate Opinion Data.

I then split the sample of analysts into two groups: analysts located within 100 miles from the weather event (treated analysts) and analysts located farther than 100 miles from the weather event (control analysts), both forecasting firms that are more than 100 miles distant from the weather event. The control group includes both analysts who have never been exposed to climate events (never treated) and analysts who in a given period have not yet been exposed to a weather shock but who will be exposed in the future (yet-to-be-treated).[3] My sample includes 1,231 analysts located

climate-related information, may be less effective if emotional reactions or cognitive biases primarily drive responses to this information disclosure.

[3] My setting implies that never-treated analysts have never experienced a weather shock since they start working as analysts. Unfortunately, I lack information on the analysts' locations before they enter the sample.

in 24 states and covering 2,770 firms. About 40% of the analysts included in the sample have been exposed to an extreme weather event and are classified as first-time treated.

To account for the multiple events occurring across different locations, I use a staggered difference-in-differences estimation. My variables of interest are forecast bias and forecast error. Following Hong and Kacperczyk (2010), I define forecast bias as the difference between actual and forecasted earnings per share, scaled by the stock price from the previous period, and forecast error as the absolute value of this bias. While forecast bias reflects analysts' relative optimism or pessimism, forecast error measures the overall accuracy of their forecasts.

The underlying assumption is that forecasted earnings, besides capturing perceptions of firms' future financial performance based on all available public information, also incorporate analysts' non-observable beliefs, including beliefs about climate risks. Given that I focus on weather shocks that do not directly impact firms' earnings—occurring more than 100 miles from the firm's location and without any accompanying changes in fundamentals— it is plausible that changes in analysts' forecasts in response to these shocks are driven by shifts in their beliefs rather than actual firm performance.

My baseline results show that exposure to weather shocks improves analysts' forecasts in terms of both forecast bias and error. The point estimates indicate a statistically significant reduction in forecast error of 0.07 percentage points (4% of the average forecast error) and 0.06 percentage point reduction in the forecast bias (8% of the average forecast bias; however, the reduction in forecast bias is not statistically significant at conventional confidence levels).

To discriminate between the *behavioral* and *information* hypothesis, I match detailed information about firms' climate risk profiles sourced from Trucost with analysts' exposure to specific climate events. Consistent with the information hypothesis, I find that exposure to heatwaves increases accuracy for firms that are subject to that heatwave risk.

To probe further, I show that analysts improve their forecast accuracy, especially when forecasting firms with high information asymmetry. Specifically, they become more accurate when forecasting firms with fewer analysts covering them, no earnings calls in the previous year, no disclosure of climate risks through the Carbon Disclosure Program, or in sectors less exposed to climatic risks. In line with the information hypothesis, they also become more accurate when working in areas less affected by these large weather events.

As a final step in discriminating between the behavioral and the information channels, I study the persistence of the exposure effect documented above. The underlying assumption of this test is that a behavioral response should be short-lived, whereas a shock that affects an analyst's ability to process information related to climate risk should have a long-lasting effect. The fact that I find that exposure leads to more accurate forecasts for a period of up to 18 months provides further support for the information channel.[4]

To provide a more granular understanding of the *information hypothesis*, I show that my findings align with the 'rational attention allocation' theory proposed by Kacperczyk et al. (2016). When weather events occur, the potential payoff from focusing analysts' attention on the impact of climate-related events on earnings forecasts increases, leading to the observed improvement in forecasting ability. Consistent with this theory, I find a significant increase in accuracy among skilled analysts—defined as those with more industry experience, top performers with smaller forecast errors in their sector, and lead analysts known for the timeliness of their forecasts—who are better at acquiring and processing information.

Moreover, analysts at larger brokerage firms show greater improvements in accuracy, suggesting that these events help mitigate conflicts of interest (Michaely and Womack, 1999). A heightened effort is also evident, as analysts particularly improve their forecasts for high market capitalization firms (Glode, 2011). However, the increase in accuracy seems specific to first-time exposure. Analysts become less accurate with subsequent shocks, suggesting they may overestimate their forecasting abilities after experiencing such events (Anagol et al., 2021).

I also document that alternative theories do not fully explain my findings. I rule out the possibility that weather events act solely as attention shocks, as both attentive and inattentive analysts exhibit improved accuracy (Baker et al., 2020). One concern is that treated analysts, being located near the event, may have less access to information about firms included in my sample (recall that I only consider firms distant to the event), as in Malloy (2005). However, I find no significant difference in outcomes between treated and control analysts at similar distances for a given firm.

To assess whether this response extends beyond earnings forecasts, I examine other aspects of

[4]The results of persistence estimations should be taken with caution because they implicitly assume that no additional information about climate risks is realized in the aftermath of the event. This assumption is less likely to hold when I extend the horizon of my analysis.

analysts' work. I find that analysts also become more accurate in forecasting stock prices, but they do not change their stock recommendations. While I focus on first-time experiences, examining analysts with at least one experience of a weather event shows that they are more likely to ask questions about physical risks during earnings calls and less likely to follow firms with high climate risks. Additionally, when a firm is affected by a weather event, these analysts demonstrate greater accuracy in their forecasts compared to those without such experience.

I also examine the responses of other financial players, including brokerage houses, distant analysts, and investors. I find that larger brokerage houses hire more analysts as the number of treated analysts increases, prompting these firms to cover more companies with higher climate risks. However, distant analysts, who also forecast companies covered by analysts exposed to the event, become more pessimistic, without showing any improvement in forecast accuracy. Similarly, investors do not appear to react to the more accurate forecast revisions made by analysts exposed to weather events. This suggests that while brokerage firms capitalize on the improved accuracy, this information acquisition does not spill over to other analysts and investors.

I conduct a battery of robustness checks and show that my results hold when I exclude analysts based in New York and California (where most analysts are located) and when excluding firms with plants near the events (my main analysis uses companies' headquarters locations). The results are also robust —in fact, they become stronger— if I only consider analysts located within a 50-mile radius of extreme weather events. I also conduct a placebo test by randomly generating shocks across the U.S. over the 20-year period of the study and find no significant effect on analysts' forecast bias or error. This suggests that the observed impact on analyst pessimism and accuracy is not driven by mean reversion.

The rest of the paper is organized as follows. Section 2 provides a review of the literature. Section 3 develops the conceptual framework and section 4 presents the methodology. Section 5 presents the data and the descriptive statistics. Section 6 discusses the results, and Section 7 concludes.

2 Related Literature

This study contributes to three streams of literature: 1) the formation and updating of beliefs about climate change, 2) the impact of weather events on financial markets, and 3) analysts' behavior, particularly in relation to climate-related information and events.

The first stream of literature focuses on how individuals form and update their beliefs about climate change. Deryugina (2013) demonstrates that local temperature fluctuations influence individuals' beliefs about global warming, affected by both rational and behavioral updates. In the financial context, investors believe that only risks related to climate regulation have started to materialize (Krueger et al., 2020) and positive information about climate transitions increase investment preferences for 'green' assets (Ceccarelli and Ramelli, 2024). Bauer et al. (2024) explore how financial experts' mental models of climate risk mispricing shape return expectations, emphasizing the role of cognitive biases and external events. This study extends this research by showing how subjective beliefs influence financial forecasts and market behavior, demonstrating that these beliefs can be elicited through means other than surveys, such as earnings forecasts.[5]

The second stream examines the impact of weather events on financial markets. Choi et al. (2020) find that retail investors are more likely to sell stocks of high-carbon-footprint firms after experiencing heatwaves. Anderson and Robinson (2019) show increased household investment in green funds following such events, while Alekseev et al. (2021) demonstrate that mutual fund managers alter their portfolio allocations across industries after extreme heat events.[6] Although prior studies don't fully disentangle whether the effects are due to changes in preferences or beliefs, this study shows how weather events affect the earnings forecasts of analysts, who play a key role in financial markets.

The third stream focuses on analysts' behavior, particularly in relation to climate shocks. Evidence on how climate events affect analysts' forecasts is mixed. Some studies show improved forecast accuracy and increased dispersion for firms with specific characteristics such as low market capitalization, low institutional ownership, less salience (Han et al., 2020), earnings sensitivity to

[5]Regarding ESG beliefs, which do not fully reflect climate beliefs, Giglio et al. (2024) document significant heterogeneity in beliefs about ESG returns, noting that these beliefs differ substantially from traditional variables used in investment decisions.

[6]Similarly, Huynh and Xia (2021) demonstrates that investors overreact to firms exposed to natural hazards, leading to a decline in the bond and stock prices of affected companies. Additionally, Alok et al. (2020) finds that managers near such events overreact and significantly underweight stocks in disaster zones compared to distant managers.

weather seasonality (Zhang, 2021), and location in countries with greater climate risk (Kim et al., 2021). Conversely, other studies find limited or no impact of extreme temperature events on earnings forecasts (Addoum et al., 2020, and Pankratz et al., 2023). A key difference between these studies and my work is that while they focus on firm-specific events, I concentrate on analysts' specific exposures.

A few prior papers examine how direct experiences of weather events affect analysts' forecasts. Addoum et al. (2020) report no significant effect on forecast revisions, while Bourveau and Law (2021) show increased forecast pessimism following hurricanes but do not address forecast accuracy or firms' climate risks. After experiencing wildfire smoke, Israelsen and Kong (2024) demonstrated that analysts become less accurate and ask more questions about climate risks during earnings calls. Consistent with this study's findings, Cuculiza et al. (2021) observed improved accuracy in forecasting firms with high climate risks after abnormal temperatures.[7] However, none of these studies test the specific hypotheses I address here, nor are they as comprehensive in demonstrating that analysts also rationally acquire information from experiencing climate-related events.

Overall, my findings align with prior research showing that increased access to information about climate risks enhances analyst accuracy. This has been observed when firms disclose climate risks in their annual reports (Wang et al., 2017), in firms with mandatory ESG disclosures (Krueger et al., 2021), following ESG-related incidents (Derrien et al., 2021), and among firms participating in the Carbon Disclosure Project (Chan, 2022).

3 Hypotheses Development

This study provides a methodology to extract climate beliefs by examining variations in earnings forecasts around weather events. Analysts' earnings forecasts can be defined as a function of their beliefs, including climate beliefs, and all available market data.[8] If the market data remains constant and firms are not affected by the weather event, any changes in analysts' forecasts can only be attributed to shifts in their beliefs. Unlike studies examining how changes in investments may be

[7] In a recent paper, Reggiani (2022) show that analysts revise down their forecast for high climate risks companies after weather events.

[8] Formally, analysts' forecasts can be represented as $(beliefs) * (market\ data)$, where analysts' *beliefs* include climate beliefs as well as beliefs about firms' fundamentals and the economy.

influenced by either preferences or beliefs—such as those involving mutual fund managers (Alekseev et al., 2021), households (Anderson and Robinson, 2019), and investors (Choi et al., 2020)—this analysis focuses solely on belief-driven changes.

I make two primary assumptions regarding how experiences of weather events influence changes in climate beliefs. First, first-hand experiences of salient natural hazards matter for changing beliefs (Malmendier and Nagel, 2011, and Anderson and Robinson, 2019). Second, weather shocks do not directly or indirectly impact the firms. Thus, forecasted firms in the sample must be at a significant distance from the weather event and their fundamentals remain constant around the event period.[9]

My null hypothesis is that weather events do not change climate beliefs, either because analysts' beliefs already correctly incorporate climate risks or because these events have no effect on their beliefs. While I cannot fully disentangle these two hypotheses, I focus on very large events, hoping they are salient enough to drive changes in beliefs that are reflected in analysts' earnings forecasts.

Why would analysts change their forecasts if the firms are unaffected by the event? My two hypotheses are the *information* and *behavioral* hypothesis. These mechanisms are not mutually exclusive and may operate in tandem.

3.1 Information Hypothesis

Under the information hypothesis, analysts gain valuable insights from experiencing a weather event. I propose two conceptual frameworks to explain the information channel.

First, according to the 'rational attention allocation' theory proposed by Kacperczyk et al. (2016), analysts must decide each period how much attention—whether in terms of time or cognitive resources—to allocate to specific information or stocks. When weather events occur, the potential payoff from focusing on the impact of climate-related events on earnings forecasts increases, leading to a shift in attention allocation that results in improved forecast accuracy.

The model's first prediction is selective attention: analysts are expected to enhance their skills by concentrating on higher-risk and more opaque firms, which increases accuracy and leads to more frequent forecasts for these companies. The second prediction posits that skilled analysts—identified

[9]Firms can be impacted indirectly by their suppliers or competitors. This is a second-order effect. In a perfectly competitive market, a climate shock to a supplier or competitor would be insignificant. In an imperfect market, controlling for industry-fixed effects or concentration indexes should mitigate the issue.

as experienced analysts, top performers, and lead analysts—are more effective at acquiring and processing information. The model anticipates a more pronounced improvement in accuracy among these skilled analysts, who possess an information advantage over their less skilled peers.

Second, under the 'attention model' of Baker et al. (2020), being near a weather event acts merely as an attention shock. Thus increasing uncertainty for all agents and causing inattentive forecasters to become more attentive and update outdated forecasts.[10] The primary prediction of this theory is that the effect on forecast accuracy will be more pronounced among analysts who were previously less attentive. In this case, the improvement in accuracy would largely stem from the general increase in attention rather than from any understanding of firm-specific climate risks. As a result, I would not expect to observe any heterogeneous effect across firms with varying climate risks.

The main predictions for the *information* hypothesis are as follows: 1) analysts' forecast accuracy improves following the weather event, 2) this effect is more pronounced in environments with higher information asymmetry, such as for more opaque firms or analysts with less prior information, and 3) the effect is persistent, leading to a long-lasting increase in accuracy.[11]

While the 'rational attention allocation' theory predicts an increase in accuracy for more experienced analysts and firms with high climate risks, the 'attention shock' hypothesis suggests an improvement primarily among previously inattentive analysts, with no significant changes for firms with high climate risks.

3.2 Behavioral Hypothesis

Under the behavioral hypothesis, changes in forecasts are driven by emotional reactions or cognitive biases, such as heuristics (Kahneman and Tversky, 1972) or mood effects (Dehaan et al., 2017).

Analysts' forecast changes may stem from the traumatic impact of the weather event, which could influence their risk-taking behavior (Bourveau and Law, 2021; Bernile et al., 2017). In this framework, analysts might become more pessimistic about all firms (*availability heuristic*)

[10] In this context, the shock does not alter the data-generating process. On the contrary, in the 'man-bites-dog' signal of Nimark (2014), analysts perceive greater uncertainty and treat the event as highly unusual, which could initially lead to less accurate forecasts due to increased uncertainty and greater dispersion.

[11] In the 'Other Mechanisms' section, I discuss alternative explanations the could potentially drive an increase in forecast accuracy.

or specifically about firms with higher levels of climate risk (*representativeness heuristic*).[12] Given that analysts are typically over-optimistic, any reduction in optimism could potentially lead to more accurate forecasts. However, this relationship is unclear, as similar studies have documented behavioral effects that result in increased forecast errors (Israelsen and Kong, 2024) or no effect at all (Addoum et al., 2020).

Weather-induced negative moods could make analysts more pessimistic and impair their assessment abilities. For example, analysts have been found to respond more slowly and pessimistically following adverse weather conditions (Dehaan et al., 2017), terrorist attacks (Cuculiza et al., 2020), and sports events (Wu, 2023). The effects on analysts' accuracy are mixed: Cuculiza et al. (2020) report an increase, Dehaan et al. (2017) observe no change, and Wu et al. (2021) note a decline. Since mood is unobservable, ruling out this mechanism is challenging. However, if mood effects are present, increases in pessimism are likely to be consistent across all firms.

Overall, the main predictions for the *behavioral* hypothesis are: 1) analysts will exhibit shifts in optimism or pessimism, but these shifts may not necessarily impact forecast accuracy, and 2) any changes in optimism or pessimism are expected to dissipate after a few months. While heuristic approaches suggest increased pessimism for firms with high climate risks, mood effects would predict a general increase in pessimism across all firms.

4 Empirical Strategy

In this section, I explain how I define salient weather shocks, the methodology used, the main assumptions for the validity of my methodology, and how to test the previously discussed hypotheses.

Salient weather event. My definition of natural disaster includes shocks that have at least one of the following three criteria: 1) more than 10 fatalities; 2) more than 100 injured people; 3) more than 1 billion dollars total economic damages.[13][14] By selecting only the largest disasters in terms of

[12] The *representativeness heuristic* suggests that an agent, after an event, tends to overestimate the probability of representative types (Kahneman and Tversky, 1972).

[13] Taylor and Thompson (1982) characterize a salient event as "a phenomenon that when one's attention is differentially directed to one portion of the environment rather than to others, the information contained in that portion will receive disproportionate weighing in subsequent judgments".

[14] Criteria 1 and 2 are commonly employed as standard criteria to classify weather events as natural disasters (Wirtz et al., 2014), while the 3rd criteria is from Barrot and Sauvagnat (2016).

economic and health-related damages in any state, I hope to discard seasonal and common climate events that may not be attributed to climate change realization. A weak definition of salient event risks would include natural disasters that are not informative for equity analysts, hence biasing the estimators downwards.

Difference-in-differences. To study the effect of salient climate shocks on analysts' forecasts, I start by dividing my sample of analysts into my treatment and control groups. I use analysts within a 100-mile radius of a salient shock as a treated group. The control group is represented by analysts who issue forecasts for firms in the same sectors as the firms followed by treated analysts.

To ensure that a change in forecasts is driven by changes in beliefs, I exclude all analysts forecasting firms located 100 miles from the event, using the firm's headquarters location as a proxy for the firm's location.[15] The analysis is conducted at the monthly level: keeping the last forecasts in the pre-treatment months and the first forecast in the post-treated months. By exploiting the staggered arrival of the extreme natural events at the analysts' location, I use the following regression:

$$\mathrm{Y}_{\ i,f,h,t} = \beta_1 post_{i,f,h,t} + \beta_2 treat_{i,f,h} + \beta_3 treat * post_{i,f,h,t} + \theta X_{i,f,t-1} + \gamma_{h*s} + \varepsilon_{i,f,h,t} \qquad (1)$$

for an analyst i, firm f, for a forecast horizon h and at month-year t. β_3 is the coefficient of interest, indicating the effect of experiencing a weather event on treated analysts after the event compared to the control group and $\theta X_{i,f,t-1}$ controls for analysts' and firms' pre-trend differences. Fixed effects (FE) included are γ_{h*s} which is an interaction between the shock indicator and the forecast horizon. Since climate shocks occur within a 100-mile radius of the analyst's office location, standard errors are clustered by the analyst's office location.[16]

Two types of dependent variables are then used to study whether analysts change their forecasts

[15] I demonstrate that the findings remain robust even when excluding firms with an establishment location near the event.

[16] Fast-growing literature highlights the problem arising by implementing a staggered differences-in-differences methodology (see Baker et al., 2022). When using multiple treatments over time, the estimated staggered DID coefficient can be seen as a weighted average across shocks. The problem arises when analysts experiencing a weather shock are compared to analysts that already received treatment in the recent past. Notice that this concern is addressed by using a control group composed of analysts that are never been treated or are yet to be treated. Thus, analysts are removed from the control group after experiencing a weather shock. Furthermore, I control this problem by implementing a standard differences-in-differences analysis across shock and forecast horizons, which is captured by the γ_{h*s}.

after a weather shock. Specifically, I follow Hong and Kacperczyk (2010) and use analysts' forecast bias and forecast error. Forecast bias is defined as $BIAS_{ift} = (F_{ift} - A_{ft}) / P_{f,t-1}$, where F_{ift} is the earnings forecast of an equity analyst i for a firm f in the month t, and A_{ft} is the realized earnings for a firm f at time t divided by $P_{f,t-1}$, the stock price for firm f in the previous quarter $t-1$. Since the bias could be positive as well as negative, I use forecast error to explore whether the analyst becomes more accurate (lower forecast errors). Forecast error is defined as $FERROR_{ift} = |F_{ift} - A_{ft}|/P_{f,t-1}$, which differs from BIAS only by having the numerator in absolute terms.

The set of additional covariates $X_{i,f,t-1}$ included are common control variables used in previous studies (Addoum et al., 2020, Cuculiza et al., 2020, Cuculiza et al., 2021, Hong and Kacperczyk, 2010, etc.) such as (i) days to end, the difference in days between the forecast and earnings announcement date; (ii) broker size, how many analysts are issuing forecasts for a brokerage firm in a year; (iii) companies followed, how many firms are forecasted by an analyst in a year; (iv) industries followed, how many industries are forecasted by an analyst in a year; (v) general experience, the difference in years between the first forecast issued on IBES and the analyzed forecasts; and (vi) firm experience, the difference in years between the first forecast analysts issued for a firm j and the analyzed forecasts. Additionally, I control for firm size (measured by total assets) and analysts' bias and error during the pre-treatment period.

Parallel trend assumption. To ensure the internal validity of my econometric methodology, I check whether the parallel assumption holds. A common test is to run a regression with pre-treatment interaction dummies between periods and treated groups, such as:

$$\begin{aligned} \mathrm{Y}_{i,f,h,t} = &\sum_{j\neq 0} \beta_j \text{relative month}_{i,f,h,t+j} + \beta \text{treat}_i \\ &+ \sum_{j\neq 0} \beta_j \text{treat} \times \text{relative month}_{i,f,h,t+j} + \theta X_{i,f,t-1} + \gamma_{s*h} + \varepsilon_{i,f,h,t} \end{aligned} \tag{2}$$

Where *relative months* is a binary variable that indicates the month when the forecasts were issued (from -3 months to + 3 months) relative to the reference month ($t = -1$), and *treat* takes value one for a treated analyst. The regression also includes pre-treatment controls ($\theta X_{i,f,t-1}$) and shock indicators interacted by forecasts' horizon (γ_{s*h}). The parallel assumption is satisfied if the difference between the control and treated group needs is either not significantly different or statistically different but constant throughout the pre-treatment months.

5 Data and Descriptive Statistics

5.1 Data

The dataset used in the paper is based on five main databases: (i) Climate events are obtained from the Storm Events Database (NOAA); (ii) Analyst forecasts are retrieved from IBES; (iii) analysts' office location is found on Refinitiv and Capital-IQ; (iv) Stock price are from CRSP; (v) firms headquarter location is from Compustat and FactSet Reserve.

Weather events. The Storm Events Database, obtained from the official National Oceanic and Atmospheric Administration (NOAA) website, contains data on 169,570 weather episodes from 1999 to 2020, covering 50 different event types reported by various sources, such as meteorological stations, media outlets, and call centers. When available, the dataset includes information on direct and indirect fatalities and injuries, geographical coordinates, event timing, as well as property and crop damages resulting from climate events. I define total fatalities and injuries as the sum of both direct and indirect cases, and total economic damages as the sum of property and crop damages, adjusted to real terms using 2013 as the base year.

I select salient weather events based on the following criteria: at least 10 fatalities, 100 injuries, or $1 billion in economic damages. This results in 185 unique episodes. For 72% of these events, precise geographical coordinates are provided. For events missing coordinates, I use the reported FIPS code of the county where the event occurred (with FIPS code translations obtained from the Storm Prediction Center WCM page, NOAA, 2016) and determine the centroid of the county as the event location.[17] In doing so, I determine the geographical location for 98% of the salient events in the dataset.

[17] The FIPS code is a unique identifier assigned to each county by the National Institute for Standards and Technology (NIST). These codes are obtained from Wikipedia's "Table of United States counties" (https://en.wikipedia.org/wiki/User:MichaelJ/Countytable).

Equity information. Stock-price data are from CRSP and they are matched with the IBES dataset of earnings forecasts by both TICKER and Cusip identifiers. To retrieve firms' location and industry classification (SIC code), I merge the IBES dataset with Compustat Quarterly by IBES TICKER. To proxy for the firm's location, I follow previous literature that uses the headquarters address (for example, Alok et al., 2020, and Barrot and Sauvagnat, 2016). Headquarter information (City, State, and ZIP code) are from Compustat Quarterly and they are linked by firms' ZIP code to the respective latitude and longitude coordinates using a large public dataset from CivicSpace Labs (opendatasoft). Out of the 9,182 firms in my dataset, about 50% can be linked by IBES TICKER using Compustat Quarterly. Following Pankratz et al. (2023), I match the remaining 50% firms by using FactSet Reserve by both TICKER and Cusip identifiers.

As a proxy for firms' climate risks, I use firms' specific forecasted climate physical and transition risks from Trucost. For physical risks, Trucost reports the composite score of a company's physical risk exposure (ranging from 1-low risk to 100-high risk) as a weighted average across 8 different physical risks (wildfire, coldwave, heatwave, hurricane, sea level rise, flood, and water stress) for three forecasts horizons (the year 2020, 2030 and 2050) and scenarios (high, medium and low). For my analysis, I use the composite physical risk forecasts of the year 2020 averaged across all future scenarios (high, medium, and low). In my sample, the average firm composite physical risk score is 60 points. Each physical risk averages from 3 points for flood and sea level rise to 57 points for water stress.[18]

To assess transition risks, I rely on the Unpriced Carbon Cost adjusted EBITDA provided by Trucost. This metric captures the difference between a company's current carbon cost and projected future costs based on industry, operations, and various pricing scenarios. Firms with high transition risk are those in the top tercile of earnings at risk, using carbon earnings at risk for the year 2020 across multiple scenarios (high, medium, and low). The remaining companies are categorized as non-risky. In Appendix D, I also conduct a robustness check by analyzing firms' absolute emissions, defining firms in the top tercile of MSCI absolute scope 1 emissions data as high-risk entities.

Lastly, to assess the information available about climate risks for each company, I use their voluntary disclosures to the Carbon Disclosure Project (CDP) and discussions of climate risks

[18] As physical risks remain relatively consistent across time and geography, my choice of using the year 2020 should not significantly impact the results.

during earnings calls from Sautner et al. (2023).

Analysts forecasts. I use the Details of Institutional Brokers' Estimate System (I/B/E/S) to collect short-term as well as long-term earnings forecasts (EPS) by analysts located in the US from 1999 to 2020. The data are then merged with the IBES Recommendation file to obtain the analyst's last name, initial of the first name, and brokerage house abbreviation. To de-anonymize the broker ID, I use the IBES Translation file.[19]

To obtain information on analysts' locations, I manually downloaded analysts for a sample of firms in Refinitiv, obtaining full names, email, brokerage names, and phone numbers. However, Refinitiv only provides information on active analysts that are currently producing forecasts and it does not provide any information on analysts' office locations. Luckily, the US uses a numbering plan area (NPA) that allows me to find the location of the analyst by exploiting analysts' first 3-digits of their phone number.

To expand the sample, I use Capital IQ - Professional to search for professionals located in the US and for which the profession title includes the term "Analyst" (for example, "Equity Analyst", "Research Analyst", "Former Analyst", etc.). Since the available version of Capital IQ - Professional provides only the US state location of the analysts, to find the city of the analyst's office location, I assume that analysts working for the brokerage firms in a given state are located in the same city as analysts previously found in Refinitiv. To avoid mismatch I manually check analysts, which moved at least once in my sample, using BrokerCheck.[20]

Lastly, the dataset is further cleaned by: (i) only including forecasts made in US dollars; (ii) excluding all forecasts with an absolute forecast error (difference between the forecast and the actual earning) greater than \$10 (Hong and Kacperczyk, 2010); (iii) excluding all firms that have an average share price lower than \$5 (Hong and Kacperczyk, 2010); (iv) excluding all firms that are followed by less than five analysts (Hong and Kacperczyk, 2010); (v) winsorizing the data at 0.5% for each tail and forecast horizon; (vi) excluding forecast less than 30 days from the forecast announcement.

This leads to a final dataset of 2,894 equity analysts in 29 different US states covering 1,588,202

[19]Since the IBES Translation file was discontinued in 2018, my study focuses on brokerage houses for which I possess the translation, assuming that IBES brokerage matches remained consistent thereafter.

[20]BrokerCheck is an open-source database provided by the Financial Industry Regulatory Authority (FINRA). See https://brokercheck.finra.org/

earnings forecasts for 5,109 firms from 2000 to 2020. For my sample of analysts, I also collect a large set of individual characteristics that are explained in detail in the Appendix B.

5.2 Descriptive Statistics

In this section, I present the descriptive statistics of analysts and weather shocks in my sample. Using the empirical strategy outlined in section 4, my final sample includes weather shocks located 100 miles from analysts who provide earnings forecasts for firms (unaffected by the weather event) and a control group of analysts forecasting firms in the same sector. After applying these filters, the sample under study shrinks to 1,231 analysts located in 24 states and covering 2,770 firms from 2000 to 2020.

Analysts characteristics. Figure 1 maps the location of my sample of analysts throughout the US (filtered by control and treated). Not surprisingly, 58% of equity analysts are located in the state of New York, followed by 7% in California and 4% in Illinois. It is well known in the literature that a significant portion of forecasts comes from analysts in New York; for example, Malloy (2005) found that 56% of all analysts are based in NYC. Importantly, my results remain consistent even when analysts from New York are excluded from the sample (see Table 11).

Table 1 reports the summary statics for my sample of analysts and firms. The average bias for analysts is 0.75% while the average forecast error is 1.84% (respectively with a standard deviation of 3.5 and 3.3). An analyst in my sample follows on average 15 firms, with an average of 2 years of forecasting a single firm and approximately 4 years overall of work experience. Moreover, the average analyst follows two sectors and works in a brokerage firm alongside 67 other analysts.

Weather shocks characteristics. Table 2 reports the characteristics of the salient events within a 100-mile radius of an analyst's location. For each type of weather event, the Table indicates the average total number of damages (in millions $), the total number of deaths and injuries, and the number of events. The Table shows that coastal floods are the most disastrous type of weather shock in terms of economic damages. In terms of health-related damages, debris flows, heat and tropical storms have the highest number of deaths, while winter storms have the highest number of injuries. In my sample, weather events with the most occurrence are tornadoes and heat.[21]

Climate beliefs after a weather event. To validate that my selected weather events affect beliefs, I follow Alekseev et al. (2021) and download Google trends about climate change in the state where analysts are situated. By regressing state-monthly Google trends on the constructed indicator for extreme events with state and year-fixed effects, I investigate if states with salient weather events present more Google searchers about climate change than states with no events.[22] Columns 1-3 of Table 3 report the coefficients of interest for the different types of damages caused by the salient events. All indicators are positive, while only fatalities and economic damages are statistically significant. Similar to Alekseev et al. (2021), experiencing any fatalities or economic damages caused by extreme events increases relative interest in climate change by respectively 9.5% and 8.6%. Similarly, table A2 uses data from the Yale Climate Opinion Map to demonstrate that in states where these events occur, a larger proportion of the population believes that global warming is happening and expresses increased concern in the following year.[23] This finding aligns with trends observed in Google search data, highlighting that selected extreme events affect climate change beliefs.

6 Empirical Findings

6.1 Main Results

The following results are conducted for analysts' yearly forecasts. Since analysts issue forecasts for different horizons, this sector reports the results aggregated by all horizons.

Baseline results. Table 4 reports the baseline differences-in-differences (DID) for both analysts' forecast bias and error using only one month before and after the weather event. The estimated coefficients indicate that, after a weather shock, first-time treated analysts become more accurate

[21] In the appendix, Figure A3 maps the selected salient weather shocks that occurred near an analyst's office location while Figure A2 plots all the salient shocks in NOAA from 2000 to 2020 across the US.

[22] In this analysis I used the entire sample of selected climate events, not just those near the analyst's location, such to prevent the misclassification of month-states as non-treated, which could result in underestimated findings.

[23] I use state-level data instead of county-level to ensure more accurate and representative results, as larger populations provide more reliable estimates.

(i.e. smaller forecast error) and less optimistic (i.e. smaller bias) compared to never-treated analysts, while the latter is not statistically different than zero given conventional confidence interval levels. The difference between the treated and control groups is 0.058 p.p. and 0.072 p.p., for bias and error respectively. Comparing the estimated results to the average bias and error in the sample, the effects correspond to an 8% decrease in forecast bias and 4% in forecast error.[24]

In the Appendix, Figure A6 plots the estimated DID for each sector. It suggests an increase in pessimism among analysts in the education, accommodation and food service, scientific, and mining sectors, while analysts have enhanced accuracy in sectors such as scientific and wholesale. Turning to Figure A7, specific climate events, including wildfires, surge tides, and floods, contribute to a more pessimistic outlook. On the other hand, analysts appear to be more (less) accurate following storm surges, heatwaves, hail, and debris flow (extreme cold and heavy snow). This highlights how heterogeneous are analysts' reactions to different sectors and weather-related incidents.

Parallel trend. I test the validity of my DID by assessing whether the parallel trend assumption holds. Figure 2 plots the estimated coefficients of pre and post-period interaction terms between treatment, near a weather event, and month indicators for both forecast error and forecast bias, using the month before the event ($t = -1$) as the reference month. The figures corroborate the findings that the forecast bias and error of control and treated groups are not statistically different in pre-treatment periods.[25]

Change in firms' fundamentals. To confirm that changes in analysts' forecast bias and error are attributed to changes in their climate beliefs, I verify that forecasted firm fundamentals remain stable between one quarter before and the quarter of the event. Figure 3 illustrates that, aside from a small statistically significant increase in Capex, there is no significant statistical variation in firm fundamentals surrounding the event. In the Appendix, Figure A4 shows that this also applies when compared to one quarter after the event.

[24] In the Appendix, Table A4 presents the results using analyst*firm fixed effects, year*firm fixed effects, and year*state fixed effects. When both year*firm and year*state fixed effects are introduced, the effect on forecast error becomes small and statistically insignificant. To confirm the robustness of these findings, Table A5 presents the results for 1-year horizon forecasts, these results remain invariant to the introduction of FE reassuring the robustness of the baseline results.

[25] Figure 2 fills in missing data for analysts during the pre-treatment period if they did not issue forecasts in a given month. In the Appendix, Figure A5 focuses on analysts who issued at least one forecast in each pre-treatment month, confirming that the parallel trend remains robust.

Breakdown by forecast horizon. The baseline results are reported aggregated for all analysts' forecast horizons (from 1 year to 4 years ahead).[26] Since climate risks affect both short and long-term expectations, I investigate whether analysts believe that climate risks threaten short- and long-term firms' earnings. Table 5 presents the estimated coefficients separately for each year's forecast horizons. The reduction in forecast error following a weather shock is primarily driven by 1 year-ahead forecasts. Looking at a longer horizon, the 4-year horizon shows an increase in optimism, making analysts less accurate. At the same time, the long-term growth (LTG) seems to decrease after the event, providing contrasting results since LTG generally indicates three- to five-year horizon growth. This result, however, is not surprising given that analysts tend to focus on shorter horizons, which are more likely to be updated in response to new information (Décaire and Graham, 2024). This opens avenues for further research into why future earnings perceptions remain overly optimistic.

6.2 Behavioral Versus Information

The study aims at disentangling whether changes in forecasts are driven by analysts' emotional response or information acquisition. To do so, I exploit the type of shock experienced by the analysts, the amount of information available about the companies they forecast, and the duration of their forecast revisions.

Shocks' characteristics. I leverage company-level data on specific climate-related physical risks to examine whether analysts who experience a particular climate event revise their forecasts for companies exposed to the same risks.

I focus on heatwaves for two main reasons. First, from a data perspective, heatwaves occur more frequently, resulting in a larger number of analysts located near these events, as well as more firms exposed to heatwave risk. Second, extensive literature highlights the costly impact of heatwaves on companies, affecting their operating performance (Pankratz and Schiller, 2021) and leading to lost sales (Barrot and Sauvagnat, 2016; Hugon and Law, 2019, Custodio et al., 2021), as well as influencing climate-related beliefs (Choi et al., 2020; Anderson and Robinson, 2019; Alekseev et al., 2021).

[26] Given the limited number of observations the table excludes 5-year forecasts.

Table 6 presents findings for firms categorized into high (above-median) and low heatwave risk, specifically for analysts who experienced a heatwave. I focus on the 1-year-ahead horizon due to the smaller sample size for longer horizons, however, the results for all forecast horizons are reported in Appendix Table A6. Given the smaller sample size compared to the baseline analysis, the first two columns replicate the baseline results, showing consistent patterns. Columns 3-4 show that analysts improve their forecast accuracy specifically for firms with high physical risks following a heatwave, while no significant effect is observed for firms with different risk profiles (columns 5-6). This suggests that analysts are specifically gaining new insights related to heatwave risk.

While my main analysis focuses on physical risks, the Appendix also explores transition risks due to their interconnected nature; for example, lenient carbon regulations could lead to more extreme weather events in the future. I categorize firms as having high (above-median) or low composite physical risks, and high (top tercile) or low transition risks, using Trucost's carbon earnings at risk weighted by EBITDA. Table A9 shows that analysts become more pessimistic and accurate for firms with high physical and transition risks, but overly optimistic and less accurate for firms with low climate risks. This suggests a potential behavioral response for low-risk companies while confirming increased accuracy for high-risk firms.[27]

Asymmetric information. If analysts gain new information from a weather event, it should significantly improve their forecast accuracy, particularly in situations of high information asymmetry where acquiring information is costly.

To test this hypothesis, I use several proxies for asymmetric information: i) companies' climate risk disclosure through the Carbon Disclosure Project (CDP), ii) companies operating in climate-sensitive sectors, iii) companies covered by many analysts, iv) analysts located in high climate-risk states, and v) companies discussing climate risks during earnings calls.

I start by collecting data on companies that voluntarily disclose climate-related information to the CDP, identifying 276 companies in my sample with such disclosures. Additionally, I categorize firms into high and low climate-sensitive sectors based on the IPCC classifications used in Choi et al. (2020). The rationale is that companies disclosing their climate risks, or operating in more climate-sensitive sectors, are likely to have more information about climate risks, both at the company and

[27] Table A12 corroborates that analysts are more pessimistic and accurate for high-transition risk firms but more optimistic and less accurate for low-risk firms.

sector level.

Next, I identify states with higher climate risks, defined as those experiencing more than four large events, which is above the median in my dataset for the entire period. The assumption is that analysts in states with fewer climate events have less exposure to climate risk information. Finally, I calculate the number of analysts following each company, as greater analyst coverage is associated with increased competition and reduced forecast bias (Hong and Kacperczyk, 2010). Companies in the top quartile of analyst coverage are classified as having high coverage, while all others are categorized as having low coverage.

Table 7 Panel A presents the results for firms broken down by CDP disclosure and climate risk of the sector. The estimated coefficients indicate that analysts become more accurate for firms without CDP disclosure. Interestingly, analysts become more pessimistic but not more accurate for firms with CDP disclosure. When looking at the climate risk of the sector, the effect is only on accuracy, and it is twice as large for companies in low climate-risk sectors.[28] [29]

Panel B shows that firms covered by fewer analysts experience a stronger effect on the accuracy of analysts' forecasts. Additionally, analysts in low-risk states become five times more accurate. These results suggest that analysts benefit from exposure to weather events by acquiring information in areas that are typically more opaque and have a lower flow of information.

I then look at companies that had discussed about climate change exposure during their earnings calls in the previous year, as well as those with no such discussions or no data. If there was any discussion of climate change exposure in any of the previous year's earnings calls, the indicator is set to one. The data is sourced from Sautner et al. (2023), which created scores based on whether climate change exposure was discussed (*exposure*), whether the discussion related to uncertainty and risk (*risk*), whether it was *positive* or *negative*, and whether it pertained to *physical* and *transition risks*.

Figure 4 shows no significant effect on either relative pessimism or accuracy when climate change

[28]In the appendix, Table A7 reports the results for firms with environmental violations above 5,000 dollars in the previous year. In my sample, 327 firms have been fined for environmental violations, but only 97 of these companies also have CDP disclosure. The results align with Table 7; analysts become more pessimistic for companies with more environmental violations but more accurate only for companies with no violations.

[29]Analysts forecast dispersion is also a proxy for uncertainty, table A8 reports the results for firms divided based on the previous year's quartiles of dispersion into high (top quartile) or low dispersion (all others). The coefficients confirm that for firms with higher dispersion, the effect on accuracy and bias is stronger, i.e., analysts become more pessimistic and accurate.

is discussed or not discussed during an earnings call. This suggests that analysts may have already incorporated climate risks into their forecasts, so the weather events provide no additional information. Interestingly, for companies without available earnings call information—whether due to data unavailability or because the company did not conduct the call—I find that analysts become more accurate in their forecasts, without a corresponding change in forecast bias. While these results should be interpreted cautiously due to potential measurement errors, they support the hypothesis of asymmetric information, indicating that analysts improve their accuracy for companies lacking prior information.

Persistence. If weather events convey no information on climate risks, equity analysts' forecasts should eventually revert to their fundamental values. However, empirically testing this hypothesis is challenging as it requires assuming no additional information about climate risks is released after the event. Despite these limitations, I explore whether analysts adjust their forecasts back to previous levels following the weather event.

I analyze changes in forecast bias and error at 4, 6, 12, and 18 months after the event, relative to the last forecast issued before the event for treated analysts. I focus on 1-year forecast horizons, as this horizon primarily drives the main results.[30] Figure 5 shows that analysts maintain an accurate stance up to 18 months post-event for 1-year-ahead forecasts, though the results are not statistically significant at 6 and 12 months.

Overall, the results suggest that analysts gain valuable information from experiencing the event, as evidenced by improved accuracy for high-risk firms and regions with more asymmetric information. Furthermore, this effect appears to persist over time. Collectively, these shifts in analysts' beliefs support the *information hypothesis*.

6.3 Mechanisms

I propose two theories to explain why analysts become more accurate after weather events. First, climate events may act as attention shocks, prompting inattentive analysts to revise their forecasts (Baker et al., 2020). Second, these events may lead analysts to focus on more valuable information

[30] Appendix A9 provides results for all forecast horizons, but their results are effectively zero. This is not surprising since table 5 indicates that there is no effect for other horizons or they become more inaccurate for 4 years ahead.

(Kacperczyk et al., 2016; Loh and Stulz, 2011). The first theory suggests inattentive analysts see the largest gains in accuracy, while the second indicates that skilled analysts, who can adapt and refocus, benefit the most.

I start by defining skilled analysts—defined as experienced analysts with more years in the industry—who can extract information about future climate change costs from weather shocks. For inattentive analysts, I calculate an attention score based on the number of forecasts an analyst issues in a year relative to the number of companies they follow, using data from the year before the weather shock. Analysts with a score of 10 or higher are considered attentive, while those below are classified as inattentive. In my DID sample, there are 318 attentive analysts and 865 inattentive analysts.

Table 8 presents results for analysts categorized by attention and experience, reporting findings for both 1-year (Panel A) and all horizons (Panel B). For 1-year forecasts, both attentive and inattentive analysts become more accurate, but high-experience analysts show more than double the increase in accuracy compared to low-experience. Across all horizons (Panel B), attentive analysts do not exhibit increased accuracy, as their optimism for long-term forecasts leads to a decrease in forecast errors that masks the increase in errors for short-term horizons. However, high-experience analysts continue to show an increase in their accuracy.

To further validate the results, I incorporate additional measures of analyst skill: forecast timeliness (Cooper et al., 2001), as shown in Table A14, and performance, calculated based on analysts' forecast errors (Hong et al., 2000), in Table A13. The findings align with the hypothesis that highly skilled analysts become more accurate post-event.[31]

The previous findings indicate that analysts demonstrate greater accuracy after weather shocks, particularly for high climate risk companies and more opaque firms, and that this effect is persistent. However, does this improvement in accuracy apply to all analysts?

To compare other analyst characteristics, I collect data on mindset (ex-ante optimism or pessimism), gender (male or female), and political donations (Republican or Democratic). Previous literature has linked characteristics such as Republican affiliation and male gender with lower prior beliefs about climate change (see, for example, Cuculiza et al., 2021 and Li et al., 2022).[32]

[31] The tables report both lead and performance at the firm and sector levels. The sector-level results are consistent with the main findings, suggesting that sector experience plays a crucial role.

[32] For a detailed description of each subgroup, refer to Tables 19 and 20, as well as Appendix B.

Figure A15 offers a breakdown of the baseline results by analyst characteristics. The findings reveal that male and Democratic analysts exhibit the most significant improvements in forecast accuracy, without a noticeable change in forecast bias. Reassuringly, the effect seems to be driven by analysts who were more pessimistic on average in the previous year and are now becoming even more pessimistic and accurate. This indicates that the observed effect is not merely due to a general decrease in optimism among previously optimistic analysts.

Overall, the findings support the rational attention allocation theory (Kacperczyk et al., 2016). When weather events occur, analysts are incentivized to focus on climate-related impacts on earnings forecasts, leading to improved accuracy, particularly among experienced analysts who leverage their information advantage.

6.4 Other Mechanisms

This section discusses other potential mechanisms that could explain my results, including geographical distance (Malloy, 2005), distraction (Han et al., 2020; Liu et al., 2022), effort (Glode, 2011), learning about one's abilities (Anagol et al., 2021), and reduced conflicts of interest (Michaely and Womack, 1999).[33]

Geographical distance. In a seminal paper by Malloy (2005), the author shows that analysts with closer geographical proximity to the companies have greater information flow. By design, my study defines treated analysts as those who are near the weather event, yet the event itself is far from the forecasted companies, thus making these analysts inherently distant from the companies. This could imply that the treated analysts had a higher level of information asymmetry to begin with. However, it is challenging to differentiate whether the control group did not update their forecasts because they already had this information about the forecasted firm (due to their proximity to firms) or because they did not experience the event (i.e., they did not acquire new information).

If my results are driven by geographical distance the effect is expected to be significantly larger for analysts who are further from the companies, given the larger information asymmetry. To do so, I divide my sample into analysts near the companies (i.e., equal to or below 751 miles, which is

[33]Since I do not observe a specific increase in pessimism, the effect is unlikely to be driven by mood effects (Dehaan et al., 2017). However, the rise in pessimism for firms with CDP disclosures may suggest a heuristic effect (Kahneman and Tversky, 1972).

the median distance from analysts to firms in my sample) and analysts distant from the companies (i.e., above the median). Table A17 indicates that the majority of the results come from analysts closer to firms, which is not consistent with the geographical distance hypothesis.

Distraction. Large weather events could potentially distract analysts, leading to increased forecast errors following the event (Han et al., 2020; Liu et al., 2022). However, the observed increase in accuracy does not support this hypothesis.

Effort. An alternative explanation is that analysts may concentrate their attention on companies critical to their professional careers (Glode, 2011), such as firms with high institutional ownership and large market capitalizations within their portfolios. Additionally, this effect on accuracy may be stronger for analysts in larger brokerage firms, as they typically have greater resources and better capabilities to deal with extreme weather events.

Table 14 shows no statistically significant results for both high and low institutional ownership firms; however, analysts do demonstrate increased accuracy for high market capitalization companies. Furthermore, the results indicate that analysts in larger brokerage firms become more accurate, while no significant effects are observed for analysts in smaller firms. In summary, these findings suggest that analysts may increase their efforts given their focus more on significant companies, with this tendency being particularly pronounced for those in larger brokerage firms.

Less conflict of interest. Higher uncertainty caused by weather events may reduce the impact of conflicts of interest, often leading analysts to be overly optimistic. This reduction in bias could result in more accurate forecasts, particularly for analysts working at larger brokerage firms (Michaely and Womack, 1999). The results in Table 14 support this, as the observed decrease in optimism and increase in accuracy among analysts at large brokerage firms may indicate a reduction in conflicts of interest.

Learning about one's abilities. Analysts might interpret experienced weather events as signals regarding their forecasting abilities rather than just learning about climate risks (Anagol et al., 2021). According to this model, analysts would become more responsive to future signals as a result. While my primary analysis focuses on analysts experiencing weather events for the first time, I also examine analysts who encounter between 2 and 8 shocks throughout their careers. As a control group, I use two types of analysts: those who have never experienced a shock and those who have experienced one less shock than the treated analysts.

Figure 6 illustrates the effects on analysts' bias (a) and error (b). The results for bias are mixed: no significant change is observed after the second shock, but pessimism decreases after the third and fourth shocks, with inconsistent effects beyond that. Regarding forecast error, analysts generally become less accurate with subsequent shocks. These findings suggest a potential behavioral effect aligned with 'learning about their forecasting abilities'. Nonetheless, the results also highlight the importance of the baseline results, showing that only initial experiences with weather events lead to increased accuracy, while subsequent shocks do not have a similar impact.

6.5 Additional Analysis

In addition to updating their forecasts, analysts provide stock price targets and recommendations, exercise discretion in selecting which firms to cover, and decide on the questions they pose during earnings calls. In this section, I also investigate whether analysts with prior experience of weather events demonstrate greater accuracy in forecasting the performance of firms affected by such events.

Forecasting stock prices is more complex than forecasting earnings due to the need to predict long-term payout ratios and appropriate discount rates (Stotz and von Nitzsch, 2005). Recommendations, meanwhile, tend to be overly optimistic and less volatile than earnings forecasts (Michaely and Womack, 2005), partly due to pressure from investment banks for more favorable recommendations (Malmendier and Shanthikumar, 2014). Despite these challenges, Table A18 shows that analysts after experiencing a weather event exhibit a statistically significant 7% reduction in target price forecast errors. In contrast, Table A19 reveals no significant change in analysts' recommendations following weather events.[34]

Next, I examine whether analysts with prior weather event experience demonstrate enhanced forecasting abilities when assessing the impact of such events on firms. For this analysis, I categorize analysts into two groups: those who have previously experienced a weather shock (treated) and those who have never experienced or have yet to encounter a weather shock (control), focusing on firms directly affected by weather events (within a 100 miles radius from the event).

[34] For a detailed explanation, see Section F in the Appendix.

Table 15 shows that analysts with weather event experience become more optimistic and accurate in their firm-level forecasts post-event (columns 1-2). This effect is particularly pronounced for firms with high physical risk and those in climate-sensitive sectors (columns 3-4). These findings suggest that firsthand weather event experience helps analysts acquire valuable insights, improving their ability to forecast the impact on companies.[35]

Lastly, I analyze the firms covered by analysts and the nature of the questions they ask during earnings calls. Given that firm coverage is assessed annually, while earnings call questions occur sporadically over time, this analysis focuses on the average differences among analysts who have experienced at least one significant weather event. The key independent variable is a binary indicator reflecting whether an analyst has encountered at least one such event, incorporating both analyst and quarter-year fixed effects.

In the coverage analysis, my dependent variables include the number of firms an analyst follows each quarter and the proportion of firms with high physical, transition, and climate risks within the total set of firms forecasted. For the analysis of earnings call questions, I sourced transcripts from 2006 to 2018 from WRDS and matched them with my sample of analysts, identifying 1,398 analysts who asked at least one question.[36] Using unigrams and bigrams from Sautner et al. (2023), I identified climate-related questions.[37] The dependent variable is the number of questions an analyst asks about physical, regulatory, and opportunity risks within a given quarter.

Table 16 presents a regression where the independent variable indicates whether an analyst has experienced at least one significant weather event. The findings suggest that, on average, analysts increase their overall coverage but tend to shift towards firms with lower climate risks across all risk categories. Table 17 reveals that analysts with weather event experience are more likely to ask questions about physical risks, aligning with Israelsen and Kong (2024), who found that analysts exposed to wildfire smoke are more inclined to discuss climate-related issues. No significant effects

[35]The results remain consistent when analysts are near both the firm and the event. However, restricting the sample to analysts distant from the forecasted firm reduces the overall effect, leading to decreased accuracy and increased optimism for firms with low physical risk and in climate-sensitive sectors, while results for high-risk firms remain similar but lose statistical significance.

[36]Since I am using analysts with more than one climate event experience, the number of analysts is greater than in the baseline analysis.

[37]The complete list of bigrams is not publicly available, I used the top 100 bigrams from "Table IA.IX: Top Bigrams for Topic-Based Climate Change Exposure Measures" in Sautner et al. (2023), removing deceptive terms and supplementing with additional unigrams and bigrams. See Table A26 for the full list.

were observed for transition or opportunity risks. Overall, these results indicate that analysts are becoming more conscious of climate risks, as evidenced by their increased focus on climate-related questions and a shift away from covering high climate-risk firms.

6.6 Aggregate Market Effect

In this section, I examine whether the impact on analysts near the event extends to those who are distant, as well as how brokerage houses respond to firms and analysts affected by these events.

Spillover to distant analysts. I examine whether an increase in forecast accuracy by analysts directly affected by a weather event impacts other analysts who forecast the same firm but are geographically distant from the event. In this analysis, the counterfactual group consists of distant analysts who forecast firms that have no analysts located near the event. In contrast, the treated group includes distant analysts who forecast the same firm as an analyst who is near the weather event. As in the baseline setting, all firms considered remain distant from the event itself.

To ensure comparability, I retain only firms that, around each event, are in the same sector and forecasted over the same horizon. I then apply propensity score matching based on firm characteristics such as analyst coverage, sales, size, leverage, operating income, ROA, stock price, and market value. The dependent variables in this analysis are forecast bias, forecast error, consensus (average forecast), and dispersion (standard deviation of forecasts) across distant analysts for each company in a given month. The regression controls include company coverage, size, leverage, sales, and operating income, with fixed effects applied at the shock, forecast horizon, and firm sector levels. Standard errors are clustered at the firm state level to ensure robustness.

Table 9 shows that analysts exposed to a treated analyst forecasting the same firm exhibit reduced forecast dispersion and increased pessimism, but with no overall effect on forecast accuracy. This suggests that while some analysts become more pessimistic (increasing forecast errors), others improve accuracy by reducing optimism, leading to a neutral overall impact on forecast error.[38] [39]

These findings confirm that such events are highly salient, affecting all analysts, even those

[38]Table A22 examines whether treated analysts who are highly skilled (based on experience, performance, or lead roles) influence distant analysts, and the results (except for experienced analysts) align with the baseline findings.

[39]In an additional analysis, I find that dispersion decreases particularly for firms with high physical and transition risks.

distant from the event. However, analysts who do not experience the event firsthand do not gain indirect insights from treated analysts, resulting in no improvement in their forecast errors.

Brokerage effect. To investigate whether brokerage firms recognize and capitalize on the increased forecast accuracy of treated analysts, I ideally would compare brokerage firms located near weather events to those that are distant. However, due to the limited sample of geographically localized analysts, it is challenging to pinpoint the exact locations of large brokerage firms. As a result, this analysis is conducted at the aggregate brokerage level, assessing the average impact across brokerage firms with multiple locations across the US.

For each brokerage firm, I calculate the total number of analysts employed, the number of firms they forecast within a given year, the number of firms located near weather events, and the number of analysts situated near such events. It is important to note that the measure of treated analysts may contain measurement errors due to my limited sample size. Standard errors are clustered at the brokerage level, and the analysis includes brokerage and year-fixed effects to account for unobserved heterogeneity.

Table 10 presents regressions of the number of forecasted firms, treated firms, and treated analysts in the previous year on the number of analysts in the following year. As expected, the more firms forecasted, the more analysts are employed in the next period. Interestingly, Column 2 shows that the number of treated analysts is also associated with hiring more analysts. Columns 3-4 and 5-6 further break down the results by large versus small brokerages (using 13 analysts as a threshold). The findings suggest that large brokerages drive this effect, as they tend to hire more analysts. In contrast, smaller brokerages see a reduction in the number of analysts for firms forecasted near the event.

In the appendix, Table A20 investigates the types of firms these brokerages are more likely to follow. This analysis keeps track of each firm followed by a brokerage house annually and uses a linear probability model to examine if having more treated analysts or firms predicts the types of firms they follow. The table shows that large brokerage firms are more likely to follow firms with high transition risks, especially when they have more treated analysts. These results are robust to the inclusion of brokerage house fixed effects.

Stock Price Reaction. To assess the economic impact of my findings and the informative value of analysts' revisions, I examine the effect of post-event analyst revisions on firms' stock prices. Following Malloy (2005), I compare stock price changes triggered by treated analysts' revisions to those by control analysts after a weather event (see Appendix F for details). I focus on revisions indicating "good" or "bad" news, defined as news revisions are higher (lower) than previous forecasts and consensus.

Table A24 shows the impact of treated versus control analysts' forecast revisions on stock returns over various time windows around the event. Results are broken down by positive and negative signals, as well as by firms with high and low physical risks. No significant results are found within three days or two weeks of the event. Significant effects of forecast revisions by treated analysts are found one to two months after the revision, especially for high-risk firms. Positive revisions for high-risk firms lead to higher returns one and two months after the revision, while negative signals result in lower returns for high-risk firms after two months. No effect is found for low-risk firms. These results suggest that while the market may eventually incorporate this information, it takes time.[40]

6.7 Robustness Tests

A series of robustness checks are conducted to test the validity of the results.

Analysts' location. I start by showing that analysts in New York and California do not drive my results. Table 11 shows that the findings remain robust when excluding analysts from these states. Then, I examine the robustness of the results by controlling for different analyst distances from the event. Table 12 shows that analysts within a 50-mile radius of the event exhibit a larger impact on bias and error, with a decrease of 13 and 23 percentage points, respectively. In the 100-200 mile range, the effect on bias is even larger in magnitude but not statistically significant, and beyond 200 miles, there is no effect.

[40]The results are robust to using cumulative abnormal return as a dependent variable (Table A25 in Appendix F presents results for trading volume, indicating a significant increase in trading volume only when stocks receive a negative signal and have low physical risks.

Firms' location. One major limitation of this study is the reliance on firms' headquarters as the firms' location. This is problematic since firms often have multiple locations, thus there is a risk of incorrectly assuming that a firm was not affected when it was. However, this concern is partly addressed by ensuring that the fundamentals of the firms remain unchanged during the events.

To address this concern, I use the National Establishment Time-Series (NETS) Database to add information about establishments of US firms along with their corresponding coordinates. Among the firms in my baseline analysis, I successfully link 899 firms with their respective establishment locations. In columns 1 and 2 of Table 13, I observe that when firms are 100 miles distant from the event, analysts' accuracy and pessimism tend to increase.

On the other hand, columns 3 and 4 show that firms with establishments within 100 miles from the event's location cause analysts to adopt a notably optimistic outlook with a larger, but not significant, forecast error increase. Lastly, in columns 5 and 6 I replicate the baseline analysts by excluding firms that in the NETS database are near the event's location. This reiteration of my primary findings corroborates the baseline results.[41]

Analysts' social connection. I investigate if the analysis is downward biased because treated analysts may be communicating with control analysts. To proxy for social connection, I use the Social Connectedness Index (SCI) obtained from Bailey et al. (2018), which represents the relative probability of a Facebook friendship connection between individuals in two locations. I then repeat the analysis by considering only those control and treated analysts in counties that are socially connected (i.e., the top decile of the SCI index for the treated analysts' county) and not socially connected (i.e., all the others). Table A16 shows that the results are driven by socially connected counties, indicating that this effect could potentially be a lower bound. However, small and statistically insignificant effects are found when looking at analysts in counties that are not connected.

Mean-reversion. To rule out the possibility that the observed effect is due to mean reversion, I perform a placebo test by randomly generating weather event dates at the locations where actual

[41] In the Appendix, section E shows that the results seem to be driven by companies with business locations in the same state as the shock. While this could be problematic, it also suggests that analysts can gather some information from the event about the firm's business operations, even if these events do not directly affect the companies' fundamentals.

weather events occurred.[42] Figure A8 shows the estimated effects on forecast bias and error, with mixed results and only out of 30 events being statistically significant at 5% significant level. This suggests that the study's findings are not driven by mean reversion.

7 Conclusion

This study enhances our understanding of how weather shocks influence beliefs about climate risks. It demonstrates that analysts adjust their forecasts following significant weather events, resulting in increased accuracy.

The evidence suggests that analysts gain valuable insights into climate change costs through their experiences with weather events. They become more accurate in predicting earnings for high climate risk firms and when facing higher asymmetric information. Notably, skilled analysts—those with higher experience, performance, and timeliness forecasts—exhibit the greatest improvements in accuracy, aligning with the rational allocation theory.

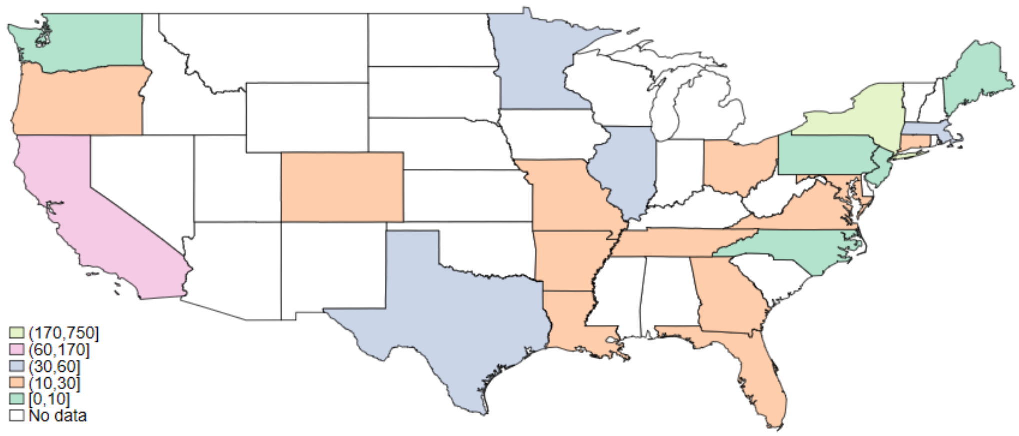
Additionally, I find that analysts also improve their accuracy for stock price forecasts and they ask more climate-related questions during earnings calls. However, investors do not react to these forecast revisions, and improvements in forecast accuracy do not seem to spill over to other analysts who are geographically distant from the event. Furthermore, large brokerage firms capitalize on this increased accuracy by hiring more analysts and following more companies with high climate risks.

Overall, the results indicate that improved disclosure should be accompanied by policy efforts to incentivize training and education programs for analysts. These measures will help ensure that climate-related risks are accurately incorporated into forecasts, ultimately enhancing climate risk awareness and responsiveness within the financial industry.

[42] The results remain consistent when generating 51 random dates at 51 different locations across the US.

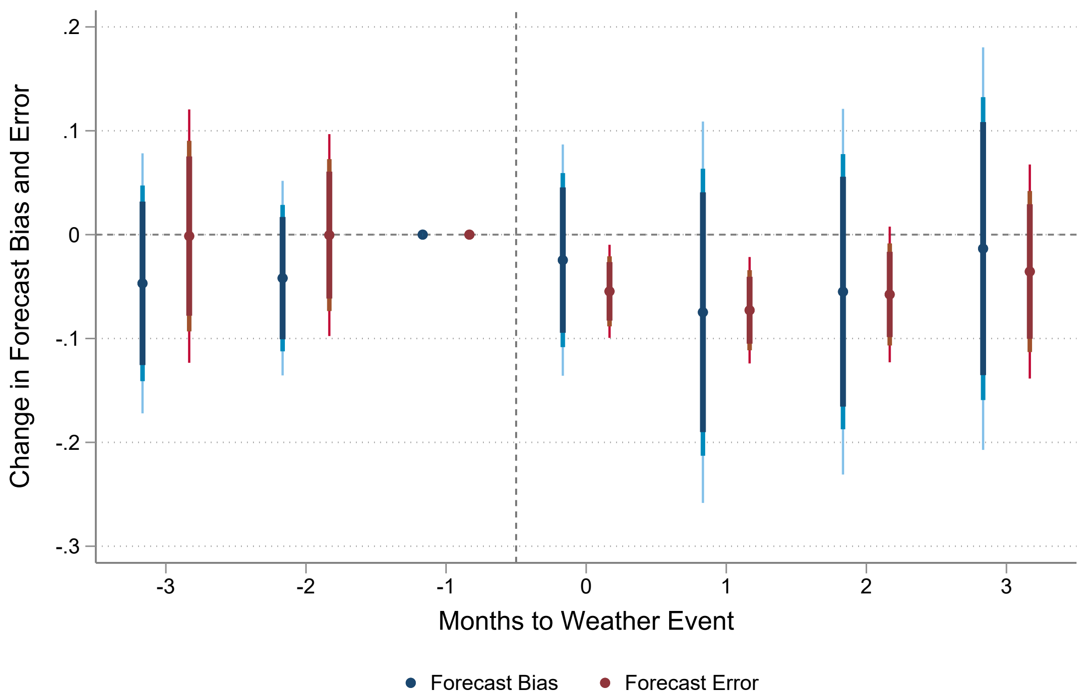
Figures

Figure 1: Analysts' location from 1999 to 2020 by state



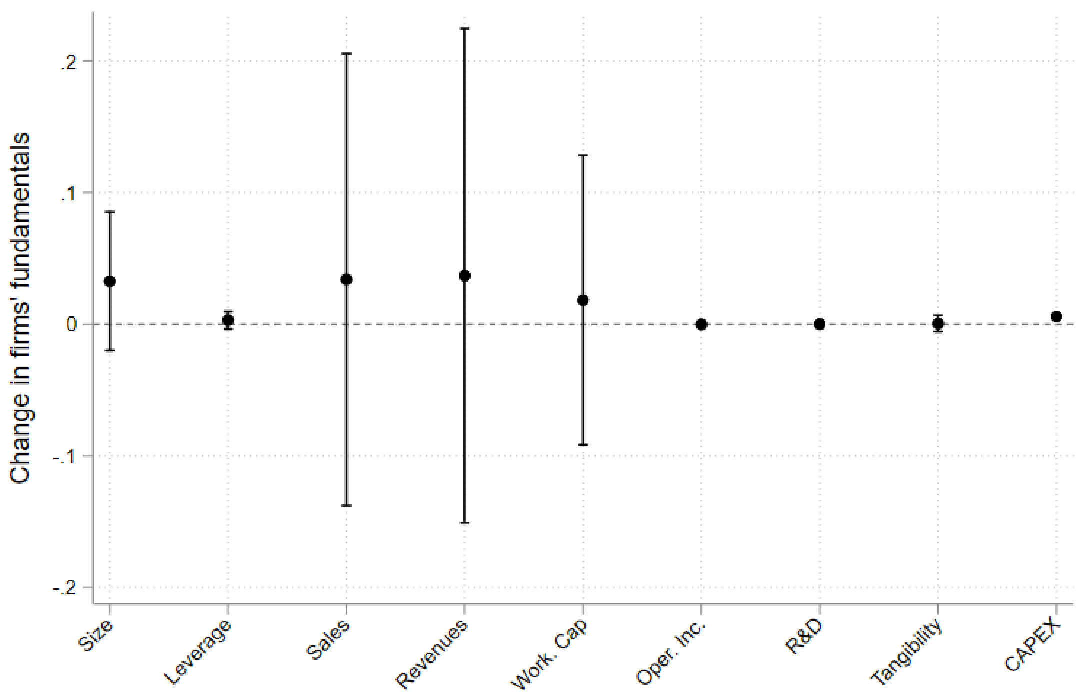
Note: The graph maps the sample of matched IBES analysts' locations to weather shock from 1999 to 2020 by US state. The state of New York has the highest number of analysts with 734 individuals, followed by 162 in California, 54 in Minnesota, and 53 in Illinois.

Figure 2: Parallel trend



The figures plot the estimated coefficients for the pre- and post-period interactions between treatment and the time variable, with 99%, 95%, and 90% confidence intervals. The omitted month is the one immediately preceding the weather event. The specification includes all covariates, with the shock interacted with horizon fixed effects. The event window spans 3 months before and 3 months after the event. The figure shows forecasts for all analysts in the sample; if an analyst did not issue a forecast in months -3 or -2, their data is filled backward. In the Appendix, Figure A5 presents results for analysts who issued at least one forecast in each month before the event, showing robust findings. Standard errors are clustered by the analysts' office location.

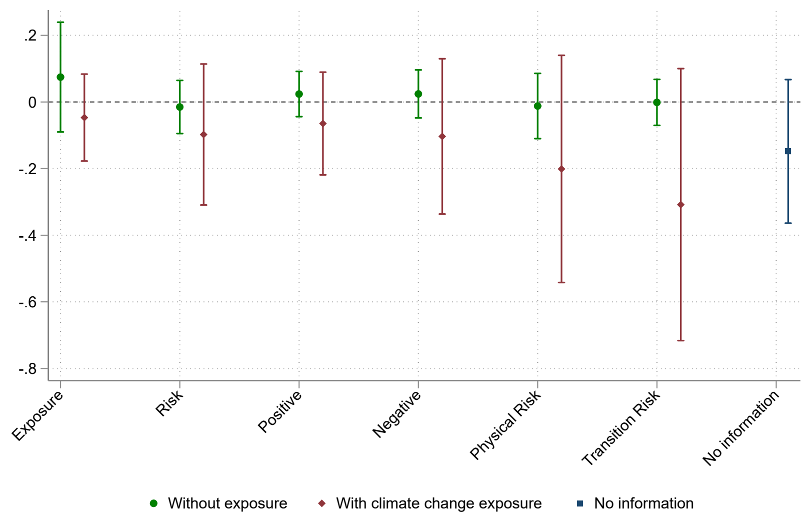
Figure 3: Changes in firms' fundamentals around the weather shock



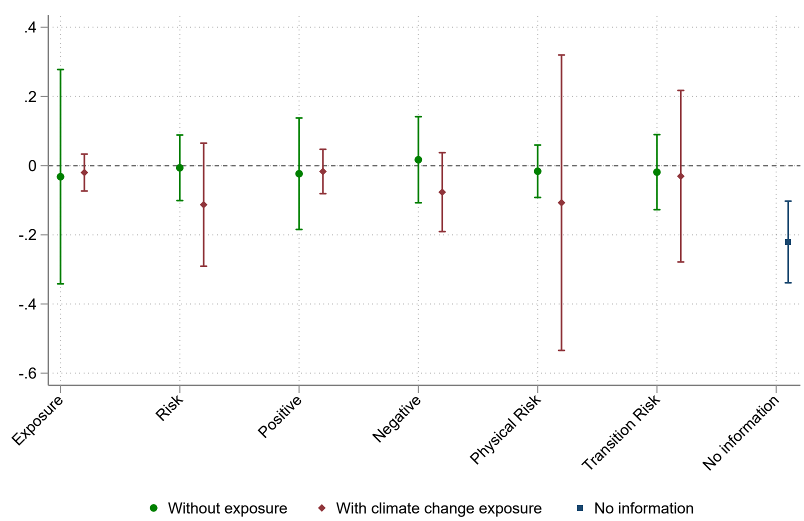
Note: the graph illustrates differences in firms' fundamentals for firms forecasted by analysts before and after a weather event. The independent variable is an indicator that takes the value of one after the weather event and zero before. We selected the first available data for the forecasted firm one quarter before and the quarter of the weather shock. Sales, revenue, and working capital are scaled by 1000 to ensure compatibility and ease of comparison. The table includes fixed effects for the weather event. To ensure that any effect of the shock is incorporated, figure A4 shows the results using only fundamentals announced one quarter after the weather shock quarter.

Figure 4: Effect on analysts forecast by previous year firms' climate risks exposure

(a) Forecast Bias

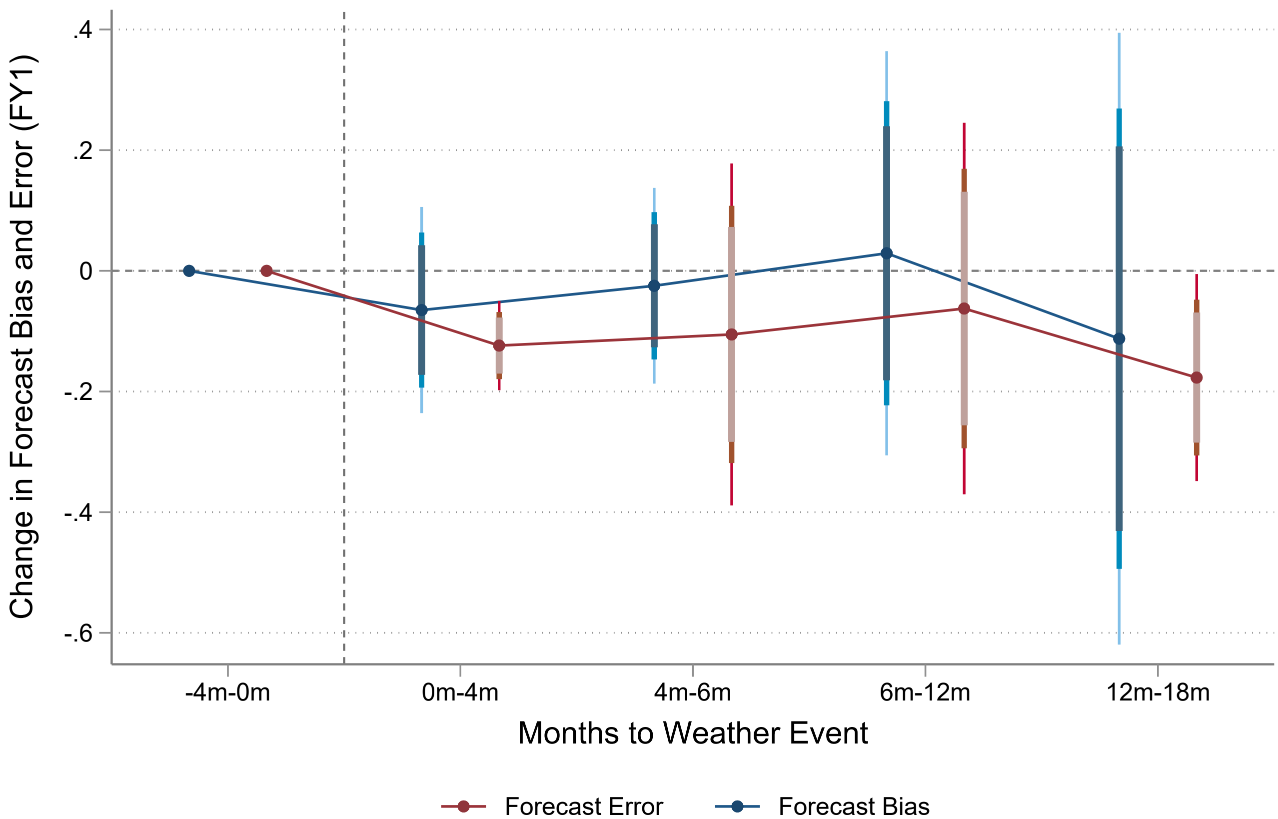


(b) Forecast Error



The figure plots the estimated effect size for firms without exposure to climate change (green), firms with exposure (red) and firms with no information (blue). Climate change exposure is estimated using earnings call transcripts from the previous year by Sautner et al. (2023). Note that the difference between "no information" and "without exposure" is that the former refers to companies with no estimated score due to a lack of data or an earnings call conducted by the company. The effect size is computed by running the baseline DID regression separately for firms in each group. For each bigram score constructed by Sautner et al. (2023), I create a binary variable indicating whether there was a non-zero mention in any earnings call in the previous year. *Exposure* refers to any mention of climate-related words, while *Risk* is defined as a climate-related word appearing in the same sentence as the words *risk* or *uncertainty*. *Positive* and *Negative* depend on whether the climate-related word was near a positive or negative tone word. *Transition* and *Physical risk* represent specific bigrams associated with these specific risks. The confidence intervals are at a 5% significant level.

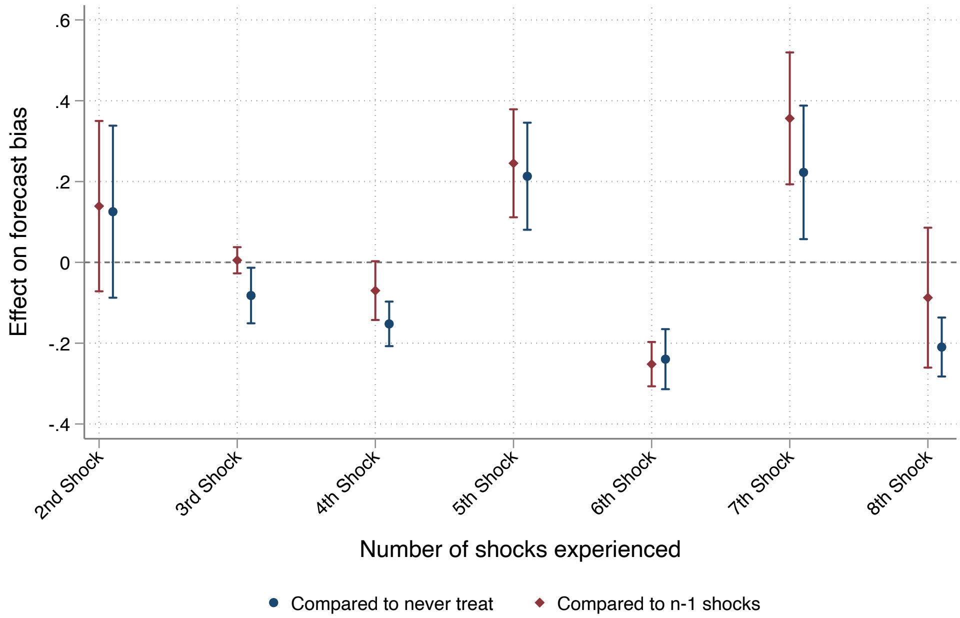
Figure 5: Persistence of the effect after the event



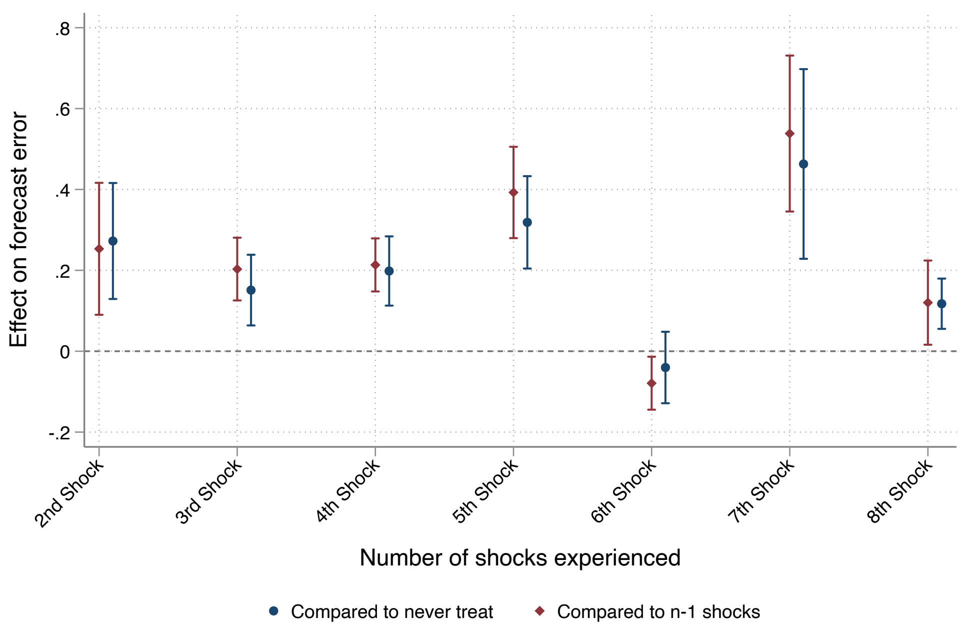
Note: figures plot the estimated coefficients from the staggered difference in difference in bar plots with 99%, 95%, and 90% confidence intervals for error (red) and bias (blue) for 1-year ahead horizon (FY1). The specification includes all covariates and forecasted horizon*weather shock fixed effect. The analysis keeps only one forecast before and after the event. The standard errors are clustered at the analyst's office location. In the appendix, figure A9 plots for all horizons.

Figure 6: Multiple experiences of weather events

(a) Forecast Bias



(b) Forecast Error



Note: figures plot the estimated coefficients from the staggered difference in difference in bar plots with 95% confidence intervals for forecast bias (top graph) and error (bottom graph). The treated group are analysts experiencing other weather events, while the control group is either analysts with no experience of weather events (blue) or one less experience compared to the treated group (red). The specification includes all covariates and forecasted horizon*weather shock fixed effect. The analysis keeps only one forecast before and after the event. The standard errors are clustered at the analyst's office location.

Tables

Table 1: Summary statistics for the staggered DID

	Mean	p50	SD	Min	Max
forecast bias (%)	0.75	0.06	3.53	-29.90	60.38
forecast error (%)	1.84	0.66	3.33	0.00	60.38
companies followed	14.57	14.00	6.44	1.00	47.00
firm experience	1.98	1.00	2.26	0.00	19.00
general experience	4.27	3.00	3.85	0.00	19.00
Industries followed	1.79	1.00	1.10	1.00	11.00
brokerage size	67.01	51.00	52.96	1.00	284.00
firm size	7.61	7.58	1.79	1.43	14.72
leverage	0.19	0.15	0.21	0.00	3.95
market value	2.01	1.45	2.02	0.02	45.48
stock price	39.43	28.31	45.80	0.62	2027.09
ROA	0.00	0.01	0.08	-3.98	0.67
N	57586				

Note: The table reports the summary statistics used in the analysis. Forecast bias is defined as the difference between the earnings forecast of an equity analyst i for a firm f in the month t minus the actual earnings divided by the stock price for a firm f in the previous fiscal year $t-1$, while forecast error differs from forecast bias only by having the numerator in absolute terms. Both are expressed in percentages. See tables 19 and 20 for a description of the variables used.

Table 2: Description merged salient storm event

Event Type	Av. Total Damage (Mil. $)	Av. Total Deaths	Av. Total injuries	Number of Events
Extreme Cold/Wind Chill	0	10	0	1
Thunderstorm Wind	0	1	100	1
Winter Weather	0	1	200	1
Heavy Snow	0.80	0	100	1
Heat	45.94	11	54	10
Tornado	76.31	7	120	7
Tropical Storm	109.20	11	77	2
Debris Flow	289.37	18	89	2
Storm Surge/Tide	1082.22	0	0	1
Flood	1155.12	0	0	2
Wildfire	1391.57	4	45	1
Hurricane (Typhoon)	1850.46	1	10	3
Hail	2185.69	0	0	1
Flash Flood	3850.36	7	0	2
Coastal Flood	5073.30	1	0	1

Note: The table reports the selected salient weather events that are 100 miles from an analyst location. The table shows the average economic damages caused by each type of shock (converted in 2013 USD), the average number of related deaths and injuries, and the respective number of shocks across the dataset. Given our empirical strategy filters (i.e. only forecasts for firms 100 miles distant from the event, the control group composed of never-treated analysts, and the treated group composed of analysts treated only once), only a small number of shocks are selected.

Table 3: Climate beliefs after a weather shock

	Google Search of "Climate Change"		
	(1)	(2)	(3)
Fatalities	0.0955*		
	(0.0496)		
Injuries		0.00942	
		(0.0868)	
1 bil. $ damages			0.0860**
			(0.0327)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
R^2	0.825	0.825	0.825
N	5028	5028	5028

Note: I use the Alekseev et al. (2021) methodology to estimate the log scaled Google search interest of the topic "climate change" in the states where analysts are located. The standard errors are clustered at the month and state level, and observations are weighted by each state's population size.

Table 4: Baseline

	(1) Bias	(2) Error	(3) Bias	(4) Error
post	0.00861	-0.0350**	0.00858	-0.0350**
	(0.0331)	(0.0144)	(0.0330)	(0.0144)
treat	-0.159	-0.0760	-0.102	0.0501
	(0.164)	(0.0814)	(0.144)	(0.0769)
treat*post	-0.0587	-0.0721**	-0.0587	-0.0721**
	(0.0660)	(0.0274)	(0.0658)	(0.0274)
Controls	No	No	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.0720	0.113	0.264	0.309
N	57586	57586	57576	57576

Note: the table shows the baseline staggered difference-in-differences (DID) for 1 to 4 years EPS forecasts of an analyst i forecasting a firm f. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 5: Forecast horizons decomposition

	Forecast Bias				Forecast Error				LTG
	(1) 1-Year	(2) 2-Year	(3) 3-Year	(4) 4-Year	(1) 1-Year	(2) 2-Year	(3) 3-Year	(4) 4-Year	(1) LTG
treat*post	-0.0598	-0.0904	-0.0283	0.299	-0.153***	-0.0376	0.0465	1.530**	-0.877***
	(0.0568)	(0.0964)	(0.140)	(0.438)	(0.0330)	(0.0242)	(0.0780)	(0.533)	(0.290)
Shock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.268	0.243	0.239	0.291	0.270	0.249	0.261	0.391	0.873
N	30566	24832	1832	286	30566	24832	1832	286	2173

Note: the table shows the baseline staggered difference-in-differences for yearly forecasts dis-aggregated at different forecast horizons: 1 to 4 years and long-term growth rate. The control variables are forecast days gap, broker size, companies followed, firms experience, industries followed and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the analyst's office location.

Table 6: Heatwave exposure and heterogeneous firm risk

Firms	All		High Heatwave Risk		Low Heatwave Risk	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
treat*post	0.0454	-0.126*	-0.00540	-0.267**	0.0662	-0.0392
	(0.0768)	(0.0723)	(0.150)	(0.126)	(0.0529)	(0.0584)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.336	0.291	0.271	0.201	0.387	0.376
N	6210	6210	2172	2172	4038	4038

The table presents the baseline difference-in-differences for firms with high and low heatwave risks when an analyst experienced a heatwave for 1-year ahead forecasts (table A6 reports for all horizons). Firms with high heatwave risk are those with above-median heatwave risks compared to other firms in the sample. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 7: Asymmetric Information

Panel A	CDP Disclosure		No CDP Disclosure		High Climate Risk		Low Climate Risk	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(1) Bias	(2) Error	(3) Bias	(4) Error
treat*post	-0.240***	-0.0252	-0.0428	-0.0703**	-0.119	-0.0570*	0.0181	-0.102**
	(0.0853)	(0.102)	(0.0671)	(0.0305)	(0.0889)	(0.0292)	(0.0287)	(0.0383)
R^2	0.174	0.176	0.279	0.326	0.199	0.260	0.516	0.526
N	4372	4372	53204	53204	39102	39102	18474	18474
Panel B	**High Analysts Coverage**		**Few Analysts Coverage**		**High Risk State**		**Low Risk State**	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(1) Bias	(2) Error	(3) Bias	(4) Error
treat*post	-0.0792	-0.0369*	-0.00195	-0.159*	-0.0657	-0.0372	-0.00431	-0.193***
	(0.0837)	(0.0207)	(0.0672)	(0.0864)	(0.0813)	(0.0240)	(0.0541)	(0.0469)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.233	0.268	0.323	0.382	0.208	0.266	0.372	0.400
N	41430	41430	16146	16146	36080	36080	21496	21496

Note: the table shows the baseline difference-in-differences for firms with high and low asymmetric information (gray area). *CDP Disclosure* refers to companies that disclose their environmental impact to the Carbon Disclosure Project. *High Climate Risk* includes companies in sectors classified as high climate as in Choi et al. (2020). *High Analyst Coverage* refers to companies in the top quartile of analyst coverage, while *Low Analyst Coverage* includes all others. *High-Risk State* denotes states that have experienced more than four significant weather events. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 8: Mechanisms of attention vs. rational allocation: analysts attention and experience

Panel A (FY1)	Inattentive		Attentive		High Experience		Low Experience	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.131*	-0.186***	0.0300	-0.170**	-0.0626	-0.281**	-0.0640	-0.120***
	(0.0780)	(0.0584)	(0.0522)	(0.0706)	(0.0919)	(0.111)	(0.0564)	(0.0286)
R^2	0.296	0.301	0.167	0.172	0.361	0.353	0.239	0.244
N	19892	19892	7096	7096	8998	8998	21568	21568
Panel B (all horizons)	Inattentive		Attentive		High Experience		Low Experience	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.112	-0.133***	0.0735	-0.0755	-0.0365	-0.149*	-0.0722	-0.0612*
	(0.0833)	(0.0341)	(0.0845)	(0.0630)	(0.106)	(0.0788)	(0.0633)	(0.0318)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.319	0.352	0.220	0.280	0.335	0.357	0.247	0.299
N	36042	36042	15027	15027	16513	16513	41063	41063

Note: The table presents the baseline difference-in-differences for subgroups of analysts based on high and low attention and experience. Analysts with an attention score (number of forecasts/number of companies followed) equal to or above the median (10) in the previous year are labeled as 'attentive.' 'High experience' refers to analysts in the top decile of years worked (more than 13 years) as analysts. Each specification includes a weather shock times horizon fixed effect to account for shock- and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, analyst experience, industries covered, and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the office location.

Table 9: Spillover effect to analysts distant to the event

	(1) Consensus	(2) Dispersion	(3) Bias	(4) Error
1(one analyst near)*post	-0.0287	-0.0166**	-0.161***	0.0326
	(0.0180)	(0.00743)	(0.0527)	(0.0645)
Controls	Y	Y	Y	Y
Shock*horizon*Sector FE	Y	Y	Y	Y
R^2	0.440	0.290	0.216	0.330
N	11740	11740	11740	11740

Note: The table presents the baseline staggered difference-in-differences for the aggregate effect on consensus, dispersion, bias, and error at the company level for analysts distant from the event. *1(one analyst near)* takes value one for companies at least one analyst near the event, and treated analysts are those forecasting the company but distant from the event. Control analysts are those without any treated analysts near the event. Treated and control companies are required to be in the same sector, state, shock event, and forecast horizon, and are matched based on coverage, sales, firm size, leverage, operating income, ROA, stock price, and market value. The control variables include company coverage, firm size, leverage, sales, and operating income. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the state level and at the office location.

Table 10: Impact of analyst and firm exposure on future analyst numbers at brokerage firms

	Number of analysts by brokerage firm$_t$					
	All		Large Brokerage		Small Brokerage	
	(1)	(2)	(3)	(4)	(5)	(6)
N. treated firms$_{t-1}$	0.00161		-0.000963		-0.0285**	
	(0.0117)		(0.0163)		(0.0140)	
N. treated analysts$_{t-1}$		0.113***		0.108**		-0.0000312
		(0.0420)		(0.0500)		(0.0700)
N. firms$_{t-1}$	0.0592***	0.0578***	0.0672***	0.0656***	0.0591***	0.0582***
	(0.00491)	(0.00478)	(0.00720)	(0.00716)	(0.00509)	(0.00502)
Brokerage	Y	Y	Y	Y	Y	Y
Year	Y	Y	Y	Y	Y	Y
R^2	0.953	0.954	0.941	0.941	0.777	0.777
N	3665	3665	1097	1097	2554	2554

Note: The dependent variable in the regression is the number of analysts at the brokerage level. The main independent variables are the number of firms forecast by the company in the previous year (N. firms), the number of analysts near a weather event in the previous year (N. treated analysts), and the number of forecasted firms near a weather event in the previous year (N. treated firms). Large (small) brokerages are defined as those with 13 or more (fewer) analysts. The regression includes year and brokerage fixed effects. Standard errors are clustered at the brokerage level.

Table 11: Robustness: excluding New York or California

Excluding:	New York		California	
	(1) Bias	(2) Error	(3) Bias	(4) Error
treat*post	0.0752	-0.131***	-0.0992	-0.0761**
	(0.0520)	(0.0426)	(0.0608)	(0.0297)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.287	0.328	0.303	0.345
N	43215	43215	41248	41248

Note: the table shows the baseline staggered difference-in-differences for 1 to 5 years EPS forecasts of an analyst i forecasting a firm f. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 12: Robustness: analyst' distance from the weather shock

Distance event (miles)	≤ 50		100-200		200-300	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
treat*post	-0.133	-0.214***	-1.032	-0.203	-0.0791	0.0795
	(0.139)	(0.0576)	(0.889)	(0.333)	(0.138)	(0.164)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.247	0.295	0.561	0.596	0.236	0.280
N	53116	53116	7882	7882	22290	22290

Note: This table presents the baseline staggered difference-in-differences estimates for analysts at different distances from the weather events. Columns 1-2 replicate the analysis for analysts within 50 miles from the event, columns 3-4 for analysts within 100 and 200 miles, and columns 3-4 for 200 to 300 miles. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 13: Robustness: firms' NETS establishment

Establishment	NETS > 100 miles		NETS < 100 miles		drop if NETS < 100 miles	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.136	-0.258***	0.0816	0.114	-0.0811	-0.0992***
	(0.158)	(0.0866)	(0.0900)	(0.0962)	(0.0733)	(0.0225)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.258	0.300	0.213	0.240	0.277	0.321
N	12148	12148	4304	4304	53272	53272

Note: This table presents the baseline difference-in-differences estimates using the firm's NETS establishment location. Each specification includes forecast horizon and shock ID interacted. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 14: Distraction hypothesis

	Institutional Owner				Relative Importance				Brokerage Firms			
	High		Low		High		Low		High		Low	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Bias	Error	Bias	Error	Bias	Error	Bias	Error	Bias	Error	Bias	Error
post*treat	-0.0788	-0.126	-0.0272	-0.0551	-0.0768	-0.112***	-0.0535	-0.0250	-0.0518	-0.0625*	-0.146	-0.190
	(0.111)	(0.0890)	(0.0626)	(0.0350)	(0.0800)	(0.0269)	(0.0417)	(0.0502)	(0.0585)	(0.0349)	(0.197)	(0.192)
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.303	0.329	0.271	0.319	0.317	0.373	0.289	0.326	0.256	0.305	0.408	0.430
N	6701	6701	49001	49001	11557	11557	28123	28123	53552	53552	4024	4024

Note: This table presents the baseline staggered difference-in-differences estimates for yearly forecasts. High Institutional Owners take the value 1 if firms are ranked in the top 25th percentile in the number of institutional owners among all covered firms in an analyst's portfolio and 0 otherwise (from Thomson-Reuters 13F Database). Relative Importance takes value 1 if a firm is ranked among the top 25th percentile of market cap in an analyst's portfolio. Small Brokerage takes the value 1 an analyst is employed within a brokerage firm above the median of its size, as quantified by the number of employees (13 employees). The control variables are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the analyst's office location.

Table 15: Forecasting a firm near a weather event

	All		High Physical Risk		Low Physical Risk		High Climate Sector		Low Climate Sector	
	Bias	Error	Bias	Error	Bias	Error	Bias	Error	Bias	Error
post*1(experienced a weather event)	0.0718*	-0.0993**	0.0743	-0.0791*	0.0591	0.0500	-0.00888	-0.215**	0.123**	-0.0213
	(0.0365)	(0.0477)	(0.0445)	(0.0453)	(0.0833)	(0.0958)	(0.0826)	(0.0926)	(0.0527)	(0.0227)
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Horizon*Firm*Shock FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Analyst FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.233	0.132	0.286	0.123	0.252	0.163	0.277	0.122	0.277	0.122
N	98914	98914	55613	55613	17903	17903	41091	41091	57821	57821

Note: the table reports the regression of the difference between previously treated analysts forecasting affected firms in comparison to analysts with no experience of a weather event. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table 16: Climate risks of firms followed by analysts

		Share of Forecasted Firms		
	N. Firms	High Physical Risk	High Transition Risk	High Climate Risk
1(experienced a weather event)	0.222***	-0.0128**	-0.0162***	-0.0312***
	(0.0730)	(0.00524)	(0.00505)	(0.00496)
Controls	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Quarter*Year FE	Yes	Yes	Yes	Yes
R-squared	0.862	0.481	0.506	0.611
N	61511	61511	61511	61511

Note: The table presents a regression where the variable *treat* equals 1 if the analyst has experienced at least one significant weather event. The table includes analyst and quarter-year fixed effects. It also includes analyst covariates such as brokerage size, number of industries and firms followed in a year, an indicator if an analyst was a top performer in the past three years, and if the analyst has many years of experience. The standard errors are clustered at the analyst level.

Table 17: Climate-related questions during earnings calls

	Number of Questions			
	Physical Risks	Regulatory Risks	Climate Opportunity	Any Climate Question
1(experienced a weather event)	0.0103*	-0.00116	0.00251	0.0113*
	(0.00576)	(0.00143)	(0.00172)	(0.00633)
Controls	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Quarter*Year FE	Yes	Yes	Yes	Yes
R-squared	0.0849	0.0517	0.0696	0.0880
N	16615	16615	16615	16615

Note: The table presents a regression where the variable *treat* equals 1 if the analyst has experienced at least one significant weather event. The dependent variable is the number of questions an analyst asks in a quarter regarding physical, regulatory, and opportunity risks, as well as any of the three. The table includes analyst and quarter-year fixed effects. It also includes analyst covariates such as brokerage size, number of industries followed in a year, an indicator if an analyst was a top performer in the past three years, and if the analyst has many years of experience. The sample includes 1,398 analysts who asked at least one question during earnings calls from 2006-2018. The table includes analysts and quarter*year fixed effects. The standard errors are clustered at the analyst level.

Table 18: Variables description - Dependent Variable

Variable Name	**Description**
Forecast Bias	The difference between the analyst forecast and the actual earnings divided by the stock price in the previous quarter
Forecast Error	The difference between the analyst forecast and the actual earnings in absolute terms, divided by the stock price in the previous quarter
Google Search of "Climate Change"	the log scaled Google searches of the world 'climate change' in a given state in a month as in Alekseev et al. (2021)
Long-term growth rate (LTG)	The long-term growth forecast is a forecast of the growth rate in earnings per share over a three to five-year horizon as in IBES
Consensus	The average forecast for a company in a given month
Dispersion	The standard deviation of forecasts for a company in a given month
Number of analysts by brokerage firm	The total number of analysts working for a brokerage firm in a given year
Share of Forecasted Firms	Calculates the share of firms with climate risks (both transition, physical and by sector) over the total number of companies followed by an analyst in a year
Number of Questions	the number of questions an analyst asks in a quarter regarding physical, regulatory, and opportunity risks, as well as any of the three

Table 19: Variables description - Analyst level

Variable Name	**Description**
Forecast Day Gap	The difference in days between the forecast and earnings announcement date
Brokerage Size	How many analysts are issuing forecasts for a brokerage firm in a year
Companies Followed	How many firms are forecasted by an analyst in a year
Industry Followed	How many industries are forecasted by an analyst in a year
Firm Experience	The difference in years between the first forecast issued for a firm j and the analyzed forecasts
Analyst Experience	The difference in years between the first forecast issued on IBES and the analyzed forecasts
Experienced Analysts	analysts in the top decile of years of experience in the IBES dataset (13 years)
Lead Analysts	I construct a ratio, the Leader-Follower Ratio (LFR), defined as the cumulative days of preceding forecasts divided by the cumulative days of following forecasts, excluding the analyst's own forecasts. Analysts with a LFR score above one and in the top decile in a given year, are classified as lead analysts.
Attentive Analysts	Analysts with an attention score (number of forecasts/number of companies followed) equal to or above the median (10) in the previous year are labeled as 'attentive.'
High Risk States	the state has more than the median climate shocks (4 weather shocks)
Ex-ante Optimistic (Pessimistic)	in the previous quarter the analyst was in the top tercile as an optimistic (pessimistic) analyst, i.e. the average of their forecasts was above (below) consensus
High Performance	I create analysts' score following Hong et al. (2000) and I select the top tercile performer based on the average performance score in the previous 3 years, both within a firm and a sector.
Analysts' Political Donation	takes the value 1 if the analysts donate to a democratic party (from FEC)
State Climate Beliefs	states with high (low) climate beliefs are states in the top percentile (bottom 5 percentiles) as the percentage of the population believing that climate change is happening in 2021 (from Yale Climate Opinion Maps for 2021)
Sex	takes the value 1 if the analyst is female (estimated from the analyst's first name)
Small Brokerage	takes value 1 if analysts are employed within a brokerage firm above average regards to its size (more than 13, proxied by the number of employees)
Socially Connected	if analysts are in counties socially connected calculate by Social Connectedness Index (SCI) from Bailey et al. (2018).

Table 20: Variables description - firm level

Variable Name	Description
Firm Size	Logarithm of total assets
Leverage	Total debt (short term debt+long term debt) divided by book assets
Operating Income	Operating income before depreciation divided by book assets
Market Leverage	Market value of firm equity from CRSP divided by book assets
ROA	Income before extraordinary items divided by book assets
Stock Price	Stock price at $t-1$
High Climate Risk (Sector)	follow Choi et al. (2020) that categorized as high climate risk according to the IPCC, which includes agriculture, mining, utilities, construction, manufacturing, transportation, and warehousing, while classifying all other sectors as low climate risk.
Physical Risk	Composite score of the company's physical risk exposure, i.e. wildfire, coldwave, heatwave, hurricane, sea level rise, flood, and water stress (from Trucost Climate Change Physical Risk Data). Physical risk scores are represented as values from 1 (lowest risk) to 100 (highest risk) and forecasted for the year 2020 averaged across all future scenarios (high, medium, and low)
CDP Disclosure	takes the value 1 if companies that disclose their environmental impact to the Carbon Disclosure Project
High Analyst Coverage	takes the value 1 if companies in the top quartile of analyst coverage
High Physical Risk firm	takes the value 1 if the firm's physical risk score is greater than the average physical risk composite score in the sample (i.e. more than 60 points)
High Heatwave Risk	takes value 1 if the firm individual score for heatwave risk is greater than the average physical risk in the sample
High Transition Risk	takes value 1 if the firm's transition risks are in the top tercile and zero otherwise. Transition risks are proxied by Unpriced Carbon Cost adjusted EBITDA of the year 2020 (Carbon Earnings at Risks) forecasted for the year 2020 averaged across all future scenarios (high, medium, and low)
Establishment Location	geographical coordinates of establishment location from NETS Database
High Institutional Owners	takes the value 1 if firms that are ranked in the top 25th percentile in the number of institutional owners among all covered firms in an analyst's portfolio and 0 otherwise (from Thomson-Reuters 13F Database)
Relative Importance	takes value 1 if a firm is ranked among the top 25th percentile of market cap in an analyst's portfolio

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Online Appendix

A Descriptive Statistics

Figure A1 maps the location of my total sample of analysts throughout the US (not filtered by control and treated). Not surprisingly, 68% of equity analysts are located in the state of New York, followed by 7% in California and 4% in Illinois.

Figure A3 maps the selected salient weather shocks that occurred near an analyst's office location from 1999 to 2020 by the US state. The states with the highest number of shocks are California and Oregon with 46 and 47 shocks. The state with the lowest number of weather events is Washington with three weather shocks.

B Analysts' Characteristics

For studying individuals' beliefs, I construct a series of analysts' characteristics commented below.

Climate variables. *Climate-sensitive states* are constructed using the entire natural hazard dataset and looking at the median number of shocks per state and by setting high climate-sensitive states as states with more than 4 natural events. These states are Texas, Tennessee, Connecticut, Florida, Ohio, California, Pennsylvania, Maryland, and New York and 87% of analysts are located there.

Experience. As for *analysts' experience*, it is quantified by the number of years an analyst has been included in the IBES dataset. Looking at the entire IBES dataset possess, I define analysts in the top decile as high experienced analysts (with more than 13 years).

Gender. The determination of the gender of equity analysts is accomplished using Chat GPT, which categorizes analysts' names as female, male, or uncertain. This categorization results in 14% of the total analyst sample being identified as female, while 5% remain uncertain.

Lead. Another approach to categorizing skilled analysts is based on the timeliness of their forecasts. Following the methodology of Cooper et al. (2001), I construct the Leader-Follower Ratio

(LFR), defined as the cumulative days of preceding forecasts divided by the cumulative days of following forecasts, excluding the analyst's own forecasts. Analysts with an LFR score above one and who are in the top decile for a given year are classified as lead analysts. This metric can be calculated at both the firm and sector levels, although Cooper et al. (2001) argue that the sector level is more representative of the industry.

Mindset. The value of optimism is assigned as value 1 if an analyst's forecast exceeds the consensus forecast (calculated as the average forecast for a specific firm over a month for a specific forecast horizon) and 0 otherwise. Subsequently, I compute the average optimism score for each analyst within a fiscal year. Based on these scores, analysts are categorized into terciles within a fiscal year. *Ex-ante pessimistic analysts* are the ones in the top tercile of pessimism scores in the previous year, and all the others are defined as 'non-pessimistic'. Conversely, the opposite holds for optimisms.

Performance. The performance measurement methodology, as described in Hong et al. (2000), follows a systematic process. Firstly, the forecast error is computed for each analyst by taking the absolute difference between their forecasted values and the actual values. Subsequently, analysts are ranked within their respective firms based on the forecast error, and this ranking is adjusted according to the number of analysts associated with each firm. The resulting rankings yield individual performance scores for analysts within a given year .To determine an analyst's overall performance score, the average score from the past three years is used. Analysts who fall into the top tercile of performance scores from the previous year are identified as the 'top-performing' analysts. At the sector level, an analyst's performance score is ranked within their sector each year, with top performers similarly defined as those in the top tercile.

Political variables. I proxy for political affiliation using Political Donation Data from the FEC dataset, which reports any individual donation above 200 dollars for a party. The merge is conducted by analysts' names and states. Moreover, I manually checked that the reported companies match the brokerage firm with which the analyst is working. Using the data from 2000 to 2018, I find 203 analysts of which 51% conducted democratic donations. [43]

[43] Note that in Jiang et al. (2016) they can find a sample of 673 donor analysts, during the 1993 to 2008 period.

C Climate News

I explore whether the news about climate change increases after an extreme event. This is important because changes in analysts' beliefs should be driven by first-hand experience shocks and not other types of occurring events, such as an increase in climate news. Ideally, I would like to examine variations in climate-related news at both the local and national levels. However, due to data limitations, the currently available indexes are only constructed at the national level. This implies that I can only utilize variations over time and not across states; hence, I cannot include state-fixed effects.

Two climate news indexes are used as dependent variables in table A3. Columns 1-2 use the Sentometric index on news about global warming constructed by Ardia et al. (2020), while columns 3-4 use the Wall Street Journal (WSJ) climate news indices created by Engle et al. (2020). The results in table A3 are not statistically significant. These findings highlight that selected extreme events affect climate change beliefs, but not national climate news.

D Firms' Climate Risks

Defining firms' climate risk is challenging. One way to leverage the data is to categorize firms by high and low-risk sectors. However, this approach is also not trivial, as most climate risk assessments focus on transition risks and often fail to capture physical risks. In the main analysis, I use the IPCC definition of risky sectors, as outlined by Choi et al. (2020), which primarily relates to transition risks. Table A21 categorizes firms into high and low climate risk sectors based on criteria from Addoum et al. (2020), which emphasize the direct impact of high temperatures (physical risks) on companies. Consequently, columns 9-12 present a mixed definition that incorporates both transition and physical risks. The results still show a stronger effect for low-risk sectors, but this effect is somewhat mitigated compared to the IPCC definition.

For firm-specif classification, I use Trucost estimates to categorize firms based on high and low climate physical and transition risks. For overall physical risks, I use Trucost's composite score, while for transition risks, I consider actual emissions and carbon earnings at risk. High physical risks are defined as above-average composite physical risk scores, given the smaller variation, while high transition risks are represented by firms in the top tercile of carbon earnings at risk.

Table A10 presents results broken down by firms with above-median and below-median physical risks. Consistent with sector-level findings, it shows that the increase in inaccuracy is more pronounced for firms with low physical risks. This isn't surprising, as physical risks span various hazards, and analysts (at least for heatwaves) tend to be more accurate when dealing with familiar risks.

For transition risks, I use two variables: actual emissions (a backward-looking measure) and carbon earnings at risk (a forward-looking measure). Table A11 shows that analysts become more pessimistic overall, with a marked increase in forecast inaccuracy for high-transition-risk firms, while becoming more optimistic—but less accurate—for low-transition-risk firms. The results for actual emissions in Table A12 are similar, but in this case, increased optimism for low-transition-risk firms (bottom tercile) also translates into worse forecast errors.

Finally, Table A9 presents the results for firms classified by both high and low physical and transition risks (earnings at risk). Forecast revisions are most prominent for firms with both high physical and high transition risks, compared to those with lower risks. This suggests that analysts possess a level of sophistication in distinguishing between firms, indicating that the broad climate sector classification may be too general to capture the nuances of climate risk impacts effectively.

E Robustness

Firms' business location

In this analysis, I examine whether companies mention a state as their business location in their 10-K filings from Garcia and Norli (2012).[44] This allows me to control for firms with extensive geographic dispersion, which may present challenges for analysts in accurately estimating climate risks. Additionally, it enables me to exclude firms with business locations in the treated states, thus allowing for a more precise analysis.

Table A23 presents the results for firms divided into two categories. Columns 1-2 include firms with at least one business location mentioned in their 10-K filings in the same state as the weather shock onset, while columns 3-4 consist of firms with business locations in other states. The results indicate that the effects are driven by firms with at least one business location in the same state as

[44] I thank Mandeep Singh for sharing the updated version of the geographical dispersion data up to 2018.

the shock onset. While this may be problematic, as it could indicate that analysts perceive these firms as directly impacted by the event, it may also suggests that analysts are able to gather some information from the event about the firms' business operations (even if there is no direct impact on the overall business).

F Additional Analysis

Analysts' target price and recommendation. I start cleaning the target price IBES dataset. Following Ou and Wang (2024), I exclude observations where the target price to current price ratio is below 0.7, above 4, or equal to 1. I then construct the absolute target price forecast error as the deviation of the "realized" price (the stock price 12 months after the target price announcement) from the target price in absolute terms. More formally, this is the logarithm of the stock price 12 months ahead divided by the current stock price minus the logarithm of the target stock price 12 months ahead forecasted by an analyst divided by the current stock price.

For analysts' recommendations, I follow Loh and Stulz (2011) and include only those outstanding ratings where the difference between the revision date and the announcement is no more than 12 months. Additionally, I include only companies that received at least one recommendation in a year from a minimum of three analysts. I also reverse the coding so that 5 = strong buy, 4 = buy, 3 = hold, 2 = sell, and 1 = strong sell, meaning a higher recommendation is better. To address the changes in rating distribution caused by the National Association of Securities Dealers (NASD) Rule 2711 implemented in 2002, I only keep recommendations from 2004.

For my analysis, I use the methodology outlined in Section 3. Analysts located within 100 miles of an event are defined as treated, and those farther away are controls that forecast firms in the same sector as the treated analysts. All the firms have to be distant from the event, and analysts forecasting at least one firm near the shock are excluded. I keep the last observation in the months before the event and the first right after (allowing a 3-month window). The sample for target price includes 46 events with 736 analysts near the event and 1,600 control analysts forecasting 2,585 companies. Due to the lower number of recommendations, the sample size is relatively small, comprising 29 events involving 254 analysts and 415 controls, totaling 700 firms.

Table A18 shows a decrease in target price forecast error after analysts experience a weather event. The effect is statistically significant and stable in magnitude across a set of different fixed effects. Given the average target forecast error of 35%, the results indicate a 7% decrease in forecast error. Table A19 shows no statistically significant effect on analysts' recommendations after the event. This finding is robust to the inclusion of controls and a battery of fixed effects.

Stock price. To test whether forecast revisions made by analysts near a weather event have a greater impact on stock prices than revisions made by control analysts, I follow Malloy (2005) and run Fama and MacBeth (1973) cross-sectional regressions of average excess return on the magnitude of the revision. Such a finding would provide strong evidence that treated analysts have an information advantage over other analysts. This type of test also helps gauge the economic significance of my prior results.

My main regression is:

$$AAR_{jt} = \alpha_0 + \alpha_1 LNME_{j,-30} + \alpha_2 SUF_{ij0} + \alpha_5 treat_i + \epsilon_{ijt} \tag{3}$$

Where AAR is computed following Beaver et al. (1979) and Atiase (1985) by running a rolling regression of stock return on market return from 2 years to 6 months before the event episode for a firm j. I then keep the estimated coefficients from the regressions ($\hat{\alpha}$ and $\hat{\beta}$) constant for the entire post-event period to estimate the unexpected price changes during the period as $u_{jt} = R_{jt} - (\hat{\alpha} + \hat{\beta} R_{mt})$. AAR_{jt} is then defined as the average unexpected price return for the window period of interest ($\underline{x}$ to $\bar{x}$) after the forecast revision:

$$AAR_{jt} = \frac{\sum_{t=+\underline{x}}^{+\bar{x}} u_{jt}}{T} \tag{4}$$

In the regression, I also control for the logarithm of the firm's market capitalization in the month before the event. The standardized unexpected forecast equals analyst i's forecast for firm j on day 0 minus analyst i's prior forecast for firm j (before the event), scaled by the cross-sectional standard deviation of all prior outstanding forecasts for firm j. As in Malloy (2005), all standard deviations below 0.25$ are set to 0.25$ to mitigate small denominators. *treat* is the dummy variable that takes the value 1 if the analyst is near a salient weather event.

Trading volume. A proxy for the information content of analysts' forecast revisions is abnormal trading volume (Cooper et al., 2001, and Michaely and Vila, 1996). If analysts provide information that was not previously reflected in the stock price, I should observe an increase in trading volume after a forecast release. Otherwise, if the information is redundant, no effect is observed. A drawback of this methodology is that abnormal trading volumes could also be driven by market-wide news (in line with Alekseev et al., 2021) or an important corporate announcement.

In our empirical analysis, I consider the abnormal volume around the forecast revision, that is, the volume in excess of regular volume as in Cooper et al. (2001). My regressions are similar to the stock price analysis but I also control for idiosyncratic risk scaled by the variance of market return and systemic risk.

To calculate abnormal trading volume, I follow Michaely and Vila (1996) and Cooper et al. (2001) to estimate the cumulative abnormal volume (CAV) for the number of days after the forecast revision.

I start by first calculating the mean volume in the estimation period for each event. The mean volume for event j is defined as the mean daily turnover for days before and after the event. For this event window, I rely on Cooper et al. (2001) and use $[81 - 41]$ days before and after.

$$ATO_j = \frac{\sum_{t\in[-81,-41]\cup[+41,+81]} TO_{jt}}{T} \tag{5}$$

where TO_{jt} is the daily turnover for a stock j on a day t, and T is the number of days in the estimated period.

Then for each day in the event period, I calculate the abnormal values as:

$$AV_{jt} = \frac{TO_{jt}}{ATO_j} - 1 \qquad t \in \underline{x}, \ldots, \bar{x} \tag{6}$$

My estimated event window goes from $\underline{x}$ to $\bar{x}$. Since I look at the effect after a forecast revision, I focus on post-event days.

The cumulative abnormal volume is the sum of the daily abnormal volume for the days around the event:

$$CAV_j = \sum_{t=+\underline{x}}^{+\bar{x}} AV_{jt} \tag{7}$$

The regression used to estimate the impact of the forecast revision on trade volume is the following:

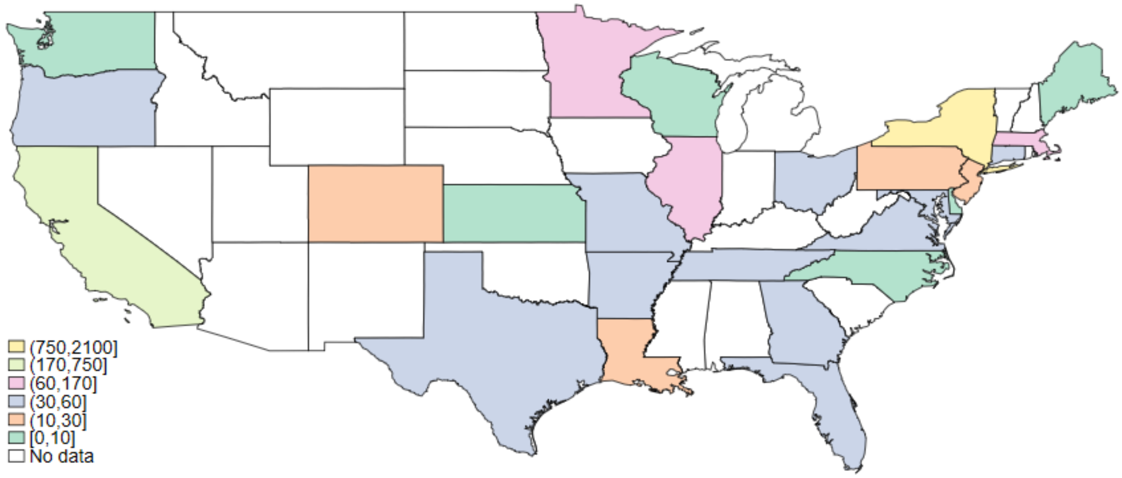
$$CAV_j = \alpha_0 + \alpha_1 LNME_{j,-30} + \alpha_2 SUF_j + \alpha_3 \frac{\sigma_{\epsilon j}}{\sigma_m} + \alpha_4 Beta_j + \alpha_5 treat_i + \epsilon_{ij} \quad (8)$$

Market capitalization, *SUF*, and *treat* are equivalent to the variables used in the previous regression on the stock price. The systemic risk, denoted as *Beta*, is estimated using a rolling regression of the market excess return on the stock excess return over a period ranging from 6 months to 2 years prior, and $\sigma_{\epsilon j}$, the idiosyncratic variance, is calculated as the variance of the residuals from the regression. The market variance is σ_m.

Table A25 reveals that the only significant finding is an increase in trade volume for firms with low physical risks in response to bad news, lasting up to two weeks post-event. This suggests that investors react more strongly to negative information when it's less expected for firms considered less vulnerable to external shocks.

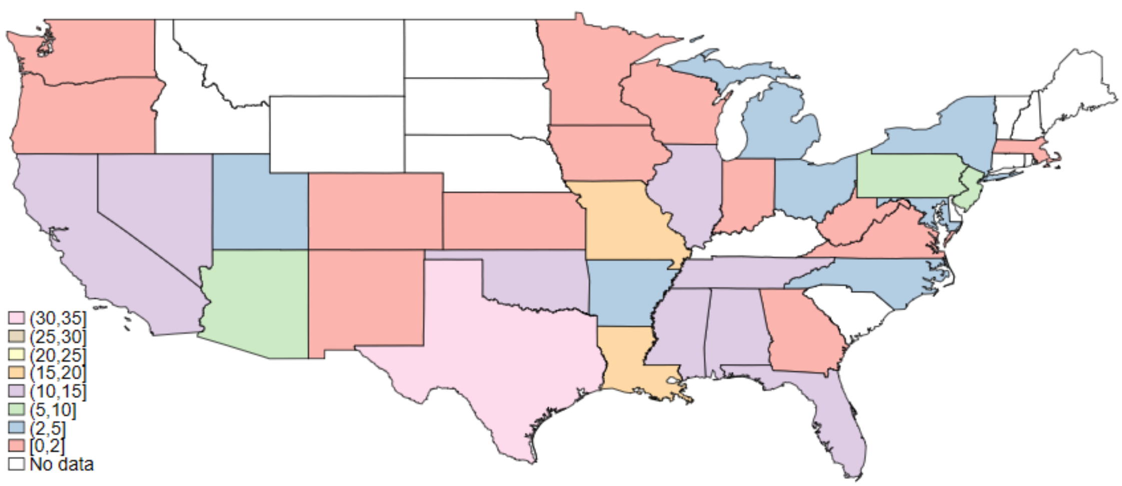
G Appendix Figures

Figure A1: Analysts' location from 1999 to 2020 by state - Full Sample



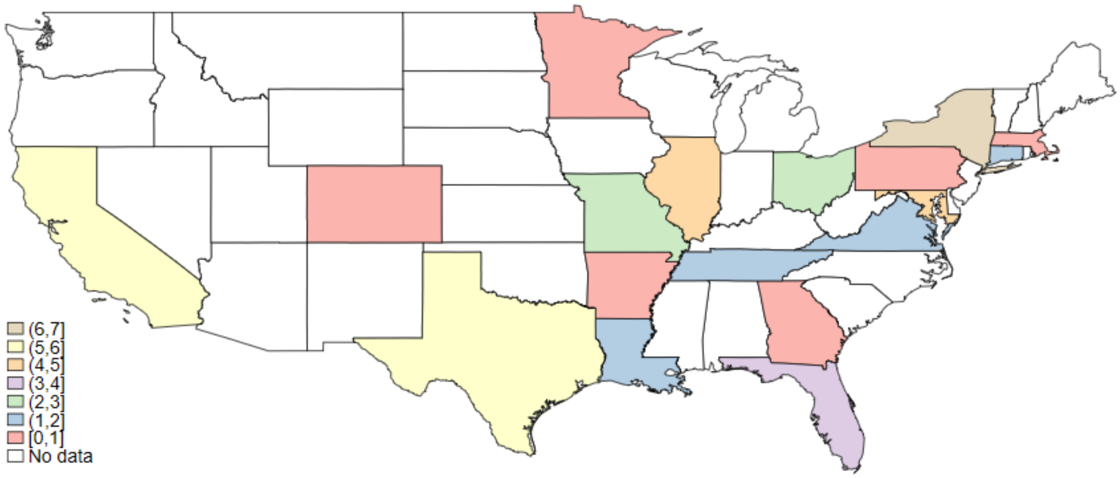
Note: The graph maps the IBES analysts' locations from 1999 to 2020 by US state obtained from Refinitiv and Capital IQ-Professional. Among 2894 analysts, the state of New York has the highest number of analysts with 2017 individuals, followed by California with 235 analysts, 105 analysts in Illinois, and 85 in Massachusetts.

Figure A2: All salient weather events from 1999 to 2020 by state



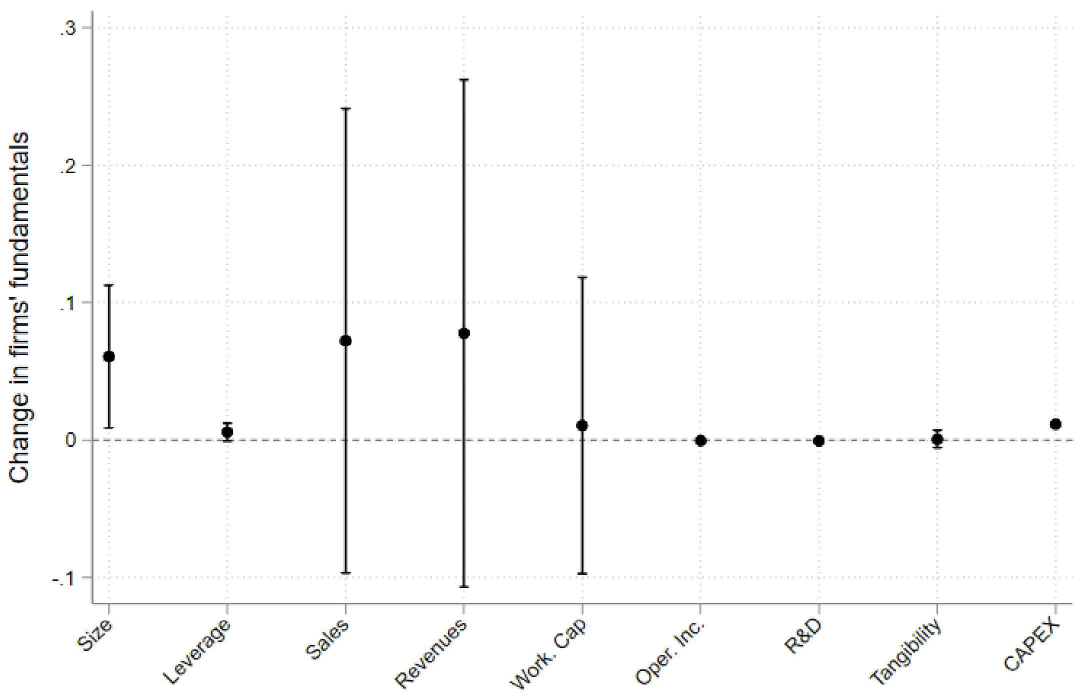
Note: The graph maps the Selected Storm Events from 1999 to 2020 by US state. The state with the highest number of shocks is Texas.

Figure A3: Salient weather events from 1999 to 2020 by state near treated analysts



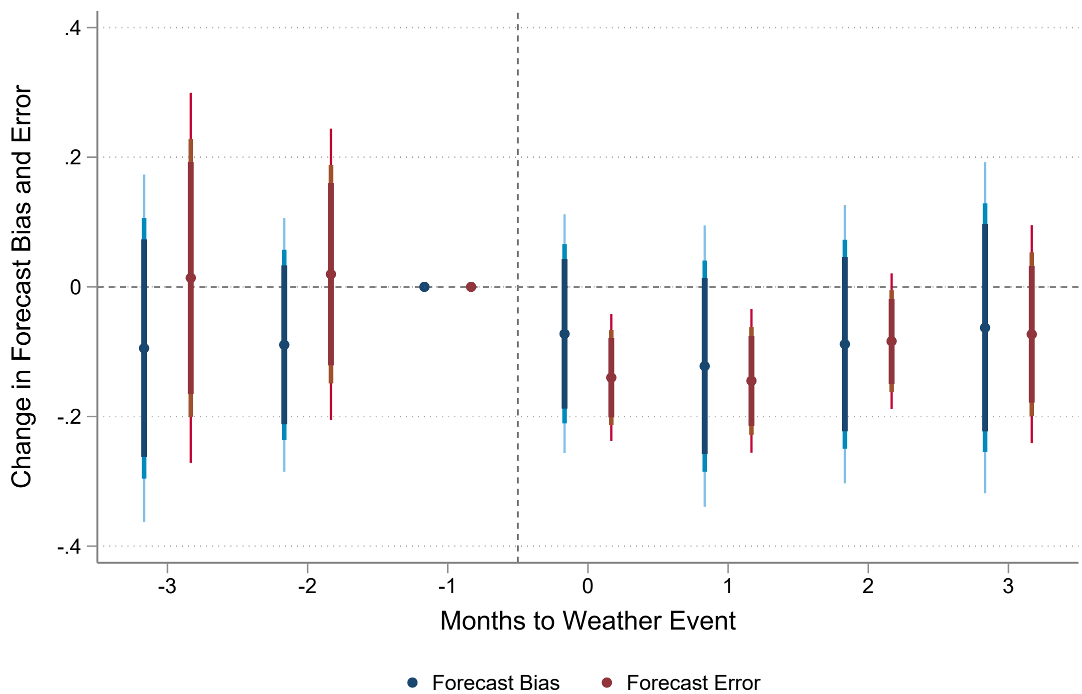
Note: The graph maps the Selected Storm Events from 1999 to 2020 merged to analysts location by US state. Notice that only weather shocks that occur near analysts are reported in the graph. The states with the highest number of shocks are New York, California, and Texas.

Figure A4: Changes in firms' fundamentals around the weather shock



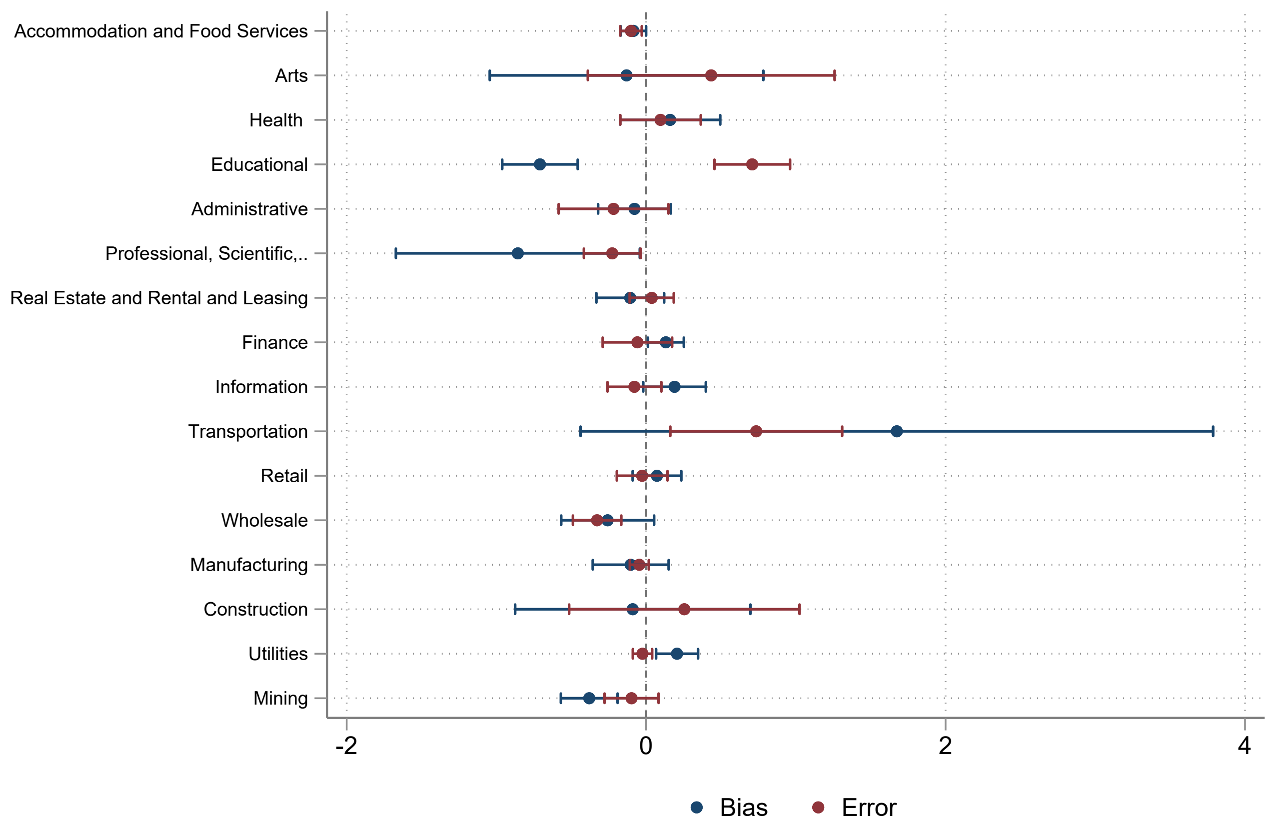
Note: the graph illustrates differences in firms' fundamentals for firms forecasted by the analysts before and after a weather event. The independent variable is an indicator that takes the value of one after the weather event and zero before. Sales, revenue, and working capital are scaled by 1,000 to ensure compatibility and ease of comparison. We selected the first available data for the forecasted firm one quarter before and one quarter after the weather shock. The table includes fixed effects for the weather event.

Figure A5: Parallel trend



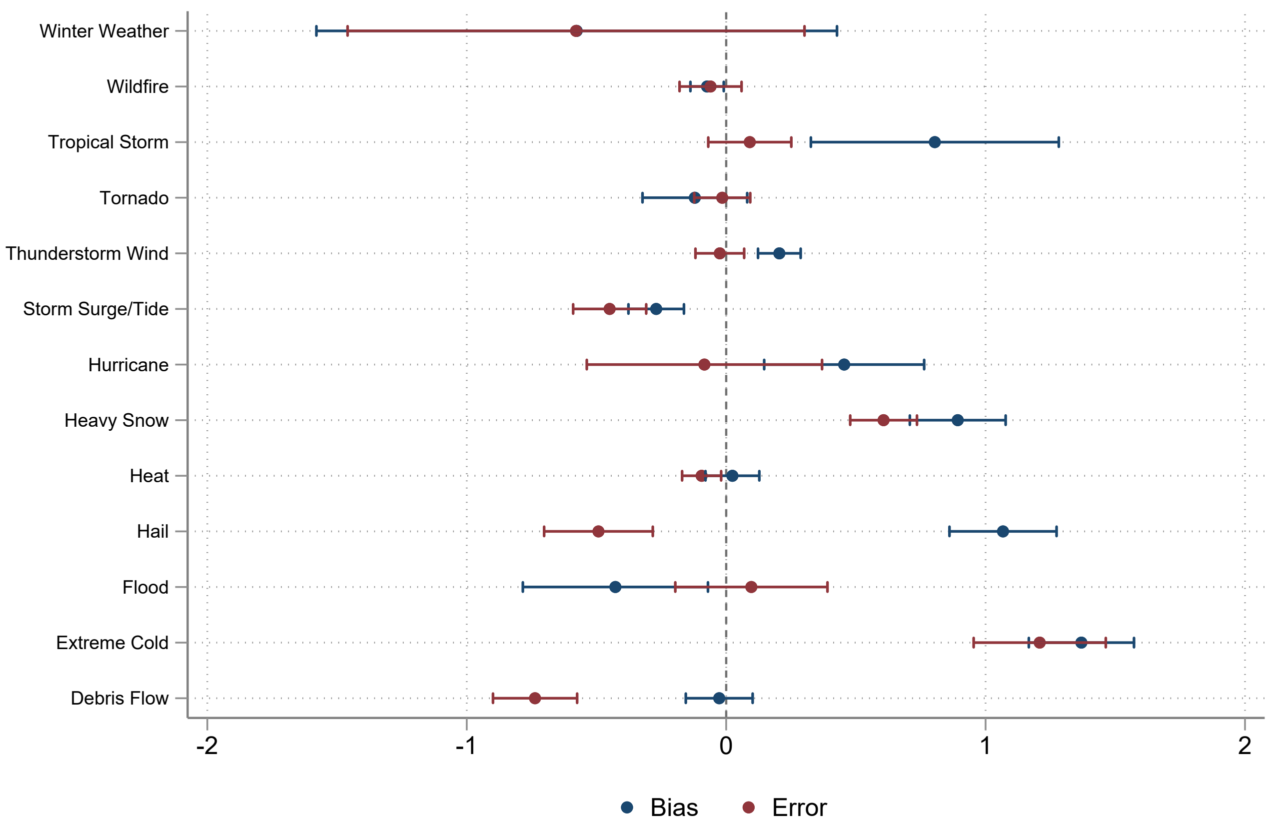
Note: The figures plot the estimated coefficients for the pre- and post-period interactions between treatment (proximity to a weather event) and month indicators for both bias (blue) and error (maroon), along with 99%, 95%, and 90% confidence intervals. The reference point is the month prior to the weather event. The analysis includes all analysts who issue forecasts in each month leading up to the event. The specification incorporates all covariates, with the shock interacted with horizon-fixed effects. The event window spans from 3 months before to 3 months after the event. Standard errors are clustered by the analysts' office location.

Figure A6: Effect on analysts forecasts by firms sector



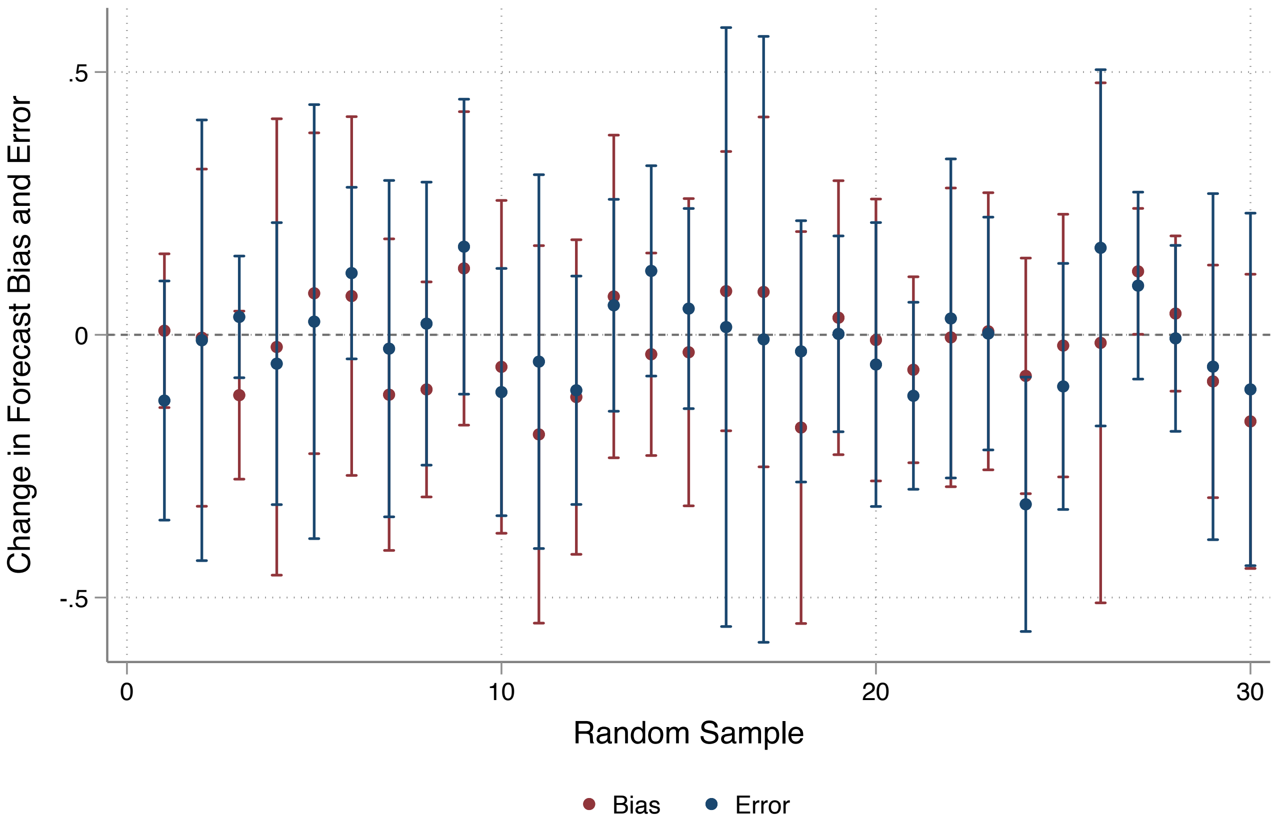
Note: The graph plots the estimated coefficients from the difference in difference in bar plots with 95% confidence intervals for bias (blue) and error (maroon). The DIDs are run separately for each sector. The specification includes all baseline covariates and includes forecasted horizon*weather shock fixed effect. The analysis keeps only one forecast before and after the event. The standard errors are clustered at the analyst's office location.

Figure A7: Effect on analysts forecasts by type of event



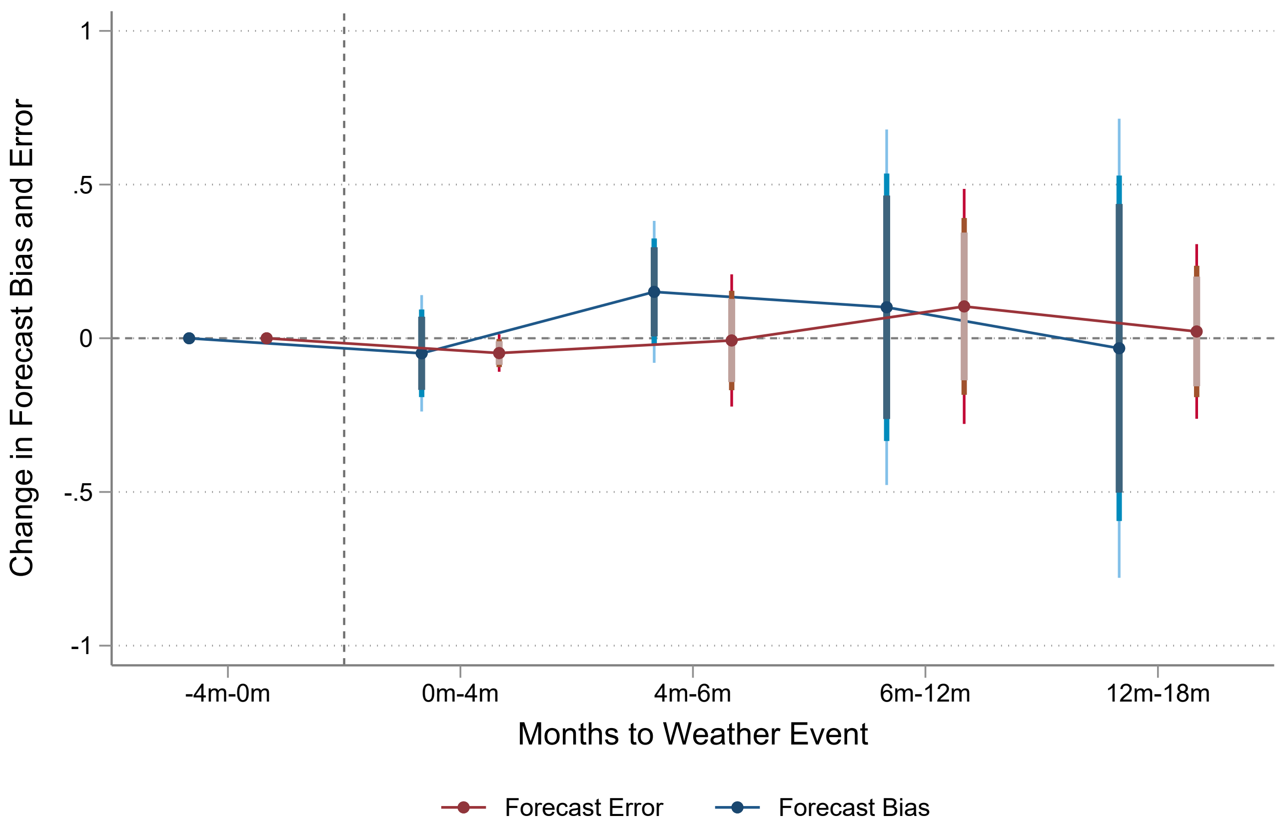
Note: The graph plots the estimated coefficients from the difference in difference in bar plots with 95% confidence intervals for error (black) and bias (maroon). The DIDs are run separately for each event's type. The specification includes all covariates and forecasted horizon*weather shock fixed effect. The analysis keeps only one forecast before and after the event. The standard errors are clustered at the analyst's office location.

Figure A8: Placebo exercise: randomly generated weather shocks date



The figure displays the estimated effect sizes for 30 randomly generated weather event dates, using locations from the actual weather events in the sample. Standard errors are reported at the 5% significance level. The Difference-in-Differences (DID) regression used for these estimates is the baseline regression with controls and fixed effects.

Figure A9: Persistence of the effect after the event (all horizons)



Note: figures plot the estimated coefficients from the staggered difference in difference in bar plots with 99%, 95% and 90% confidence intervals for error (red) and bias (blue). The specification includes all covariates and forecasted horizon*weather shock fixed effect. The analysis keeps only one forecast before and after the event. The standard errors are clustered at the analyst's office location.

H Appendix Tables

Table A1: Summary statistics for the entire dataset

	Mean	p50	SD	Min	Max
forecast bias (%)	0.81	0.04	4.31	-33.08	66.38
forecast error (%)	2.19	0.78	4.14	0.00	83.20
companies followed	17.47	17.00	7.83	1.00	80.00
firm experience	3.47	2.00	3.60	0.00	21.00
general experience	7.45	7.00	5.12	0.00	21.00
Industries followed	2.12	2.00	1.37	1.00	11.00
brokerage size	85.78	70.00	56.00	1.00	284.00
firm size	8.46	8.41	1.93	-0.86	14.83
leverage	0.25	0.22	0.23	0.00	5.10
market value	1.76	1.08	25.26	0.01	12253.26
stock price	50.79	35.94	67.24	0.32	3808.41
ROA	0.01	0.01	0.19	-166.00	10.69
N	1588202				

Note: The table reports the summary statistics for the whole sample of analysts (before matching with weather shocks). Forecast bias is defined as the difference between the earnings forecast of an equity analyst i for a firm f in the month t minus the actual earnings divided by the stock price for a firm f in the previous fiscal year $t-1$, while forecast error differs from forecast bias only by having the numerator in absolute terms. Both are expressed in percentages. See tables 19 and 20 for a description of the variables used.

Table A2: State population belief in global warming and concern after a weather event

	Global warming is happening			Worried about global warming		
	(1)	(2)	(3)	(4)	(5)	(6)
Fatalities	0.365			0.579***		
	(0.242)			(0.198)		
Injuries		0.518			0.265	
		(0.475)			(0.472)	
1 bil. $ damages			0.528*			0.401**
			(0.275)			(0.180)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.966	0.966	0.966	0.978	0.977	0.977
N	561	561	561	561	561	561

Note: I use data from the Yale Climate Opinion Maps 2023 (Howe et al., 2015). The dependent variables 'Is Happening' represent the estimated percentage who think that global warming is happening, and 'Worried' represents the estimated percentage who are somewhat/very worried about global warming, expressed as percentages at the state-year level. Both variables are led by one year. The standard errors are clustered at the state levels, and observations are weighted by each state's population size.

Table A3: Climate news after a weather event

	Sentometrics			WSJ		
	(1)	(2)	(3)	(4)	(5)	(6)
Fatalities	0.0334			-0.0185		
	(0.0401)			(0.0421)		
Injuries		-0.00776			-0.00518	
		(0.0408)			(0.0414)	
1 bil. $ damages			0.0220			-0.0348
			(0.0541)			(0.0556)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	No	No	No
R^2	0.382	0.382	0.382	0.309	0.308	0.309
N	4563	4563	4563	4239	4239	4239

The table reports the estimated coefficients from the regression of the Sentometric index on news about global warming (Ardia et al., 2020) and the Wall Street Journal (WSJ) climate news indices created by Engle et al. (2020). State fixed effects are not included because these indices are constructed at the national level, and unfortunately, I do not have data on local news.

Table A4: Baseline result with FE

	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
post time	0.00858	-0.0350**	-0.0514*	-0.124***	-0.0525*	-0.122***
	(0.0354)	(0.0155)	(0.0292)	(0.0293)	(0.0277)	(0.0276)
treat	-0.377	-0.126	-0.0866	-0.0134	0.258*	-0.373***
	(0.243)	(0.140)	(0.0984)	(0.0504)	(0.148)	(0.113)
treat*post	-0.0587	-0.0721**	-0.0429	-0.0176	-0.0432	-0.0181
	(0.0706)	(0.0293)	(0.0664)	(0.0413)	(0.0585)	(0.0418)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*Horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst*Firm FE	Yes	Yes	No	No	No	No
Year*Firm FE	No	No	Yes	Yes	No	No
Year*State FE	No	No	No	No	Yes	Yes
R^2	0.585	0.653	0.669	0.718	0.283	0.329
N	57576	57576	57274	57274	57573	57573

Note: the table shows the baseline staggered differences-in-differences (DID) for 1 to 5 years EPS forecasts of an analyst i forecasting a firm f. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The table also includes analyst*firm fixed effects, year interacted with firm ID fixed effects, and year interacted with state ID fixed effects. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A5: Baseline result for 1-year horizon with FE

	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
post	0.0155	-0.0424**	0.000445	-0.112***	0.000910	-0.111***
	(0.0250)	(0.0186)	(0.0263)	(0.0310)	(0.0228)	(0.0273)
treat	-0.225**	0.0318	0.0315	0.0345	0.289***	-0.194**
	(0.105)	(0.110)	(0.0427)	(0.0269)	(0.0783)	(0.0762)
treat*post	-0.0598	-0.153***	-0.0621	-0.106**	-0.0696	-0.111**
	(0.0646)	(0.0374)	(0.0683)	(0.0508)	(0.0542)	(0.0446)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst*Firm FE	Yes	Yes	No	No	No	No
Year*Firm FE	No	No	Yes	Yes	No	No
Year*State FE	No	No	No	No	Yes	Yes
R^2	0.694	0.731	0.800	0.827	0.285	0.290
N	30566	30566	29921	29921	30561	30561

Note: the table shows the baseline staggered differences-in-differences (DID) for 1 year EPS forecasts of an analyst i forecasting a firm f. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The table also includes analyst*firm fixed effects, year interacted with firm ID fixed effects, and year interacted with state ID fixed effects. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A6: Heatwave exposure and heterogeneous firm risk (all horizons)

	All		High Heatwave Risk		Low Heatwave Risk	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bias	Error	Bias	Error	Bias	Error
treat*post	0.0799	-0.0346	0.00474	-0.121	0.119***	0.0131
	(0.0553)	(0.0505)	(0.0932)	(0.0885)	(0.0435)	(0.0398)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.35	0.37	0.25	0.30	0.46	0.47
N	12434	12434	4358	4358	8076	8076

The table presents the baseline difference-in-differences for firms with high and low heatwave risks when an analyst experienced a heatwave for all horizons. Firms with high heatwave risk are those with above-median heatwave risks compared to other firms in the sample. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The regression also controls for bias and error from the pre-treatment period period. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A7: Firms' environmental violation

	Environmental Violation		No Environmental Violation	
	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error
treat*post	-0.244***	-0.0461	-0.0200	-0.0799**
	(0.0775)	(0.0438)	(0.0665)	(0.0328)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.250	0.266	0.278	0.327
N	10081	10081	47495	47495

Note: the table shows the baseline difference-in-differences for firms with and without environmental violations in the previous year above 5,000 dollars. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A8: Firms' forecast dispersion

	Low Forecast Dispersion		High Forecast Dispersion	
	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error
treat*post	0.0402	-0.0417	-0.404***	-0.229***
	(0.0580)	(0.0313)	(0.118)	(0.0775)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.361	0.381	0.237	0.266
N	44118	44118	8863	8863

Note: The data is divided using the firms' forecast dispersion. First, I calculate dispersion as the standard deviation of all forecasts made for a firm over a given horizon in a month. Then, in each month, I create quartiles of forecast dispersion. For a given firm, I average the quartiles at the monthly level for the year. Forecast dispersion is classified as high dispersion if it falls into the top quartile of dispersion in the previous year for a given forecast horizon; the bottom quartile includes all the other quartiles. Each specification includes weather shock times horizon fixed effects to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms' experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A9: Firms' climate physical and transition risk

			High Physical Risk				Low Physical Risk			
	All		High Transition Risk		Low Transition Risk		High Transition Risk		Low Transition Risk	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Bias	Error	Bias	Error	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.136**	-0.0507	-0.257***	-0.129**	0.202***	0.177*	-0.231	-0.0774**	-0.0805	-0.159
	(0.0578)	(0.0325)	(0.0520)	(0.0529)	(0.0544)	(0.0906)	(0.152)	(0.0373)	(0.159)	(0.130)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.191	0.221	0.183	0.211	0.239	0.268	0.279	0.293	0.311	0.338
N	29168	29168	13566	13566	6840	6840	6656	6656	2106	2106

Note: the table shows the baseline differences-in-differences for firms in high and low physical and transition risks. The firm's physical risk is a composite score of all the company's physical risk exposure, i.e. wildfire, coldwave, heatwave, hurricane, sea level rise, flood, and water stress (from Trucost Climate Change Physical Risk Data). The score takes values from 1 (lowest risk) to 100 (highest risk). Firms with more (less) than the average physical risk composite score in the sample (i.e. more than 60 points) are defined as high (low) risk. I use Trucost's Unpriced Carbon Cost adjusted EBITDA to gauge transition risks, which compares a company's current carbon expenses with projected future costs. High-risk firms are those in the top tercile of earnings at risk for 2020 across different scenarios (high, medium, low), while the rest are considered non-risky. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A10: Firms' climate physical risk

	All		High Physical Risk		Low Physical Risk	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.0562	-0.0337	-0.0168	-0.0131	-0.150	-0.0824**
	(0.0476)	(0.0507)	(0.0382)	(0.0765)	(0.136)	(0.0355)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.203	0.256	0.190	0.264	0.275	0.287
N	35314	35314	25112	25112	10202	10202

Note: the table shows the baseline differences-in-differences for firms in high and low composite climate physical risk. The firm's physical risk is a composite score of all the company's physical risk exposure, i.e. wildfire, coldwave, heatwave, hurricane, sea level rise, flood, and water stress (from Trucost Climate Change Physical Risk Data). The score takes values from 1 (lowest risk) to 100 (highest risk). Firms with more (less) than the average physical risk composite score in the sample (i.e. more than 60 points) are defined as high (low) risk. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A11: Firms' climate transition risk proxied by earnings at risk

	All		High Transition Risk		Low Transition Risk	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bias	Error	Bias	Error	Bias	Error
treat*post	-0.137**	-0.0528	-0.247***	-0.114***	0.127**	0.0926
	(0.0577)	(0.0332)	(0.0574)	(0.0344)	(0.0619)	(0.0719)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.192	0.222	0.202	0.218	0.234	0.266
N	29586	29586	20552	20552	9034	9034

Note: the table shows the baseline differences-in-differences for firms in high and low climate transition risk. I use Trucost's Unpriced Carbon Cost adjusted EBITDA to gauge transition risks, which compares a company's current carbon expenses with projected future costs. High-risk firms are those in the top tercile of earnings at risk for 2020 across different scenarios (high, medium, low), while the rest are considered non-risky. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A12: Transition risk proxied by carbon emission

Firm's Emissions	All		Top Tercile		Bottom Tercile	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
treat*post	-0.0386	0.0363	-0.242***	-0.0877*	0.233***	0.194*
	(0.0623)	(0.0460)	(0.0800)	(0.0462)	(0.0589)	(0.100)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.266	0.322	0.295	0.335	0.297	0.375
N	13638	13638	7074	7074	6564	6564

Note: the table shows the baseline differences-in-differences for firms in high and low transition risks. High transition risk firms are defined as those in the top tercile of MSCI absolute scope 1 emissions data, while all others are classified as low risk. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A13: Performance

	Firm Performance				Sector Performance			
	High		Low		High		Low	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error	(7) Bias	(8) Error
treat*post	0.146**	0.0583	-0.100	-0.106**	-0.0673	-0.113**	0.0380	-0.0992
	(0.0693)	(0.0584)	(0.0669)	(0.0461)	(0.0541)	(0.0439)	(0.0959)	(0.0757)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Shock*Hor FE	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.340	0.388	0.252	0.296	0.259	0.311	0.280	0.309
N	11503	11503	46073	46073	31259	31259	19059	19059

Note: the table shows the baseline difference-in-differences for high and low performance analysts constructed following Hong et al. (2000). Firm (sector) performance is constructed using the top analysts regarding forecast error for a firm (sector). The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A14: Lead and Follower

	Firm Lead-Follower				Sector Lead-Follower			
	Lead		Follower		Lead		Follower	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error	(7) Bias	(8) Error
treat*post	0.157*	-0.0667	-0.0669	-0.0998***	-0.0769	-0.239**	-0.0218	-0.0724**
	(0.0815)	(0.0877)	(0.0658)	(0.0309)	(0.112)	(0.0832)	(0.0654)	(0.0336)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Shock*Hor FE	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.263	0.130	0.298	0.126	0.277	0.160	0.291	0.124
N	8005	8005	33656	33656	4671	4671	36989	36989

Note: the table shows the baseline difference-in-differences for lead-follower analysts constructed at the firm and sector level following Cooper et al. (2001). The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A15: Analysts gender, mindset and political donation

Analyst's gender	Female		Male	
	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error
treat*post	0.0590	0.0145	-0.0946	-0.101***
	(0.134)	(0.108)	(0.0721)	(0.0324)
R^2	0.290	0.375	0.276	0.317
N	4926	4926	49716	49716
Analyst's mindset	Pessimistic		Optimisitc	
	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error
treat*post	-0.102*	-0.143***	-0.0324	-0.0302
	(0.0562)	(0.0410)	(0.0903)	(0.0300)
R^2	0.219	0.255	0.312	0.356
N	21799	21799	35777	35777
Analyst's donation	Democratic		Republican	
	(1)	(2)	(3)	(4)
	Bias	Error	Bias	Error
treat*post	-0.0477	-0.510***	0.207	-0.0182
	(0.0848)	(0.0362)	(0.135)	(0.128)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.333	0.338	0.576	0.642
N	4554	4554	2408	2408

Note: The table presents the baseline difference-in-differences estimates for analysts' gender, mindset, and political donations. Gender is inferred from the analyst's first name and categorized as either female or male. Mindset is determined by calculating the average bias in the previous year and classifying analysts into the top tercile of pessimism (where the top tercile represents the most pessimistic analysts) while all others are categorized as optimistic. Similar results are obtained when defining optimists as only those in the top tercile of optimism. Political donation data is compiled by merging individual donations to political parties from the Federal Election Commission (FEC) database. The weather shock indicator and the horizon fixed effect are incorporated to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A16: Socially connected analysts

Social Connectedness Index	High		Low	
	(1) Bias	(2) Error	(3) Bias	(4) Error
treat*post	-0.0944	-0.103***	-0.0305	-0.0439
	(0.0679)	(0.0326)	(0.0678)	(0.0384)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.268	0.305	0.302	0.348
N	33992	33992	28512	28512

Note: The table presents the baseline differences-in-differences results for analysts in the treated group, categorized by their level of US counties' Social Connectedness Index (columns 1-2 for socially connected and columns 3-4 for not socially connected). The Social Connectedness Index (SCI), obtained from Bailey et al. (2018), representing the relative probability of a Facebook friendship connection between individuals in two locations. For each treated analyst's county, high connection is defined as being in the top decile of the SCI, while low connection includes all others. The results are shown for treated analysts with control analysts in socially connected cities (columns 1-2) and those without control analysts in socially connected cities (columns 3-4). The weather shock indicator and horizon fixed effects are included to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, number of companies followed, firm experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the office location.

Table A17: Geographical distance of analysts from forecasted firms

	Companies Near Analysts		Companies Distant from Analysts	
	(1) Bias	(2) Error	(3) Bias	(4) Error
treat*post	-0.0682	-0.117***	-0.0334	-0.0237
	(0.0638)	(0.0420)	(0.0765)	(0.0509)
Controls	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes
R^2	0.261	0.293	0.278	0.338
N	28823	28823	28753	28753

Note: The table shows the baseline differences-in-differences results, broken down by the geographical distance between firms and analysts. *Companies Near Analysts* are within 752 miles of the analysts, which is the median distance, while *Companies Distant from Analysts* are farther than the median. The results remain consistent when breaking down the distance by quartiles, comparing the 1st-2nd quartiles to the 3rd-4th quartiles. The weather shock indicator and horizon fixed effects are included to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, number of companies followed, firm experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the office location.

Table A18: Analysts' target price forecast error

	Price Target Forecast Error				
	(1)	(2)	(3)	(4)	(5)
post	-0.0135***	-0.0134***	-0.0134***	-0.0134***	-0.0133***
	(0.00272)	(0.00271)	(0.00283)	(0.00283)	(0.00268)
treat	0.00118	0.0101	0.0203*	0.0192**	0.00921
	(0.0122)	(0.0116)	(0.0112)	(0.00733)	(0.0123)
post*treat	-0.0246***	-0.0246***	-0.0246***	-0.0249***	-0.0247***
	(0.00514)	(0.00512)	(0.00530)	(0.00530)	(0.00511)
Controls	No	Yes	Yes	Yes	Yes
Shock FE	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	No	Yes	No	No
Firm FE	No	No	No	Yes	No
Brokerage FE	No	No	No	No	Yes
R^2	0.195	0.214	0.381	0.564	0.235
N	46858	46846	46846	46846	46845

Note: The table reports the estimated coefficients for the baseline regression using analyst recommendations as the dependent variable. The dependent variable is constructed as the logarithmic return of stock i (the price in 12 months divided by the price today) minus the logarithmic implied return of stock i (the target price of stock issued by analyst with a 12-month forecast horizon divided by the current price of stock), all in absolute terms. The last columns also include a triple interaction for firms in high climate sectors, high physical risk, and high transition risks. Each specification includes weather shock indicator fixed effects (FE). I also include analyst FE, firm FE, and brokerage FE. The controls used are broker size, companies followed, firm experience, industries followed, and firm size. The standard errors are clustered at the office location.

Table A19: Analysts' recommendation

	Analyst Recommendation				
	(1)	(2)	(3)	(4)	(5)
post	-0.0896	-0.0895	-0.0925	-0.0903	-0.0942
	(0.0645)	(0.0645)	(0.0693)	(0.0732)	(0.0677)
treat	0.0654	0.0926	-0.0183	0.131**	0.105
	(0.0736)	(0.0725)	(0.0897)	(0.0564)	(0.0665)
post*treat	-0.0419	-0.0424	-0.0322	-0.0401	-0.0342
	(0.0623)	(0.0626)	(0.0668)	(0.0713)	(0.0626)
Controls	No	Yes	Yes	Yes	Yes
Shock FE	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	No	Yes	No	No
Firm FE	No	No	No	Yes	No
Brokerage FE	No	No	No	No	Yes
R^2	0.03	0.03	0.34	0.40	0.11
N	2728	2728	2728	2728	2727

Note: The table reports the estimated coefficients for the baseline regression using analyst target price forecast error as the dependent variable. A higher recommendation is better, with 5 = strong buy, 4 = buy, 3 = hold, 2 = sell, and 1 = strong sell. The last columns also include a triple interaction for firms in high climate sectors, high physical risk, and high transition risks. Each specification includes weather shock indicator fixed effects (FE). I also include analyst FE, firm FE, and brokerage FE. The controls used are broker size, companies followed, firm experience, industries followed, and firm size. The standard errors are clustered at the office location.

Table A20: Firms followed by brokerage firms

	1(High Market Cap) (1)	1(High PR) (2)	1(High TR) (3)	1(High Market Cap) (4)	1(High PR) (5)	1(High TR) (6)
N. treated firms	0.00563	-0.00901*	0.00636	0.00510	0.000707	-0.00365
	(0.00411)	(0.00530)	(0.00401)	(0.00358)	(0.00347)	(0.00283)
N. treated analyst	0.00666*	-0.0000281	0.0133***	0.00510	-0.000487	0.00463*
	(0.00384)	(0.00439)	(0.00403)	(0.00374)	(0.00332)	(0.00275)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage FE	No	No	No	Yes	Yes	Yes
R^2	0.412	0.0585	0.375	0.418	0.0652	0.382
N	124592	124592	124592	124592	124592	124592

Note: The table reports the linear probability model of a brokerage firm following a company with a high market cap, high physical risk, and high transition risk, based on the number of analysts and firms near an event in the previous year. Control variables include ROA, stock price, market value, leverage, number of analysts covering the company, firm size, and sales from the previous year. Standard errors are clustered at the firm level.

Table A21: Firms' climate-sector risk

Definition	Addoum et al. (2020)				Addoum et al. (2020) and Choi et al. (2020)			
Climate Sector Risk	High		Low		High		Low	
	(5) Bias	(6) Error	(7) Bias	(8) Error	(9) Bias	(10) Error	(11) Bias	(12) Error
treat*post	-0.0822	-0.0625**	-0.0398	-0.0829*	-0.102	-0.0607**	0.0305	-0.0971*
	(0.109)	(0.0294)	(0.0399)	(0.0430)	(0.0864)	(0.0249)	(0.0239)	(0.0488)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.178	0.239	0.461	0.488	0.205	0.264	0.518	0.527
N	36090	36090	21486	21486	41382	41382	16194	16194

The table presents the baseline differences-in-differences estimates for firms with high and low climate sector risks. The first four columns use the Addoum et al. (2020) climate sector classification, which categorizes the following industries as low risk: mining, retail, transport, information, finance, real estate, professional services, administration, education, and arts. High-risk industries include utilities, construction, manufacturing, wholesale, health, and accommodation. The last four columns apply a combined classification from both Choi et al. (2020) and Addoum et al. (2020), where industries are categorized as follows: low-risk sectors include retail, information, finance, real estate, professional services, administration, education, and arts, while high-risk sectors include mining, utilities, construction, manufacturing, wholesale, transport, health, and accommodation. The weather shock indicator and horizon fixed effects are included to account for shock and horizon-specific characteristics. The controls used in the model are forecast days gap, broker size, number of companies followed, analyst experience, number of industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the office location.

Table A22: Distant Analysts & Skilled Near Analysts

Panel A	Treated Experienced Analyst			
	(1) Consensus	(2) Dispersion	(3) Bias	(4) Error
1(treated analyst)*post	-0.295	-0.0138	-0.140	0.282***
	(0.214)	(0.0402)	(0.125)	(0.101)
Controls	Y	Y	Y	Y
Shock*horizon*Sector FE	Y	Y	Y	Y
R^2	0.432	0.290	0.207	0.317
N	10477	10477	10477	10477
Panel B	**Treated Top Performance Analyst**			
	(1) Consensus	(2) Dispersion	(3) Bias	(4) Error
1(treated analyst)*post	0.118	-0.0302**	-0.243**	-0.145
	(0.155)	(0.0146)	(0.115)	(0.119)
Controls	Y	Y	Y	Y
Shock*horizon*Sector FE	Y	Y	Y	Y
R^2	0.437	0.295	0.206	0.317
N	10640	10640	10640	10640
Panel C	**Treated Lead Analyst**			
	(1) Consensus	(2) Dispersion	(3) Bias	(4) Error
1(treated analyst)*post	0.532	0.0148	-0.319**	-0.00883
	(0.361)	(0.0180)	(0.130)	(0.117)
Controls	Y	Y	Y	Y
Shock*horizon*Sector FE	Y	Y	Y	Y
R^2	0.440	0.290	0.204	0.308
N	10007	10007	10007	10007

: The table presents the baseline staggered difference-in-differences for the aggregate effect on consensus, dispersion, bias, and error at the company level for analysts distant from the event. *1(one analyst near)* takes value one for companies at least one analyst near the event, and treated analysts are those forecasting the company but distant from the event. Panel A reports results for analysts who have experienced a weather event, Panel B focuses on top-performing analysts, and Panel C highlights results for lead analysts. Control analysts are those without any treated analysts near the event. Treated and control companies are required to be in the same sector, state, shock event, and forecast horizon, and are matched based on coverage, sales, firm size, leverage, operating income, ROA, stock price, and market value. The control variables include company coverage, firm size, leverage, sales, and operating income. The dependent variables are multiplied by 100 for interpretability. Standard errors are clustered at the state level and at the office location.

Table A23: Robustness: firms' business location

Firm business location	All		= shock's state		≠ shock's state	
	(1) Bias	(2) Error	(3) Bias	(4) Error	(5) Bias	(6) Error
treat*post	-0.0587	-0.0721**	-0.118	-0.136***	-0.0127	-0.0299
	(0.0658)	(0.0273)	(0.0862)	(0.0342)	(0.0620)	(0.0429)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Shock*horizon FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.262	0.303	0.362	0.392	0.204	0.255
N	57586	57586	22138	22138	35448	35448

Note: The data is divided using the firm's business location index developed by Garcia and Norli (2012), which counts the number of times a firm mentions having at least one business location in a particular state within a year in their 10-k filing. The results are reported by = *shock's state* focus on firms that mention the same state as the weather shocks at least once a year, ≠ *shock's state* examine firms that do not mention the state as the weather shock. Each specification includes weather shock times horizon fixed effect to account for shock and horizon-specific characteristics. The controls used are forecast days gap, broker size, companies followed, firms experience, industries followed, and firm size. The dependent variables are multiplied by 100 for interpretability purposes. The standard errors are clustered at the office location.

Table A24: Effect of forecast revision on stock return

	All	Good	Bad	Good & LP	Good & HP	Bad & LP	Bad & HP
				AAR [-1,+1]			
treat	-0.0197	-0.252**	0.387***	-0.148	-0.256	0.388	0.227
	(0.0761)	(0.124)	(0.134)	(0.219)	(0.238)	(0.277)	(0.179)
R^2	0.0565	0.0419	0.0732	0.119	0.0541	0.168	0.0786
N	29517	10035	9625	2056	4640	1647	3912
				AAR [2,15]			
treat	-0.0127	0.0147	-0.0289	-0.0567	0.0537	-0.059	0.075
	(0.0181)	(0.0257)	(0.0347)	(0.0437)	(0.0342)	(0.0741)	(0.0456)
R^2	0.0267	0.0502	0.037	0.154	0.0906	0.173	0.0752
N	29959	10200	9740	2100	4741	1680	3955
				AAR [2,30]			
treat	-0.00344	0.0284	-0.0204	0.00498	0.0585**	-0.0506	0.0117
	(0.0123)	(0.0183)	(0.0229)	(0.0326)	(0.0245)	(0.0495)	(0.0308)
R^2	0.0352	0.057	0.05	0.121	0.117	0.161	0.102
N	29959	10200	9740	2100	4741	1680	3955
				AAR [2,62]			
treat	0.0101	0.0279**	-0.00854	-0.00521	0.0410**	-0.0218	-0.0456**
	(0.00843)	(0.0134)	(0.0149)	(0.023)	(0.0168)	(0.031)	(0.0207)
R^2	0.0513	0.0804	0.0659	0.153	0.144	0.156	0.107
N	29959	10200	9740	2100	4741	1680	3955

Note: The table reports the estimated coefficient for the Malloy (2005) regression of forecast revision on stock price. The regression is segmented by analysts' signal—classified as good if the forecast revision is above the previous forecast and consensus, and bad if below. Additionally, the regression is divided by a firm's climate physical risks: high (HP) if above average, and low (LP) if below average, as per the composite physical risk measure. The coefficient of interest is the variable *treat* equals 1 if the analyst has experienced at least one significant weather event. The table includes month-year fixed effects. It also includes covariates such as market capitalization and the standardize value of the forecast revision. The standard errors are clustered at the firm level.

Table A25: Analysts' revision and trade volume

	All	Good	Bad	Good & LP	Good & HP	Bad & LP	Bad & HP
				CAV [+1,+8]			
treat	-0.0710	-0.130	-0.0446	0.0461	-0.118	0.360*	0.0235
	(0.0662)	(0.104)	(0.111)	(0.243)	(0.144)	(0.215)	(0.145)
R^2	0.0526	0.0713	0.0670	0.126	0.0941	0.188	0.108
N	29480	10074	9553	2086	4708	1671	3905
				CAV [+2,+15]			
treat	0.0904	0.0626	0.0587	0.614	0.103	0.677**	0.183
	(0.0854)	(0.150)	(0.136)	(0.479)	(0.165)	(0.274)	(0.197)
R^2	0.0805	0.100	0.0824	0.138	0.124	0.164	0.138
N	29480	10074	9553	2086	4708	1671	3905
				CAV [+2,+21]			
treat	0.113	0.0228	0.112	0.692	0.180	0.480	0.268
	(0.102)	(0.176)	(0.167)	(0.544)	(0.201)	(0.330)	(0.240)
R^2	0.0930	0.113	0.0898	0.154	0.131	0.180	0.153
N	29480	10074	9553	2086	4708	1671	3905
				CAV [+2,+40]			
treat	-0.0323	-0.268	0.0799	0.510	0.0161	0.0869	0.139
	(0.145)	(0.250)	(0.241)	(0.641)	(0.339)	(0.488)	(0.343)
R^2	0.113	0.133	0.108	0.180	0.169	0.196	0.184
N	29480	10074	9553	2086	4708	1671	3905
month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports the estimated coefficient for the Michaely and Vila (1996) regression of forecast revision on trade volume. The dependent variable is cumulative abnormal trade volume for the period of interest. The regression is segmented by analysts' signal—classified as good if the forecast revision is above the previous forecast and consensus, and bad if below. Additionally, the regression is divided by a firm's climate physical risks: high (HP) if above average, and low (LP) if below average, as per the composite physical risk measure. The coefficient of interest is the variable *treat* equals 1 if the analyst has experienced at least one significant weather event. The table includes month-year fixed effects. It also includes covariates such as market capitalization, the standardize value of the forecast revision, market beta and idiosyncratic variance divided by the market variance. The standard errors are clustered at the firm level.

Table A26: Earnings Calls' Dictionary: lists of unigrams and bigrams used to identify climate-related questions.

List Name	**Unigrams/Bigrams**
Unigram Physical Risk	hurricane, heat, storm, flood, wildfire, heatwave, tornado, hail, tide
Bigram Physical Risk	global warm, global warming, climate change, natural hazard, warm climate, coastal area, snow ice, sea level, storm water, heavy snow, water scarcity, thunderstorm wind, winter weather, extreme cold, excessive heat, wind chill, winter storm, debris flow, storm surge, flash flood, tropical storm
Bigram Physical Risk (Sautner et al., 2023)	coastal area, global warm, snow ice, friendly product, forest land, area florida, sea level, provide water, nickel metal, storm water, heavy snow, air water, natural hazard, sea water, warm climate, water discharge, ice product, security energy, water act, management district, weather snow, service reliable, management water, ability party, hurricane, flood, wildfire, heatwave, ice control, inland area, non coastal, storm january, sale forest, value forest, land forest, particularly coastal, golf ground, especially coastal, sewer overflow, combine sewer, area coastal, large desalination, plant algeria, warm product, solution act, fluorine product, area inland, fight global, sell forest, exposure coastal, city coastal, marina east, day desalination, snow storm, typhoon, heat

Bigram Opportunity Risk (Sautner et al., 2023)	renewable energy, electric vehicle, clean energy, new energy, wind power, wind energy, solar energy, plug hybrid, heat power, renewable resource, solar farm, battery electric, electric hybrid, reinvestment act, issue rfp, construction megawatt, rooftop solar, grid power, recovery reinvestment, solar generation, energy standard, sustainable energy, vehicle charge, guangdong province, hybrid car, charge infrastructure, micro grid, grid connect, clean efficient, carbon free, hybrid technology, generation renewable, energy wind, battery charge, gas clean, vehicle lot, vehicle place, meet energy, vehicle type, vehicle future, energy commitment, electronic consumer, expand energy, gigawatt install, bus truck, ton waste, energy research, focus renewable, pure electric, ev charge, grid technology, geothermal power, type energy, solar program, vehicle development, energy important, install solar, vehicle battery, energy vehicle, energy bring, vehicle space, opportunity clean, demand wind, vehicle good, medical electronic, incremental content, supply industrial, energy target, term electric, power world, vehicle small, renewable electricity, wave power, carbon neutral, auction new, cost renewable, vehicle talk, vehicle offer, customer clean, power solar, vehicle opportunity, community solar, energy goal, vehicle hybrid, invest renewable, incorporate advance, talk solar, ton carbon, small hydro, base solar, target gigawatt, charge network, capacity generation, vehicle add, vehicle infrastructure, solar array, energy auction, product hybrid, product solar, exist wind

Bigram Regulatory Risk (Sautner et al., 2023)	greenhouse gas, reduce emission, carbon emission, carbon dioxide, gas emission, air pollution, reduce carbon, energy regulatory, carbon tax, carbon price, environmental standard, nox emission, emission trade, dioxide emission, epa regulation, energy independence, carbon reduction, know clean, standard requirement, development renewable, carbon market, trade scheme, deliver clean, mercury emission, reduce air, save technology, talk clean, energy alternative, place energy, reduce nox, air resource, target energy, change climate, impact climate, issue air, promote energy, emission free, implement energy, recovery pollution, control regulation, florida department, commission license, gas regulation, appeal district, source electricity, effective energy, nitrous oxide, impact clean, think carbon, global climate, produce carbon, clean job, efficient natural, emission monitor, emission issue, quality permit, product carbon, china air, reduce sulfur, available control, emission rate, regulation low, capture sequestration, nation energy, emission year, efficient combine, carbon economy, comply environmental, glacier hill, hill wind, nox sox, tax australia, way comply, emission intensity, oxide emission, emission improve, emission increase, install low, commission public, castle peak, capture carbon, wait commission, emission compare, clean electricity, high hydrocarbon, emission come, weight fuel, stability reserve, quality regulation, request public, additive process, gas carbon, epa requirement, liter diesel, meet reduction, talk climate, expect carbon, emission ton, ambient air, know carbon