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The spillover effect of natural disaster on analyst forecast inaccuracy: Evidence from shared analyst coverage

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ABSTRACT

This paper studies the spillover effect of natural disasters on analyst forecast performance for unaffected firms. Controlling for analyst, firm, and brokerage characteristics, we find that analysts, who track disaster-stricken firms, issue inaccurate forecasts for unaffected firms due to limited attention. Our analysis further reveals that analysts are more likely to issue inaccurate forecasts when they have less experience and when the impact of natural disasters is more severe. These findings show how natural disasters can alter the information output of analysts within the shared analyst-coverage network.

1. Introduction

Investors, academics, and regulators are increasingly concerned about the economic impact of natural disasters. The Global Assessment Report, 2022 (GAR, 2022), released by the United Nations Office for Disaster Risk Reduction (UNISDR), states that natural disasters cause an average of 90,000 deaths annually. A growing body of research explores the effects of natural disasters on various aspects of business, including firms' operational efficiency (Hsu et al., 2018), supply chains (Barrot & Sauvagnat, 2016), cash reserves (Dessaint & Matray, 2017), mutual fund stock portfolios (Alok et al., 2020), and analyst information production (Bourveau & Law, 2021; Han et al., 2024). Analysts are crucial to capital markets, as they collect, analyze, interpret, and disseminate information, forecast earnings and prices, and make stock recommendations for companies and industries (e.g., Givoly & Lakonishok, 1979; Lys & Sohn, 1990; Stickel, 1991). However, our understanding is limited regarding the potential spillover effects of natural disasters on analyst information production for other unaffected firms.

Analysts are often believed to possess specific informational expertise to make investment recommendations (Caylor et al., 2017; Wang, 2019). Brown et al. (2015) find that analysts allocate tremendous effort in acquiring information, through private phone calls with management, conference calls, road shows, site visits, and hosting an investor day, etc. However, biased analyst forecasts can lead

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to more forecast revisions and affect analysts' market influence (Keskek & Tse, 2018). Although natural disasters have become more frequent in recent decades, and both the costs and death associated with them have increased, research on the impact of natural disasters on capital markets remains limited. Given the vital role analysts play as information intermediaries in capital markets, we aim to uncover whether natural disasters lead to spillover effect that causes analysts to produce inaccurate forecasts for unaffected firms. While existing literature has documented the spillover effect based on proximity (e.g., Han et al., 2024), our focus is on the spillover effect from shared analyst coverage.

An analyst's coverage of specific firms can indirectly influence the information generation of other companies within the analyst's coverage domain. An analyst's information production should depend not only on her own characteristics but also on those of the other stocks she follows. Ali and Hirshleifer (2020) demonstrate that shared analysts help expedite the impounding of information between linked firms. Prior literature indicates that disaster shocks can be transmitted through common geographical locations (Dessaint & Matray, 2017), and supply-chain links (Barrot & Sauvagnat, 2016).[1] Unlike these studies, we investigate whether an analyst's forecast accuracy for unaffected firms is impacted when a natural disaster strikes another firm they cover.[2]

There are two potential channels through which natural disasters can affect analysts' information production for unaffected firms. First, a natural disaster is a salient event that may require analysts to dedicate more time and attention to investigate its actual impact on affected firms. Consequently, this may leave them with less time to focus on unaffected firms. Analysts' attention will be greatly affected during disasters, and the lack of attention leads to reduced accuracy of their forecasts not only for affected firms but also for unaffected peer firms. Continuous coverage of disaster-affected firms requires tremendous time and effort because analysts must respond to constant client inquiries for information about the affected companies. This channel is supported by Klibanoff et al. (1998) and Daniel et al. (2002), who show that humans have limited time and capacity to process information. Previous research has documented limited attention among individual and institutional investors, fund managers, and board of directors (Da et al., 2011; Ben-Rephael et al., 2017; Brown et al., 2018; Masulis & Zhang, 2019). Similarly, financial analysts have also been found to be affected (Driskill et al., 2020). We label this phenomenon as the "limited attention" hypothesis.

Second, analysts may provide less accurate forecasts for unaffected firms due to changes in their risk perceptions. Hirshleifer (2001) argues that salience and availability effects cause humans to focus on information that stands out or is being repeated. Due to the availability heuristic, people tend to overestimate the frequency of salient events until the local context changes, as these events come to mind more easily. Previous studies find that a firm's risk change can spill over into other firms, especially for firms that belong to the same industry or are located in the same region (Barrot & Sauvagnat, 2016; Beatty et al., 2013; Dessaint & Matray, 2017). Therefore, when analysts become aware of the affected firms' risk following the natural disaster, they may perceive a higher likelihood that unaffected firms will be impacted by future natural disasters. We label this as the "perceived risk hypothesis."

Conversely, it is also possible that analysts do not change the quality of their forecasts for unaffected firms because they might drop the disaster-impacted firms or other firms in their portfolio to ensure high-quality coverage. Moreover, as experts in acquiring information from multiple sources, analysts may correct their behavioral biases periodically. If this is the case, we should not observe a spillover effect. Therefore, it is important to empirically investigate whether a spillover effect from disasters through shared analyst coverage exists.

We employ a sample of all major natural disasters in the U.S. between 1992 and 2017 to investigate the effect of disasters on analysts' information production under a staggered difference-in-differences (DID) framework. We restrict our sample to four quarters before and after the disaster event to capture the short-term transient shock of natural disasters. The staggered nature of natural disaster shocks provides a unique opportunity to identify causal relationships.

Our empirical results show that analysts who follow other disaster-stricken firms issue inaccurate forecasts towards those unaffected firms after controlling for firm, year-quarter, analyst, and brokerage fixed effects, compared with analysts who do not follow disaster-stricken firms. Our estimates indicate that analyst forecast inaccuracy increases by about 4.5% following the disaster event. To confirm the parallel trend assumption in the DID design, we conduct a dynamic analysis that examines the spillover effect of natural disasters on analyst forecast performance for unaffected firms around the disaster-year-quarter. We find that there are no differential trends between the treated and the control firms before the event. We also present a battery of robustness tests to show that our main results are robust when we use alternative samples, different fixed effects, alternative measures for key variables, and alternative research designs.

Next, we seek to provide supportive evidences on the mechanisms through which the spillover effect manifests. First, we find that analysts pay less attention to, and take longer time to update information for firms that are not directly affected by a natural disaster. In contrast, analysts who cover firms impacted by the disaster allocate more attention to these affected firms. Additionally, we observe an increase in institutional investor attention and news coverage for disaster-affected firms following the event, which further supports the rationale why analysts focus more on firms directly impacted by disasters. Second, we rule out the perceived risk channel. We analyzed the direction of analyst forecasts, noting that while the limited attention hypothesis applies to both upward and downward biased forecasts, the perceived risk hypothesis is likely to apply only to downward biased forecasts. Consistent with limited attention hypothesis, we observed only an upward bias. Finally, there is a concern that our results may simply reflect fundamental changes in unaffected firms due to their connections with disaster-affected firms (e.g., as industry peers, suppliers, or customers). We address and rule out this alternative explanation, concluding that the spillover effect we document operates primarily through the limited attention

[1] Barrot and Sauvagnat (2016) show that if a firm's supplier is affected by a hurricane, its sales growth decreases by 2–3 percentage points. Dessaint and Matray (2017) show that firms located in the neighborhoods of disaster areas tend to increase cash holdings.

[2] Within this study, the term "forecasts" refers to estimates of quarterly earnings per share (EPS) provided by sell-side analysts.

channel.

Lastly, in cross-sectional analyses, we find that inaccurate forecasts for unaffected firms are less pronounced when analysts are more experienced. Experienced analysts may possess a better ability to manage and allocate their attention across multiple firms, allowing them to minimize the spillover effects caused by the disaster. On the other hand, we observe that the inaccuracy becomes more pronounced as the severity of the disaster increases. In such cases, the heightened attention required for disaster-affected firms appears to amplify the neglect of unaffected firms, leading to greater forecast errors. These findings underscore the importance of analyst experience and the scale of disaster impact in shaping the accuracy of forecasts for unaffected firms.

This paper contributes to the nascent literature on analyst's cognitive limits. Previous literature mainly focuses on managers', or investors' behavior bias (Abreu & Mendes, 2012; Shefrin, 2002). For example, Tversky and Kahneman (1974) show that individuals are based on beliefs affected by a limited number of heuristic principles and result in cognitive biases. Although previous studies show that analysts have economic incentives to provide their earnings forecasts in a biased manner, recent studies identify factors with the potential to affect analysts' earnings forecast unintentionally (Dehaan et al., 2017; Dong et al., 2021; Hirshleifer et al., 2019, 2021). For example, Hirshleifer et al. (2021) find that equity analysts' forecasts, target prices, and recommendations suffer from first impression bias. Hirshleifer et al. (2019) find that decision fatigue affects analysts' judgments. We provide direct evidence illustrating the constraints on analysts' ability to effectively process information in the context of the spillover effect of natural disasters on unaffected firms.

This paper also contributes to the literature investigating the economic impact of natural disasters (Belasen & Polachek, 2008; Eccles & Krzus, 2018; Kelly & Jiang, 2014). Early economic studies have extensively studied the long-term effect of natural disasters on human life, physical assets, and local economies (e.g., Ellson et al., 1984). Recently, researchers began to examine the effects of natural disasters on corporate activities through the supply chain and common geographical location. For example, Barrot and Sauvagnat (2016) show that if a firm's supplier is affected by a hurricane, its sales growth decreases by 2–3 percentage points. Dessaint and Matray (2017) show that firms located in the neighborhoods of disaster areas tend to increase cash holdings. In contrast to research focused on understanding the risk spillover transmitted through the common industry, common geographical location, supply-chain links, technology links, and similar firm divisions, we focus on the spillover effect of natural disasters on analyst forecast performance for unaffected firms through shared analyst coverage. This is consistent with the finding of Ali and Hirshleifer (2020), who show that shared analysts help expedite the impounding of information between linked firms.

Our findings contribute most directly to the large literature in accounting and finance on the behavioral biases of analysts caused by natural disasters. Our paper is closely related to Han et al. (2024) who document that earnings forecasts by analysts in the disaster zone become less accurate compared to those by unaffected analysts. However, different from their findings, we investigate the spillover effect of the disaster on analyst forecast performance with unaffected firms. Han et al. (2024) primarily examine the distraction effect of the hurricane event on the earnings forecast accuracy by analysts who experienced the hurricane. On the contrary, we examine how natural disasters affect analysts' forecast accuracy for those unaffected firms within the same portfolio as disaster-stricken firms. Our research focuses on spillover effects from disaster-affected firms to unaffected firms that are followed by the same analyst. We compare unaffected firms that are followed by an analyst who has disaster-stricken firms in her portfolio (i.e., treated analyst) with those that are followed by another analyst who never follows disaster-stricken firms (untreated analyst). For the same unaffected firm, we compare the within-firm difference between the treated analyst and untreated analyst. This can alleviate the concern that the effect of disaster is due to other omitted variables or firm changes in fundamentals caused by the disaster. We document that due to natural disasters, analysts have to devote more effort to the research and analysis of affected firms, and therefore are unable to provide accurate forecasts for unaffected firms, which highlights the problem of the limited attention channel. Our results complement Han et al. (2024)'s findings. In addition, our analysis covers a full spectrum of natural disasters including flooding, hurricanes, and wildfires, etc. whereas Han et al. (2024) focus on hurricanes only.[3]

The remainder of our paper is structured as follows: Section 2 presents our hypothesis, followed by Section 3, which covers data and summary statistics. Section 4 describes the empirical analysis and robust tests, while Section 5 provides direct channel tests. Section 6 includes cross-sectional tests, and Section 7 discusses the impact of natural disasters on disaster-hit firms. Finally, Section 8 concludes the paper.

2. Hypothesis

Our study focuses on the relationship between disaster and analyst forecast behavior for other unaffected firms before and after the disaster, if at least one analyst's following firm suffers from a natural disaster. The existing research already examined multiple determinants of forecast accuracy in disaster-affected firms.[4] However, our focus is on the shared analyst coverage transmission channel, that is, disaster can affect analyst forecasts towards other peer firms through shared analyst coverage. There are at least two reasons for the existence of the spillover effect:

First, an analyst who follows an affected firm may pay more attention to the affected firm and thus pays less attention to unaffected

[3] In Han et al. (2024), they use a sample of 30,270 forecasts which cover 2280 analysts involving 22 major hurricanes. In comparison, we use a sample of 225,808 forecasts which cover 10,470 analysts involving 213 climatic disasters.

[4] Loh and Stulz (2018) find that analysts' forecasts are less accurate, more frequent, and have a more significant price impact during market declines (i.e., down markets, market crises, and recessions). Whereas Loh and Stulz (2018) focus on market declines, our study differs from Loh and Stulz (2018) by focusing on uncertainty/risk arising from natural disasters.

firms. Due to the lack of attention, forecast inaccuracy for unaffected firms increases. We refer to this as the "limited attention channel". Clement (1999) shows that factors such as analysts' ability, available resources, and portfolio complexity significantly influence forecast accuracy. However, when individuals face a rich supply of information, attention may become a scarce cognitive resource (Falkinger, 2008). The accounting and finance literature has already accumulated evidence that investors are subject to limited attention (DeHaan et al., 2015; DellaVigna & Pollet, 2009; Hirshleifer et al., 2009; Louis & Sun, 2010). Choi and Gupta-Mukherjee (2016) assume that analysts have limited attention and rely more on industry information than on firm-specific information. It may seem obvious that when analysts face the task of analyzing multiple firms simultaneously, their information production is hindered. Dong and Heo (2014) provide evidence that analysts have limited attention when their region experiences flu epidemics. In their study, analysts' limited attention is due to the distractions of the sickness of family members, relatives, and colleagues. In contrast, Driskill et al. (2020) examine analysts' limited attention that arises from a rich supply of information in their normal course of work. They find that analysts are less likely to provide timely and thorough earnings forecasts for firms when another firm in their coverage portfolio is announcing earnings concurrently. In our study, analysts' limited attention arises from firms affected by natural disasters. Analysts who follow affected firms may pay more attention to them and thus pay less attention to unaffected firms.

Second, the analyst may overestimate the frequency of natural disasters for unaffected firms and thus provide less accurate forecasts for them. We call this channel the "risk perception channel." According to the risk perception mechanism, the salience of the events causes humans to focus on information that stands out or is often mentioned. Theoretical work on saliency by Thakor (2015) and Bordalo et al. (2012) provides foundations for our hypotheses. Thakor (2015) proposes a model of financial crisis based on the availability heuristic. Specifically, in his setting, banks, investors, and regulators overestimate bankers' skills following long periods of banking profitability (as recent history is salient), resulting in a risky lending boom and eventually a crisis. Bordalo et al. (2012) model choice under salient risks and argue that people may overweight the downside of a risky event when it is salient and may act in a risk-averse manner. People using the availability heuristic will then overestimate the frequency of salient events. Researchers document the behavioral bias of analysts (Dehaan et al., 2017; Dong et al., 2021; Hirshleifer et al., 2019, 2021; Liu & Kong, 2024). For example, Hirshleifer et al. (2021) find that equity analysts' forecasts, target prices, and recommendations suffer from first impression bias. Hirshleifer et al. (2019) find that decision fatigue affects analysts' judgments. Tversky and Kahneman (1973) and Tversky and Kahneman (1974) show that individuals base their beliefs on a limited number of heuristic principles, resulting in cognitive biases. Liu and Kong (2024) investigate how analysts' exposure to dangerous infectious diseases affect analysts' risk assessments and find that analysts in more affected regions issue more optimistic forecasts compared to their less-affected peers. Natural disasters can cause an unexpected negative deviation from an expected outcome, and thus can be considered as salient events to the analyst. In our setting, this implies that analysts may overestimate the adverse impact of salient natural disasters on firms and the probability of such natural disaster strikes in the future. Accordingly, we hypothesize that analysts' perceived disaster risk temporarily increases even though the real risk does not change for the unaffected firms.

However, if analysts drop the disaster firms or other firms in their portfolio to ensure high-quality coverage or if analysts work much harder and have a full investigation about the firms, then there should be no spillover effect in analyst forecast inaccuracy. Our hypothesis, stated as the alternative, is as follows.

Hypothesis. Analysts who follow disaster-stricken firms tend to provide inaccurate forecasts towards unaffected firms.

3. Data and summary statistics

3.1. Sample

To construct our dataset, we collect firm year-quarter fundamental data in the Compustat database spanning from 1992 to 2017.[5] Analyst forecast data is obtained from the unadjusted file in IBES. Beginning with the universe of firms and analysts in IBES, we eliminate firms that experienced major disasters because we want to focus on unaffected firms. We then delete those observations with an absolute value of measured forecast errors exceeding 100 percent, which seem to be problematic and probably result from a data input error (Lim, 2001).

Our natural disaster data is from the Spatial Hazard Events and Losses Database (SHELDUS) maintained by Arizona State University. We focus on all the major natural disasters. The SHELDUS database provides comprehensive coverage of natural hazards occurring in the U.S. such as thunderstorms, hurricanes, floods, wildfires, and tornados as well as perils such as flash floods and heavy rainfall (reference: SHELDUS Metadata). From SHELDUS, we identify the severity of each disaster (i.e., personal and property damage) and the affected counties, we base on the counties to define the affected focal firm. We drop firm-quarter observations if the price per share as of the end of the fiscal quarter is smaller than $1 (Cen et al., 2013; Malmendier & Shanthikumar, 2014). We further exclude observations with missing price information in the Center for Research in Security Prices (CRSP) database. We drop forecasts made by unidentified analysts (i.e., forecasts with an analyst identifier equal to zero). We further restrict our sample to 4 quarters before and after the disaster event. Our final sample consists of 225,808 observations at the analyst-firm-year-quarter level, of which there are 10, 470 analysts and 5,673 firms. To minimize the effects of outliers, we winsorize all continuous variables at the 1st and 99th percentiles.

[5] According to Appendix D, our results remain robust when we use more recent data from 2018 to 2022.

Table 1
Summary statistics.

Variable	N	Mean	Std.	Min	P25	P50	P75	Max
Key Variable of Interest								
Inaccuracy (%)	225,808	0.541	1.127	0.000	0.055	0.171	0.488	7.692
Post_Disaster	225,808	0.326	0.469	0.000	0.000	0.000	1.000	1.000
Firm Level Controls								
ROA	225,808	0.004	0.041	−0.216	0.001	0.008	0.021	0.083
Coverage	225,808	2.267	0.672	0.693	5.000	2.303	2.773	3.584
Size	225,808	7.338	1.782	3.523	6.076	7.307	8.559	11.650
Loss	225,808	0.235	0.424	0.000	0.000	0.000	0.000	1.000
MTB	225,808	2.092	1.663	0.725	1.095	1.509	2.360	10.520
STD_Ret	225,808	0.029	0.015	0.009	0.018	0.026	0.037	0.083
STD_ROA	225,808	0.028	0.049	0.000	0.006	0.013	0.029	0.378
RET_1y	225,808	0.134	0.554	−0.799	−0.196	0.068	0.341	2.592
Firm Age	225,808	2.323	0.865	0.000	4.000	2.398	3.045	4.043
Analyst Level Controls								
Horizon	225,808	4.050	0.957	0.693	3.555	4.431	4.595	5.308
Lag Inaccuracy (%)	225,808	0.484	0.984	0.000	0.053	0.160	0.451	6.794
Exp_Gen	225,808	3.027	0.961	0.693	2.303	3.045	3.850	4.875
Exp_Firm	225,808	2.088	0.767	0.693	1.609	1.946	2.565	4.331
Analyst_Sic2	225,808	1.272	0.484	0.693	0.693	1.099	1.609	2.485
Bsize	225,808	3.479	1.036	0.693	2.773	3.526	4.317	5.517
Analyst_Firm	225,808	2.435	0.600	0.693	2.079	2.485	2.833	3.850
Days Since Last Forecast	225,808	3.736	0.973	0.693	3.258	4.007	4.466	5.897

This table presents the sample summary statistics and univariate T test. The sample period is from 1992 to 2017. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are presented in Appendix A.

3.2. Model specification and variable construction

To test our hypothesis, we specify the following empirical model:

$$\begin{aligned} Inaccuracy = {} & \beta_0 + \beta_1 Post\_Disaster + \beta_2 ROA + \beta_3 Coverage + \beta_4 Size + \beta_5 Loss + \beta_6 MTB + \beta_7 STD\ Ret + \beta_8 STD\ ROA + \beta_9 RET_{1y} \\ & + \beta_{10} Firm\ Age + \beta_{11} Horizon + \beta_{12} Lag\ Inaccuracy + \beta_{13}\ Exp\ Gen + \beta_{14}\ Exp\ Firm + \beta_{15} Analyst\ Sic2 + \beta_{16} BSize + \beta_{17} Analys\_firm \\ & + \beta_{18} Days\_since\_last\_forecast + \sum YearQuarter\ FE + \sum Analyst\ FE + \sum Firm\ FE + \sum Brokerage\ FE + e \end{aligned} \tag{1}$$

The dependent variable *Inaccuracy* is the absolute difference between the value of analyst forecast earnings per share and the actual earnings per share for the unaffected peer firm followed by a disaster-stricken analyst, divided by the stock price at the beginning of the reporting period. We closely follow Diether et al. (2002) using the unadjusted forecast data, and then we further adjust for stock splits and stock dividends. To facilitate comparison across firms, we scale forecast inaccuracy by the prior year-end stock price.

The key independent variable *Post_Disaster* is an indicator that equals one if the earnings forecast is made after an analyst experienced the first natural disaster in our sample, and zero otherwise. To define *Post_Disaster*, we first need to clarify the treatment group and the control group. To identify the treatment group, we provide a simplified example in Appendix B. Let us assume that there are three analysts, each following a different set of firms: Analyst 1 follows Firm A and Firm B; Analyst 2 follows Firm C; Analyst 3 follows Firm A, Firm C, and Firm D. Among these firms, let us say that Firm A and D are hit by a disaster, while Firm B and C are not affected. In this case, we choose the combination of Analyst 1-Firm B and Analyst 3-Firm C as the treatment group, and Analyst 2-Firm C as the control group. The key is to classify the pre- and post-disaster periods for unaffected firms followed by the same analyst who also follows at least one disaster-stricken firm (such as Analyst 3). For analysts that only follow one disaster-stricken firm (such as Analyst 1), we can easily match the disaster event-time. If the affected analysts follow multiple disaster-stricken firms, we keep the first disaster event following the methodologies of Bertrand and Mullainathan (1999, 2003) and Huang and Wang (2021). More specifically, if Firm A is hit by a disaster in 1992Q1 and Firm D is hit in 1991Q4, then we use 1991Q4, the earliest time, to classify the pre- and post-disaster period. Alternatively, we also define the pre- and post-disaster data using the event with the highest inflation-adjusted dollar amount of property lost (*Post_Disaster 1*) and using the event with the highest number of fatalities (*Post_Disaster)*.

As for the benchmark group, it comprises the unaffected firms followed by analysts who never follow any disaster-stricken firm during the entire sample period (such as Analyst 2-Firm C). We eliminate the firms affected by any disaster because we want to focus on unaffected firms and differentiate the spillover effects from the direct effects of the disaster. We compare the unaffected firms followed by analysts who follow disaster-stricken firms with the unaffected firms followed by analysts who never follow disaster-stricken firms.

We draw upon existing literature to identify and control for a wide range of variables. To control for the demand for investment advice, we control for firm profitability (*ROA*), analyst coverage (*Coverage*), and firm size (*Size*) (Atiase, 1985; Barron et al., 2002; Barth et al., 2001). Analysts face forecasting difficulties for firms with losses (*Loss*), volatile market returns (*STD_Ret*), and volatile

Table 2
Baseline regression.

Dep. Var.	(1)	(2)	(3)
	Inaccuracy	Inaccuracy	Inaccuracy
Post_Disaster	0.045***	0.037***	0.025***
	(5.02)	(4.65)	(3.46)
ROA		−0.623***	−0.380***
		(-4.25)	(-2.65)
Coverage		−0.054***	−0.035***
		(-4.38)	(-3.21)
Size		−0.389***	−0.327***
		(-38.50)	(-37.19)
Loss		0.163***	0.113***
		(15.24)	(11.46)
MTB		0.031***	0.029***
		(7.49)	(8.13)
STD_Ret		16.370***	13.142***
		(21.53)	(20.51)
STD_ROA		−0.521***	−0.481***
		(-3.31)	(-3.47)
RET_1y		0.009	−0.017***
		(1.36)	(-2.92)
Firm Age		0.294***	0.224***
		(20.49)	(17.97)
Horizon			0.073***
			(29.23)
Lag_Inaccuracy			0.197***
			(28.53)
Exp_Gen			0.047***
			(3.10)
Exp_Firm			0.018***
			(4.26)
Analyst_Sic2			0.030*
			(1.81)
BSize			−0.009
			(-0.64)
Analyst_Firm			−0.020*
			(-1.73)
Days Since Last Forecast			0.004*
			(1.91)
Year-Quarter FE	YES	YES	YES
Analyst FE	YES	YES	YES
Firm FE	YES	YES	YES
Brokerage FE	YES	YES	YES
N	225,808	225,808	225,808
Adj. R^2	0.354	0.402	0.423

This table presents the regression results for the relation between natural disasters and analyst forecast inaccuracy excluding the focal firms. Our sample spans the period from 1992 to 2017. The regressions are performed by OLS. Year-quarter, firm, brokerage, and analyst fixed effects are included. Standard errors are clustered by the analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

earnings (*STD_ROA*) (Lang and Lundholm, 1993, 1996). Other firm characteristics, such as market-to-book (*MTB*), market performance (*RET_1y*) (Abarbanell & Bernard, 1992), and firm age (*Firm Age*) (Ertimur et al., 2011) are also likely to affect analyst coverage and hence forecast precision.

We also control for analyst-level characteristics. In particular, we control for *Horizon*, the number of days from the analyst forecast date to the actual earnings announcement date; *Lag_Inaccuracy*, forecast inaccuracy in previous year quarter; *Exp_Gen*, the number of years since an analyst first appeared in IBES; *Exp_Firm*, the number of years an analyst covers a specific firm; *Analyst_Sic2*, the number of industries (2 digit SIC codes) covered by an analyst, where a larger number of industries typically is associated with more forecast errors (Clement, 1999); *BSize*, the number of analysts who work in a brokerage house each quarter where analyst belongs to. In addition, we also include *Analyst_Firm*, the number of companies an analyst covered, to control for analysts' portfolio complexity, and *Days Since Last Forecast*, the number of days since the last earnings forecast, to control for the timeliness of the information dissemination by analysts.[6]

To adjust for possible cross-sectional correlations, we cluster all of the standard errors by the analyst. We control for analyst fixed

[6] Please refer to Appendix A for detailed definitions of variables.

Table 3
Parallel trend test.

Dep. Var.	(1) Inaccuracy	(1) Inaccuracy
PreQuarter 2	−0.005	−0.005
	(-0.55)	(-0.54)
PreQuarter 1	0.007	0.007
	(0.66)	(0.66)
Post	0.028***	
	(3.98)	
PostQuarter 1		0.033***
		(3.62)
PostQuarter 2		0.026***
		(2.81)
PostQuarter 3+		0.025***
		(2.99)
ROA	−0.390***	−0.389***
	(-2.73)	(-2.73)
Coverage	−0.036***	−0.036***
	(-3.33)	(-3.34)
Size	−0.319***	−0.319***
	(-36.23)	(-36.22)
Loss	0.112***	0.112***
	(11.33)	(11.33)
MTB	0.028***	0.028***
	(7.78)	(7.78)
STD_Ret	11.952***	11.957***
	(21.89)	(21.90)
STD_ROA	−0.468***	−0.468***
	(-3.39)	(-3.39)
RET_1y	−0.017***	−0.017***
	(-2.88)	(-2.88)
Firm Age	0.232***	0.232***
	(18.48)	(18.49)
Horizon	0.073***	0.073***
	(28.88)	(28.88)
Lag_Inaccuracy	0.199***	0.199***
	(28.79)	(28.79)
Exp_Gen	0.105***	0.105***
	(8.92)	(8.91)
Exp_Firm	0.022***	0.023***
	(5.22)	(5.25)
Analyst_Sic2	0.025	0.025
	(1.49)	(1.49)
BSize	−0.024*	−0.024*
	(-1.66)	(-1.67)
Analyst_Firm	−0.017	−0.017
	(-1.47)	(-1.47)
Days Since Last Forecast	0.004*	0.004*
	(1.95)	(1.95)
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	225,808	225,808
Adj. R^2	0.421	0.421

This table presents the parallel trend test. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Firm, brokerage, and analyst fixed effects are included errors are clustered by the analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

effect, and year-quarter fixed effect to alleviate the concern on potential cross-sectional and time-series omitted variables. We also control for brokerage houses fixed effect and firm fixed effect to ensure that the results are not driven by heterogeneity across brokerage houses and firms.

3.3. Summary statistics

Table 1 presents the summary statistics for our sample. We report the observation, the mean, the standard deviation, the min, the median, the 25th and 75th percentile, and the max for each variable. *Inaccuracy* has a mean of 0.541 and a median of 0.171. The mean

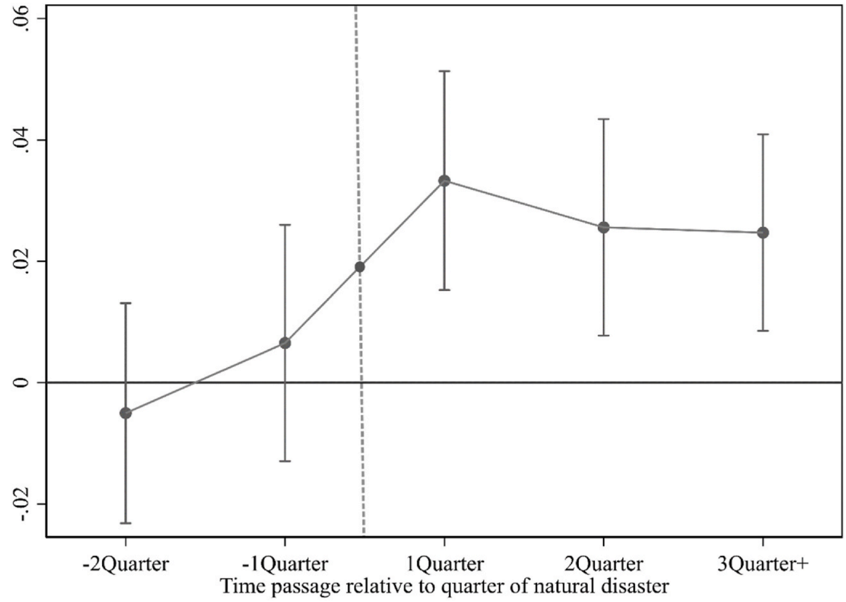

Fig. 1. Dynamic Effect of Disaster on Analyst Forecast Inaccuracy for Unaffected Firms
This figure shows the dynamic effect of disaster on analyst forecast inaccuracy for unaffected firms. We plot the coefficients and 5% confidence interval of *PreQuarter 2*, *PreQuarter 1*, *PostQuarter 1*, *PostQuarter 2*, *PostQuarter 3+* in Column (2) of Table 3. The dashed line represents the disaster event quarter. Detailed variable definitions are provided in Appendix A.

and median of *Post_Disaster* are 0.326 and 0.000. On average, the firm is about 13.38 quarters old. The mean value of *ROA* indicates that a firm in our sample has, on average, a *ROA* of 0.4%. Regarding analyst level control variables, an analyst on average has appeared in IBES (*Exp_Gen*) for 30 quarters, covers 3.034 industries (*Analyst_Sic2*) and 12.4 firms (*Analyst_Firm*) in each quarter, and works in a brokerage firm with 50.84 analysts.

4. Empirical results

4.1. Baseline results

In this section, we employ a staggered difference-in-differences (DID) estimation approach to investigate whether analysts exposed to natural disasters through covered firms tend to issue inaccurate forecasts toward other non-affected firms. Table 2 presents the results. The first column reports the regression result without control variables. The second column reports the results with firm level controls only and the third column reports the results as specified in equation (1). In Column (1) of Table 2, we first report the effect of natural disasters on forecast inaccuracy without adding any controls. We find that the coefficient of our key variable of interest *Post Disaster* is positive and statistically significant. This result is also economically significant: analyst forecast inaccuracy increase by about 8.3% (0.045/0.541) compared with the mean value of inaccuracy. In Column (2), we control for firm characteristics and in Column (3), we further control for analyst characteristics. We find consistent results across different model specifications. Our baseline regression in Column (3) demonstrates that when a firm experiences a natural disaster, there is a discernible increase in the forecast inaccuracy for other firms followed by the same analyst, amounting to approximately 4.5% (0.025/0.541) compared to the mean value of inaccuracy, which stands at 0.541%. Hence, we conclude that the effect of the disaster on analyst forecast inaccuracy on peer firms is not only statistically significant but also economically meaningful.

Regarding the control variables, we find that the coefficients on *ROA*, *Coverage*, and *Size* are significantly negative across all regressions, indicating that larger firms, firms with more analyst coverage, and more profitable firms exhibit lower forecast inaccuracy. We also find that analysts with higher forecast inaccuracy in the previous quarter, with longer forecast horizon, and covering more industries in the current quarter, have a higher level of forecast inaccuracy. Overall, the coefficients of the control variables are generally consistent with previous literature (Duru & Reeb, 2002; Gu & Wu, 2003; Wong & Zhang, 2014).

4.2. Parallel trend

An important underlying assumption of the DID research design is the parallel trend assumption. There is no statistically significant difference between the treatment group and the control group before natural disasters. If disaster and non-disaster analysts exhibit different trends in forecast inaccuracy before the natural disaster, then they are likely to continue exhibiting different trends after the disaster. We hereby conduct parallel trend analysis to rule out the possibility that our results are driven by the trends before the natural

Table 4
Entropy balancing.

Panel A. Sample before Entropy Balanced Matching						
	Disaster = 1 (N = 73,710)			Disaster = 0 (N = 15,2098)		
	Mean	Variance	Skewness	Mean	Variance	Skewness
ROA	0.004	0.002	−2.872	0.003	0.002	−2.720
Coverage	2.281	0.457	−0.345	2.260	0.448	−0.314
Size	7.224	3.020	0.136	7.394	3.243	0.090
Loss	0.224	0.174	1.327	0.240	0.182	1.218
MTB	3.227	19.440	3.176	3.284	18.470	3.194
STD_Ret	0.030	0.000	1.183	0.029	0.000	1.273
STD_ROA	0.027	0.002	4.711	0.028	0.003	4.670
RET_1y	0.143	0.331	1.743	0.129	0.296	1.563
Firm Age	2.362	0.766	−0.156	2.305	0.738	−0.129
Horizon	4.078	0.857	−1.298	4.037	0.943	−1.261
Lag Inaccuracy (%)	0.473	0.978	4.399	0.489	0.963	4.263
Exp_Gen	3.128	0.845	−0.196	2.977	0.953	−0.180
Exp_Firm	2.095	0.524	0.590	2.085	0.619	0.526
Analyst_Sic2	1.358	0.244	0.204	1.231	0.224	0.487
Bsize	3.531	0.934	−0.385	3.455	1.139	−0.251
Analyst_Firm	2.665	0.211	−0.331	2.324	0.395	−0.569
Days Since Last Forecast	3.747	0.938	−1.146	3.731	0.950	−1.091

Panel B. Sample after Entropy Balanced Matching						
	Disaster = 1 (N = 73,710)			Disaster = 0 (N = 15,2098)		
	Mean	Variance	Skewness	Mean	Variance	Skewness
ROA	0.004	0.002	−2.872	0.004	0.002	−2.814
Coverage	2.281	0.457	−0.345	2.281	0.457	−0.33
Size	7.224	3.02	0.136	7.224	3.02	0.103
Loss	0.224	0.174	1.327	0.224	0.174	1.327
MTB	3.227	19.44	3.176	3.227	19.43	3.313
STD_Ret	0.03	0	1.183	0.03	0	1.193
STD_ROA	0.027	0.002	4.711	0.027	0.002	4.864
RET_1y	0.143	0.331	1.743	0.143	0.331	1.628
Firm Age	2.362	0.766	−0.156	2.362	0.766	−0.173
Horizon	4.078	0.857	−1.298	4.078	0.857	−1.28
Lag Inaccuracy (%)	0.473	0.978	4.399	0.473	0.978	4.433
Exp_Gen	3.128	0.845	−0.196	3.128	0.845	−0.323
Exp_Firm	2.095	0.524	0.59	2.095	0.524	0.453
Analyst_Sic2	1.358	0.244	0.204	1.358	0.244	0.202
Bsize	3.531	0.934	−0.385	3.531	0.934	−0.294
Analyst_Firm	2.665	0.211	−0.331	2.665	0.211	−0.337
Days Since Last Forecast	3.747	0.938	−1.146	3.747	0.938	−1.144

Panel C. Regression Results	
Dep. Var.	(1)
	Inaccuracy
Post_Disaster	0.022***
	(2.84)
ROA	−0.297*
	(-1.76)
Coverage	−0.041***
	(-3.38)
Size	−0.306***
	(-34.96)
Loss	0.117***
	(10.21)
MTB	0.000
	(0.30)
STD_Ret	14.433***
	(19.41)
STD_ROA	−0.382**
	(-2.51)
RET_1y	−0.006
	(-0.88)
Firm Age	0.227***
	(17.17)
Horizon	0.071***

(*continued on next page*)

Table 4 (*continued*)

Panel C. Regression Results	
Dep. Var.	(1)
	Inaccuracy
	(25.39)
Lag_Inaccuracy	0.194***
	(24.84)
Exp_Gen	0.035*
	(1.78)
Exp_Firm	0.016***
	(3.44)
Analyst_Sic2	0.041**
	(2.20)
BSize	−0.004
	(-0.26)
Analyst_Firm	−0.016
	(-1.13)
Days Since Last Forecast	0.004*
	(1.81)
Year-Quarter FE	YES
Analyst FE	YES
Firm FE	YES
Brokerage FE	YES
N	225,808
Adj. R^2	0.432

This table presents the tests of natural disaster' effect on forecast inaccuracy using entropy balancing matched sample. Panel A presents the descriptive statistics for the matching variables before entropy balancing. Panel B presents the descriptive statistics for the matching variables after entropy balancing. We balance the covariates of our sample on three moments (mean, variance, and skewness). Panel C presents the multivariable regressions using the entropy balancing matched sample. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Year-quarter, firm, brokerage, and analyst fixed effects are included errors are clustered by analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

disaster. Following Bertrand and Mullainathan (2003) and Fauver et al. (2017), we first replace *Post_Disaster* in Equation (1) with dummies *PreQuarter_2, PreQuarter_1*, *Post* which are indicator variables set to one if the firm-year-quarter is two quarters before, one quarter before, and one or more quarters after the natural disaster, respectively. We then replace *Post_ Disaster* in Equation (1) with dummies *PreQuarter_2, PreQuarter_1*, *PostQuarter_1, PostQuarter_2* and *PostQuarter_3+* which are indicator variables set to one if the firm-year-quarter is two quarters before, one quarter before, one quarter after, two quarters after, and three or more quarters after the natural disaster, respectively.

Table 3 presents the results of the parallel trend analysis. In Columns (1) and (2), the interaction terms between *Post_Disaster* and the year quarter dummies before the natural disaster (i.e., *PreQuarter_2* and *PreQuarter_1*), both carry insignificant coefficients. On the other hand, for the interaction terms between *Disaster* and the year quarter dummies after the natural disaster (i.e., *Post*, *PostQuarter_1, PostQuarter_2* and *PostQuarter_3+*), both carry significantly positive coefficients. The results suggest that before the natural disaster, treated and non-treated analysts exhibit no significant differences in forecast inaccuracy. However, after the natural disaster, the treated analysts have higher forecast inaccuracy than non-treated ones. We also visually display these effects in Fig. 1, where we can clearly observe a significant increasing trend of analyst forecast inaccuracy in the years post natural disaster. This analysis lends support to the validity of the parallel trend assumption and reinforces that natural disasters cause treated-analysts and non-treated analysts to diverge in their forecast accuracy.

4.3. Entropy balancing test and robustness tests

To enhance the validation of our empirical design, in this section, we apply entropy balancing to assemble a control sample that exhibits covariate balance with the treatment group. Entropy balancing constructs a set of matching weights on control sample units to achieve covariate balance, controlling for random and systematic inequalities in the variable distributions between the treatment and control groups (Hainmueller, 2012). It enables us to achieve a covariate balance between disaster-affected and unaffected groups along the mean, variance, and skewness of the control variable distributions without requiring design choices that can affect the composition of the control sample and, thus, the results of the analysis. Panels A and B of Table 4 present summary statistics of both samples before and after the entropy balancing matching, respectively, and Panel C presents the regression results using the entropy balanced sample. Consistent with our main findings, the coefficient on *Post_Disaster* remains significantly positive.

We next conduct a battery of robustness tests to reinforce the validity of our findings. First, in Panel A of Table 5, we provide a robustness table by including state fixed effects to account for institutional and structural heterogeneity across U.S. states. We find our results remain unchanged. We also conduct a split sample analysis by showing that firms in urban cities are more affected than those in rural areas. Due to space limit, we provide this analysis in Appendix C.

Table 5
Robustness tests.

Panel A. Controlling for State Fixed Effects

Dep. Var.	(1)	(2)
	Inaccuracy	Inaccuracy
Post_Disaster	0.020***	0.019**
	(2.71)	(2.43)
PerCap Income		−0.000**
		(-2.42)
Unemployment Rate		1.167**
		(2.30)
Control	YES	YES
State FE	YES	YES
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	188,615	166,454
Adj. R^2	0.420	0.423

Panel B. Robustness Test with Additional Controls

Dep. Var.	(1)	(2)	(3)
	Inaccuracy	Inaccuracy	Inaccuracy
Post_Disaster	0.025***	0.026***	0.026***
	(3.53)	(3.25)	(3.18)
IO	−0.063***		−0.095***
	(-5.32)		(-6.00)
ALLSTAR		0.015	0.015
		(0.83)	(0.83)
Controls	YES	YES	YES
Year-Quarter FE	YES	YES	YES
Analyst FE	YES	YES	YES
Firm FE	YES	YES	YES
Brokerage FE	YES	YES	YES
N	225,808	138,756	138,756
Adj. R^2	0.423	0.388	0.389

Panel C. Robustness Test Using Alternative Sample

Dep. Var.	(1)
	Inaccuracy
Post_Disaster	0.028**
	(2.50)
Controls	YES
Year-Quarter FE	YES
Analyst FE	YES
Firm FE	YES
Brokerage FE	YES
N	151,287
Adj. R^2	0.423

Panel D. Robustness Test Using Alternative Measures of Disaster

Dep. Var.	(1)	(2)
	Inaccuracy	Inaccuracy
Post_Disaster 1	0.023***	
	(2.60)	
Post_Disaster 2		0.023**
		(2.52)
Controls	YES	YES
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES

(*continued on next page*)

Table 5 (*continued*)

Panel D. Robustness Test Using Alternative Measures of Disaster

Dep. Var.	(1)	(2)
	Inaccuracy	Inaccuracy
N	225,808	225,808
Adj. R^2	0.423	0.423

Panel E. Robustness Test Using Alternative Measures of Analyst Forecast Inaccuracy

Dep. Var.	(1)	(1)
	Inaccuracy 1	Inaccuracy 2
Post_Disaster	0.179***	0.017***
	(3.62)	(2.95)
Controls	YES	YES
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	225,808	222,291
Adj. R^2	0.323	0.059

Panel F. Alternative Design

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
	Inaccuracy	Inaccuracy	Inaccuracy	Inaccuracy	Inaccuracy	Inaccuracy
	Keep Horizon [2100]	Keep Horizon [2365]	Two-way cluster	Same Industry	Different Industry	No financial and utility firms
Post_Disaster	0.021***	0.026***	0.025***	0.032**	0.032***	0.023***
	(2.86)	(3.56)	(3.43)	(1.99)	(3.41)	(2.93)
Controls	YES	YES	YES	YES	YES	YES
Year-Quarter FE	YES	YES	YES	YES	YES	YES
Analyst FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Brokerage FE	YES	YES	YES	YES	YES	YES
N	169,636	222,950	225,808	66,261	158,976	178,559
Adj. R^2	0.409	0.425	0.423	0.451	0.418	0.419

Panel G. Testing Relationship Between Residualized Outcomes and Residualized Treatment

Dep. Var.	Residualized Inaccuracy
	(1)
Residualized treatment	−0.002
	(-0.17)
Treatment group	−0.001
	(-0.20)
Treatment group x Residualized treatment	0.007
	(0.37)

Panels A–G present the results of robustness tests. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Year-quarter, firm, brokerage, and analyst fixed effects are included.
Panel A presents the results while controlling for state fixed effects. Column (1) adds state fixed effect to the main regression. Column (2) adds state fixed effect to the main regression and additionally controls for PerCap Income, which is natural logarithm of GDP per capita at year t and Unemployment Rate which is the unemployment rate of the state.
Panel B presents the results by adding additional control variables including institutional ownership and all-star analysts.
Panel C shows the results by removing firms that are in both treatment and control groups in quarter t.
Panel D presents the results using an alternative definition of disaster. Post_Disaster 1 is an indicator variable that equals one if the current year quarter is after the year quarter that the analyst experienced the most serious property damage (inflation-adjusted) disaster and 0 otherwise. Post_Disaster 2 is an indicator variable that equals one if the current year is after the year quarter that the analyst experienced the most fatality and 0 otherwise.
Panel E presents the results using an alternative definition of forecast inaccuracy. Inaccuracy 1 is the square of difference in earnings forecast and earnings announcement scaled by the lagged year beginning stock price. Inaccuracy 2 is the proportional mean absolute forecast error calculated as the difference between the absolute forecast error for analyst i on firm j in quarter t and the mean absolute forecast error for firm j in quarter t scaled by the mean absolute forecast error for firm j in quarter t.
Panel F presents the robustness results obtained by using alternative designs. Column (1) and (2) restricts the forecast horizon. Column (3) clusters the standard by analyst and year-quarter. Column (4)/(5), we keep unaffected firms in the same/different 2-digit sic code with affected firms respectively. Column (6) removes observations from financial and utility industries.
Panel G presents the validation of the treatment effect homogeneity assumption. The dependent variable is the residual from a regression of the inaccuracy on all the fixed effects. Residualized treatment is the residual from a regression of the disaster dummy on all the fixed effects.
Standard errors are clustered by the analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

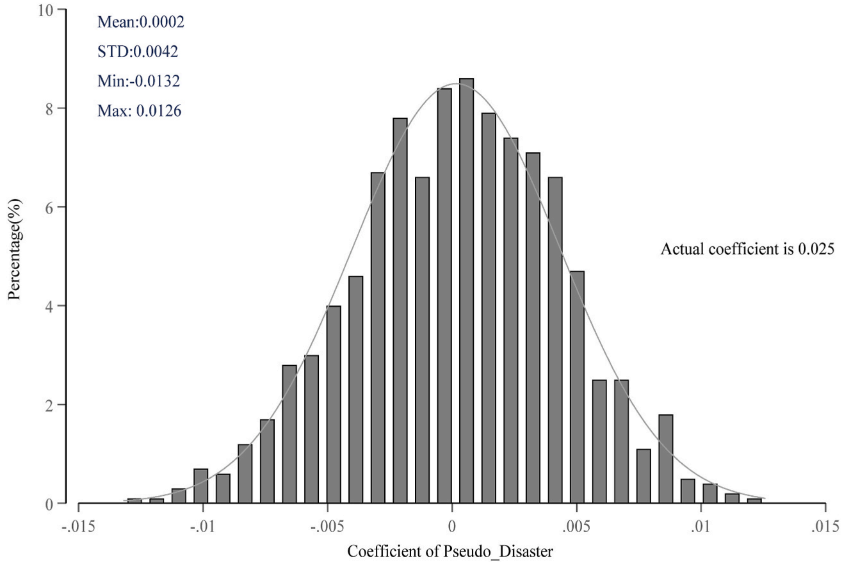

Fig. 2. Placebo Test
Fig. 2 shows the results of the placebo test. We randomly assign treated firms in our sample and create *Pseudo_Disaster*. Then, we re-estimate the baseline regression in Equation (1) using *Pseudo_Disaster*. We repeat the process 1000 times and plot the distribution of the coefficients on *Pseudo_Disaster* in the figure. We also plot the actual coefficient of *Disaster* in Column (3) of Table 2. Detailed variable definitions are provided in Appendix A.

Second, another potential concern in our paper is that institutional ownership and analyst ability impacts the analyst forecast accuracy, which may introduce concerns of omitted variables issues. To address this potential endogeneity problem, we incorporate controls for institutional ownership of firms (*IO*) and All-Star analyst (*ALLSTAR*) in our analysis. *IO* is measured as the institutional ownership of a firm in a given quarter, where we collect institutional holdings data from *Thomson Reuters Institutional Holdings (13F) database*. *ALLSTAR* is an indicator that equals one if the analyst is named as an "All-America Research Team Analyst" by *Institutional Investor Magazine* in year t and 0 otherwise. As the *ALLSTAR* analyst data is only available to us until 2008, including this variable has reduced the sample by nearly half. Importantly, we find that even after accounting for these factors, the coefficient loading on *Post_Disaster* remains significantly positive, as shown in Panel B of Table 5. This further strengthens and validates our conclusions.

Third, while our analysis is conducted at the firm-analyst level, and the comparison is between treated and untreated analysts, it is possible to include one firm in both treatment and control groups. This may also contaminate our results because the forecast difference can be caused by different analyst skills. To address this concern, we conduct a robustness test by removing firms in both treatment and control groups in quarter t and rerun our baseline regression. This result is reported in Panel C of Table 5. We find that our main findings do not change.

Forth, in our baseline test, *Post_Disaster* is defined as the first natural disaster that an analyst experienced. In Panel D of Table 5, we report the empirical analysis by using alternative measures of natural disaster, where *Post_Disaster1* (*Post_Disaster2*) is an indicator variable that equals one if the current year quarter is after the first year that the analyst experienced the most serious inflation-adjusted property damage (fatality) disaster and 0 otherwise. We find that the coefficients of *Post_Disaster1* and *Post_ Disaster2* are still statistically significant.

In Panel E of Table 5, we explore the sensitivity of the main result to the use of alternative measures of analyst forecast inaccuracy. In our baseline test, *Inaccuracy* is defined as the absolute difference between forecasted earnings and reported earnings scaled by the stock price at the beginning year. In line with the existing empirical literature on analyst forecast inaccuracy, we use two alternative proxies for forecast inaccuracy, *Inaccuracy 1* and *Inaccuracy 2*, where *Inaccuracy 1* is the square of the difference between forecasted earnings and reported earnings scaled by stock price at the beginning year. *Inaccuracy 2* is the proportional mean absolute forecast error. As shown in Panel E of Table 5, we find that the coefficients of *Post_ Disaster* remain positive and statistically significant (coefficient = 0.179, t = 3.62 in Column (1); coefficient = 0.017, t = 2.95 in Column (2)) when using these two alternative forecast accuracy measures.

In Panel F of Table 5, we adopt two alternative research designs. As the horizon becomes shorter, analyst forecasts gain more accuracy due to the increase in information certainty (Dong et al., 2021). Therefore, in Column (1), we limit our sample to those forecasts within the forecast horizon between 2 days and 100 days. In Column (2), we limit our sample to those forecasts within the forecast horizon between 2 days and 365 days. The results are almost unchanged. We also report results using two-way cluster-robust standard errors by analyst and year-quarter in Column (3). Consistent with the prediction, the estimated coefficient on *Post_ Disaster* is positive (coefficient = 0.022, t = 3.66) and statistically significant at 1%. In Column (4) (Column (5)), we keep the unaffected firm in the same (different) two SIC code with the affected firm. The result is basically unchanged. Finally, in Column (6), we remove financial and utility firms from our sample and find similar results.

Table 6
The effect of natural disasters on analyst attention and revision untimeliness for unaffected firms.

Dep. Var.	(1)	(2)
	Analyst attention	Revision untimeliness
Post_Disaster	−0.010*	0.012***
	(-1.77)	(7.12)
ROA	−0.049	−0.033
	(-0.53)	(-1.35)
Coverage	0.795***	−0.003
	(81.71)	(-1.25)
Size	0.107***	−0.011***
	(17.56)	(-6.96)
Loss	−0.007	−0.005**
	(-0.93)	(-2.35)
MTB	−0.001*	0.000
	(-1.76)	(0.63)
STD_Ret	1.008***	−0.119
	(3.10)	(-1.39)
STD_ROA	−0.210**	0.061***
	(-2.55)	(2.61)
RET_1y	−0.042***	−0.001
	(-8.55)	(-0.47)
Firm Age	−0.012	0.012***
	(-1.04)	(4.43)
Year-Quarter FE	YES	YES
Firm FE	YES	YES
N	53,584	53,584
Adj. R^2	0.766	0.091

This table presents the results of the disaster's effect on analyst attention and revision untimeliness towards unaffected firms. Analyst attention is defined as the number of unique earnings per share revisions count issued for each firm-quarter, including forecasts for all time horizons. Revision untimeliness is defined as the natural logarithm of the average number of days between earnings forecast revision of an analyst in quarter t. The analysis is implemented at the firm and year-quarter level. The regressions are performed by OLS. Year-quarter and firm fixed effects are included. Standard errors are clustered by firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

In Panel G of Table 5, we address the concern related to the staggered difference-in-differences design (i.e., Cengiz et al., 2019; Bourveau & Law, 2021). We follow Jakiela (2021) to test whether the treatment effect homogeneity assumption is likely to be valid. To answer the question, we exploit the relationship between the residual of our dependent variable and residual treatment. Under the assumptions of treatment effect homogeneity and common trends, a linear relationship indicates that the slope does not differ between the treatment group and the control group. We regress *Residualized Inaccuracy* on *Treatment group x Residualized treatment*, where *Residualized Inaccuracy* is the residual from a regression of the inaccuracy on all the fixed effects, and *Residualized treatment* is the residual from a regression of the disaster dummy on all the fixed effects. As shown in Panel G of Table 5, we find insignificant results on this interaction term suggesting no obvious evidence that the slope differs between the treatment group and the control group. This finding supports the assumptions required for two-way fixed effects estimation.[7]

Lastly, to ensure the validity of our results, we also performed a placebo test. In particular, we randomly reassign our treated sample in the dataset and rerun our analyses. We repeat this procedure one thousand times and generate the distribution of the coefficient loadings on the randomly assigned pseudo-*Disaster* variable, and we display in Fig. 2. We find that the average coefficient loading on this pseudo- *Disaster* variable is remarkably close to zero and is statistically different from the estimated coefficient in our main analysis (0.025 in Column (3) of Table 2). This finding confirms that our results are unlikely to be driven by chance.

5. Channel test

5.1. The limited attention channel

Our main results document the spillover effect of natural disasters on unaffected peers' forecast accuracy, and one possible explanation for this result is due to the limited attention of financial analysts. Driskill et al. (2020) argue that financial analysts are subject to limited attention and Harford et al. (2019) suggest that analysts will allocate their efforts strategically based on the importance of their portfolio firms. To demonstrate the limited attention channel, we first directly test analysts' attention on

[7] In our setting, this bias is less likely since we only focus on a short window.

Table 7
The effect of natural disaster on affected firms.

Panel A. Disaster and Analyst Attention on Affected Firms

Dep. Var.	(1)
	Analyst attention
Post_Disaster	0.020**
	(2.40)
ROA	0.147
	(1.15)
Coverage	0.897***
	(69.23)
Size	0.106***
	(13.05)
Loss	0.011
	(1.03)
MTB	−0.002***
	(-2.79)
STD_Ret	1.520***
	(3.63)
STD_ROA	−0.282**
	(-2.13)
RET_1y	−0.042***
	(-6.30)
Firm Age	0.007
	(0.38)
Year-Quarter FE	YES
Firm FE	YES
N	39,406
Adj. R^2	0.766

Panel B. The Effect of Disaster on Affected Firms' Forecast Inaccuracy and Investors' Attention

Dep. Var.	(1)	(2)
	Inaccuracy	Bloomberg-attention
Post_Disaster	0.023**	0.026*
	(2.26)	(1.90)
Controls	YES	YES
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	81,715	24,380
Adj. R^2	0.372	0.799

Panel C. The Change of Media Coverage for Affected Firms

Dep. Var.	(1)	(2)
	Delta_News1	Delta_News2
Post_Disaster	0.016*	0.027*
	(1.77)	(1.66)
STD_Ret	0.392**	−1.823***
	(2.37)	(-5.44)
RET_1y	0.045***	0.022***
	(9.14)	(2.92)
Turnover	1.158**	14.374***
	(2.20)	(8.80)
ROA	0.275***	0.577***
	(8.01)	(7.74)
MTB	−0.000	0.003***
	(-0.99)	(2.62)
Leverage	0.004	−0.000
	(0.25)	(-0.00)
Year-Quarter FE	YES	YES
Firm FE	YES	YES
N	635,298	674,721
Adj. R^2	0.314	0.735

This table presents the validation tests utilizing the sample of firms affected by disasters. Panel A displays the impact of natural disasters on analyst attention for disaster-affected firms. Panel B presents the regression findings concerning the effect of disasters on analyst forecast inaccuracy and investors' attention. Panel C presents the regression results regarding the impact of disasters on media coverage. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Year-quarter, firm, brokerage, and analyst fixed effects are included. Standard errors are clustered by firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

disaster-unaffected and disaster-affected firms. The assumption is that, if financial analysts are subject to limited attention, our finding of increased forecast inaccuracy for unaffected firm after natural disaster is due to the following joint condition: analyst will allocate more attention to and spend more time on disaster-affected firms and thereby focus less on disaster-unaffected firms.

To investigate this assumption, we first examine whether analysts pay less attention on disaster-unaffected firms. Our results are presented in Table 6. We show that analysts paid less attention and took more time to update the information for those unaffected firms. In Table 6, *Analyst attention* is defined as the number of unique earnings per share revisions counts issued for each firm-quarter, including forecasts for all time horizons following Frankel et al. (2006) and Drake et al. (2017). *Revision untimeliness* is defined as the natural logarithm of the average number of days between earnings forecast revision of an analyst in quarter t. The results show that analysts issued less unique earnings per share revisions, and the number of days between earnings forecast revision by an analyst for disaster unaffected firms increases after a natural disaster. These results support the limited attention channel that analysts paid less attention to unaffected firms due to natural disasters.

We next examine whether analysts allocate more attention to disaster-affected firms. Analysts care about factoring disasters into their forecast updates. With the actual physical damage, there would be numerous news on safety concerns, economic losses, and responses to the aftermath, which may draw analysts' attention from devoting all their energy to the disaster-affected firms. Attention is a limited resource (Han et al., 2024), particularly for brain-intensive professions such as analysts. They have to spend more time on disaster-affected firms due to a notable increase in the complexity and urgency of the financial analysis required. The affected companies may engage in earnings management practices (Rueangsuwan & Jevasuwan, 2022) and so analysts will have to scrutinize these actions to provide accurate financial forecasts. The increased workload on disaster-affected firms could impact analyst's attention to unaffected firms. We therefore directly test the analyst's attention towards affected firms.

In Panel A of Table 7, we find evidence in support of our conjecture. We regress *Analyst attention* on *Post_Disaster* using the sample of disaster-affected firms, where *Analyst attention* is defined as before, i.e., the number of unique earnings per share revisions count issued for each firm-quarter, including forecasts for all time horizons for disaster-affected firms. The positive coefficient on *Post_Disaster* suggests that analysts increase attention on the affected firms after the natural disaster. Panel B of Table 7 further examines the relation between natural disasters and focal firms' forecast inaccuracy and institutional investor attention. In Column (1), the dependent variable is *Inaccuracy*, defined as the absolute difference between forecasted earnings and reported earnings scaled by the stock price at the beginning year. We find that the estimated coefficient on *Post_Disaster* is positive and statistically significant suggesting that natural disasters increase affected firms' forecast inaccuracy.

In Column (2) of Panel B in Table 7, we use the average Bloomberg searching activities of the focal firm in quarter t to proxy for institutional investors' attention and show an increase in institutional investors' attention of affected firms after a natural disaster. The results show that *Post_Disaster* is positively correlated with *Bloomberg-attention* (0.026, $t = 1.90$), indicating that institutional investors increase their attention on disaster-affected firms after the natural disaster. This suggests that analysts have allocated more effort and time to issue timely forecasts for firms that attract high levels of attention from institutional investors. Indirectly, this also implies that analysts may be particularly drawn to firms affected by disasters.

Panel C of Table 7 examines the changes in media coverage of disaster-affected firms. We use two measures *Delta_News1* and *Delta_News2* as dependent variables to capture the changes of media sentiment and media coverage, respectively, where *Delta_News1* is measured as the natural logarithm of changes in media sentiment (the mean value of ESS score) from quarter -1 to t, and *Delta_News2* is measured as the natural logarithm of changes in number of media coverage from quarter t-1 to t. The media coverage data is obtained from Ravenpack. As shown in Columns (1) and (2), the coefficients on *Post_Disaster* are both positive and significant, suggesting that media coverage for affected firms increases. This result suggests that analysts may ned to spend more time on disaster-affected firms due to a significant increase in media attention, creating an urgency for timely financial analysis. Taken together, our findings support the limited attention channel, indicating that the reduction in forecast accuracy of disaster-unaffected firms is due to analysts allocating less attention to them.

5.2. Ruling out alternative channels

One possibility that analysts provide less accurate forecasts on unaffected firms after natural disaster could be changes in their risk perceptions. While the limited attention hypothesis works for both upward and downward biased forecasts, the perceived risk hypothesis can only work for downward biased forecasts. To rule out the possibility of perceived risk channel, we, performed the analysis on the direction of analyst forecast to determine which hypothesis predominates. Specifically, we employ a staggered difference-in-differences (DID) estimation approach to investigate whether analysts exposed to natural disasters through covered firms tend to issue upward or downward bias forecasts toward other non-affected firms. The results are presented in Panel A of Table 8, where we proxy forecast bias with (1) *Inaccuracy*, defined as the absolute difference between forecasted earnings and reported earnings scaled by the stock price at the beginning of the year and (2) *Forecast bias*, defined as the difference between forecasted earnings and reported earnings scaled by the stock price at the beginning of the year. The first (second) two columns report the regression result with forecast bias greater (less) than zero. As shown, the coefficients of the main variable of interest, *Post_Disaster*, are positive and significant in the

Table 8
Ruling out alternative channels.

Panel A. Ruling Out Alternative Explanation on Change in Risk Perception

Dep. Var.	Forecast bias ≥ 0		Forecast bias<0	
	(1)	(2)	(3)	(4)
	Inaccuracy	Forecast bias	Inaccuracy	Forecast bias
Post_Disaster	0.048***	0.042***	0.009	−0.002
	(3.75)	(3.80)	(1.27)	(-0.33)
ROA	0.314	0.195	−0.873***	0.696***
	(1.32)	(0.99)	(-5.53)	(6.93)
Coverage	−0.030*	−0.022	−0.067***	0.046***
	(-1.73)	(-1.47)	(-5.43)	(5.68)
Size	−0.386***	−0.339***	−0.258***	0.205***
	(-27.92)	(-29.52)	(-26.28)	(31.70)
Loss	0.181***	0.163***	0.036***	−0.045***
	(10.90)	(11.60)	(3.21)	(-5.90)
MTB	0.035***	0.023***	0.021***	−0.005*
	(6.21)	(4.90)	(4.75)	(-1.83)
STD_Ret	13.766***	11.184***	12.092***	−7.841***
	(14.16)	(14.46)	(16.10)	(-17.09)
STD_ROA	−0.896***	−0.663***	−0.174	−0.153
	(-4.37)	(-3.89)	(-0.88)	(-1.32)
RET_1y	−0.032***	−0.025***	0.002	−0.018***
	(-2.85)	(-2.62)	(0.32)	(-3.82)
Firm Age	0.264***	0.231***	0.166***	−0.116***
	(12.92)	(13.07)	(11.81)	(-12.29)
Horizon	0.108***	0.099***	0.035***	−0.029***
	(25.11)	(26.62)	(13.34)	(-15.37)
Lag_Inaccuracy	0.176***	0.140***	0.202***	−0.133***
	(18.48)	(18.46)	(22.56)	(-28.15)
Exp_Gen	0.052**	0.042*	0.039**	−0.027**
	(2.10)	(1.96)	(2.14)	(-2.09)
Exp_Firm	0.030***	0.025***	0.002	0.001
	(4.19)	(4.03)	(0.46)	(0.22)
Analyst_Sic2	0.021	0.017	0.020	−0.016
	(0.73)	(0.68)	(1.08)	(-1.29)
BSize	−0.029	−0.021	0.006	−0.005
	(-1.16)	(-0.98)	(0.35)	(-0.45)
Analyst_Firm	−0.018	−0.015	−0.013	0.012
	(-0.90)	(-0.92)	(-0.91)	(1.21)
Days Since Last Forecast	0.005	0.005	0.002	−0.001
	(1.27)	(1.52)	(0.76)	(-0.31)
Year-Quarter FE	YES	YES	YES	YES
Analyst FE	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES
Brokerage FE	YES	YES	YES	YES
N	103,467	103,467	119,525	119,525
Adj. R^2	0.440	0.451	0.460	0.490

Panel B. Ruling Out Alternative Explanation on Firm Fundamental Change

Dep. Var. = Inaccuracy	(1)	(2)	(3)
	In different industries of disaster-affected firms	No supply chain relationship with disaster-affected firms	Neither in the same industry nor has supply chain relationship with disaster-affected firms
Post_Disaster	0.026***	0.029***	0.026**
	(2.74)	(3.69)	(2.56)
Controls	YES	YES	YES
Year-Quarter FE	YES	YES	YES
Analyst FE	YES	YES	YES
Firm FE	YES	YES	YES
Brokerage FE	YES	YES	YES
N	158,976	206,963	152,607
Adj. R^2	0.418	0.423	0.418

This table presents the results to rule out other possible channels. Panel A presents the results for our main regression in the upward and downward forecast bias sample. Forecast bias is the difference between forecasted earnings and reported earnings scaled by the stock price at the beginning of the year. Panel B presents the results obtained by excluding firms with connections to the disaster-affected firm. Connection1 is a dummy variable that equals 1 if the focal firm and disaster affected firm pair is from the sample industry, and zero otherwise. Industry is defined by two digits SIC code. Connection2 is a dummy variable that equals 1 if the focal firm and disaster-affected firm pair have any connection defined by FactSet Relationships

Data and zero otherwise. Connection3 is a dummy variable that equals 1 if Connection1 = 1 or Connection2 = 1, and zero otherwise. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Firm, Brokerage and Analyst fixed effects are included errors are clustered by analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

Table 9
Cross-sectional tests – the effect of analyst experience.

Dep. Var.	(1)	(2)	(3)
	Inaccuracy	Inaccuracy	Inaccuracy
Post_Disaster	0.037***	0.039***	0.043***
	(3.88)	(4.40)	(4.57)
High Forecast Tenure	0.027***		
	(4.01)		
Post_Disaster × *High Forecast Tenure*	−0.021**		
	(-2.01)		
High BSize		0.016	
		(1.30)	
Post_Disaster × *High BSize*		−0.032**	
		(-2.79)	
High Exp_gen			−0.072***
			(-3.32)
Post_Disaster × *High Exp_gen*			−0.028**
			(-2.26)
Controls	YES	YES	YES
Year-Quarter FE	YES	YES	YES
Analyst FE	YES	YES	YES
Firm FE	YES	YES	YES
Brokerage FE	YES	YES	YES
N	225,808	225,808	225,808
Adj. R^2	0.423	0.423	0.423

This table shows the estimates obtained from examining whether our main results are affected by the analyst experience. Analyst experience is measured by (1) High Forecast Tenure, defined as a dummy variable that equals 1 if the analyst's Forecast Tenure, the number of quarters an analyst covers a specific firm, of the focal firm is larger than the mean value of Forecast Tenure in the sample, and 0 otherwise, (2) High BSize, defined as a dummy variable that equals 1 if the analyst's Bsize is larger than the mean value of Bsize in the sample, and 0 otherwise, and (3) High Exp_gen, defined as a dummy variable that equals 1 if the analyst's exp_gen is larger than the mean value of exp_gen in the sample, and 0 otherwise. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Year-quarter, firm, brokerage, and analyst fixed effects are included. Standard errors are clustered by the analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

first two columns only. Specifically, *Post_Disaster* is significant at one percent level when the dependent variable is either *Inaccuracy* (Coefficient = 0.048, t = 3.75) or *Forecast bias* (Coefficient = 0.042, t = 3.80), This result is also economically significant. For example, the results in Column (1) suggest that analyst forecast inaccuracy is upward biased by about 8.9% (0.048/0.541) compared with the mean value of inaccuracy. In contrast, we do not find statistically significant results in the second two columns when using forecast bias less than zero as the dependent variable. This finding allows us to rule out the perceived risk channel, leading us to conclude that the effect of the inaccuracy in analyst forecasts for peer firms is driven by the limited attention channel. In addition, this finding also suggests that while financial analysts focus more on the negative news surrounding disaster-affected firms, they may exhibit relative optimism towards unaffected firms. This upward bias could stem from the tendency to view unaffected firms as safer bets, or from analysts defaulting to a more optimistic outlook, assuming business as usual for these firms in the absence of negative news.

Another potential cause of forecast inaccuracy for unaffected firms could be fundamental changes in these firms due to their connections with disaster-affected firms, whether as industry peers, suppliers, or customers. To rule out this alternative channel, in Panel B of Table 8, we provide a robustness test by excluding observations with connections to disaster-affected firms. In Column (1), we exclude those sample observations if the unaffected firm is from the same industry as the affected firm. In Column (2), we exclude firms that are suppliers or customers of the disaster-affected firms. In Column (3), we exclude both firms in the same industry and along the same supply chain. Across various sample specifications, we find that our results remain statistically similar, leading us to conclude that the spillover effect we document operates primarily through the limited attention channel.

6. Cross-sectional analysis

6.1. The effect of analyst experience

So far, we have documented a significant increase in analyst forecast inaccuracy for unaffected peer firms after natural disasters through shared analyst coverage. In this section, we examine whether and how analysts' experience affects the relationship between disaster and analyst forecast inaccuracy of peer firms. Analysts who have covered a specific firm for many quarters are more familiar

Table 10
Cross-sectional tests – the effect of disaster severity.

Dep. Var.	(1) Inaccuracy	(2) Inaccuracy
Post_Disaster	0.034***	0.035***
	(2.86)	(2.74)
High Number of Subsidies	/	
	/	
Post_Disaster × High Number of Subsidies	−0.041**	
	(-2.43)	
High Intangibility		/
		/
Post_Disaster × High Intangibility		−0.025**
		(-1.99)
Controls	YES	YES
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	119,466	119,466
Adj. R^2	0.427	0.427

This table shows the estimates obtained from examining whether our main results are affected by the influence of natural disasters. *High Number of Subsidies* is defined as a dummy that equals 1 if the number of disaster firm's subsidiaries is larger than the mean value of the number of subsidiaries all disaster firms, and 0 otherwise. *High Intangibility* is defined as a dummy that equals 1 if the number of disaster firm's intangible assets is larger than the mean value of the intangible asset of all disaster firms, and 0 otherwise. Year-quarter, firm, brokerage, and analyst fixed effects are included. Our sample spans the period 1992 to 2017. The regressions are performed by OLS. Firm, Brokerage and Analyst fixed effects are included errors are clustered by analyst. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively. Variable definitions are presented in Appendix A.

with the specific firm and may do a better job in incorporating information into their earnings forecast. Similarly, analysts who have more working experience may be more likely to make better forecasts. We expect that the positive impact on forecast inaccuracy will be mitigated as analysts gain more work experience or as they are more likely to receive assistance from others.

To test this conjecture, we include interaction terms of *Post_Disaster* × *High Exp_gen* and *Post_Disaster* × *High Forecast Tenure* into our baseline, respectively. Consistent with previous literature, we use *Forecast Tenure* to capture analyst firm-specific experience and *Exp_gen* to capture general experience, while *Forecast Tenure* is measured as the number of quarters an analyst covers a specific firm and *Exp_gen* is the number of quarters since an analyst first appeared in IBES. As shown in Columns (1) and (2) of Table 9, we find that both interaction terms are negatively significant,[8] offering evidence on the limited attention hypothesis. The increase in forecast errors on peer firms post-natural-disasters is attenuated by analysts' general and firm-specific experience.

Large brokerage houses typically have more prestige and resources. So, when analysts from large brokerage houses experience natural disasters, they can make use of the resources of their brokerage house to maintain their forecast accuracy for other unaffected firms. We predict that the effect of natural disasters on analyst forecast inaccuracy will be weakened when analysts work for larger brokerage houses. To test this conjecture, we interact *Post_Disaster* with *High Bsize* and add this interaction term into our baseline model. As shown in Column (3) of Table 9, we find that the coefficient of *Post_Disaster x High Bsize* is negative and statistically significant at the 5% level. Overall, we find that the effect of natural disasters on analyst forecast inaccuracy will be affected by analysts' experience and work environment.

6.2. *The effect of disaster severity*

In this section, we examine how the effect of natural disasters on analyst forecast inaccuracy for unaffected firms varies depending on the potential damage to the disaster-affected firms. In the previous sections, we have observed a spillover effect of natural disasters through shared analyst coverage from disaster-affected firms to unaffected firms. We anticipate that this effect to be stronger when the potential damage to disaster-affected firms is greater.

To validate our prediction, we first investigate how the concentration of firm activities moderates our main results. If firms' activities are located in counties different from their headquarter, the impact of natural disasters on firms' operations may be weakened, potentially leading to lower damage to firms. We use the number of subsidiaries of the disaster-affected firms to proxy for the concentration of the firms' activities (*High Number of Subsidies*) and interact this with *Post_Disaster*. *High Number of Subsidies* is a dummy variable that equals 1 if the number of disaster firm's subsidiary is larger than the mean value of the number of subsidiaries of all

[8] The prefix "D_" here means that this variable is an indicator variable that equals one if the variable is larger than the mean value of the variable in the sample and 0 otherwise.

disaster firms, and 0 otherwise. In Column (1) of Table 10 we find that the coefficient on the interaction term *High Number of Subsidies x Post_Disaster* is negative and significant at the 5% level. This suggests that the effect of disasters on forecast inaccuracy is weakened when the disaster-affected firms have a lower level of activity concentration.

Similarly, the damage suffered by disaster-affected firms may also vary with the level of the firms' intangible assets. The larger the firm's intangible assets, the lower the damage that the firm will suffer from natural disasters. We create *High Intangibility*, a dummy variable that equals 1 if the number of disaster firm's intangible assets is larger than the mean value of the intangible asset of all disaster firms, and 0 otherwise and interact *High Intangibility* with *Post_Disaster*. In Column (2) of Table 10, we find that the coefficient on *High Intangibility* × *Post_Disaster* is negative and statistically significant, suggesting that our documented effect is less pronounced when the level of disaster affected firms' intangible assets is high.

7. Conclusion

We investigate whether disaster risk is systematically associated with the forecasting behavior of sell-side security analysts' annual EPS forecasts. Specifically, we explore how the effects of natural disaster spillover through shared analyst coverage. Our results suggest that when an analyst follows a firm suffering from a natural disaster, this analyst tends to issue less accurate forecasts towards the rest of the unaffected firms as she could become more decision-fatigue or concerned about the uncertainty of other firms. We show that the spillover effects through firms that share the same analyst are presented through the limited attention channel. In addition, when an analyst is more experienced, she is less likely to be affected by the salient event of a natural disaster, and when the disaster is more severe, the spillover effect is more pronounced, which is consistent with the limited attention hypothesis. Future work can explore the effect of natural disasters on other market participants and how the effect is transmitted through different connections.

We caution that the spillover effect of the natural disaster we document in this study does not alleviate the concerns that analysts themselves may also be affected by the disaster due to data availability of analyst location information. Besides, we acknowledged that our identification is based on the county where the firm's headquarter is located. It is likely that some firms' activity is located in a different county other than the headquarter county. Finally, a disaster can be flagged as occurring in one county but actually not affect a firm located in that county. Our identification is an approximation, and the actual effect may be larger. Future researchers can provide a more nuanced and accurate understanding of the spillover effect of natural disasters.

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Appendix A. Variable Definition

Variable	Definition	Source
Variables in Table 2		
Inaccuracy	The absolute difference between forecasted earnings and reported earnings scaled by the stock price at the beginning of year.	IBES
Post_Disaster	An indicator variable that equals one if the current year quarter is after the year quarter that the analyst experienced the natural disaster and 0 otherwise.	SHELDUS
ROA	Returns on assets are defined as the income before extraordinary income (ibq) to total assets (atq) in a given quarter.	Compustat
Coverage	Natural logarithm of the number of analysts following the firm in time t.	IBES
Size	Natural logarithm of a firm's market value of equity (prcc_q * cshoq) at the beginning of the reporting period.	Compustat
Loss	An indicator variable equal to one for negative income before extraordinary income(ibq) and zero at time t.	Compustat
MTB	The market value of equity (prcc_q × cshoq), divided by the book value of equity (ceqq).	Compustat
STD_Ret	The standard deviation of stock returns during the prior 12 months.	CRSP
STD_ROA	The standard deviation of returns on assets over the past 8 quarters. Returns on assets are defined as the income before extraordinary income (ibq) to total assets (atq) in a given quarter.	Compustat
RET_1y	Cumulative return of stock returns during the prior 12 months.	CRSP
Firm Age	Natural logarithm of one plus firm age.	Compustat
Horizon	Natural logarithm of the number of days until the actual earnings announcement.	IBES
Lag_Inaccuracy	The absolute difference in earnings forecast and earnings announcement scaled by the lagged year beginning stock price in t-1.	IBES
Exp_Gen	Natural logarithm of the number of quarters since an analyst first appeared in IBES.	IBES
Exp_Firm	Natural logarithm of the number of quarters an analyst covers a specific firm.	IBES

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(continued)

Variable	Definition	Source
Analyst_Sic2	Natural logarithm of the number of industries (proxied by two-digit sic codes) an analyst covers each quarter.	IBES
BSize	Natural logarithm of brokerage size, which is measured as the number of analysts employed in the brokerage of analyst i at time t.	IBES
Analyst_Firm	Natural logarithm of the number of firms that an analyst covered in quarter t.	IBES
Days Since Last Forecast	Natural logarithm of the number of days between the current forecast and previous forecast of an analyst in quarter t.	IBES
Additional Variables in Table 3		
PreQuarter 2	An indicator variable that equals one if the current quarter is 2 quarters before the disaster event quarter and 0 otherwise.	SHELDUS
PreQuarter 1	An indicator variable that equals one if the current quarter is 1 quarter before the disaster event quarter and 0 otherwise	SHELDUS
Post	An indicator variable that equals one if the current quarter is 1 quarter before the disaster event quarter and 0 otherwise	SHELDUS
PostQuarter 1	An indicator variable that equals one if the current quarter is after the disaster event quarter and 0 otherwise.	SHELDUS
PostQuarter 2	An indicator variable that equals one if the current quarter is 2 quarters after the disaster event quarter and 0 otherwise.	SHELDUS
PostQuarter 3+	An indicator variable that equals one if the current quarter is more than 2 quarters after the disaster event quarter and 0 otherwise.	SHELDUS
Additional Variables in Table 5		
Post_Disaster 1	An indicator variable that equals one if the current year quarter is after the year quarter that the analyst experienced the most serious property damage (inflation-adjusted) disaster and 0 otherwise.	SHELDUS
Post_Disaster 2	An indicator variable that equals one if the current year is after the year quarter that the analyst experienced the most fatality and 0 otherwise.	SHELDUS
Inaccuracy 1	Inaccuracy 1 is the square of difference in earnings forecast and earnings announcement scaled by the lagged year beginning stock price.	IBES
Inaccuracy 2	The proportional mean absolute forecast error is calculated as the difference between the absolute forecast error for analyst i on firm j in quarter t and the mean absolute forecast error for firm j in quarter t scaled by the mean absolute forecast error for firm j in quarter t.	IBES
Unemployment_rate	The unemployment rate of a state in year t.	U.S. Bureau of Economic Analysis (BEA)
Percap_income	Income Per Capita, which is Natural logarithm of GDP per capita at year t	U.S. Bureau of Economic Analysis (BEA)
IO	Institutional ownership in quarter t.	Thomson Reuters Institutional Holdings (13F)
ALLSTAR	An indicator that equals 1 if the analyst is named as an All-America Research Team Analyst by *Institutional Investor magazine* in year t and 0 otherwise.	Institutional Investor Magazine
Additional Variables in Table 6		
Analyst attention	Natural logarithm of the number of unique EPS revisions issued for each firm-quarter, including forecasts for all time horizons (i.e., one-year, two-year, and all other horizons).	IBES
Revision Untimeliness	Natural logarithm of the average number of days between earnings forecast revision of an analyst in quarter t.	IBES
Additional Variables in Table 7		
Bloomberg-attention	Natural logarithm of the average Bloomberg searching activities of the focal firm (ticker) in quarter t	Bloomberg
Delta_News1	Natural logarithm of changes in media sentiment (the mean value of ESS score) from quarter t-1 to quarter t.	Ravenpack
Delta_News2	Natural logarithm of changes in number of media coverage from quarter t-1 to quarter t.	Ravenpack
Additional Variables in Table 8		
Forecast bias	The difference between forecasted earnings and reported earnings is scaled by the stock price at the beginning of the year.	IBES
Connection1	An indicator variable that equals 1 if the focal firm and disaster-affected firm pair are from the sample industry, and zero otherwise.	Compustat
Connection2	An indicator variable that equals 1 if the focal firm and disaster-affected firm pair have any connection defined by FactSet Relationships Data and zero otherwise (relationship in FactSet Data includes: customer, partner, supplier, and competitor.)	FactSet
Connection3	An indicator variable that equals 1 if *Connection1* = 1 or *Connection2* = 1, and zero otherwise	FactSet & Compustat
Additional Variables in Table 9		
Forecast Tenure	The number of quarters an analyst covers a specific firm.	IBES
High Forecast Tenure	An indicator variable that equals 1 if the analyst's Forecast Tenure of the focal firm is larger than the mean value of Forecast Tenure in the sample, and 0 otherwise.	IBES
High Bsize	An indicator variable that equals 1 if the analyst's Bsize is larger than the mean value of Bsize in the sample, and 0 otherwise.	IBES
High Exp_gen	An indicator variable that equals 1 if the analyst's exp_gen is larger than the mean value of exp_gen in the sample, and 0 otherwise.	IBES
Additional Variables in Table 10		
High Number of Subsidies	An indicator variable that equals 1 if the number of disaster firm's subsidiaries is larger than the mean value of the number of subsidiaries in all disaster firms, and 0 otherwise.	Compustat
High intangibility	An indicator variable that equals 1 if the number of disaster firm's intangible assets is larger than the mean value of the intangible asset of all disaster firms, and 0 otherwise.	Compustat

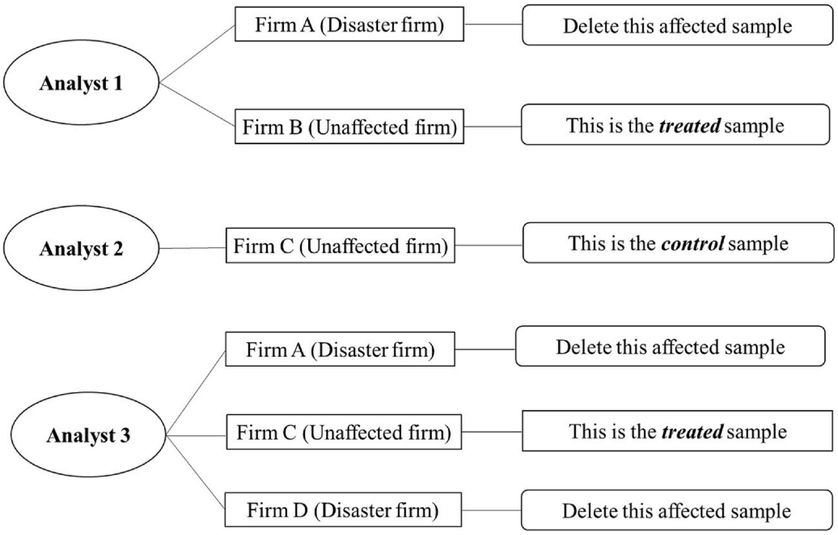
(continued on next page)

(*continued*)

Variable	Definition	Source
Delta_News1	Natural logarithm of changes in media sentiment (the mean value of ESS score) from quarter t-1 to quarter t.	Ravenpack
Delta_News2	Natural logarithm of changes in number of media coverage from quarter t-1 to quarter t.	Ravenpack

Appendix B. Examples of Treat and Control Sample

This figure illustrates three examples of how to identify treated samples and control samples.



Appendix C. Urban vs. Rural Area

Dep. Var. =	(1)	(2)
	Urban	Rural
Post_Disaster	0.042* [a]	0.023* [b]
	(1.79)	(1.66)
ROA	0.167	−0.142
	(0.40)	(-0.64)
Coverage	−0.059**	−0.047***
	(-2.02)	(-2.92)
Size	−0.345***	−0.328***
	(-13.83)	(-24.76)
Loss	0.119***	0.139***
	(4.24)	(8.35)
MTB	0.001	0.001
	(0.39)	(0.54)
STD_Ret	17.347***	14.094***
	(9.97)	(14.23)
STD_ROA	0.052	−0.038
	(0.10)	(-0.20)
RET_1y	0.015	0.017**
	(0.86)	(1.97)
Firm Age	0.229***	0.224***
	(5.54)	(12.47)
Horizon	0.065***	0.066***
	(10.35)	(16.71)
Lag_Inaccuracy	0.086***	0.149***
	(5.16)	(13.18)
Exp_Gen	−0.011	0.036
	(-0.20)	(1.14)
Exp_Firm	0.016	0.013**

(*continued on next page*)

(*continued*)

Dep. Var. =	(1) Urban	(2) Rural
	(1.46)	(2.03)
Analyst_Sic2	0.119***	0.030
	(2.69)	(1.05)
BSize	−0.019	−0.031
	(-0.41)	(-1.23)
Analyst_Firm	−0.035	−0.019
	(-1.03)	(-0.93)
Days Since Last Forecast	−0.005	0.005*
	(-0.86)	(1.71)
Year-Quarter FE	YES	YES
Analyst FE	YES	YES
Firm FE	YES	YES
Brokerage FE	YES	YES
N	32,475	88,282
Adj. R^2	0.455	0.423
Coeff Diff (a-b)	$p < 0.001$	

This table presents the outcomes of the subsample analysis categorized by whether the firms affected by disasters are located in urban or rural areas. Our dataset covers the timeframe from 1992 to 2017. The regressions are conducted via Ordinary Least Squares (OLS) method, incorporating fixed effects for Year-Quarter, Firm, Brokerage, and Analyst. Standard errors are clustered by analyst. Statistical significance levels are denoted by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are presented in Appendix A.

Appendix D. Baseline Regression Results for the Period 2018–2022

Dep. Var. =	(1) Inaccuracy	(2) Inaccuracy	(3) Inaccuracy
Post_Disaster	0.275**	0.214*	0.214*
	(2.50)	(1.90)	(1.83)
ROA		0.130	0.654
		(0.20)	(1.37)
Coverage		−0.012	−0.001
		(-0.08)	(-0.00)
Size		−0.458***	−0.426***
		(-5.81)	(-5.48)
Loss		0.088	0.103*
		(1.48)	(1.79)
MTB		−0.000	0.000
		(-0.25)	(0.06)
STD_Ret		7.606*	6.734
		(1.70)	(1.52)
STD_ROA		−0.975	−0.537
		(-1.01)	(-0.56)
RET_1y		0.119***	0.119***
		(2.61)	(2.65)
Firm Age		0.898***	0.641**
		(3.13)	(2.23)
Horizon			0.055***
			(3.04)
Lag_Inaccuracy			0.011***
			(5.10)
Exp_Gen			0.400*
			(1.88)
Exp_Firm			0.059**
			(2.11)
Analyst_Sic2			−0.387*
			(-1.73)
BSize			−0.247
			(-1.10)
Analyst_Firm			−0.059

(*continued on next page*)

(*continued*)

Dep. Var. =	(1) Inaccuracy	(2) Inaccuracy	(3) Inaccuracy
			(-0.50)
Days Since Last Forecast			−0.013
			(-0.74)
Year-Quarter FE	YES	YES	YES
Analyst FE	YES	YES	YES
Firm FE	YES	YES	YES
Brokerage FE	YES	YES	YES
N	7720	7720	7720
Adj. R^2	0.501	0.510	0.516

This table presents the baseline results using recent period from 2018 to 2022. The regressions are conducted via Ordinary Least Squares (OLS) method, incorporating fixed effects for Year-Quarter, Firm, Brokerage, and Analyst. Standard errors are clustered by analyst. Statistical significance levels are denoted by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are presented in Appendix A.

Data availability

The authors do not have permission to share data.

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