Flu Fallout: Information Production Constraints and Corporate Disclosure

Chen Chen
Monash University
Department of Accounting
Monash Business School
900 Dandenong Road, Caulfield East
VIC 3145, Australia
chen.chen2@monash.edu

Leonard Leye Li
UNSW Sydney
School of Accounting, Auditing & Taxation
The UNSW Business School
College Road, Kensington
NSW 2052, Australia
leye.li@unsw.edu.au

Louise Yi Lu
Australian National University
Research School of Accounting
Kingsley Street, Acton
ACT 2601, Australia
louise.lu@anu.edu.au

Rencheng Wang*
Singapore Management University
School of Accountancy
60 Stamford Road
178900, Singapore
rcwang@smu.edu.sg

This Version: March 2023

Acknowledgement: Accepted by Rodrigo Verdi. We are indebted to an anonymous associate editor and an anonymous reviewer for highly valuable comments and suggestions that helped significantly improve the paper, and Sonali Walpola for detailed feedback on each revision. We appreciate constructive and insightful suggestions from John Campbell, Xia Chen, Qiang Cheng, Vivian Fang (discussant), Wayne Guay, Charles Hsu, Ting Jiang, Robert Knechel, Wayne Landsman, Ningzhong Li, Gerald Lobo, Yun Lou, Xiumin Martin, Gary Monroe, Venky Nagar, Jeffrey Ng, Zihang Peng, Jeff Pittman, Greg Shailer, Lakshmanan Shivakumar, Jin Wang (discussant), Holly Yang, Yangxin Yu, Liandong Zhang, and workshop participants at Fudan University, Hong Kong Polytechnic University, Monash University, University of Sydney, Wuhan University, the 2020 Accounting and Finance Association of Australia and New Zealand (AFAANZ) Annual Conference, the 2020 Midyear Meeting of the Financial Accounting and Reporting Section (FARS), and the 2020 SMU Accounting Symposium. We thank four practitioners in S&P 500 firms for sharing their experiences of the forecasting processes. We express appreciation to Kathy Xuejun Jiang, Yongtao Jiang, Madhukar Singh, Ben Wang, and Kathy Dongyue Wang for their excellent research assistance. We also thank our respective schools for their financial support. Wang acknowledges the support from University of Melbourne with which he was affiliated while part of the work on this paper was done. All errors and omissions are our own. An Online Appendix to this paper can be downloaded at https://www.chicagobooth.edu/jar-online-supplements.

*Corresponding author.

Flu Fallout: Information Production Constraints and Corporate Disclosure

Abstract

Using influenza epidemic data, we examine how constraints on corporate information production affect disclosure policies. We find that firms in areas with higher flu activity are less likely to issue short-run earnings forecasts and more likely to issue long-run earnings forecasts. These results are more pronounced when the information production process is more complex, when managers face a greater reputational loss for issuing low-quality short-run forecasts, and when firms' costs of switching the forecast horizon are lower. Further analysis implies that the effect of flu activity on these forecast issuance decisions is not driven by firm performance or information uncertainty. Our results suggest that managers do not simply avoid issuing forecasts in response to information production constraints. Instead, they shift the forecast horizon from short-run to long-run, appearing to balance the costs of issuing low-quality forecasts with those of not issuing forecasts at all.

JEL codes: D8, M41, I10, I18, J10, J32

Keywords: information production constraints, flu epidemic, management forecast, corporate disclosure

1. Introduction

Previous studies find that investors' capacity to process information is constrained by their need to allocate time and effort across various activities (Blankespoor, deHaan, and Marinovic [2020]). Survey evidence suggests that firms may face similar constraints on the production of high-quality information due to the demands on employees in terms of time and effort (Hsieh, Koller, and Rajan [2006]; Ernst and Young [2015]). However, empirical evidence of the impact of these constraints on firms' information production is limited. We help fill this gap in the literature by examining how firms' constraints on information production affect managers' forecast issuance policies.

Theoretical work and empirical evidence suggest that managers have strong incentives to issue credible forecasts and maintain a transparent information environment (e.g., Trueman [1986]; Healy and Palepu [2001]; Graham, Harvey, and Rajgopal [2005]). We argue that managers have two potential disclosure choices when firms face constraints on generating high-quality information. The first choice, as previous researchers suggest, is to cease issuing forecasts (e.g., Feng, Li, and McVay [2009]; Dorantes et al. [2013]; Call et al. [2017]; Chen et al. [2018]). Managers make this decision because such constraints tend to increase information production costs and decrease the accuracy of forecasts. This exposes managers to reputational loss should a forecast later prove inaccurate. However, this strategy of silence may also be costly since it violates firms' commitment to following transparent disclosure policies and contributes to information asymmetry (e.g., Grossman [1981]; Milgrom [1981]; Houston et al. [2010]; Chen et al. [2011]; Baginski and Rakow [2012]).

These concerns may lead to the pursuit of the second choice, in which managers may take a middle position between non-issuance and issuing dubious forecasts by issuing long-run instead of short-run forecasts. This trade-off strategy allows managers to continue to provide forecasts, thereby reducing the costs of cutting all forecasts. It can also address managers' reputational concerns about issuing low-quality forecasts, as the ex-post costs of issuing inaccurate forecasts for managers are lower for long-run forecasts. Specifically, the mistakes in long-run forecasts can be more easily attributed to "unavoidable errors" than those in short-run forecasts (Gong, Li, and Xie [2009], p. 502). As managers have more opportunities to revise and correct errors in long-run forecasts, investors may view such corrections as informative updates (e.g., Trueman [1986]).

To investigate this issue, we use flu activity in the area surrounding a firm's headquarters during a period of intensive information production to measure the information production constraints on a firm. The flu imposes staffing constraints on firms when employees take sick leave or work while ill, care for infected family members, or cover sick colleagues. Therefore, infectious illness is a major cause of lost work time and effort (for a review, see Keech and Beardsworth [2008]). Considering that information production is labor-intensive and requires coordination and collaboration among employees, the flu imposes severe constraints on this activity.

We analyze a sample of 86,483 firm-quarter observations from 2003 to 2018. We define the period between the end of the fiscal period and the earnings announcement (EA) date as a firm's "information production window" (see Figure 1). We then measure flu activity using the average weekly data for outpatient visits to healthcare providers for influenza-like illness (ILI) in a U.S. state where a firm's headquarters is located during its information production window. First, as a validation test, we demonstrate that firms experience longer reporting lags and are more likely to produce financial statements with errors when the level of flu activity is higher. A one-standard-deviation increase in flu activity corresponds to a one-day increase in reporting lag and 6% more financial statement errors compared to the baseline rate. This finding supports our assumption that the level of flu activity is positively associated with information production constraints.[1]

In our main analysis, we examine changes in managers' policies of issuing forecasts in response to these constraints. Focusing on earnings forecasts bundled with EAs—those issued at the end of the information production window—we show that, among the firms headquartered in states with higher levels of flu activity, there is no change in the likelihood of issuing a forecast (when short- and long-run forecasts are considered in aggregate). However, these firms are less likely to issue short-run forecasts (i.e., for the immediate next fiscal quarter) and more likely to issue long-run forecasts (i.e., for the fiscal periods beyond the immediate next fiscal quarter). In short, our results suggest that managers continue to issue forecasts when their firms face constraints on information production, but they shift their focus from short- to long-run forecasts.[2] Our results are consistent with the view that managers are mindful of the costs associated with issuing low-quality forecasts and not issuing any forecasts and make a strategic trade-off in selecting forecasts to reduce both types of costs.

Next, we conduct multiple cross-sectional analyses based on our prediction that managers may adopt this trade-off strategy. First, we expect that the flu has a larger effect on firms in which the information production process is more complex because these firms face more binding constraints. Second, the flu should have a larger effect on managers who are more concerned about the reputational loss associated with issuing low-quality short-run forecasts. Third, we expect the effects of the flu to be negatively associated with the cost of changing the forecast horizon. As predicted, we find a stronger flu effect when the information production

[1] In all of our subsequent analyses, we control for reporting errors and lags and ensure that our findings are not simply driven by reporting quality in general.

[2] Importantly, our findings do not suggest that managers always prefer long-run forecasts. Previous studies suggest that managers' decisions to issue short-run forecasts may be motivated by various factors other than information production constraints. We discuss them in detail in section 2.3. In addition, if firms are already used to issue only long-run forecasts for other reasons, they may serve as a part of benchmark cases in our analysis and our results are robust after excluding them (untabulated). We further discuss how the forecast regularity affects our main findings in section 4.4.3.

process is more complex, as measured by factors such as the number of business segments, involvement in acquisitions, and the number of XBRL tags in financial reports. The flu effect is more pronounced for firms with a stronger track record of short-run forecast accuracy. Additionally, firms that have regularly issued forecasts and have changed their forecast horizons experience a greater impact of flu. These cross-sectional results support our main findings and highlight the conditions where managers are most likely to use this trade-off strategy.

We perform several additional tests. We show that our results relating to the trade-off strategy are robust to controlling for the effect of flu activity on firms' performance and information uncertainty and the effect of weather-induced mood (deHaan, Madsen, and Piotroski [2017]; Chen et al. [2022]). Our results continue to hold after using various approaches to mitigate potential measurement errors of flu activity and possible correlated omitted variables. Furthermore, we find that long-run forecasts are less specific and with longer horizons for firms with high flu activities. Overall, our results are consistent with the view that managers continue to issue forecasts strategically to reduce reputational loss associated with issuing inaccurate forecasts and the penalties for guidance cessation when their firms face information production constraints.

In all our main and additional analyses, we control for reporting lag. In our final set of analyses, we examine whether there can be a trade-off between reporting timeliness and forecasting thoroughness. On the one hand, a longer reporting lag could indicate a more severe constraint on information production, making managers more concerned about the impact of the flu on forecast quality. On the other hand, such a lag could provide managers with additional time to improve their forecasts and mitigate their concerns about the impact of flu. We find that our results regarding forecast issuance decisions become stronger when the reporting lag is longer and EAs are more delayed (relative to the 10-Q/K filing statutory deadline set by the

SEC). These findings suggest that the increase in firms' reporting lags reflects the negative impact of flu on information production constraints.

To provide more direct evidence, we further examine the interaction between reporting timeliness and forecasting thoroughness and find that the accuracy of long-run forecasts is negatively associated with the level of flu activity around firms' headquarters. This effect is more pronounced when the reporting lag is longer. These results are consistent with the view that devoting additional time to preparing forecasts when flu activity is higher cannot entirely offset the negative effects of flu activity on forecast quality. Instead, managers may be more likely to be aware of the negative impact of flu activity on information production constraints because of delays.[3]

Our study contributes to the literature on information processing constraints. Previous research mainly focuses on the users of information (Hirshleifer, Lim, and Teoh [2009], deHaan, Shevlin, and Thornock [2015]; Drake, Gee, and Thornock [2016]; Akbas et al. [2018]; Blankespoor et al. [2020]; Driskill, Kirk, and Tucker [2020]). In response to the call by Blankespoor et al. [2020] to study the effect of information processing constraints on other players in the capital market, our study examines the effect on the providers of information, firms. We show that constraints on firms' information production prompt managers to issue forecasts strategically in the manner described above. Given that management forecasts provide over 50% of relevant accounting-based information to the capital market (Beyer, Cohen, Lys and Walther 2010), documenting the effect of information production constraints from the perspective of information providers enables a more comprehensive understanding of these constraints.

[3] Conversely, we do not find the flu or the extension of reporting lags to significantly impact short-run forecasts' accuracy. It is consistent with our main findings that managers are self-selected to continue to issue fairly accurate short-run forecasts when the flu is high. This is because the reputational loss associated with issuing low-quality short-run forecasts is higher than long-run forecasts.

Second, we build on prior studies that document a positive relation between internal information quality and external disclosure quality (e.g., Hemer and Labro [2008]; Feng et al. [2009]; Gong et al. [2009]; Call et al. [2017]; Chen et al. [2018]). From an empirical perspective, prior research on management forecasts either does not explicitly distinguish the horizons or focuses on one type of forecast horizon. More importantly, from a theoretical perspective, previous studies emphasize either managers' concerns about issuing low-quality forecasts (Feng, Li, and McVay [2009]; Dorantes et al. [2013]; Call et al. [2017]; Chen et al. [2018]) or the costs of not issuing forecasts (Houston et al. [2010]; Chen et al. [2011]; Baginski and Rakow [2012]) without considering the interaction of the two. We present a more comprehensive picture by showing that when information production is constrained, managers tend to make a strategic trade-off in their selection of forecast horizon. Our findings suggest that the positive relationship between internal information quality and external disclosure quality documented in the literature may be associated with the strategic trade-off made by managers, as they self-select to issue long-run forecasts when internal information quality is low.

2. Background, Motivation, and Empirical Predictions

2.1. Background and Motivation

Previous research suggests that investors' information processing is constrained by the time and effort they devote to other activities (e.g., Hirshleifer et al. [2009], deHaan et al. [2015]; Drake et al. [2016]; Akbas et al. [2018]; Driskill et al. [2020]). Little is known about whether and how corporate disclosure policies are affected by constraints on firms' information processing capacity, which may prevent them from generating high-quality information (Blankespoor et al. [2020]).[4] This is an important managerial concern because managers have

[4] A few recent studies have examined managers' constraints regarding information-processing capacity (Chen, Demers, and Lev 2018; Chen et al. 2022). Our study is distinct from the previous research as we do not posit that such constraints are only for managers. More importantly, we focus on managers' strategic disclosure choices in

strong incentives to cultivate their reputations regarding forecast quality. In particular, theoretical work suggests that forecast quality serves as a public signal regarding managers' knowledge of their firms' economic environments and their abilities to manage future business prospects (Trueman [1986]; Healy and Palepu [2001]). Consistent with this view, survey evidence suggests that managers consider their forecasting credibility to be critical and believe that they pay a career penalty for being "seen as either an incompetent executive or a poor forecaster" (Graham et al. [2005], p.28).

On the demand side, empirical studies show that both investors and boards of directors value managers' forecast quality. Some, for example, indicate that investors react strongly to forecasts by forecasters who have been accurate in the past (Yang [2012]; Ng, Tuna, and Verdi [2013]; Hutton and Stocken [2021]). Similarly, previous research on long-run forecasts suggests that investors infer the quality of corporate investment decisions based on forecast quality, and the managerial labor market uses it as a measure of managerial quality (Goodman et al. [2014]; Hui and Matsunaga [2015]). Previous studies of short-run forecasts show that boards of directors may replace their CEOs for issuing inaccurate forecasts (Lee et al. [2012]). On the supply side, recent studies document that managers have incentives to avoid issuing forecasts when they anticipate the forecast quality to be low. For example, firms with repeated internal control weaknesses, which have not implemented enterprise systems and have lower-quality local human capital, are reluctant to issue forecasts (Feng et al. [2009]; Dorantes et al. [2013]; Call et al. [2017]).[5] Chen et al. [2018] show that information asymmetry between divisional and top managers reduces forecast frequency.

response to the constraints rather than their unconscious reactions. We address the possibility that the effect of mood may have influenced our results in section 6.2.

[5] Note that these studies examine the frequency or likelihood of issuing annual earnings forecasts ("ANN" in the First Call and I/B/E/S Guidance databases). However, these annual forecasts range from years to a few days before the end of the fiscal year (e.g., Tang, Yao, and Zarowin [2016]).

We identify two major questions that remain unanswered in the literature. First, researchers focus primarily on managers' concerns about issuing low-quality forecasts while paying relatively little attention to the costs of not issuing forecasts (e.g., Milgrom [1981]; Diamond and Verrecchia [1991]; Kasznik and Lev [1995]; Tucker [2007]; Houston et al. [2010]; Chen et al. [2011]; Baginski and Rakow [2012]). Therefore, it is unclear whether and how managers balance the reputational loss associated with issuing low-quality forecasts and the costs of not issuing forecasts. Second, prior research either does not distinguish forecast horizons explicitly or focuses on one type of forecast horizon. Therefore, it is unclear whether reputational concerns for managers vary with the forecast horizon and whether this subsequently affects decisions relating to the issuance of forecasts.[6]

2.2. Flu and Constraints on Information Production

Flu is a highly contagious, acute, febrile respiratory viral disease that is regarded as a major cause of employee time and effort losses in the workplace (see Keech and Beardsworth [2008] for a review).[7] Its debilitating effects typically leave sufferers bed-ridden during the acute phase of the infection and may require several weeks of recovery, so infected employees face substantial absences, or are usually capable of less effort and produce lower-quality outcomes if they continue to work while being ill (Palmer et al. [2010]; Smith [2013]). Additionally, because patients with influenza usually require care, the disease can constrain the time and effort of uninfected individuals who need to care for infected family members or friends (Principi et al. [2004], Teo et al. [2005]).[8] Moreover, healthy employees may be required to assume additional workload to complete the work usually performed by infected

[6] Additionally, while these studies suggest that internal information quality and external disclosure quality are associated, it is unclear whether this positive association is an outcome of managerial, strategic forecast policy (i.e., a self-selected outcome made by managers). We explore this issue in section 5.2.

[7] Research in health economics documents the significant adverse macro-consequences of influenza (Stewart et al. [2003]; Molinari et al. [2007]; Keech and Beardsworth [2008]; Peasah et al. [2013]; Petrie et al. [2016]).

[8] Studies show that caring for flu-infected family members impairs employees' ability to function normally at work and places them at risk of infection, thereby increasing the potential loss of work time (Principi et al. [2004]; Bosis et al. [2005]; Esposito et al. [2005]; Palmer et al. [2010]; Van Wormer et al. [2017]).

employees or experience delays in delivering their work because of reliance on the inputs from infected employees (Severens et al. [1998]; Krol et al. [2012]; Rost et al. [2014]).

Therefore, flu activity can constrain a firm's information production capacity and increase managers' concerns about their firms' ability to generate high-quality information for forecasting purposes. Information production—and particularly forecasting—is a labor-intensive task that requires coordination and collaboration in a team setting such that the input of one employee is likely to affect the quality of others' work.[9] Moreover, firms face pressure to produce high-quality information within certain timeframes, and this pressure intensifies in the context of issuing forecasts in EAs (Sherin [2010]; Gong et al. [2011]). Therefore, the negative effect of absenteeism caused by the flu when employees tend to continue working, is compounded when illness constrains their information production capacity, and the likelihood of producing low-quality corporate information increases.[10] Hence, flu activity can constrain a firm's information production capacity and diminish the quality of the information used for forecasting.

2.3. Empirical Predictions

Managers naturally develop disclosure policies by evaluating the costs and benefits associated with issuing forecasts (Verrecchia [1983], [1990]; Lennox and Park [2006]). We expect managers to develop disclosure policies that minimize these negative impacts if flu-induced constraints on information production limit firms' capacity to produce high-quality internal information. In this context, managers have two options. The first option is not to issue

[9] In our discussion with them, four practitioners in S&P 500 firms located in the United States indicate that the forecasting process requires inputs and attention from employees across departments and levels. Specifically, forecasts are prepared primarily by a firm's financial planning and analysis (FP&A) team, which summarizes, reviews, and communicates with other business units (e.g., sales, operations, and financial reporting) regarding the information required to produce forecasts.

[10] Practitioners in S&P 500 firms emphasize the importance of preparing forecasts that align with EAs, but note that it can be challenging due to time constraints and heightened public scrutiny. Our discussions with these practitioners reveal that they typically work two to three additional hours per day when EA dates are approaching, and that other teams within the firm are also expected to increase their efforts to support the FP&A team. Sherin [2010] provides similar anecdotal evidence.

a forecast, as prior research suggests (Feng et al. [2009]; Dorantes et al. [2013]; Call et al. [2017]; Chen et al. [2018]). Managers may decide not to issue short-run forecasts for the following reasons. As short-run forecasts are made close to the realization of actual earnings, investors may believe that managers should have sufficient information to make fairly accurate predictions. Consequently, managers may expect to bear reputational damage when issuing short-run forecasts with errors. For example, low-quality short-run forecasts could be attributed to a deliberate manipulation by managers or their inferior forecasting ability (Kasznik [1999]; Lee et al. [2012]; Hutton and Stocken [2021]). Managers may avoid issuing short-run forecasts under flu-induced capacity constraints because of this reputational concern.

Managers may also choose not to issue long-run forecasts for a different reason. As these forecasts are issued further in time from the realization of actual earnings, they are subject to greater uncertainty (e.g., because of unforeseen shifts in market demand or competitors' strategies) than are short-run forecasts (Gong et al. [2011]; Hribar and Yang [2016]). Hence, long-run forecasts require more time and effort to produce accurate and relevant information compared to short-run forecasts. They are more likely to be materially impaired by flu-induced constraints on information production. Consequently, managers may avoid issuing long-run forecasts, especially under such constraints.

However, the decision not to issue earnings forecasts also involves costs; in this case, the costs include an increase in information asymmetry, as firms violate their commitment to providing transparent disclosure (e.g., Grossman [1981]; Milgrom [1981]; Houston et al. [2010]; Chen et al. [2011]; Baginski and Rakow [2012]). For example, Houston et al. [2010] find that analyst following decreases and analyst forecast errors and dispersion increase after managers reduce their firms' forecast frequency. Chen et al. [2011] find that the stock market tends to react negatively when firms cease issuing forecasts. Thus, managers may decide to issue forecasts, even though they are relatively inaccurate, considering the costs of not issuing them.

These concerns may lead to the development of a second option. Managers may choose between non-issuance and dubious forecasts by issuing long-run forecasts instead of short-run forecasts. This strategy can minimize the potential reputational loss associated with issuing low-quality forecasts because investors' threshold for tolerating forecast errors may be lower for short-run than long-run forecasts. First, it is easier to attribute the mistakes in long-run forecasts to "unavoidable errors" (Johnson, Kasznik, and Nelson [2001]; Gong et al. [2009], p.502; Hutton and Stocken [2021]). Second, as long-run forecasts are issued further away from actual earnings realizations, they give managers more opportunities to revise erroneous predictions than short-run forecasts (Tang, Yao, and Zarowin [2016]). Investors may view such revisions positively as informative updates from managers' assessments of business prospects (e.g., Trueman [1986]). Accordingly, when firms face flu-induced constraints on information production, we expect that their managers balance the costs of issuing low-quality forecasts and those of not issuing forecasts by avoiding short-run forecasts and issuing long-run forecasts.

Notably, we do not argue that managers always prefer long-run forecasts, even without information production constraints. Previous studies suggest that managers' decisions to issue short-run forecasts may be motivated by various factors other than information production constraints. These factors include firm performance (Dye [1985]; Beyer et al. [2010]; Houston et al. [2010]; Chen et al. [2011]; Rogers and Van Buskirk [2013]), information demand relating specifically to short-run performance (Ajinkya, Bhojraj, and Sengupta [2005]; Palter et al. [2008]; Kim et al. [2017]), the incentive to meet or beat earnings targets in the near future (e.g., Kross et al. [2011]), and the pressure to follow the disclosure practices of industry peers (Houston et al. [2010]).

Given these considerations, the effect of information production constraints on forecast issuance policies remains unclear. In summary, managers may avoid issuing forecasts or make a strategic trade-off in their choices of forecast horizons to reduce information asymmetry and protect their reputations as credible forecasters.

3. Sample and Research Design

3.1. Sample Construction

We begin with 525,849 firm-quarter observations of U.S.-incorporated firms from the Compustat database for 2003 through 2018. We exclude 250,013 firm-quarter observations that lack EA dates from I/B/E/S or for which the EA date is after the end of the next fiscal quarter ($EA_{i,t}$ is later than $FQE_{i,t+1}$ in Figure 1). We then merge the data with weekly state-level flu activity data from the Centers for Disease Control (CDC) website and Google Flu Trends (GFT) using the information on the states in which firms are headquartered from the historical 10-K header data provided by the Notre Dame Software Repository for Accounting and Finance (SRAF). The flu-activity data coverage is available from June 2003 to September 2009 and extends from October 2010 onward. We exclude the missing timing between these two periods because of the data availability of our sample. Next, we exclude firms with no earnings forecasts during the sample period (144,766 observations). We merge the firms' financial information, stock returns, and analyst information from Compustat, CRSP, and I/B/E/S, respectively, and exclude 42,265 observations that lack sufficient data to calculate the control variables. Finally, we exclude the 215 singleton observations within the FE groups in equation (1). These steps provide us with a final sample of 86,483 firm-quarter observations. Table 1 summarizes the sampling procedure.

3.2. Regression Model

To test our empirical question, we examine the effect of flu activity during a specific information production window on a firm's forecast issuance at the end of the window. We define this window as the period between the end of the fiscal period and the EA date (i.e., from $FQE_{i,t}$ to $EA_{i,t}$ in Figure 1) and focus on the issuance of bundled earnings forecasts. Bundled earnings forecasts refers to the earnings-per-share (EPS) forecasts issued within the window of [−1, +1] of the EA date ($EA_{i,t}$ in Figure 1; Zhang [2012], Rogers and Van Buskirk [2013]). We estimate the regression model as follows:

$$Forecast\ Issuance_{i,t} = \beta_1 Flu_{s,t} + Controls_{i,t} + Fixed\ Effects + \varepsilon_{i,t}, \quad (1)$$

where i, s, and t denote the firm, headquarters' state, and year quarter, respectively. We employ three proxies for forecast issuance (*Forecast Issuance*): (1) *Issue*, an indicator of the issuance of at least one bundled earnings forecast; (2) $Issue^{Short}$, an indicator of the issuance of bundled earnings forecasts for fiscal quarter $t + 1$ (i.e., the next fiscal quarter); and (3) $Issue^{Long}$, an indicator of the issuance of bundled earnings forecasts for fiscal periods beyond quarter $t + 1$ (i.e., the fiscal periods after the next fiscal quarter).

We construct our variable of interest, *Flu*, as the flu activity in the state in which a firm is headquartered averaged during the information production window measured in weeks. The CDC calculates flu activity by the ILI rate, which is the number of influenza-like illness (ILI) cases scaled by the number of total outpatient visits to healthcare providers in a state weekly.[11] Both numbers are reported voluntarily by local healthcare providers to the CDC. Since both are measured on the same basis, ILI is more comparable across periods and states than scaling the number of flu cases by other factors (e.g., state population) before including fixed effects in Equation (1) (Jennings et al. [2023]). The average flu activity during the CDC-defined season-peak periods is 2.1%, while it is 1.4% during other periods in our sample.[12] In our

[11] The CDC defines ILI as a complex of symptoms, including fever (a temperature of 100°F [37.8°C] or greater) and cough and sore throat without a known cause other than influenza. Our results are robust to using alternative measures (for which see Tables OA3 and OA5 in the Online Appendix).
[12] As an illustration, the national-level estimates by the CDC underscore the significance of season-peak flu activity of the 2015–2016 season, with a season-peak ILI rate of approximately 3.5% corresponding to 24 million influenza cases, 11 million influenza-associated medical visits, 280,000 influenza-related hospitalizations, and 23,000 influenza-associated deaths. See https://www.cdc.gov/flu/about/burden/index.html for the CDC's burden

setting, the value of *Flu* reflects the severity of ILI in the state where a firm's headquarters are located such that the time and effort employees allocate to information production decline, thereby constraining corporate capacity for such production.

We include firm fixed effects (FE) to control for time-invariant firm characteristics that may impact forecast behavior. As we aim to capture the effect of abnormal variations in flu activity on corporate disclosure, we include headquarters' state × calendar year-quarter FEs. This is to control for macroeconomic conditions, seasonal flu patterns across years, and other unobserved time-varying heterogeneity within the states where the firms are headquartered with the potential to affect state-level flu activity and corporate disclosures. We cluster the standard errors by headquarters' state × calendar year-quarter, defining the calendar year-quarter based on the EA date ($EA_{i,t}$ in Figure 1).[13]

Following previous research (e.g., Houston et al. [2010]; Chen et al. [2011]; Rogers and Van Buskirk [2013]), we include a set of control variables that affect firms' decisions regarding the issuance of management forecasts (bundled with EAs): firm size (*Size*) (Kasznik and Lev [1995]), market-to-book ratio (*MTB*) (Bamber and Cheon [1998]; Chen, Cheng, and Lo [2010]), analyst coverage (*Coverage*) (Ajinkya et al. [2005]; Kross, Ro, and Suk [2011]), percentage of institutional ownership (*IOR*) (Ajinkya et al. [2005]), performance volatility (*EPSVolt*), analyst forecast dispersion (*Dispersion*) (Kross, Lewellen, and Ro [1994]; Heflin, Kross, and Suk [2016]), the record of meeting or beating market expectations (*MBanalyst*) (Houston et al. [2010]), and firm performance (*Loss*, *ΔEPS*, *FutureEPS*, and *Return*) (Baginski, Hassell, and Kimbrough [2002]; Miller [2002]; Houston et al. [2010]). We also control for

estimates, and https://www.cdc.gov/flu/season/past-flu-seasons.htm for the CDC's definitions of season-peak periods.

[13] The magnitude of flu is highly variable over locations and time, being predominantly dependent on the properties of the infecting virus (e.g., Charu et al. [2017]; Coletti et al. [2018]; Dalziel et al. [2018]). In a robustness check, our results do not change when we exclude the firms that relocate their headquarters to a different state during our sample period (untabulated). Our results are also unaffected when considering alternative FE specifications (Table OA4 in the Online Appendix).

disclosure practices in a firm's industry (Dye and Sridhar [1995]; Gul and Lundholm [1995]; Tse and Tucker [2010]), measured by industry peer's average bundled forecast tendency (*IndIssue*, $IndIssue^{Short}$, and $IndIssue^{Long}$) (Houston et al. [2010]). Finally, we control for lags in financial reporting (*Lag*) and financial statement errors (*Error*) and mitigate the concern that our results are purely driven by quality of other information production activities. Appendix A presents the definitions of these variables. We winsorize all continuous variables at the 1st and 99th percentiles.

4. Main Empirical Results

4.1. Descriptive Statistics

Panel A of Table 2 presents the descriptive statistics of the sample. On average, 58.0% of the firm-quarters in our sample issue bundled earnings forecasts, 36.3% issue bundled earnings forecasts for the next quarter, and 36.7% issue bundled earnings forecasts for the fiscal periods beyond the next quarter.[14] The mean value of *Flu* is 0.015, suggesting that on average 1.5% of outpatient visits to healthcare providers in our firms' headquarters states are flu cases. Following deHaan (2021), we also report the distributional statistics of within-FE variations. The third column presents the standard deviations of our main variables after they are orthogonalized to all FE groupings (i.e., firm and state × year-quarter). The fourth column presents the within-FE standard deviation divided by the pooled standard deviation, which captures the reduction in variance caused by FE. After considering the FE for flu activity (*Flu*), the reduction in variation is closer to that of firm size (*Size*) than for the other variables.

Figure 2A presents the distribution of our baseline construct—weekly flu activity—measured as the proportion of outpatient visits to healthcare providers for ILI in a state over our sample period. Each dot in the figure represents the level of weekly flu activity in a state.

[14] Most of the long-run forecasts are for the current fiscal year end and less than 10% of the long-run forecasts in our sample have forecast horizons longer than 365 days.

There are significant variations in the magnitude and timing of flu peaks. Specifically, we find that between 2003 and 2018, the average flu activity during the CDC-defined season-peak periods ranges from 1.5% to 7.8%. These season-peak periods can last between 12 and 21 weeks, with the season peak, the highest level of flu activity in a year, occurring in December, January, or February. Figure 2B presents the year-by-year influenza spatial dynamics across states measured as the average value of weekly flu activity in each state and year. An increase in the size of a circle or a shift from yellow to red in the figure indicates an increase in flu activity. Consistent with the properties of flu activity described by the CDC and in epidemiological studies, as seen in Figures 2A and 2B, while activity exhibits some seasonality, the spatiotemporal patterns of flu distribution vary greatly over the years and across states (e.g., Charu et al. [2017]; Dalziel et al. [2018]).[15] This result reflects the importance of including headquarters' state × calendar year-quarter FEs.

Panel B in Table 2 reports the correlations between our variables. Focusing on firms' decision to issue forecasts bundled with an EA, we find that, while the level of flu activity, *Flu*, is not significantly correlated with the issuance of forecasts in general (*Issue*), *Flu* is negatively and significantly correlated with the issuance of short-run forecasts ($Issue^{Short}$) and positively and significantly correlated with the issuance of long-run forecasts ($Issue^{Long}$). These results provide preliminary support for our conjecture that managers may be concerned with balancing the costs of issuing low-quality forecasts and not issuing forecasts, hence making a trade-off between forecast horizons. Finally, the correlations between *Flu* and the control variables are small. The highest variance inflation factor is only 1.67 (untabulated), indicating relatively low multicollinearity among the predictors of forecast issuance decisions.

[15] In a given week during the peak 2017 flu season, the level of flu activity ranged from no activity (e.g., Oregon) to widespread activity (e.g., Texas). In addition, over our sample period, flu activity is not always concentrated in a certain state and flu activity is spatially distributed across time. For example, although Texas generally has more flu activity than other states, its flu activity level also varies across years.

4.2. Flu Activity and Constraints on Information Production

Before conducting our main tests, we examine whether flu activity in the state in which a firm is headquartered during the information production window constrains the firm's information processing capacity. First, we examine whether firms experience longer reporting lags (i.e., the time between the end of the fiscal period and the EA) during periods when flu activity increases. We regress *Lag* on *Flu* with the control variables defined in equation (1). As column 1 in Table OA1 of the Online Appendix shows, the coefficient on *Flu* is positive and significant. This result is consistent with the notion that managers delay their earnings reports when flu activity surges. Second, we focus on errors in financial statements that do not refer to financial fraud, irregularities, intentional misrepresentation, or other behaviors relevant to investigations by regulators. We develop an indicator variable, *Error*, with a value of 1 when the financial report issued in fiscal quarter t is subsequently restated because of errors and 0 otherwise. As column 2 in Table OA1 shows, we find the coefficient on *Flu* to be positive and significant, consistent with flu constraining firms' information production. The economic significance of *Flu* is also comparable to the prior studies (e.g., Jha and Chen [2015]; Call et al. [2017]; Hoitash and Mkrtchyan [2022]). Specifically, a one-standard-deviation increase in *Flu* be associated with an increase of 1.04 day ($= e^{[3.075 \times 0.013]}$) in reporting lag and 6.26% ($= 0.289 \times 0.013 \div 0.06$) in the likelihood of reporting errors (relative to the average *Error*).[16]

4.3. Main Analysis of Flu Activity and Forecast Issuance

4.3.1. Baseline Analysis

Panel A of Table 3 reports the results based on equation (1). We find the coefficient on *Flu* to be insignificant at the conventional level ($p > 0.10$) when the dependent variable is *Issue*

[16] We also follow deHaan (2021) and re-calculate the economic significance using within-FE variation instead of the pooled standard deviation. Take the reporting lag as an example, we find that a within-FE one-standard-deviation increase in *Flu* is associated with a 4.37% (= 3.075 × 0.003 ÷ 0.211) within-FE standard-deviation decrease in *Lag*. This effect of *Flu* on *Lag* is approximately twice the effect of *Size*. We repeat the same analysis for *Error* and find that the *Flu* effect is approximately 81% of the effect of *Size*.

in column 1. In column 2, $Issue^{Short}$ is the dependent variable, and the coefficient on *Flu* is negative and significant ($p < 0.01$). However, the coefficient on *Flu* is positive and significant for the $Issue^{Long}$ specification in column 3 ($p < 0.01$). The coefficients of the control variables are consistent with the findings of prior studies (e.g., Houston et al. [2010]). For example, the propensity to issue earnings forecasts (either short- or long-run) is positively associated with firm size (*Size*), analyst coverage (*Coverage*), institutional shareholding (*IOR*), records of meeting or beating analysts' consensus forecasts (*MBAnalyst*), and industry peers' propensity to issue earnings forecasts (*IndIssue*, $IndIssue^{Short}$, or $IndIssue^{Long}$). Meanwhile, firms with more volatile earnings (*EarnVolt*), greater analyst forecast dispersion (*Dispersion*), and loss reported (*Loss*) are less likely to issue forecasts.

In terms of economic significance, a within-FE one-standard-deviation increase in *Flu* is associated with a 2.13% within-FE standard-deviation decrease in $Issue^{Short}$ and a 1.07% within-FE standard-deviation increase in $Issue^{Long}$. We compare these variables with firm size (*Size*), one of the most significant drivers of forecast issuance, which shows a similar reduction in variation after considering the FEs. The economic effect of *Flu* on $Issue^{Short}$ is approximately 44% of the effect of *Size*, and this effect on $Issue^{Long}$ is approximately 38% of that of *Size*. This economic significance is comparable to that calculated in prior studies.[17] Overall, after controlling for the known determinants of forecast issuance, we find that, while firms do not alter their tendency to issue forecasts in general when their capacity to produce information is more constrained, they are less likely to issue short-run forecasts and more likely to issue long-

[17] For comparability with prior studies that do not adjust for FEs in calculating the implied economic magnitudes, we find a one-pooled-sample-standard-deviation increase in *Flu* to be associated with a reduction of 8.5% (= -2.382 × 0.013 ÷ 0.363) in the likelihood of issuing a short-run forecast (relative to the average $Issue^{Short}$) and 4.5% (= -1.259 × 0.013 ÷ 0.367) for long-run forecasts. This magnitude is comparable to prior studies. For example, using OLS regressions with firm FEs, Park et al. [2019] find that commonly owned firms are approximately 9.5% more likely to issue an earnings forecast than non-commonly owned firms.

run forecasts. In subsequent tests, we concentrate on $Issue^{Short}$ and $Issue^{Long}$ to explore managers' strategic disclosure choices in greater depth.

4.3.2. Placebo Tests

We conduct two sets of placebo tests to address the two concerns regarding our results. One concern is that events occurring outside the states where the firms are headquartered may be correlated with flu activity in those states during the same information production window (e.g., because of a common shock across the states). To address this concern, for each focal firm-quarter observation, we select a random state other than that where the focal firm is headquartered to calculate $Flu^{PlaceboState}$ as the average value of weekly flu activity over the information production window (i.e., the same measurement window as that of *Flu* for the focal firm). We expect that, if our results are driven by unobservable social and economic characteristics in states outside those where the firms are headquartered, the effect of $Flu^{PlaceboState}$ will be similar to that of *Flu*.

The second concern is that the documented results can be driven by events outside the information production window yet be correlated with the flu activity during the window. To alleviate this concern, we randomly select a period in the same year but outside the actual information production window for each firm-quarter observation such that the length of the period is equal to that of the focal firm's information production window. We then calculate $Flu^{PlaceboTime}$ as the average value of weekly flu activity over this randomly selected period. We expect that if our results are driven by events outside our defined window—flu activity or any other slow-moving unobservable social/economic characteristic associated with a firm's headquarters—the effect of $Flu^{PlaceboTime}$ will be similar to that of *Flu*. As Panels B1 and B2 in Table 3 show, the magnitudes of the actual *Flu* coefficients consistently exceed those of the 1st or 99th percentile of the distributions from the simulations on $Flu^{PlaceboState}$ and $Flu^{PlaceboTime}$

when we repeat the above analysis 1,000 times. This result suggests that other time- or state-level factors are unlikely to be responsible for the observed effect.

4.4. Cross-Sectional Analysis

Next, we conduct cross-sectional tests with partitions in which (1) the information production constraints are more binding, (2) the reputational loss associated with issuing low-quality short-run forecasts is higher, and (3) the costs of not issuing any forecasts are higher, whereas the costs of switching forecasts of different horizons are lower.

4.4.1. Complexity of Information Production

The complexity of information production increases the time and effort required to produce high-quality forecasts (e.g., Clement [1999]; Plumlee [2003]; Hutton [2005]). Therefore, we expect the strength of the effects of constraints on information production to be associated with the complexity of the process. We use the following empirical proxies to assess this characteristic: (1) operational complexity based on the number of business segments in a firm (*BusSeg*), (2) financial reporting complexity measured by a count of the XBRL tags for the accounting items disclosed in the 10-K filings (*ARC*) (Hoitash and Hoitash [2018]), and (3) mergers and acquisitions (*Acquisition*), that is, events that lead to significant increases in the time and effort necessary to comply with corporate disclosure policies. We construct a composite variable, *Complexity*, as the first principal component of *BusSeg*, *ARC*, and *Acquisition*. We then examine the effect of complexity based on the indicator variable $Complexity^{High}$, the value of which is 1 when *Complexity* exceeds the sample median in a year, and 0 otherwise. Next, we estimate an extended version of equation (1) by including $Complexity^{High}$ and its interaction term with *Flu*.

Panel A reports the descriptive statistics of $Complexity^{High}$. In Panels B and C of Table 4, the first columns (labeled column 1) report the regression results for the $Issue^{Short}$ and $Issue^{Long}$ specifications, respectively. We find that the coefficient on $Flu \times Complexity^{High}$ is negative and significant for short-run forecasts and positive and significant for long-run forecasts. This result suggests that the effect of flu activity on the trade-off disclosure strategy is greater for firms with more complex information production processes because the production of complex information exacerbates a firm's information production constraints, thereby magnifying the effect of flu on forecast issuance decisions.

4.4.2. Reputation Cost of Issuing Low-Quality Short-Run Forecasts

The baseline results presented in Table 3 suggest that managers are more concerned about the reputational loss arising from issuing low-quality short-run forecasts and therefore switch to issuing long-run forecasts. We expect such concerns to be more pronounced when the managers involved have cultivated a reputation for issuing accurate short-run forecasts.

Indeed, prior studies show that a stock's price reaction to management forecast news tends to be stronger when the manager involved has a reputation for issuing more accurate forecasts (Yang [2012]; Hutton and Stocken [2021]). This result suggests that investors incorporate more information in short-run forecasts issued by managers with better records of forecast accuracy. This tendency can magnify investors' losses when a forecast later proves inaccurate. Consequently, such managers are likely to be more concerned about incurring a reputational loss for issuing low-quality short-run forecasts. Therefore, we expect the effect of flu activity on the trade-off strategy to be stronger for firms with greater accuracy in their previous short-run forecasts.[18]

We use the average forecast accuracy of short-run forecasts over the previous four quarters (t - 3 to t), denoted by *Past Accuracy*, as the partition variable. We then create an indicator variable, $Past\ Accuracy^{High}$, with a value of 1 when *Past Accuracy* exceeds the sample

[18] An alternative explanation—one inconsistent with our results—is that a record of short-run forecast accuracy contributes to managers' confidence about the quality of their short-run forecasts (e.g., Hilary and Hsu [2011]; Hribar and Yang [2016]), thereby mitigating their concerns about issuing low-quality short-run forecasts.

median in a year and 0 otherwise. Next, we re-estimate the extended equation (1) using the new partition variable. As can be seen in Table 4, the coefficient on the interaction term between *Flu* and *Past Accuracy*^{High} is significant and negative in the *Issue*^{Short} specification (Panel B, column 2) and significant and positive in the *Issue*^{Long} specification (Panel C, column 2). These findings give us greater confidence on our main results, suggesting that managers are concerned about the reputational loss associated with issuing low-quality short-run forecasts and strategically make a trade-off in response to information production constraints.

4.4.3. Costs of Changing Forecast Policies

We next explore the conditions under which managers are more concerned about the costs of not issuing forecasts and switching forecasts of different horizons. When a firm has issued bundled earnings forecasts most of the time, investors are likely to expect a bundled forecast in the current quarter (Billings, Jennings, and Lev 2015). Consistent with this assumption, Zhou [2021] shows that the pressure to meet investors' information demands is likely to drive firms to maintain consistent forecast policies. This finding suggests that firms with consistent forecast policies incur costs when these policies change and become less consistent. Therefore, we expect that managers of firms that make regular forecasts will experience greater pressure to maintain disclosure transparency and be more likely to issue long-run forecasts when short-run forecasts are omitted. We also expect that the costs of changing the forecast horizon will be lower for firms that have made such changes in the past because these changes will less likely be viewed as a move toward less consistent disclosure policies than would be the case for firms that have not previously made such changes. Overall, we expect that the managers in our sample would be more likely to implement the trade-off strategy of issuing long- rather than short-run forecasts when their firms have consistently issued forecasts in the past and have previously shifted their horizons.

To test this prediction, we create an indicator variable, *Persistence*, with a value of 1 for firms that meet all three of the following criteria in a quarter and 0 otherwise. The criteria are, first, that a firm has issued at least one bundled earnings forecast in each of the past four quarters; second, that a firm has not issued a bundled short-run earnings forecast in at least one of the past four quarters; and third, that a firm has not issued a bundled long-run earnings forecast in at least one of the previous four quarters. Based on these criteria, we are able to identify firms that regularly issue forecasts but have previously shifted the forecast horizon. In other words, investors expect these firms to issue bundled forecasts but may not be sensitive to the horizons of these forecasts. Panel A of Table 4 reports the descriptive statistics of *Persistence*. The results reveal that the mean value of *Persistence* is 17%,[19] We re-estimate the extended equation (1) using *Persistence* as the partition variable. In Panels B and C of Table 4, column 3 reports regression results. We find that the coefficient on the interaction term between *Flu* and *Persistence* is negative and significant for short-run forecasts. This coefficient is positive and significant for long-run forecasts. These findings are consistent with the notion that the effect of the flu on short- and long-run forecasts is more pronounced for managers who are under greater pressure to issue forecasts and have more flexibility regarding the horizon of their forecasts.[20]

4.5. The Confounding Effects of Performance and Uncertainty

We are concerned that the effect of flu activity on actual firm performance may drive the results in Table 3. First, as discussed, flu activity may lower firm performance (e.g., Keech

[19] We conduct a robustness check using an alternative way to define *Persistence*. Instead of using the past four quarters of forecast issuance, we use the past eight quarters, resulting in a higher mean value of 27% for the new *Persistence* variable. Untabulated results show that our findings remain unchanged. We acknowledge that the empirical proxies have inherent limitations in representing the underlying construct. Some firms with *Persistence* = 0 may still have incentives to adopt the trade-off strategy based on their past forecast policies.

[20] The three cross-sectional predictions discussed above are not entirely overlapped or mutually exclusive. We find that the correlations between our three partition variables are significant, but the magnitudes of the coefficients are small (ranging from 0.06 to 0.10). By testing multiple predictions, we can reduce the likelihood that the results are purely driven by alternative explanations. Our untabulated cross-sectional tests using three placebo partition variables suggest that it is unlikely for the measurement error issue to drive our results.

and Beardsworth [2008]), thereby leading managers to withhold information (Kothari, Shu, and Wysocki [2009]; Chen et al. [2011]). Second, flu activity may increase information uncertainty (both at the firm level and economy-wide), which in turn influences firms' forecasting behavior (e.g., Billings et al. [2015]; Guay, Samuels, and Taylor [2016]; Nagar, Schoenfeld, and Wellman [2019]).

To address this concern, we conduct a three-stage analysis. First, we examine whether flu activity affects firms' financial performance and information uncertainty. Second, we identify a subsample of firms in which financial performance and information uncertainty are relatively less likely to be affected by flu activity. Finally, we evaluate whether our baseline results continue to hold for firms in which the flu seems less likely to affect financial performance and information certainty.

In the first stage, as Panel A of Table 5 shows, flu activity in the state where a firm is headquartered during a fiscal quarter (Flu^{Qtr}) is negatively associated with the firm's EPS growth (*EPS Growth*, column 1) and positively associated with the dispersion in analysts' earnings forecasts (*AF Disp*, column 4). This result suggests that flu activity in the headquarters lowers financial performance and increases information uncertainty.

Next, we create a subsample in which the flu activity of the states where firms are headquartered is less likely to affect their financial performance and information uncertainty, thus highlighting the effect of flu activity on information production constraints. We use two criteria to identify the subsample. The first criterion deals with the concentration of operations. For the subsample of firms with business units or employees located mainly *outside* the states where their headquarters are located, we expect their financial performance and information uncertainty to be less affected by the flu activity in headquarters' states. The second criterion further limits this subsample to firms with large differences in the levels of flu activity between the states where they are headquartered and other states where they also operate. This criterion ensures that the flu activity measured in the headquarters' states does not positively correlate with that in other states and hence is not associated with firms' performance and information uncertainty.

To construct the subsample described empirically, we obtain the geographic distribution of the firms' operations from the National Establishment Time Series (NETS) database and require that (1) the percentage of employees in the state where a firm is headquartered be smaller than the sample median of a year and (2) the difference between the flu activity in the headquarters' state and the average level of flu activity in the other states where the firm also operates be larger than its sample median of a year. We label this subsample "Low HQ Pct & Large Diff in Flu" and expect that flu activity does not affect financial performance and information uncertainty for this subsample. We find consistent evidence in Columns 2 and 5 of Panel A.

As a validation check, we use a similar approach to construct another subsample and consider the opposite situation, labeled as "High HQ Pct & Small Diff in Flu". This subsample contains firms with greater percentage of employees in the headquarters' states and with smaller differences between the flu activity in the headquarters' states and the average level of flu activity in the other states where firms also operate. We expect that flu activity is negatively associated with financial performance and information uncertainty for this subsample. Columns 3 and 6 show the consistent results. This suggests that the insignificant result using the earlier "Low HQ Pct & Large Diff in Flu" subsample is not driven by the testing power issue due to the reduced sample size. Overall, our results indicate that using the "Low HQ Pct & Large Diff in Flu" subsample mitigates the effect of flu activity on financial performance and information uncertainty, as predicted.

Finally, we re-estimate our main analysis of forecast issuance using the two subsamples created. As Panel B in Table 5 shows, our baseline results for the trade-off strategy continue to hold for the "Low HQ Pct & Large Diff in Flu" subsample (columns 1 and 3). This suggests that the effect of the flu on financial performance and information uncertainty may not drive our baseline findings.[21]

5. Additional Analyses

5.1. Timing of Earnings Announcements

In Table OA1, we show that firms with high flu activity experience longer reporting lags in EAs. These delays could either reflect an effort by managers to mitigate their concerns about forecast quality or indicate the severity of constraints they are facing. To gain a deeper understanding of these possibilities, we perform two tests.

First, we examine the effect of reporting lags on the relation between flu activity and forecast issuance. The effect is unclear *ex ante*. On the one hand, if firms extend the reporting lag to allow additional time to work on their forecasts in ways that increase managers' confidence in forecast quality, the effect of flu activity on forecast issuance may diminish as the reporting lag increases. On the other hand, there may be limits to how managers can extend their reporting lags. The SEC establishes 10-Q/K statutory filing deadlines (depending on a firm's filing status, e.g., accelerated or not), and the costs of long reporting lags are not trivial (Kross and Schroder [1984]; Johnson and So [2018]; Noh, So, and Verdi [2021]). Therefore, managers may feel pressured to promptly address their concerns about forecast quality, and a lengthy reporting lag may indicate severe constraints on information production. Hence, the impact of flu activity on the trade-off strategy may increase with the length of the reporting lag.

We perform a cross-sectional test to explore these possibilities, focusing on the effect of reporting lags. We construct an indicator variable, Lag^{High}, with a value of 1 when the

[21] To address potential confounding effects of investors' demand for information, we conduct a robustness test by controlling for four additional variables measured during the information-production window in Equation (1): stock returns, stock return volatility, trading turnovers, and closing bid-ask spreads (McTier, Tse, and Wald [2013]). Our results (untabulated) are not affected after accounting for these variables.

reporting lag (*Lag*) exceeds the sample median of its fiscal year-quarter and 0 otherwise. We then estimate the extended equation (1) after including Lag^{High} and the interaction of *Flu* with Lag^{High}. Our results, presented in Table 6, show that the coefficient on the interaction term is significant and negative for short-run forecasts (column 1) and significant and positive for long-run forecasts (column 2). As an additional analysis, we consider the heterogeneity in the time that firms could delay owing to their SEC's filing status. The delay becomes more serious when the EA approaches the 10-Q/K deadline. Specifically, we replace Lag^{High} with $Lag\_Deadline^{High}$, based on *Lag_Deadline*, to capture the delay in EAs relative to firms' SEC filing deadlines (Bartov and Konchitchki [2017]).[22] Columns 3 and 4 show that the baseline effect of flu activity on the trade-off strategy is more pronounced for more delayed cases. These results are consistent with our prediction that reporting lags do not mitigate managers' concerns about forecast quality. Instead, it appears that managers who delay their EAs for longer periods are more likely to be aware of flu-induced constraints on information production and, hence, more likely to issue forecasts strategically.

5.2. Further Analysis of Timeliness versus Thoroughness

To further shed light on whether managers delay their EAs to increase the thoroughness of their forecasts and mitigate their concerns about issuing low-quality forecasts, we next investigate the effect of flu activity on the accuracy of short- and long-run forecasts once issued and then test whether this effect varies with the reporting lag. Our main findings suggest that managers, being concerned about the reputation costs of issuing low-quality short-run forecasts, tend to prefer long-run forecasts. Therefore, we expect that, for managers who issue only high-quality short-run forecasts, the accuracy of short-run forecasts is not negatively associated with

---

[22] We calculate *Lag_Deadline* as the difference between EA ($EA_{i,t}$ in Figure 1) and 10-Q/K deadline (Bartov and Konchitchki [2017]). For example, *Lag_Deadline* is equal to -25 if a firm's EA is 20 days after the end of the fiscal quarter, whereas it is necessary to submit 10-Q within 45 days after the end of the fiscal quarter. Our results are not affected when we define $Lag\_Deadline^{High}$ as equal to 1 for firms with especially serious delays (those in the highest quartile of *Lag_Deadline*) and 0 otherwise.

flu activity (i.e., a self-selected sample). In contrast, the accuracy of long-run forecasts may be negatively associated with flu activity when the constraints on information production impair forecast quality, but managers nevertheless issue them (Rogers and Stocken [2005]; Feng et al. [2009]; Call et al. [2017]).[23]

To test these predictions, we conduct firm-quarter-level analyses using observations with either short- or long-run forecasts issued. We re-estimate equation (1) by replacing the dependent variable with the forecast accuracy for short-run forecasts ($Accuracy^{Short}$) and long-run forecasts ($Accuracy^{Long}$) one at a time. In the short-run forecast sample, as column 1 in Table 7 shows, the coefficient on *Flu* is statistically insignificant, consistent with a potential selection effect. As column 2 shows, the coefficient on *Flu* is significant and negative for the long-run forecast sample, suggesting that flu-induced constraints on information production reduce the quality of long-run forecasts.

Next, we investigate whether the extension of reporting lags mitigates the negative effect of flu activity on forecast accuracy. We further include Lag^{High} or $Lag\_Deadline^{High}$ and the interaction of these variables with *Flu* in the previous regression models. Columns 3-6 of Table 7 report the results. Consistent with the results in column 1, columns 3 and 5 show that flu activity does not affect the accuracy of short-run forecasts, and this result does not vary with the duration of reporting lags. However, the coefficients on the interaction terms, $Flu \times Lag^{High}$ and $Flu \times Lag\_Deadline^{High}$ are negative and significant ($p < 0.01$), as shown in columns 4 and 6, when the dependent variable is $Accuracy^{Long}$, indicating that the reduction in accuracy of long-run forecasts is more pronounced for more delayed cases. This result also supports the findings discussed in section 5.1 that the managers may lack sufficient time to address their concerns about forecast quality. Rather than increasing forecast thoroughness,

[23] Our further analysis shows a greater extent of subsequent revisions to the long-run forecasts issued when flu activity is higher, given that there is greater error in those forecasts (Table OA8).

longer reporting lags tend to reflect the severity of flu-induced constraints on information production.[24]

5.3. Other Characteristics of Forecasts

As a supplementary analysis, we further investigate the effect of flu activity on forecast precision, horizon, and frequency. Similar to forecast accuracy, we expect no negative association between the precision of the short-run forecasts and flu activity because managers may choose not to issue short-run forecasts when flu activity is high (i.e., a potential selection effect). However, we expect the precision of long-run forecasts to be negatively associated with flu activity if the flu activity constrains a firm's information production. Accordingly, we re-estimate equation (1) by replacing the dependent variable with the width for short-run forecasts ($Width^{Short}$) and long-run forecasts ($Width^{Long}$) one at a time. Consistent with our predictions, as columns 1 and 2 in Table 8 show, the level of flu activity is associated with decreases in the precision of long-run forecasts but has no effect on the precision of short-run forecasts.[25] Additionally, we find that managers tend to extend the horizon of their long-run forecasts. Specifically, column 3 in Table 8 shows that flu activity is positively associated with the length of forecast horizon for long-run forecasts ($Horizon^{Long}$). This result suggests that managers may issue forecasts for which they have more opportunities to improve their quality strategically (Tang et al. [2016]).

Next, given that the variation in the propensity to issue an EPS forecast could be relatively small, particularly after considering various fixed effects, we examine alternative dependent variables with the potential to show larger variations. In particular, we focus on the

[24] We repeat our analysis, examining an alternative accuracy measure, $Accuracy^{Long}$ scaled by *Lag*. This variable captures the potential improvement in forecast accuracy associated with the delay, although the overall accuracy can still decline. We find that *Flu* is negatively associated with this scaled accuracy variable (untabulated), consistent with the duration of the lags in reporting not improving forecast thoroughness.

[25] Notably, the positive relation between $Width^{Long}$ and flu activity is also consistent with the strategic choice to reduce the costs associated with the issuance of inaccurate forecasts (e.g., Yang [2012]; Zhang [2012]; Cheng, Luo, and Yue [2013]; Li and Zhang [2015]).

frequency of all forecasts rather than on the issuance of EPS forecasts. Our baseline inferences do not change when we focus on the frequency of overall short- and long-run forecasts (see $Freq^{Short\text{-}All}$ and $Freq^{Long\text{-}All}$ Table OA7). More importantly, our untabulated results show that the economic effect of *Flu* on $Freq^{Short\text{-}All}$ is approximately 58% of the effect of *Size*, which is 30% greater than that of $Issue^{Short}$. Similarly, the economic effect of *Flu* on $Freq^{Long\text{-}All}$ is approximately 59% greater than that of $Issue^{Long}$.[26]

6. Robustness Analyses

6.1. Measurement Errors of Flu Activity

We perform several tests to alleviate the concern that the potential deviation between our empirical proxy based on flu activity in the *state* where a firm is headquartered, and the *firm*'s flu-induced constraints on information production drives our results. Previous studies show that more densely populated areas enable higher rates of interpersonal contact and, therefore, facilitate influenza transmission (Chandra et al. [2013]; Dalziel et al. [2018]). Suppose measurement errors exist; such errors would be large for firms located in areas of low population density but high state-level flu activity because the incidence of flu at these firms should be significantly lower than in the other parts of headquarters states.[27] Accordingly, we partition our sample into high- and low-density groups based on the annual sample median of the population density of the *counties* where firms are headquartered.[28] We focus on the county level to capture the population density around firms' locations more precisely than

[26] We acknowledge that the forecast frequency results need to be interpreted with caution because it is unclear how managers choose specific lines in the performance matrix to add/drop or the ratio of the frequencies of short-to long-run forecasts in our setting. These complex issues represent a potential avenue for future research.

[27] We note that measurement errors may exist for firms headquartered in densely populated areas with low state-level flu activity, but only if numerous other population centers in the same state have significantly higher flu activity. Given that few states meet this criterion, we conclude that when state-wide flu activity is relatively low, the flu activity at firms located in that state is also low. We thank an anonymous referee for sharing this point.

[28] Population density is calculated as a country's population divided by its size. Our sample consists of firms headquartered in 429 counties, with the mean population density of 969.90 (204.34 for all U.S. counties during our sample period), indicating that most firms are headquartered in high-density areas. Only 517 of our observations' population density is lower than the median value of all U.S. counties.

measurements of population density at the state level. We compare our key variable of interest, *Flu*, between the two groups. Our concern is that if the measurement error is severe, we may have misclassified many densely populated counties with low state-level flu activity as having low flu, and vice versa. Consequently, the mean value of *Flu* may not be significantly higher in the high-density group than in the low-density group. In contrast, we find that the mean value of *Flu* in the high-density group is significantly higher than that in the low-density group at less than the 1% level (untabulated), suggesting that any misclassification that may have occurred does not have a significant influence on our results. Next, we partition our sample into high- and low-*Flu* groups based on the median values of each quarter. Consistent with the notion that the measurement error is not severe, we find that only 5% of the observations in our full sample belong to both the high-*Flu* and the low-density groups. We exclude this 5% of the observations and re-estimate equation (1). The regression results in Table OA2 confirm that our results continue to hold.

We also use a granular measure of flu activity at the metropolitan statistical area (MSA)-level to address the potential measurement errors. Specifically, GFT estimates flu activity based on the incidence of internet searches about flu from 84 MSAs during 2003–2013 (Ginsberg et al. [2009]). As Table OA3 shows, the results indicate that such errors, if any, do not affect our findings. Overall, these tests bolster our confidence that measurement errors do not drive our findings.

6.2. Effect of Mood

Our results for reporting lags indicate that managers are aware of the constraints on information production in their firms. However, direct evidence of managerial awareness is difficult to discover. For example, weather conditions, such as humidity and temperature, play important roles in the spread of infection (Lowen and Steel [2014]; Paynter [2015]) and in determining the mood of individuals in an area (Keller et al. [2005]). Recent studies suggest that weather-induced mood changes slow information processing, increase investors' pessimism, and contribute to biases in management forecasts (deHaan et al. [2017]; Chen et al. [2022]). The additional analyses presented in section OA2 of the Online Appendix suggest that issues relating to neither managers' moods nor unobserved correlated variables significantly affect our findings.

7. Conclusions

Using state-level outpatient visits to healthcare providers for ILI, we show that firms headquartered in states with higher flu activity are less likely to issue short-run forecasts and more likely to issue long-run forecasts. This effect is stronger (1) when firms have more complex information production processes, (2) when managers face a greater reputational loss for issuing low-quality short-run forecasts, and (3) when firms face lower costs for changing forecast horizon. We also find that, for firms issuing long-run forecasts, the accuracy and precision of these forecasts are lower when flu activity is higher, but this is not the case for short-run forecasts.

Our results suggest that constraints on firms' capacity to process and analyze information affect their corporate disclosure policies. Our findings are consistent with the view that managers balance the costs of issuing inaccurate forecasts with those of not issuing forecasts and form their disclosure strategies based on the forecast horizon. Additionally, we show that flu-induced constraints on employees' time and effort may significantly impair the quality of information production, complementing the health economics research on the adverse consequences of influenza. We encourage investors, policymakers, and researchers to remain attentive to strategic managerial decisions in the context of influenza and other epidemics in the United States and other countries.

References

AJINKYA, B., BHOJRAJ, S., and SENGUPTA, P. 'The association between outside directors, institutional investors and the properties of management earnings forecasts.' Journal of Accounting Research 43 (2005): 343–376.

AKBAS, F., MARKOV, S., SUBASI, M., and WEISBROD, E. 'Determinants and consequences of information processing delay: Evidence from the Thomson Reuters institutional brokers' estimate system.' Journal of Financial Economics 127 (2018): 366–388.

BAGINSKI, S. P., and RAKOW, K. C. 'Management earnings forecast disclosure policy and the cost of equity capital.' Review of Accounting Studies 17 (2012): 279–321.

BAGINSKI, S. P., HASSELL, J. M., and KIMBROUGH, M. D. 'The effect of legal environment on voluntary disclosure: Evidence from management earnings forecasts issued in US and Canadian markets.' The Accounting Review 77 (2002): 25–50.

BAMBER, L. S., and CHEON, Y. S. 'Discretionary management earnings forecast disclosures: Antecedents and outcomes associated with forecast venue and forecast specificity choices.' Journal of Accounting Research 36 (1998): 167–190.

BARTOV, E., and KONCHITCHKI, Y. 'SEC filings, regulatory deadlines, and capital market consequences.' Accounting Horizons 31 (2017): 109-131.

BEYER, A., COHEN, D. A., LYS, T. Z., and WALTHER, B. R. 'The financial reporting environment: Review of the recent literature.' Journal of Accounting and Economics 50 (2010): 296-343.

BILLINGS, M. B., JENNINGS, R., and LEV, B. 'On guidance and volatility.' Journal of Accounting and Economics 60 (2015): 161–180.

BLANKESPOOR, E., DEHAAN, E., and MARINOVIC, I. 'Disclosure processing costs, investors' information choice, and equity market outcomes: A review.' Journal of Accounting and Economics 70 (2020): 101344.

BOSIS, S., ESPOSITO, S., NIESTERS, H. G., CROVARI, P., OSTERHAUS, A. D., and PRINCIPI, N. 'Impact of human metapneumovirus in childhood: comparison with respiratory syncytial virus and influenza viruses.' Journal of Medical Virology 75 (2005): 101–104.

CALL, A. C., CAMPBELL, J. L., DHALIWAL, D. S., and MOON JR, J. R. 'Employee quality and financial reporting outcomes.' Journal of Accounting and Economics 64 (2017): 123–149.

CHANDRA, S., KASSENS-NOOR, E., KULJANIN, G., and VERTALKA, J. 'A geographic analysis of population density thresholds in the influenza pandemic of 1918–19.' International Journal of Health Geographics 12 (2013): 9–18.

CHARU, V., ZEGER, S., GOG, J., BJØRNSTAD, O. N., KISSLER, S., SIMONSEN, L., and VIBOUD, C. 'Human mobility and the spatial transmission of influenza in the United States.' PLoS Computational Biology, 13 (2017): e1005382.

CHEN, C., CHEN, Y., PITTMAN, J., PODOLSKI, E. and VEERARAGHAVAN, M. 'Emotions and managerial judgment: Evidence from sunshine exposure.' The Accounting Review 97 (2022): 179–203.

CHEN, C., MARTIN, X., ROYCHOWDHURY, S., WANG, X., and BILLETT, M. T. 'Clarity begins at home: Internal information asymmetry and external communication quality.' The Accounting Review 93 (2018): 71–101.

CHEN, J., DEMERS, E., and LEV, B. 'Oh what a beautiful morning! Diurnal influences on executives and analysts: Evidence from conference calls.' Management Science 64 (2018): 5899–5924.

CHEN, S., MATSUMOTO, D., and RAJGOPAL, S. 'Is silence golden? An empirical analysis of firms that stop giving quarterly earnings guidance.' Journal of Accounting and Economics 51 (2011): 134–150.

CHEN, X., CHENG, Q., and LO, K. 'On the relationship between analyst reports and corporate disclosures: Exploring the roles of information discovery and interpretation.' Journal of Accounting and Economics 49 (2010): 206–226.

CHENG, Q., LUO, T., and YUE, H. 'Managerial incentives and management forecast precision.' The Accounting Review 88 (2013): 1575–1602.

CLEMENT, M. B. 'Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter?' Journal of Accounting and Economics 27 (1999): 285–303.

COLETTI, P., POLETTO, C., TURBELIN, C., BLANCHON, T., and COLIZZA, V. 'Shifting patterns of seasonal influenza epidemics.' Scientific Reports 8 (2018): 1–12.

DALZIEL, B. D., KISSLER, S., GOG, J. R., VIBOUD, C., BJØRNSTAD, O. N., METCALF, C. J. E., and GRENFELL, B. T. 'Urbanization and humidity shape the intensity of influenza epidemics in US cities.' Science 362 (2018): 75–79.

DEHAAN, E. 'Using and interpreting fixed effects models.' Working paper, University of Washington, 2021. Available at: https://ssrn.com/abstract=3699777

DEHAAN, E., MADSEN, J., and PIOTROSKI, J. D. 'Do weather-induced moods affect the processing of earnings news?' Journal of Accounting Research, 55 (2017): 509–550.

DEHAAN, E., SHEVLIN, T., and THORNOCK, J. 'Market (in)attention and the strategic scheduling and timing of earnings announcements.' Journal of Accounting and Economics 60 (2015): 36–55.

DIAMOND, D. W., and VERRECCHIA, R. E. 'Disclosure, liquidity, and the cost of capital.' The Journal of Finance 46 (1991): 1325–1359.

DORANTES, C. A., LI, C., PETERS, G. F., and RICHARDSON, V. J. 'The effect of enterprise systems implementation on the firm information environment.' Contemporary Accounting Research 30 (2013): 1427–1461.

DRAKE, M. S., GEE, K. H., and THORNOCK, J. R. 'March market madness: The impact of value-irrelevant events on the market pricing of earnings news.' Contemporary Accounting Research 33 (2016): 172–203.

DRISKILL, M., KIRK, M. P., and TUCKER, J. W. 'Concurrent earnings announcements and analysts' information production.' The Accounting Review, 95 (2020):165–189.

DYE, R. A. 'Disclosure of nonproprietary information.' Journal of Accounting Research, 23 (1985): 123-145.

DYE, R. A., and SRIDHAR, S. S. 'Industry-wide disclosure dynamics.' Journal of Accounting Research 33 (1995): 15–174.

ERNST & YOUNG. Disclosure effectiveness: Companies embrace the call to action. (2015). Available at https://www.ey.com/Publication/vwLUAssets/EY-disclosure-effectiveness-report/$FILE/EY-disclosure-effectiveness-report.pdf.

ESPOSITO, S., GASPARINI, R., BOSIS, S., MARCHISIO, P., TAGLIABUE, C., TOSI, S., BIANCHI, C., CROVARI, P., and PRINCIPI, N. 'Clinical and socio–economic impact of influenza and respiratory syncytial virus infection on healthy children and their households.' Clinical Microbiology and Infection 11 (2005): 933–936.

FENG, M., LI, C., and MCVAY, S. 'Internal control and management guidance.' Journal of Accounting and Economics 48 (2009): 190–209.

GINSBERG, J., MOHEBBI, M. H., PATEL, R. S., BRAMMER, L., SMOLINSKI, M. S., and BRILLIANT, L. 'Detecting influenza epidemics using search engine query data.' Nature 457 (2009): 1012–1014.

GONG, G., LI, L. Y., and WANG, J.J. 'Serial correlation in management earnings forecast errors.' Journal of Accounting Research 49 (2011): 677–720.

GONG, G., LI, L. Y., and XIE, H. 'The association between management earnings forecast errors and accruals.' The Accounting Review 84 (2009): 497–530.

GOODMAN, T., NEAMTIU, M., SHROFF, N., and WHITE, H. 'Management forecast quality and capital investment decisions.' The Accounting Review 89 (2014): 331–365.

GRAHAM, J. R., HARVEY, C. R., and RAJGOPAL, S. 'The economic implications of corporate financial reporting.' Journal of Accounting and Economics 40 (2005): 3–73.

GUAY, W., SAMUELS, D., and TAYLOR, D. 'Guiding through the fog: Financial statement complexity and voluntary disclosure.' Journal of Accounting and Economics 62 (2016): 234–269.

GUL, F., and LUNDHOLM, R. 'Endogenous timing and the clustering of agents' decisions.' Journal of Political Economy 103 (1995): 1039-1066.

GROSSMAN, S. J. 'An introduction to the theory of rational expectations under asymmetric information.' The Review of Economic Studies 48 (1981): 541–559.

HEALY, P. M., and PALEPU, K. G. 'Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature.' Journal of Accounting and Economics 31 (2001): 405–440.

HEFLIN, F., KROSS, W. J., and SUK, I. 'Asymmetric effects of regulation FD on management earnings forecasts.' The Accounting Review 91 (2016): 119-152.

HEMMER, T., and LABRO, E. 'On the optimal relation between the properties of managerial and financial reporting systems.' Journal of Accounting Research 46 (2008): 1209-1240.

HILARY, G., and HSU, C. 'Endogenous overconfidence in managerial forecasts.' Journal of Accounting and Economics 51 (2011): 300-313.

HIRSHLEIFER, D., LIM, S. S., and TEOH, S. H. 'Driven to distraction: Extraneous events and underreaction to earnings news.' The Journal of Finance 64 (2009): 2289–2325.

HOITASH, R., and HOITASH, U. 'Measuring accounting reporting complexity with XBRL.' The Accounting Review 93 (2018): 259–287.

HOITASH, U. and MKRTCHYAN, A. 'Internal governance and outside directors' connections to non-director executives.' Journal of Accounting and Economics 73 (2022): 101436.

HOUSTON, J. F., LEV, B., and TUCKER, J. W. 'To guide or not to guide? Causes and consequences of stopping quarterly earnings guidance.' Contemporary Accounting Research 27 (2010): 143–185.

HRIBAR, P., and YANG, H. 'CEO overconfidence and management forecasting.' Contemporary Accounting Research 33 (2016): 204–227.

HSIEH, P., KOLLER, T., and RAJAN, S.R., 'The misguided practice of earnings guidance.' McKinsey on Finance 19 (2006): 1–6.

HUI, K. W., and MATSUNAGA, S. R. 'Are CEOs and CFOs rewarded for disclosure quality?' The Accounting Review 90 (2015):1013–1047.

HUTTON, A. P. 'Determinants of managerial earnings guidance prior to regulation fair disclosure and bias in analysts' earnings forecasts.' Contemporary Accounting Research 22 (2005): 867–914.

HUTTON, A. P., and STOCKEN, P. C. 'Prior forecasting accuracy and investor reaction to management earnings forecasts.' Journal of Financial Reporting 6 (2021): 98-107.

JENNINGS, J. N., KIM, J. M., LEE, J. A. and TAYLOR, D. J. 'Measurement error and bias in causal models in accounting research.' Review of Accounting Studies (2023): Forthcoming.

JHA, A. and CHEN, Y. 'Audit fees and social capital.' The Accounting Review, 90 (2015): 611-639.

JOHNSON, M. F., KASZNIK, R. and NELSON, K. K. 'The impact of securities litigation reform on the disclosure of forward-looking information by high technology firms.' Journal of Accounting Research 39 (2001): 297–327.

JOHNSON, T. L., and SO, E.C. 'Time will tell: Information in the timing of scheduled earnings news.' Journal of Financial and Quantitative Analysis 53 (2018): 2431–2464.

KASZNIK, R. 'On the association between voluntary disclosure and earnings management.' Journal of Accounting Research 37 (1999): 57–81.

KASZNIK, R., and LEV, B. 'To warn or not to warn: Management disclosures in the face of an earnings surprise.' The Accounting Review 70 (1995): 113–134.

KELLER, M. C., FREDRICKSON, B. L., YBARRA, O., CÔTÉ, S., JOHNSON, K., MIKELS, J., CONWAY, A., and WAGER, T. 'A warm heart and a clear head: The contingent effects of weather on mood and cognition.' Psychological Science 16 (2005): 724–731.

KEECH, M., and BEARDSWORTH, P. 'The impact of influenza on working days lost.' Pharmacoeconomics, 26 (2008): 911–924.

KIM, Y., SU, L. N, AND ZHU, X. K. 'Does the cessation of quarterly earnings guidance reduce investors' short-termism?.' Review of Accounting Studies 22 (2017): 715–752.

KOTHARI, S. P., SHU, S., and WYSOCKI, P. D. 'Do managers withhold bad news?' Journal of Accounting Research 47 (2009): 241–276.

KROL, M., BROUWER, W. B., SEVERENS, J. L., KAPER, J., and EVERS, S. M. 'Productivity cost calculations in health economic evaluations: correcting for compensation mechanisms and multiplier effects.’ Social Science & Medicine 75 (2012): 1981–1988.

KROSS, W. J., RO, B. T., and SUK, I. ‘Consistency in meeting or beating earnings expectations and management earnings forecasts.’ Journal of Accounting and Economics 51 (2011): 37–57.

KROSS, W. J., LEWELLEN, W. G., and RO, B. T. ‘Evidence on the motivation for management forecasts of corporate earnings.’ Managerial and Decision Economics 15 (1994): 187–200.

KROSS, W. J., and SCHROEDER, D. A. ‘An empirical investigation of the effect of quarterly earnings announcement timing on stock returns.’ Journal of Accounting Research 22 (1984): 153–176.

LEE, S., MATSUNAGA, S. R., and PARK, C. W. ‘Management forecast accuracy and CEO turnover.’ The Accounting Review 87 (2012): 2095–2122.

LENNOX, C. S., and PARK, C. W. ‘The informativeness of earnings and management’s issuance of earnings forecasts.’ Journal of Accounting and Economics 42 (2006): 439–458.

LI, Y., and ZHANG, L. ‘Short selling pressure, stock price behavior, and management forecast precision: Evidence from a natural experiment.’ Journal of Accounting Research 53 (2015):79–117.

LOWEN, A. C., and STEEL, J. ‘Roles of humidity and temperature in shaping influenza seasonality.’ Journal of Virology 88 (2014): 7692–7695.

MCTIER, B. C., TSE, Y., and WALD, J. K. ‘Do stock markets catch the flu?’ Journal of Financial and Quantitative Analysis, 48 (2013): 979-1000.

MILGROM, P. R. ‘Good news and bad news: Representation theorems and applications.’ The Bell Journal of Economics 12 (1981): 380–391.

MILLER, G. S. ‘Earnings performance and discretionary disclosure.’ Journal of Accounting Research 40 (2002): 173–204.

MOLINARI, N. A. M., ORTEGA-SANCHEZ, I. R., MESSONNIER, M. L., THOMPSON, W. W., WORTLEY, P. M., WEINTRAUB, E., and BRIDGES, C. B. ‘The annual impact of seasonal influenza in the US: measuring disease burden and costs.’ Vaccine 25 (2007): 5086–5096.

NAGAR, V., SCHOENFELD, J., and WELLMAN, L. ‘The effect of economic policy uncertainty on investor information asymmetry and management disclosures.’ Journal of Accounting and Economics 67 (2019): 36–57.

NG, J., TUNA, I., and VERDI, R. ‘Management forecast credibility and underreaction to news.’ Review of Accounting Studies 18 (2013): 956–986.

NOH, S., SO, E. C., and VERDI, R. S. ‘Calendar rotations: A new approach for studying the impact of timing using earnings announcements.’ Journal of Financial Economics 140 (2021): 865–893.

PALMER, L. A., ROUSCULP, M. D., JOHNSTON, S. S., MAHADEVIA, P. J., and NICHOL, K. L. ‘Effect of influenza-like illness and other wintertime respiratory illnesses on worker productivity: The child and household influenza-illness and employee function (CHIEF) study.' Vaccine 28 (2010): 5049–5056.

PALTER, R. N., REHM, W., and SHIH, J. 'Communicating with the right investors.' McKinsey Quarterly 2 (2008): 64.

PARK, J., SANI, J., SHROFF, N., and WHITE, H. 'Disclosure incentives when competing firms have common ownership.' Journal of Accounting and Economics 67 (2019): 387–415.

PAYNTER, S. 'Humidity and respiratory virus transmission in tropical and temperate settings.' Epidemiology & Infection 143(2015): 1110–1118.

PEASAH, S. K., AZZIZ-BAUMGARTNER, E., BREESE, J., MELTZER, M. I., and WIDDOWSON, M. A. 'Influenza cost and cost-effectiveness studies globally–a review.' Vaccine 31 (2013): 5339–5348.

PETRIE, J. G., CHENG, C., MALOSH, R. E., VANWORMER, J. J., FLANNERY, B., ZIMMERMAN, R. K., and OHMIT, S. E. 'Illness severity and work productivity loss among working adults with medically attended acute respiratory illnesses: US Influenza Vaccine Effectiveness Network 2012–2013.' Clinical Infectious Diseases 62 (2016): 448–455.

PLUMLEE, M. A. 'The effect of information complexity on analysts' use of that information.' The Accounting Review 78 (2003): 275–296.

PRINCIPI, N., ESPOSITO, S., GASPARINI, R., MARCHISIO, P., and CROVARI, P. 'Burden of influenza in healthy children and their households.' Archives of Disease in Childhood, 89 (2004): 1002-1007.

ROGERS, J. L., and STOCKEN, P. C. 'Credibility of management forecasts.' The Accounting Review 80 (2005): 1233–1260.

ROGERS, J. L., and VAN BUSKIRK, A. 'Bundled forecasts in empirical accounting research.' Journal of Accounting and Economics 55 (2013): 43–65.

ROST, K. M., MENG, H., and XU, S. 'Work productivity loss from depression: evidence from an employer survey.' BMC Health Services Research 14 (2014): 1–9.

SEVERENS, J. L., LAHEIJ, R. J. F., JANSEN, J. B. M. J., LISDONK, E. V. D., and VERBEEK, A. L. M. 'Estimating the cost of lost productivity in dyspepsia.' Alimentary Pharmacology & Therapeutics 12(1998): 919–923.

SHERIN, K. 'Financial planning and investor communications at GE (with a look at why we ended earnings guidance).' Journal of Applied Corporate Finance 22 (2010): 8–17.

SMITH, A. P. 'Effects of upper respiratory tract illnesses and stress on alertness and reaction time.' Psychoneuroendocrinology 38 (2013): 2003–2009.

STEWART, W. F., RICCI, J. A., CHEE, E., and MORGANSTEIN, D. 'Lost productive work time costs from health conditions in the United States: results from the American Productivity Audit.' Journal of Occupational and Environmental Medicine 45(2003): 1234–1246.

TANG, M. M., YAO, L., and ZAROWIN, P. 'Once is not enough: Properties and implications of management forecast updates.' Working paper, New York University, 2016. Available at: https://ssrn.com/abstract=2482685

TEO, S. S. S., NGUYEN-VAN-TAM, J. S., and BOOY, R. 'Influenza burden of illness, diagnosis, treatment, and prevention: What is the evidence in children and where are the gaps?' Archives of Disease in Childhood 90 (2005): 532-536.

TSE, S., and TUCKER, J. W. 'Within-industry timing of earnings warnings: Do managers herd?' Review of Accounting Studies 15 (2010): 879-914.

TRUEMAN, B. 'Why do managers voluntarily release earnings forecasts?.' Journal of Accounting and Economics 8 (1986): 53–71.

TUCKER, J. W. 'Is openness penalized? Stock returns around earnings warnings.' The Accounting Review 82 (2007): 1055–1087.

VAN WORMER, J. J., KING, J. P., GAJEWSKI, A., MCLEAN, H. Q., and BELONGIA, E. A. 'Influenza and workplace productivity loss in working adults.' Journal of Occupational and Environmental Medicine 59 (2017): 1135–1139.

VERRECCHIA, R. E. 'Discretionary disclosure.' Journal of Accounting and Economics 5 (1983):179–194.

VERRECCHIA, R. E. 'Information quality and discretionary disclosure.' Journal of Accounting and Economics 12 (1990): 365–380.

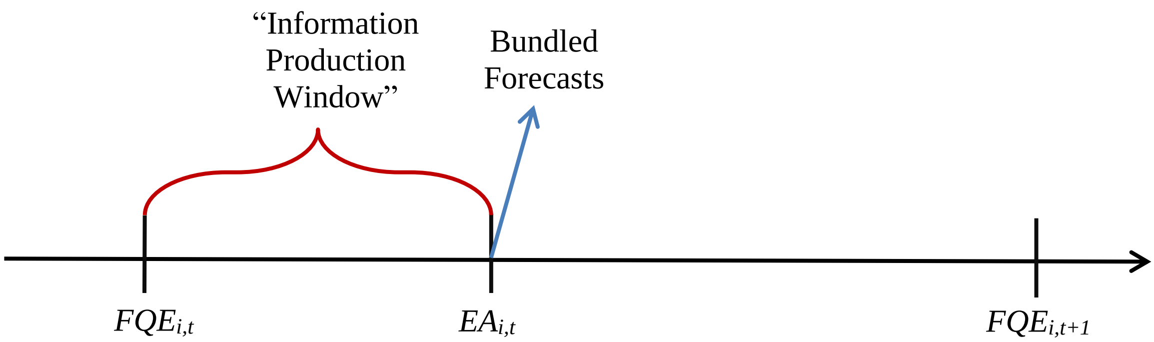
YANG, H. I. 'Capital market consequences of managers' voluntary disclosure styles.' Journal of Accounting and Economics 53 (2012): 167–184.

ZHANG, L. 'The effect of ex ante management forecast accuracy on the post-earnings-announcement drift.' The Accounting Review 87 (2012): 1791–1818.

ZHOU, F. S. 'Disclosure dynamics and investor learning.' Management Science 67(2021): 3429-3446.

FIGURE 1

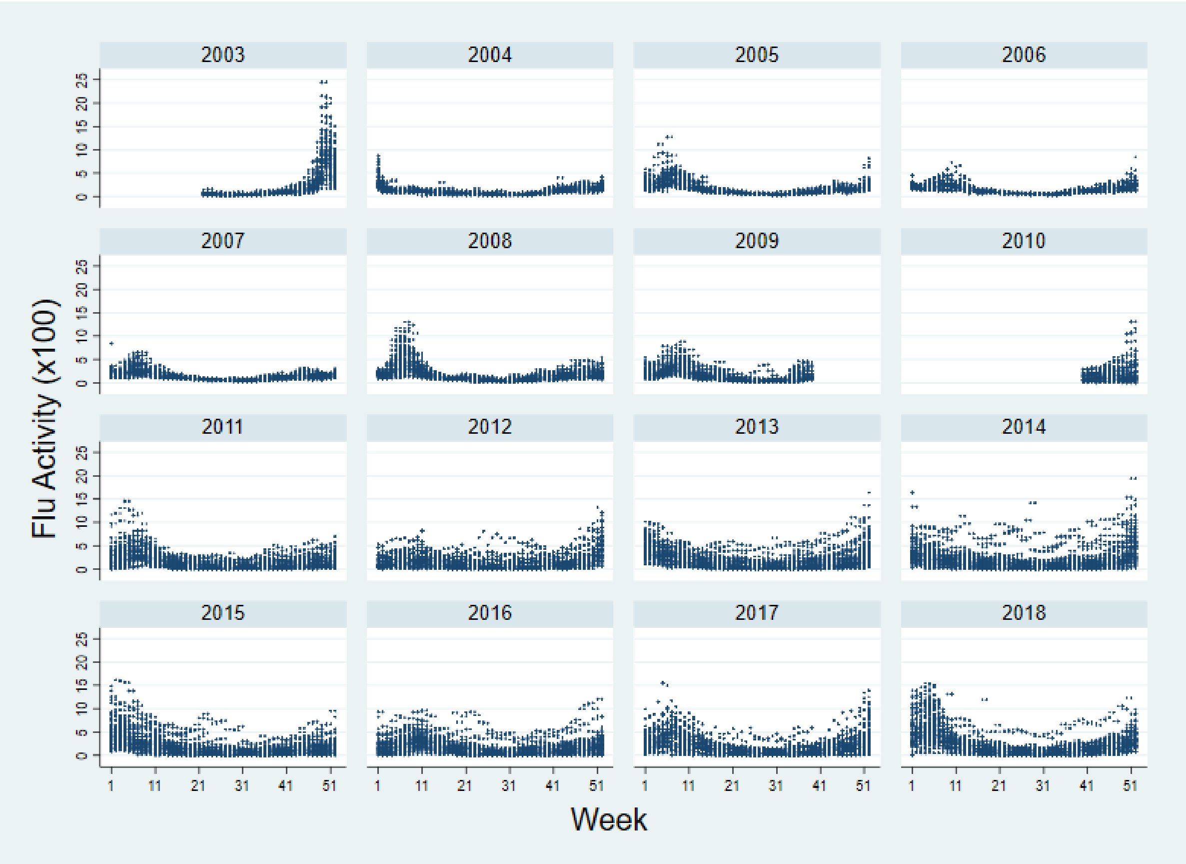
Event Window of Flu Activity



This figure illustrates the information production window. We define this window for calculating flu activity as the weeks between the end of fiscal quarter t for firm i ($FQE_{i,t}$) and its earnings announcement (EA) date for quarter t's performance ($EA_{i,t}$). The bundled short- or long-run forecasts are those issued within the window [-1, +1] of the EA date ($EA_{i,t}$). The short-run forecasts are for fiscal quarter t + 1's performance, and the long-run forecasts are for the performance associated with fiscal periods beyond quarter t + 1.

Figure 2A

Distribution of Weekly Flu Activity during 2003-2018

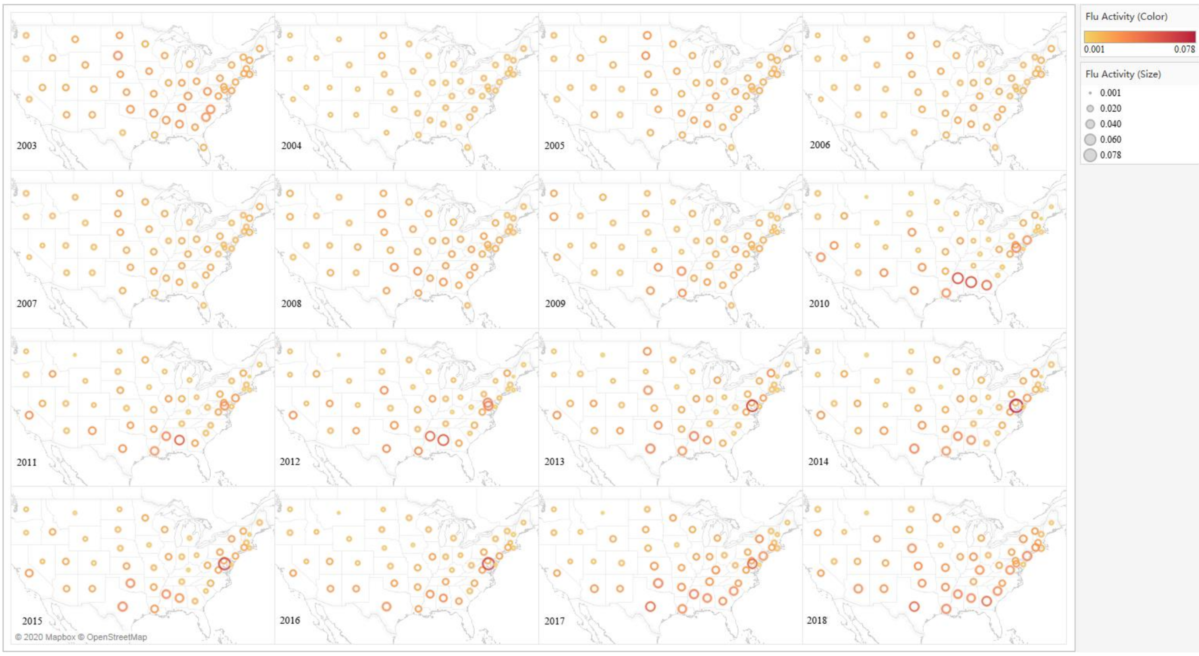


This figure illustrates weekly flu activity over a year from 2003 to 2018. Weekly flu activity is defined as the proportion of outpatient visits to healthcare providers of a state for influenza-like illness (ILI) in a week, multiplied by 100. Value 1 represents that 1% of outpatient visits to healthcare providers are for ILI relative to all outpatient visits. Each dot in the figure represents the level of weekly flu activity of a certain state in a week.

We obtain weekly flu activity data from https://gis.cdc.gov/grasp/fluview/fluportaldashboard.html and https://www.google.org/flutrends/about/data/flu/historic/us-historic-v1.txt. The weekly flu activity data are not available until June 2003. Weekly flu activity data are not available from September 2009 to October 2010. The flu activity data are not available for Florida from 2010 to 2018.

Figure 2B

Distribution of the Average Weekly Flu Activity over a Year during 2003-2018



This figure illustrates the average value of weekly flu activity over each state and year from 2003 to 2018. Weekly flu activity is defined as the proportion of outpatient visits to healthcare providers of a state for influenza-like illness (ILI) in a week. Value 0.01 represents that 1% of outpatient visits to healthcare providers are for ILI relative to all outpatient visits.

We obtain weekly flu activity data from https://gis.cdc.gov/grasp/fluview/fluportaldashboard.html and https://www.google.org/flutrends/about/data/flu/historic/us-historic-v1.txt. The weekly flu activity data are not available until June 2003. Weekly flu activity data are not available from September 2009 to October 2010. The flu activity data are not available for Florida from 2010 to 2018. This figure does not include data from Hawaii and Porto Rico for illustration purposes.

TABLE 1
Sample Selection

	Observations
All firm-quarter observations of U.S. incorporated firms in the Compustat Fundamentals Quarterly from fiscal year 2003 to 2018	525,849
excluding:	
Firm-quarters with actual earnings announcement (EA) dates missing from I/B/E/S or an actual EA date later than the end of the next fiscal quarter	(250,013)
Observations lacking headquarters location information	(2,107)
Firms not covered by I/B/E/S Guidance or that never issued an earnings forecast during the sample period	(144,766)
Observations missing values used to calculate variables in equation (1)	(42,265)
Singleton observations within fixed effects groups	(215)
Final sample	86,483

TABLE 2
Descriptive Statistics

Panel A. Summary statistics (N = 86,483)

Variable	Mean	Std. Dev.	Within-FE Std. Dev.	Within-FE Std. Dev ÷ Pooled Std. Dev.	P5	P25	Median	P75	P95
Issue	0.580	0.494	0.334	67.59%	0.000	0.000	1.000	1.000	1.000
$Issue^{Short}$	0.363	0.481	0.336	69.89%	0.000	0.000	0.000	1.000	1.000
$Issue^{Long}$	0.367	0.482	0.353	73.31%	0.000	0.000	0.000	1.000	1.000
Flu	0.015	0.013	0.003	24.00%	0.003	0.007	0.012	0.020	0.040
Size	7.355	1.848	0.358	19.35%	4.508	6.012	7.261	8.566	10.673
MTB	3.188	4.907	3.867	78.80%	0.662	1.454	2.291	3.778	9.461
Coverage	2.061	0.680	0.319	46.99%	0.693	1.609	2.079	2.565	3.135
IOR	0.734	0.238	0.133	55.68%	0.237	0.616	0.796	0.914	1.000
EPSVolt	0.023	0.052	0.042	79.65%	0.002	0.004	0.008	0.020	0.090
Dispersion	0.146	0.401	0.349	87.10%	0.000	0.015	0.035	0.097	0.599
MBanalyst	0.729	0.275	0.222	80.72%	0.250	0.500	0.750	1.000	1.000
Loss	0.210	0.408	0.315	77.39%	0.000	0.000	0.000	0.000	1.000
ΔEPS	-0.010	1.665	1.628	97.74%	-1.482	-0.193	0.000	0.191	1.436
FutureEPS	0.002	0.041	0.038	92.53%	-0.032	-0.004	0.001	0.005	0.034
Return	0.034	0.212	0.182	85.78%	-0.300	-0.080	0.029	0.137	0.374
Lag	3.399	0.340	0.211	62.08%	2.833	3.178	3.401	3.611	4.007
Error	0.060	0.238	0.200	84.13%	0.000	0.000	0.000	0.000	1.000
IndIssue	0.609	0.161	0.077	48.00%	0.250	0.551	0.643	0.710	0.800
$IndIssue^{Short}$	0.542	0.163	0.077	47.36%	0.194	0.475	0.574	0.645	0.750
$IndIssue^{Long}$	0.492	0.183	0.077	41.97%	0.158	0.397	0.523	0.600	0.769

TABLE 2 (Cont'd)
Panel B. Correlation coefficients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
(1) *Issue*		**0.643**	**0.648**	0.002	**0.096**	**0.159**	**0.144**	**0.151**	**-0.208**	**-0.220**	**0.241**	**-0.159**	0.001	**-0.033**	**0.031**	**-0.048**	0.002	**0.273**	**0.284**	**0.230**
(2) *Issue^{Short}*	**0.643**		**0.073**	**-0.012**	**-0.030**	**0.124**	**0.130**	**0.112**	**-0.123**	**-0.081**	**0.240**	**-0.065**	0.005	**-0.020**	-0.001	**-0.122**	-0.007	**0.173**	**0.203**	**0.051**
(3) *Issue^{Long}*	**0.648**	**0.073**		**0.012**	**0.136**	**0.142**	**0.096**	**0.110**	**-0.182**	**-0.228**	**0.143**	**-0.144**	0.007	**-0.017**	**0.059**	**0.057**	0.005	**0.212**	**0.196**	**0.285**
(4) *Flu*	-0.004	**-0.038**	**0.034**		**0.034**	**0.011**	**0.061**	**0.035**	**-0.013**	**-0.012**	-0.008	**0.037**	**-0.010**	**-0.009**	0.005	**0.224**	0.007	**-0.034**	**0.027**	**-0.032**
(5) *Size*	**0.088**	**-0.032**	**0.128**	**0.047**		**-0.050**	**0.584**	**0.160**	**-0.191**	**-0.145**	**0.085**	**-0.260**	0.001	-0.007	**0.016**	**-0.249**	**0.027**	**-0.066**	**-0.103**	**0.030**
(6) *MTB*	**0.068**	**0.049**	**0.065**	**0.020**	**-0.032**		**0.209**	**0.083**	**-0.415**	**-0.172**	**0.253**	**-0.148**	0.003	**0.044**	**0.198**	**-0.096**	**-0.036**	**0.115**	**0.140**	**0.110**
(7) *Coverage*	**0.146**	**0.133**	**0.095**	**0.064**	**0.590**	**0.110**		**0.283**	**-0.256**	**0.106**	**0.187**	**-0.135**	0.001	**-0.013**	0.001	**-0.239**	**0.013**	**-0.037**	-0.006	**-0.015**
(8) *IOR*	**0.161**	**0.113**	**0.118**	**0.038**	**0.197**	**0.040**	**0.301**		**-0.140**	**0.029**	**0.099**	**-0.122**	0.001	**-0.032**	**0.011**	**-0.025**	**0.039**	**0.012**	**0.049**	**0.027**
(9) *EPSVolt*	**-0.170**	**-0.098**	**-0.137**	**-0.010**	**-0.102**	**-0.101**	**-0.150**	**-0.160**		**0.374**	**-0.226**	**0.415**	-0.002	**0.087**	0.000	**0.185**	**0.030**	**-0.070**	**-0.072**	**-0.083**
(10) *Dispersion*	**-0.154**	**-0.068**	**-0.137**	0.000	**-0.119**	**-0.042**	**-0.019**	**-0.062**	**0.216**		**-0.190**	**0.317**	-0.003	**0.022**	**-0.076**	**0.048**	**0.032**	**-0.123**	**-0.087**	**-0.162**
(11) *MBanalyst*	**0.245**	**0.238**	**0.145**	**-0.012**	**0.093**	**0.108**	**0.192**	**0.121**	**-0.180**	**-0.168**		**-0.217**	**-0.013**	**0.013**	**0.136**	**-0.147**	**-0.042**	**0.061**	**0.076**	**0.028**
(12) *Loss*	**-0.159**	**-0.065**	**-0.144**	**0.038**	**-0.258**	**-0.027**	**-0.136**	**-0.148**	**0.309**	**0.298**	**-0.236**		**-0.044**	**0.092**	**-0.104**	**0.173**	0.008	**-0.026**	0.005	**-0.052**
(13) *ΔEPS*	0.004	0.006	0.001	0.002	0.000	0.001	0.003	0.002	**-0.068**	-0.007	-0.003	**-0.031**		**0.034**	0.000	0.006	0.002	0.000	0.001	0.001
(14) *FutureEPS*	**-0.052**	**-0.031**	**-0.037**	**-0.015**	**-0.023**	-0.004	**-0.029**	**-0.043**	**0.541**	**0.049**	**-0.043**	**0.106**	**0.010**		**0.151**	0.001	**-0.017**	**-0.029**	**-0.040**	**-0.028**
(15) *Return*	**0.019**	-0.002	**0.043**	-0.004	**-0.014**	**0.093**	**-0.022**	0.000	**0.072**	**-0.046**	**0.123**	**-0.090**	**-0.023**	**0.126**		**-0.021**	-0.007	-0.004	**-0.011**	0.003
(16) *Lag*	**-0.050**	**-0.126**	**0.058**	**0.250**	**-0.263**	**-0.042**	**-0.243**	**-0.061**	**0.090**	**0.060**	**-0.150**	**0.169**	-0.007	**0.013**	**-0.026**		**0.017**	**0.038**	**0.074**	**0.088**
(17) *Error*	0.002	-0.007	0.005	0.007	**0.021**	-0.004	**0.011**	**0.028**	**0.014**	**0.010**	**-0.042**	0.008	0.004	0.000	-0.008	**0.015**		**-0.014**	-0.003	**-0.014**
(18) *IndIssue*	**0.300**	**0.195**	**0.216**	**-0.065**	**-0.139**	**0.068**	**-0.061**	**0.023**	**-0.097**	**-0.063**	**0.077**	**-0.012**	0.001	**-0.033**	-0.002	**0.036**	**-0.019**		**0.891**	**0.807**
(19) *IndIssue^{Short}*	**0.307**	**0.215**	**0.206**	**-0.014**	**-0.157**	**0.077**	**-0.033**	**0.048**	**-0.086**	**-0.044**	**0.083**	**0.010**	0.002	**-0.031**	-0.007	**0.070**	**-0.009**	**0.933**		**0.640**
(20) *IndIssue^{Long}*	**0.241**	**0.060**	**0.290**	**-0.044**	**0.017**	**0.067**	**-0.027**	**0.035**	**-0.102**	**-0.084**	**0.029**	**-0.060**	0.001	**-0.033**	-0.007	**0.062**	**-0.018**	**0.816**	**0.700**	

Panel A reports the distribution of the variables in the final sample for our baseline analysis. "FE" represents firm fixed effects and headquarters' state × year-quarter fixed effects used in the baseline regressions. Panel B presents the Pearson (below) / Spearman (above) correlation coefficients of the variables. **Boldface** indicates a 0.01 significance level. See Appendix A for the variable definitions.

TABLE 3
Forecast Issuance Decisions

Panel A. Baseline results

	(1)	(2)	(3)
Dep. Var. =	*Issue*	$Issue^{Short}$	$Issue^{Long}$
Flu	-0.531	-2.382***	1.259***
	(-1.39)	(-5.58)	(2.83)
Size	0.054***	0.045***	0.028***
	(15.18)	(11.62)	(7.47)
MTB	0.000*	-0.000	0.001**
	(1.71)	(-0.16)	(2.51)
Coverage	0.041***	0.026***	0.037***
	(10.33)	(6.17)	(8.64)
IOR	0.073***	0.058***	0.053***
	(8.03)	(6.85)	(5.53)
EPSVolt	-0.472***	-0.141***	-0.416***
	(-12.50)	(-3.74)	(-11.92)
Dispersion	-0.049***	-0.022***	-0.037***
	(-12.37)	(-6.64)	(-11.32)
MBanalyst	0.130***	0.106***	0.088***
	(22.58)	(18.61)	(15.11)
Loss	-0.044***	-0.024***	-0.030***
	(-10.63)	(-6.25)	(-7.53)
ΔEPS	-0.001	0.000	-0.000
	(-0.77)	(0.00)	(-0.53)
FutureEPS	0.068*	0.024	0.082**
	(1.65)	(0.70)	(2.25)
Return	0.006	-0.019***	0.024***
	(0.83)	(-2.68)	(3.29)
Lag	-0.015**	-0.094***	0.113***
	(-2.39)	(-14.26)	(16.60)
Error	0.003	0.009*	0.013**
	(0.53)	(1.67)	(2.15)
IndIssue	0.587***		
	(36.48)		
$IndIssue^{Short}$		0.260***	
		(15.31)	
$IndIssue^{Long}$			0.534***
			(30.99)
Firm FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
Adj. R^2	0.539	0.491	0.445
N	86,483	86,483	86,483

TABLE 3 (Cont'd)

Panel B1. Summary statistics of the coefficients on $Flu^{PlaceboState}$

Dep. Var. =	Times	Mean	Std. Dev.	P1	P10	P25	P50	P75	P90	P99	Results in Table 3 Panel A
$Issue^{Short}$	1,000	-0.005	0.535	-1.234	-0.674	-0.366	-0.004	0.358	0.675	1.227	-2.382
$Issue^{Long}$	1,000	0.009	0.548	-1.204	-0.684	-0.355	0.012	0.376	0.700	1.205	1.259

Panel B2. Summary statistics of the coefficients on $Flu^{PlaceboTime}$

Dep. Var. =	Times	Mean	Std. Dev.	P1	P10	P25	P50	P75	P90	P99	Results in Table 3 Panel A
$Issue^{Short}$	1,000	0.028	0.109	-0.230	-0.111	-0.048	0.028	0.103	0.169	0.281	-2.382
$Issue^{Long}$	1,000	-0.021	0.111	-0.276	-0.164	-0.096	-0.022	0.054	0.123	0.239	1.259

Panel A presents the baseline results of regressions on the effect of flu on forecast issuance decisions. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively. Panels B1 and B2 report the summary statistics of the coefficients on $Flu^{PlaceboState}$ and $Flu^{PlaceboTime}$ for the two placebo tests. The placebo tests use the same model specifications of $Issue^{Short}$ ($Issue^{Long}$) in columns 2 and 3 of Panel A, respectively. See Appendix A for the variable definitions. We include firm fixed effects and headquarters' state × year-quarter fixed effects but do not report them. We define the year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1).

TABLE 4
Cross-sectional Tests

Panel A. Descriptive statistics for the partition variables (*Partition*)

Variable	Mean	Std.	P5	P25	Median	P75	P95
$Complexity^{High}$	0.496	0.500	0.000	0.000	0.000	1.000	1.000
$Past\ Accuracy^{High}$	0.500	0.500	0.000	0.000	0.000	1.000	1.000
Persistence	0.171	0.376	0.000	0.000	0.000	0.000	1.000

Panel B. Regression results with the dependent variable of $Issue^{Short}$

	(1)	(2)	(3)
Partition =	$Complexity^{High}$	$Past\ Accuracy^{High}$	*Persistence*
Flu	-1.464***	-3.145***	-1.520***
	(-3.11)	(-5.01)	(-3.62)
Flu × *Partition*	-1.333***	-0.583**	-2.120***
	(-5.93)	(-2.48)	(-7.86)
Partition	0.039***	0.018***	0.201***
	(7.52)	(3.00)	(29.83)
Control Variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
Adj. R^2	0.489	0.458	0.520
N	73,646	52,793	86,483

Panel C. Regression results with the dependent variable of $Issue^{Long}$

	(1)	(2)	(3)
Partition =	$Complexity^{High}$	$Past\ Accuracy^{High}$	*Persistence*
Flu	0.167	2.015***	0.660
	(0.35)	(3.20)	(1.42)
Flu × *Partition*	1.481***	1.376***	1.957***
	(6.06)	(5.18)	(5.93)
Partition	-0.021***	0.005	0.241***
	(-3.82)	(0.83)	(28.61)
Control Variables	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
Adj. R^2	0.448	0.440	0.469
N	73,646	52,793	86,483

Panel A of this table presents the descriptive statistics for the three partition variables. Panel B and Panel C present the cross-sectional results with the dependent variable of $Issue^{Short}$ and $Issue^{Long}$, respectively. In both panels, the partition variables (*Partition*) are high business and reporting complexity ($Complexity^{High}$) in column 1, high accuracy of short-run forecasts in the past ($Past\ Accuracy^{High}$) in column 2, and an indicator for regular EPS forecast issuer with prior records of switching forecasts of different horizons (*Persistence*) in column 3. See Appendix A for the other variable definitions. The sample size varies because some cross-sectional variables are not available for all observations. We include control variables, firm fixed effects, and headquarters' state × year-quarter fixed effects but do not report them. The control variables are the same as those in Panel A of Table 3. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

TABLE 5

Confounding Effects: Firm Performance and Information Uncertainty

Panel A. Subsample analyses of firm performance and information uncertainty

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	*EPS Growth*			*AF Disp*		
Sample =	Full Sample	Low HQ Pct & Large Diff in Flu	High HQ Pct & Small Diff in Flu	Full Sample	Low HQ Pct & Large Diff in Flu	High HQ Pct & Small Diff in Flu
Flu^{Qtr}	-6.551**	0.522	-9.761**	0.717*	0.368	1.455**
	(-2.13)	(0.08)	(-2.05)	(1.83)	(0.39)	(2.02)
Size	-0.472***	-1.140***	-0.244***	-0.007	0.039*	-0.007
	(-11.71)	(-6.30)	(-3.25)	(-1.45)	(1.94)	(-0.82)
Leverage	0.545***	1.253***	0.359	0.128***	0.147**	0.174***
	(4.58)	(3.24)	(1.48)	(8.77)	(2.56)	(5.91)
MTB	0.186***	0.159***	0.172***	-0.025***	-0.031***	-0.025***
	(11.59)	(3.17)	(4.36)	(-13.94)	(-4.10)	(-6.73)
R&D	-1.000	4.766	-3.021	0.163	-0.772	-0.088
	(-0.68)	(0.88)	(-1.09)	(0.96)	(-1.00)	(-0.26)
EPSVolt	-0.960**	5.237***	-0.351	0.464***	0.232	0.481***
	(-2.25)	(2.78)	(-0.42)	(7.00)	(0.92)	(4.79)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.061	0.059	0.067	0.175	0.269	0.189
N	83,136	14,313	21,661	81,392	14,191	21,194

TABLE 5 (Cont'd)

Panel B. Subsample analyses of forecast issuance

	(1)	(2)	(3)	(4)
Dep. Var. =	*Issue*^{Short}		*Issue*^{Long}	
Sample =	Low HQ Pct & Large Diff in Flu	High HQ Pct & Small Diff in Flu	Low HQ Pct & Large Diff in Flu	High HQ Pct & Small Diff in Flu
Flu	-2.981***	-2.977***	3.038***	1.820**
	(-3.07)	(-3.94)	(3.04)	(2.20)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.583	0.497	0.509	0.450
N	14,313	21,661	14,313	21,661

This table presents the results of tests that address the confounding effects of firm performance and information uncertainty. Panel A reports the results of regressions that test the effect of flu activity over a fiscal quarter (*Flu*^{Qtr}) on concurrent firm performance (*EPS Growth*) and information uncertainty (*AF Disp*). The dependent variable in columns 1–3 of Panel A is, *EPS Growth*, the change in EPS in percentage from fiscal period t - 3 to period $t + 1$. The dependent variable in columns 4-6 is *AF Disp*, the dispersion of analysts' one-quarter-ahead forecasts (*fpi* = 6) issued during the fiscal quarter $t + 1$. "Full Sample" represents the sample used in our main regression reported in Table 3 after considering non-missing variables used in the analyses (*EPS Growth* and *AF Disp*). "Low HQ Pct & Large Diff in Flu" represents the subsample of observations for which the percentage of headquarters employees is below the sample median in a year and the difference in the level of flu activity between the headquarters' state and the other states in which the firm operates exceeds the sample median in a year. "High HQ Pct & Small Diff in Flu" represents the subsample of observations for which the percentage of headquarters employees exceeds the sample median in a year and the difference in the level of flu activity between the headquarters' state and other states in which the firm operates is below the sample median in a year. Panel B reports the results of forecast issuance decisions using "Low HQ Pct & Large Diff in Flu" and "High HQ Pct & Small Diff in Flu" subsamples. See Appendix A for the variable definitions. The sample size varies because some variables are unavailable for all observations. We include but do not report firm fixed effects and headquarters' state × year-quarter fixed effects. The untabulated control variables in Panel B are the same as those in Panel A of Table 3. Robust t-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

TABLE 6

Timing of Earnings Announcements

	(1)	(2)	(3)	(4)
Dep. Var. =	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$
Flu	-2.442***	1.452***	-2.489***	1.425***
	(-5.60)	(3.10)	(-5.73)	(3.04)
$Flu \times Lag^{High}$	-0.475**	0.413**		
	(-2.38)	(2.05)		
Lag^{High}	-0.000	-0.016***		
	(-0.08)	(-3.31)		
$Flu \times Lag\_Deadline^{High}$			-0.400**	0.463**
			(-2.01)	(2.32)
$Lag\_Deadline^{High}$			-0.001	-0.016***
			(-0.27)	(-3.29)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.489	0.443	0.489	0.443
N	86,483	86,483	86,483	86,483

This table reports the results of the cross-sectional analyses conditioning on the reporting lag variables. Columns 1 and 2 present the results conditioning on reporting lag (*Lag*). Columns 3 and 4 present the results conditioning on the reporting lag adjusted for the reporting deadline (*Lag_Deadline*). See Appendix A for variable definitions. We include but do not report control variables, firm fixed effects, and headquarters' state × year-quarter fixed effects. The control variables are the same as those in Panel A of Table 3 except that we exclude *Lag*. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

TABLE 7

Timeliness versus Thoroughness

Dep. Var. =	(1) $Accuracy^{Short}$	(2) $Accuracy^{Long}$	(3) $Accuracy^{Short}$	(4) $Accuracy^{Long}$	(5) $Accuracy^{Short}$	(6) $Accuracy^{Long}$
Flu	0.002	-0.105**	0.021	-0.080*	0.011	-0.077*
	(0.07)	(-2.49)	(0.82)	(-1.76)	(0.44)	(-1.72)
$Flu \times Lag^{High}$			-0.025	-0.048**		
			(-1.58)	(-2.26)		
Lag^{High}			0.000	0.002***		
			(0.94)	(2.82)		
$Flu \times Lag\_Deadline^{High}$					-0.012	-0.054***
					(-0.76)	(-2.65)
$Lag\_Deadline^{High}$					0.000	0.002***
					(0.78)	(2.77)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.506	0.653	0.506	0.653	0.506	0.653
N	29,587	27,816	29,587	27,816	29,587	27,816

This table presents the regression results of forecast accuracy. The dependent variable is short-run forecast accuracy ($Accuracy^{Short}$) in columns 1, 3, and 5 that include 29,587 firm-quarters with short-run bundled EPS forecasts. The dependent variable is long-run forecast accuracy ($Accuracy^{Long}$) in columns 2, 4, and 6 that include 27,816 firm-quarters with long-run bundled EPS forecasts. Columns 3 and 4 present the results conditioning on reporting lag (*Lag*). Columns 5 and 6 present the results conditioning on reporting lag adjusted for the reporting deadline (*Lag_Deadline*). We include control variables, firm fixed effects, and headquarters' state × year-quarter fixed effects but do not report them. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. The control variables are the same as those in equation (1) of Table 3 Panel A in columns 1 and 2, and exclude *Lag* in columns 3–6. See Appendix A for the variable definitions. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

TABLE 8

Other Forecast Characteristics

Dep. Var. =	(1) $Width^{Short}$	(2) $Width^{Long}$	(3) $Horizon^{Long}$
Flu	0.014	0.025**	2.842***
	(1.47)	(2.04)	(4.15)
Size	-0.001***	-0.000	-0.020***
	(-5.92)	(-0.29)	(-3.94)
MB	-0.000*	-0.000***	-0.000
	(-1.79)	(-4.36)	(-0.30)
Coverage	-0.000	0.000	0.032***
	(-1.08)	(0.89)	(6.47)
IOR	-0.004***	-0.002***	0.005
	(-8.76)	(-5.90)	(0.39)
EPSVolt	0.056***	0.084***	-0.230**
	(9.75)	(14.03)	(-2.55)
Dispersion	0.001***	0.001*	-0.008
	(2.66)	(1.84)	(-1.33)
MBanalyst	-0.001***	-0.001***	0.015**
	(-7.06)	(-7.90)	(2.11)
Loss	0.001***	0.001***	0.015***
	(10.45)	(6.63)	(2.64)
ΔEPS	0.000	0.000	-0.002
	(0.44)	(1.61)	(-1.48)
FutureEPS	-0.004	-0.036***	0.135*
	(-0.95)	(-6.65)	(1.79)
Return	-0.004***	-0.004***	-0.021**
	(-15.61)	(-13.38)	(-2.24)
Lag	0.000*	0.001***	0.211***
	(1.82)	(3.30)	(18.70)
Error	-0.000	-0.000	-0.010
	(-0.95)	(-0.16)	(-1.60)
$IndIssue^{Short}$	-0.000		
	(-0.26)		
$IndIssue^{Long}$		-0.000	-0.224***
		(-0.21)	(-10.41)
Firm FE	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes
Adj. R^2	0.719	0.784	0.577
N	29,587	27,816	27,816

This table presents the regression results of the other forecast characteristics with the dependent variable of the short-run forecast width ($Width^{Short}$) in column 1, the average long-run forecast width ($Width^{Long}$) in column 2, and the average long-run forecast horizon ($Horizon^{Long}$) in column 3. Column 1 includes 29,587 firm-quarters with short-run bundled EPS forecasts. Columns 2 and 3 include 27,816 firm-quarters with long-run bundled EPS forecasts. The control variables are the same as in equation (1) of Table 3 Panel A. See Appendix A for the variable definitions. We include firm fixed effects and headquarters' state × year-quarter fixed effects but do not report them. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5%, and 1% levels, respectively.

Appendix A
Variable Definitions

Variable	Definition
Variables Used in the Baseline Analysis	
Issue	An indicator variable equal to 1 when at least one bundled EPS forecast is issued and 0 otherwise. A bundled forecast is the one issued within one day around the fiscal quarter *t*'s actual earnings announcement (EA) date ($EA_{i,t}$ in Figure 1). Source: I/B/E/S.
$Issue^{Short}$	An indicator variable equal to 1 when a bundled forecast is issued for the EPS of fiscal quarter $t + 1$ and 0 otherwise. A bundled forecast is the one issued within one day around the fiscal quarter *t*'s actual EA date ($EA_{i,t}$ in Figure 1). Source: I/B/E/S.
$Issue^{Long}$	An indicator variable equal to 1 when at least one bundled forecast is issued for the EPS of the fiscal periods beyond the fiscal quarter $t + 1$ and 0 otherwise. A bundled forecast is the issued within one day around the fiscal quarter *t*'s actual EA date ($EA_{i,t}$ in Figure 1). Source: I/B/E/S.
Flu	The average proportion of outpatient visits to healthcare providers for influenza-like illness (ILI) over the weeks between the end of fiscal period *t* and the fiscal quarter *t*'s actual EA ($EA_{i,t}$ in Figure 1). A value of 0.01 indicates that 1% of total outpatient visits to healthcare providers are for ILI. Source: https://gis.cdc.gov/grasp/fluview/fluportaldashboard.html and https://www.google.org/flutrends/about/data/flu/historic/us-historic-v1.txt.
Size	The natural logarithm of total assets (*atq*) at the end of fiscal quarter *t*. Source: Compustat.
MTB	The sum of the market value of equity and long-term debt (*prccq* × *cshoq* + *dlttq*) divided by the book value of total assets (*atq*) at the end of fiscal quarter *t*. Source: Compustat.
Coverage	The natural logarithm of the number of analysts issuing one-year-ahead earnings forecasts (*fpi* = 1) for the firm during fiscal quarter *t*. Source: I/B/E/S.
IOR	Percentage of institutional shareholding at the end of fiscal quarter *t*. Source: Thomas Reuters 13F.
EPSVolt	The standard deviation of quarterly EPS (*epspxq*) over the 12 quarters until the end of fiscal quarter *t*, divided by the stock price at the beginning of fiscal quarter *t* (*prccq*). Source: Compustat.
Dispersion	The standard deviation of one-year-ahead analysts' forecasts (*fpi* = 1) divided by the absolute value of the median analyst forecast during fiscal quarter *t*. For analysts who issued multiple forecasts for the same fiscal period during the quarter, we use the value of the first forecast. For quarters in which only one analyst issues forecasts, we set the variable to zero. Source: I/B/E/S.
MBanalyst	The proportion of quarters in the prior four periods (*t* - 3 to *t*) during which the firm meets or beats the most recent analysts' consensus forecast before the EA. Source: I/B/E/S.

Loss	An indicator variable equal to 1 when net income (*niq*) during the fiscal quarter *t* is negative and 0 otherwise. Source: Compustat.
ΔEPS	The change in EPS (*epspxq*) from the same quarter in the prior year averaged over the four previous quarters (*t* - 3 to *t*) (i.e., $[(EPS_t - EPS_{t-4}) + (EPS_{t-1} - EPS_{t-5}) + (EPS_{t-2} - EPS_{t-6}) + (EPS_{t-3} - EPS_{t-7})] \div 4)$ and deflated by the stock price at the end of fiscal quarter *t* (*prccq*). Source: Compustat.
FutureEPS	The change in EPS (*epspxq*) from the same quarter in the subsequent year averaged over the four next four quarters (*t* + 1 to *t* + 4) (i.e., $[(EPS_{t+4} - EPS_t) + (EPS_{t+3} - EPS_{t-1}) + (EPS_{t+2} - EPS_{t-2}) + (EPS_{t+1} - EPS_{t-3})] \div 4)$ and deflated by the stock price at the end of fiscal quarter *t* (*prccq*). Source: Compustat.
Return	The buy-and-hold stock return during fiscal quarter *t*. Source: CRSP.
Lag	The natural logarithm of the number of days between the end of fiscal quarter *t* ($FQE_{i,t}$ in Figure 1) and the date of the upcoming actual EA ($EA_{i,t}$ in Figure 1). Source: I/B/E/S.
Error	An indicator variable equal to 1 if a firm is during the misstatement period of an error-related misstatement in the 10-K or 10-Q report identified in Audit Analytics Restatement database, and 0 otherwise. An error-related misstatement is defined as a misstatement that does not either reference financial fraud, irregularities, or misrepresentations (*res_fraud* = 0), or indicate the involvement of the SEC, PCAOB, or other regulator in the restatement process (*res_sec_invest* = 0). Misstatement period is the time between the beginning date (*res_begin_date*) and ending date (*res_end_date*) of a restatement. Source: Audit Analytics.
IndIssue	The proportion of peers in the firm's 2-digit SIC industry that have provided at least one bundled EPS forecast during the prior four quarters (*t* - 3 to *t*). Source: I/B/E/S.
$IndIssue^{Short}$	The proportion of peers in the firm's 2-digit SIC industry that have provided at least one short-run bundled EPS forecast during the prior four quarters (*t* - 3 to *t*). Source: I/B/E/S.
$IndIssue^{Long}$	The proportion of peers in the firm's 2-digit SIC industry that have provided at least one long-run bundled EPS forecast during the prior four quarters (*t* - 3 to *t*). Source: I/B/E/S.
Variables Used in the Placebo Tests	
$Flu^{PlaceboState}$	Flu activity in placebo states measured as the average value of weekly flu activity over the information production window (i.e., the same measurement window for *Flu* for the focal firm) by randomly selecting a state outside the focal firm's headquarters' state.
$Flu^{PlaceboTime}$	Flu activity in the placebo measurement window measured as the average value of weekly flu activity over a randomly selected period in the same year for each firm-quarter observation, with the length of the period equal to that of the information production window of the focal firm.
Variables Used in the Cross-sectional Analysis:	

Partition	Represents *Complexity*^{High}, *Past Accuracy*^{High}, or *Persistence* in the cross-sectional analysis of Table 4.
Complexity	The first principal component of *BusSeg*, *ARC*, and *Acquisition*. For observations with missing *ARC*, we focus on *BusSeg* and *Acquisition* only. *BusSeg* is the number of business segments in the fiscal year of fiscal quarter *t* (Source: Compustat). *ARC* is an accounting reporting complexity measure based on the count of XBRL tags disclosed in 10-K filings in the fiscal year in which the fiscal quarter *t* falls (Source: XBRL Research). *Acquisition* is an indicator variable that equals one if the firm reports non-zero, non-missing cash flows related to year-to-date acquisitions for fiscal quarter *t* (i.e., *aqcy* is positive), and zero otherwise (Source: Compustat).
Complexity^{High}	An indicate variable equal to 1 when *Complexity* exceeds the sample median of a year, and 0 otherwise.
Past Accuracy	Accuracy of the short-run EPS forecasts issued in the past measured as the average value of forecast accuracy of all of the bundled short-run EPS forecasts over fiscal quarters *t* - 3 to *t*. Forecast accuracy is -1 multiplied by the absolute value of the difference between earnings forecast and corresponding actual EPS and scaled by the stock price at the end of the fiscal quarter immediately before the forecast is issued. We require the firm to have issued at least one short-run EPS forecast in the past four quarters. Source: I/B/E/S and CRSP.
Past Accuracy^{High}	An indicator variable equal to 1 when the accuracy of the short-run EPS forecasts issued in the past exceeds the sample median of a year, and 0 otherwise.
Persistence	An indicator variable equal to 1 when the firm (1) has issued at least one bundled EPS forecast in each of the past four quarters (*t* - 3 to *t*), (2) does not issue short-run earnings forecasts in at least one of the past four quarters, and (3) does not issue long-run earnings forecasts in at least one of the past four quarters and equal to zero otherwise. Source: I/B/E/S.
Lag_Deadline	The difference between the EA date ($EA_{i,t}$ in Figure 1) and the 10-Q/K statutory filing deadline (Bartov and Konchitchki [2017]). For example, *Lag_Deadline* is equal to -25 when a firm's EA date is 20 days after the end of the fiscal period though it is required to submit its 10-Q within 45 days after the end of the fiscal period. Source: Compustat, CRSP, and I/B/E/S.
Lag^{High}	An indicator variable equal to 1 when *Lag* exceeds the sample median of a fiscal year-quarter and 0 otherwise.
Lag_Deadline^{High}	an indicator variable equal to 1 when *Lag_Deadline* exceeds the sample median of a fiscal year-quarter and 0 otherwise.
Other Forecast Characteristics Variables	
Accuracy^{Short} (*Accuracy*^{Long})	The forecast accuracy of short- or long-run EPS forecasts bundled with $EA_{i,t}$. For long-run EPS forecasts, we take the average value of forecast accuracy. The forecast accuracy is -1 times the absolute value of the difference between a short- or long-run bundled EPS forecast and the

	corresponding actual EP, scaled by the stock price at the end of fiscal quarter t. Source: I/B/E/S and CRSP.
$Width^{Short}$ ($Width^{Long}$)	The forecast width of a short- or long-run EPS forecast bundled with $EA_{i,t}$. For long-run EPS forecasts, we take the average value of forecast width. The forecast width is the high estimate minus the low estimate scaled by the stock price at the end of fiscal quarter t. Source: I/B/E/S and CRSP.
$Horizon^{Long}$	The long-run forecast horizon measured as the natural logarithm of the average number of days between the forecast issuance date and the end of the corresponding fiscal period for the forecasted earnings for a firm's long-run EPS forecasts bundled with $EA_{i,t}$. Source: I/B/E/S.
Variables Used in Other Analyses	
EPS Growth	The difference between the actual EPS (*epspxq*) in fiscal quarter $t + 1$ and the actual EPS in quarter t - 3 divided by the absolute value of EPS in quarter t - 3. Source: Compustat.
AF Disp	The standard deviation of one-quarter-ahead analyst forecasts (*fpi* = 6) issued during the fiscal quarter $t + 1$ divided by the absolute value of the median analyst forecast during fiscal quarter $t + 1$. For analysts who issued multiple forecasts for the same fiscal period during the quarter, we use the value of the first forecast. For quarters in which only one analyst issues forecasts, we set the variable to 0. Source: I/B/E/S.
Leverage	Long-term debt (*dlttq*) divided by total assets (*atq*) at the end of fiscal quarter t. Source: Compustat.
R&D	R&D expenditures (*xrdq*, zero if missing) divided by total assets (*atq*) at the end of fiscal quarter t. Source: Compustat.
Flu^{Qtr}	The average proportion of outpatient visits to healthcare providers for influenza-like illness (ILI) during the fiscal quarter $t + 1$. A value of 0.01 indicates that 1% of all outpatient visits to healthcare providers are for ILI. Source: https://gis.cdc.gov/grasp/fluview/fluportaldashboard.html and https://www.google.org/flutrends/about/data/flu/historic/us-historic-v1.txt.

Online Appendix

Flu Fallout: Information Production Constraints and Corporate Disclosure

This Online Appendix contains additional discussions and analyses referenced in the main paper.

OA.1 Additional Results

This section contains the following components. First, Table OA1 reports the results of the positive association between flu activity and errors in financial statements. Second, Tables OA2 and OA3 address the measurement error of *Flu* by (1) excluding observations with high flu activity but low population density from the testing sample; and (2) recalculating *Flu* based on MSA-level flu activity. Tables OA4, OA5, OA6, and OA7 report the robustness of our findings using alternative fixed effects, alternative flu measures, alternative samples, and forecast frequency variables, respectively. Table OA8 reports the results of long-run forecast revisions. Finally, Table OA9 presents the untabulated summary statistics of variables used in Tables 4–8.

TABLE OA1

Validation Tests: Reporting Lags, Errors in Financial Statements, and Flu

	(1)	(2)
Dep. Var. =	*Lag*	*Error*
Flu	3.075***	0.289***
	(4.47)	(2.70)
Size	-0.013***	0.003***
	(-5.40)	(3.50)
MTB	-0.001***	0.000*
	(-4.20)	(1.86)
Coverage	-0.019***	0.001
	(-6.82)	(0.83)
IOR	-0.028***	0.004**
	(-5.01)	(2.45)
EPSVolt	0.178***	0.031***
	(7.46)	(3.88)
Dispersion	-0.000	0.000
	(-0.17)	(0.39)
MBanalyst	-0.033***	0.001
	(-9.19)	(0.51)
Loss	0.039***	-0.000
	(14.16)	(-0.22)
ΔEPS	-0.001	0.000
	(-1.29)	(0.39)
FutureEPS	-0.035	-0.025***
	(-1.48)	(-3.32)
Return	-0.027***	-0.000
	(-5.81)	(-0.11)
Lag		-0.001
		(-0.98)
Error	-0.012	
	(-0.99)	
IndIssue	-0.068***	-0.007***
	(-6.42)	(-2.72)
Firm FE	Yes	Yes
State × YQ FE	Yes	Yes
Adj. R^2	0.595	0.250
N	86,483	86,483

This table presents the results of regressions in relation to the effect of flu on reporting lags in column 1 and errors in financial statements in column 2. The variables are defined in Appendix A. We include but do not report firm fixed effects and headquarters' state × year-quarter fixed effects. The control variables are the same as those in Table 3 Panel A except that we exclude *Lag* (*Error*) from the regression in column 1 (2), respectively. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the earnings announcement (EA) date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA2

Tests to Address Measurement Error:
Excluding High Flu but Low Population Density Sample

Dep. Var. =	(1) $Issue^{Short}$	(2) $Issue^{Long}$
Flu	-2.326***	1.183**
	(-5.15)	(2.55)
Size	0.045***	0.029***
	(10.98)	(7.73)
MTB	-0.000	0.001**
	(-0.27)	(2.25)
Coverage	0.029***	0.037***
	(6.67)	(8.52)
IOR	0.057***	0.051***
	(6.55)	(5.12)
EPSVolt	-0.140***	-0.403***
	(-3.55)	(-11.34)
Dispersion	-0.021***	-0.037***
	(-6.23)	(-10.75)
MBanalyst	0.104***	0.092***
	(17.82)	(15.36)
Loss	-0.024***	-0.029***
	(-6.08)	(-7.15)
ΔEPS	0.000	-0.000
	(0.10)	(-0.19)
FutureEPS	0.027	0.081**
	(0.76)	(2.20)
Return	-0.019**	0.026***
	(-2.54)	(3.41)
Lag	-0.094***	0.115***
	(-13.84)	(16.23)
Error	0.006	0.012*
	(1.13)	(1.84)
$IndIssue^{Short}$	0.260***	
	(14.87)	
$IndIssue^{Long}$		0.528***
		(29.42)
Firm FE	Yes	Yes
State × YQ FE	Yes	Yes
Adj. R^2	0.492	0.445
N	82,231	82,231

This table presents the results of our main tests using an alternative sample to address measurement error. We exclude 4,252 observations of our full sample that are from high-*Flu* group and low-density group. We define high-*Flu* group as those as the observations with the value of *Flu* variable above the sample median of each quarter. We define low-density group as those counties in our sample whose population density is below sample median of each year. See Appendix A for detailed variable definitions. We include but do not report firm fixed effects, and headquarters' state × year-quarter fixed effects. The control variables are the same as those in Table 3 Panel A. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA3

Tests to Address Measurement Error: MSA-Level Flu Measures

Dep. Var. =	(1) $Issue^{Short}$	(2) $Issue^{Long}$
$Flu^{MSA\text{-}Google}$	-0.011***	0.007*
	(-2.62)	(1.76)
Size	0.059***	0.021***
	(7.87)	(13.15)
MTB	0.000	0.006***
	(0.44)	(8.62)
Coverage	0.032***	0.009**
	(5.22)	(1.96)
IOR	0.057***	0.160***
	(4.33)	(16.09)
EPSVolt	-0.210***	-0.608***
	(-4.06)	(-10.62)
Dispersion	-0.021***	-0.062***
	(-4.66)	(-12.71)
MBanalyst	0.095***	0.177***
	(11.16)	(20.00)
Loss	-0.040***	-0.051***
	(-7.04)	(-8.24)
ΔEPS	0.001	0.000
	(0.66)	(0.05)
FutureEPS	0.075	0.207***
	(1.48)	(3.54)
Return	-0.012	0.016
	(-1.26)	(1.42)
Lag	-0.091***	0.106***
	(-10.49)	(13.26)
Error	-0.001	0.019**
	(-0.09)	(2.11)
$IndIssue^{Short}$	0.196***	
	(8.22)	
$IndIssue^{Long}$		0.623***
		(36.19)
Firm FE	Yes	Yes
MSA × YQ FE	Yes	Yes
Adj. R^2	0.497	0.190
N	40,851	40,851

This table presents the results of regressions using MSA-level flu activity. $Flu^{MSA\text{-}Google}$ is the estimated MSA-level flu activity provided by the Google Flu Trends (GFT), calculated as the average estimated weekly proportion of outpatient visits to healthcare providers for influenza-like illness (ILI) in the Metropolitan Statistical Area (MSA) that the firm's headquarters is located over the information production window. The data is available from 2003 to 2013 and obtained from https://www.google.org/flutrends/about/data/flu/historic/us-historic-v2.txt. See http://www.google.org/flutrends/about/how.html for a more detailed description of the GFT data. Other variables are defined in Appendix A. The control variables are the same as those in Table 3 Panel A. We include but do not report firm fixed effects and headquarters' MSA × year-quarter fixed effects. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' MSA × year-quarter level. *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA4

Alternative Fixed Effects

Panel A. Diagnostic results as in deHaan [2021]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var. =	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$
Flu	-2.158***	2.314***	-0.856***	0.664***	-0.727***	0.496**	-0.536***	1.210***
	(-4.79)	(4.89)	(-5.42)	(3.62)	(-4.19)	(2.32)	(-4.43)	(7.47)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Industry × YQ, State × YQ		Firm, State × Y, State × Q		Industry, State × Y, Q		Industry, State × Y	
Unabsorbed Variation of *Flu*	22.39%		49.69%		53.06%		83.91%	
Adj. R^2	0.506	0.459	0.495	0.448	0.205	0.215	0.177	0.163
N	86,483	86,483	86,483	86,483	86,483	86,483	86,483	86,483

Panel B. CEO-related fixed effects

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$	$Issue^{Short}$	$Issue^{Long}$
Flu	-3.055***	1.600***	-3.116***	1.562***	-3.083***	1.578***
	(-6.53)	(3.34)	(-6.57)	(3.26)	(-6.54)	(3.30)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	CEO, State × YQ		CEO, Firm, State × YQ		CEO × Firm, State × YQ	
Adj. R^2	0.533	0.481	0.538	0.484	0.538	0.485
N	66,573	66,573	66,573	66,573	66,570	66,570

This table presents the results of using alternative fixed effects. "Unabsorbed Variation of *Flu*" represents the ratio of within-FE standard deviation of *Flu* and pooled of *Flu* as in Table 2 Panel A for the set of specified fixed effects. We obtain CEO information from Execucomp in Panel B. We include but do not report control variables and various fixed effects. Control variables are the same as those in Table 3 Panel A. All variables are defined in Appendix A. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level except for columns (3)-(8) with the headquarters' state × year level in Panel A. Our results continue to hold if we calculate standard errors that cluster by headquarters' state (untabulated). We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA5

Alternative Measures of Flu

Dep. Var. =	(1) $Issue^{Short}$	(2) $Issue^{Long}$	(3) $Issue^{Short}$	(4) $Issue^{Long}$
Flu_Pop	-1,132.495***	615.435*		
	(-4.63)	(1.80)		
Flu_Composite			-0.082***	0.051***
			(-4.97)	(2.82)
Size	0.048***	0.029***	0.037***	0.021***
	(6.27)	(3.38)	(5.23)	(3.18)
MTB	-0.000	-0.000	-0.000	-0.000
	(-0.43)	(-0.95)	(-0.44)	(-0.64)
Coverage	0.023***	0.038***	-0.001	0.047***
	(3.04)	(4.70)	(-0.18)	(6.50)
IOR	0.051***	0.062***	0.045***	0.063***
	(3.43)	(3.73)	(3.19)	(4.46)
EPSVolt	-0.220***	-0.587***	-0.205***	-0.504***
	(-2.77)	(-6.47)	(-3.43)	(-8.15)
Dispersion	-0.017***	-0.039***	-0.014***	-0.033***
	(-2.74)	(-6.14)	(-2.76)	(-6.41)
MBanalyst	0.059***	0.042***	0.078***	0.064***
	(6.67)	(4.42)	(8.32)	(6.95)
Loss	-0.019***	-0.021***	-0.017***	-0.029***
	(-3.20)	(-3.12)	(-2.88)	(-4.75)
ΔEPS	-0.001	0.001	-0.001	0.000
	(-0.68)	(0.78)	(-0.80)	(0.59)
FutureEPS	-0.171*	-0.132	0.031	0.146**
	(-1.78)	(-1.29)	(0.49)	(2.47)
Return	-0.011	0.035**	0.000	0.015
	(-0.93)	(2.50)	(0.00)	(1.33)
Lag	-0.135***	0.188***	-0.158***	0.213***
	(-13.14)	(15.43)	(-14.55)	(17.46)
Error	-0.004	0.015	0.009	0.006
	(-0.42)	(1.61)	(1.07)	(0.71)
$IndIssue^{Short}$	0.174***		0.213***	
	(5.43)		(7.07)	
$IndIssue^{Long}$		0.539***		0.487***
		(14.11)		(13.70)
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.564	0.489	0.538	0.473
N	35,806	35,806	37,530	37,530

This table presents the results of using two alternative measures of flu activity. *Flu_Pop* is the average number of outpatient visits to healthcare providers for influenza-like illness (ILI) for the weeks during the information production window, scaled by the total population of the state in the year of EA date. The variable is available from 2010. *Flu_Composite* is the average value of *Flu_Severity* and *Flu_WideSpread*, where *Flu_Severity* is the average level of flu severity rated by the CDC (https://gis.cdc.gov/grasp/fluview/main.html) over the information production window, rescaled to the range from zero to one; *Flu_WideSpread* is the average level of flu severity rated by the CDC (https://gis.cdc.gov/grasp/fluview/FluView8.html) over the information production period, rescaled to the range from zero to one. *Flu_Composite* is available from 2008. Other variables are defined in Appendix A. We include but do not report firm fixed effects and headquarters' state × year-quarter fixed effects. Control variables are the same as those in Table 3 Panel A. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA6

Alternative Samples

Dep. Var. =	(1) $Issue^{Short}$	(2) $Issue^{Long}$	(3) $Issue^{Short}$	(4) $Issue^{Long}$
Sample =	Excluding Extreme Weather Observations		Excl. States with Consistently High or Low Flu Activity	
Flu	-2.247***	1.342***	-2.377***	1.189***
	(-4.70)	(2.58)	(-5.42)	(2.65)
Size	0.045***	0.029***	0.046***	0.028***
	(11.07)	(7.55)	(11.66)	(7.41)
MTB	0.000	0.001*	-0.000	0.001***
	(0.11)	(1.78)	(-0.05)	(2.77)
Coverage	0.026***	0.041***	0.026***	0.039***
	(5.77)	(9.20)	(5.97)	(9.06)
IOR	0.052***	0.045***	0.059***	0.054***
	(5.82)	(4.51)	(6.83)	(5.57)
EPSVolt	-0.139***	-0.375***	-0.124***	-0.369***
	(-3.81)	(-11.13)	(-3.63)	(-11.36)
Dispersion	-0.021***	-0.037***	-0.020***	-0.036***
	(-6.24)	(-10.98)	(-6.03)	(-10.85)
MBanalyst	0.109***	0.093***	0.108***	0.089***
	(18.01)	(15.03)	(18.53)	(14.99)
Loss	-0.025***	-0.029***	-0.023***	-0.028***
	(-6.09)	(-7.03)	(-5.78)	(-7.04)
ΔEPS	-0.000	0.000	-0.000	-0.000
	(-0.29)	(0.43)	(-0.50)	(-0.04)
FutureEPS	0.032	0.084**	0.015	0.080**
	(0.90)	(2.45)	(0.46)	(2.49)
Return	-0.021***	0.022***	-0.019***	0.024***
	(-2.89)	(2.93)	(-2.64)	(3.19)
Lag	-0.098***	0.113***	-0.096***	0.113***
	(-13.88)	(15.68)	(-14.21)	(16.28)
Error	0.004	0.012*	0.010*	0.014**
	(0.73)	(1.89)	(1.75)	(2.21)
$IndIssue^{Short}$	0.269***		0.269***	
	(15.14)		(15.31)	
$IndIssue^{Long}$		0.528***		0.545***
		(28.71)		(30.79)
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.489	0.443	0.491	0.447
N	77,311	77,311	82,779	82,779

This table presents the results of using alternative samples. Observations with extreme weather conditions are those with the value of any of the six weather condition variables (*Temperature*, *Wind Speed*, *Dew Point*, *Visibility*, *Sea Level Pressure*, and *Cloud*) in the top or bottom percentile of a year in our sample. *Temperature*, *Wind Speed*, *Dew Point*, *Visibility*, *Sea Level Pressure*, and *Cloud* are defined in OA.2 of this Online Appendix. States with consistently high flu activity include Alabama, Mississippi, and Oklahoma, while states with consistently low flu activity include Colorado, Florida, and Montana. In defining consistently high or low flu activity, we first take the averaged value of *Flu* for each state-year and rank the state-year observations in quintiles. We next obtain the max (min) rank for each state and year. We define the states with consistently high (low) flu activity if their min (max) rank is not less (higher) than 4 (2), respectively. Other variables are defined in Appendix A. We include but do not report firm fixed effects and headquarters' state × year-quarter fixed effects. Control variables are the same as those in Table 3 Panel A. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA7

Forecast Frequency

	(1)	(2)
Dep. Var. =	$Freq^{Short\text{-}All}$	$Freq^{Long\text{-}All}$
Flu	-3.585***	1.789***
	(-6.51)	(3.91)
Size	0.052***	0.025***
	(9.60)	(6.43)
MTB	-0.000	0.001**
	(-0.68)	(2.40)
Coverage	0.039***	0.039***
	(7.49)	(9.03)
IOR	0.094***	0.058***
	(8.14)	(5.97)
EPSVolt	-0.130**	-0.439***
	(-2.48)	(-11.62)
Dispersion	-0.018***	-0.038***
	(-4.25)	(-11.45)
MBanalyst	0.138***	0.096***
	(18.93)	(16.34)
Loss	-0.034***	-0.034***
	(-7.06)	(-8.56)
ΔEPS	-0.000	-0.000
	(-0.51)	(-0.40)
FutureEPS	0.059	0.091**
	(1.26)	(2.48)
Return	-0.043***	0.029***
	(-4.76)	(3.83)
Lag	-0.171***	0.122***
	(-19.63)	(17.58)
Error	0.019***	0.014**
	(2.63)	(2.26)
$IndIssue^{Short\text{-}All}$	0.255***	
	(12.12)	
$IndIssue^{Long\text{-}All}$		0.516***
		(29.70)
Firm FE	Yes	Yes
State × YQ FE	Yes	Yes
Adj. R^2	0.555	0.449
N	86,483	86,483

This table presents the results of forecast frequency. $Freq^{Short\text{-}All}$ is the frequency of all short-run bundled forecasts, measured as the natural logarithm of one plus the number of all bundled forecasts for fiscal quarter $t + 1$. $Freq^{Long\text{-}All}$ is the frequency of all long-run bundled forecasts, measured as the natural logarithm of one plus the number of all bundled forecasts for fiscal periods beyond the fiscal quarter $t + 1$. $IndIssue^{Short\text{-}All}$ ($IndIssue^{Long\text{-}All}$) is the proportion of peers in the firm's 2-digit SIC industry that have provided at least one short-term (long-term) bundled forecast during the prior four quarters (t - 3 to t). Other variables are defined in Appendix A. We include but do not report firm fixed effects and headquarters' state × year-quarter fixed effects. The control variables are the same as those in Table 3 Panel A. Robust t-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA8

Long-run Forecast Revisions

	(1)	(2)	(3)	(4)
Dep. Var. =	$Issue^{Long\ Rev}$		$Issue^{Long\ Rev}\%$	
Sample =	Full Sample	$Issue^{Long} = 1$	Full Sample	$Issue^{Long} = 1$
Flu	1.731***	4.364***	1.389***	4.028***
	(3.70)	(7.09)	(3.12)	(6.31)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.444	0.423	0.446	0.477
N	86,483	31,732	86,483	31,732

This table presents the results of the effect of flu on the subsequent revisions to the long-run forecasts issued. Columns 1 and 3 use the full sample. Columns 2 and 4 use the sample with bundled long-run forecast issued ($Issue^{Long} = 1$). The dependent variable in columns 1 and 2 is $Issue^{Long\ Rev}$, an indicator variable equal to 1 when at least one long-run bundled forecast is issued and subsequently revised, and 0 otherwise. The dependent variable in columns 3 and 4 is $Issue^{Long\ Rev}\%$, calculated as the proportion of long-run bundled forecasts that are subsequently revised to the total number of long-term bundled forecast issued. We set the two variables above to be 0 if there is no bundled long-run forecast issued in a certain quarter in columns 1 and 3. Other variables are defined in Appendix A. We include but do not report control variables, firm fixed effects and headquarters' state × year-quarter fixed effects. Control variables are the same as those in Table 3 Panel A. Robust *t*-statistics are reported in parentheses and calculated using standard errors clustered at the headquarters' state × year-quarter level. We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

TABLE OA9

Summary Statistics for Other Variables Used in Tables 4–8

Variable	N	Mean	Std.	P5	P25	Median	P75	P95
Variables Used in Table 4:								
Complexity	73,646	70.974	91.164	0	0.777	1.974	126.253	251.705
Past Accuracy	52,793	-0.006	0.018	-0.020	-0.004	-0.001	-0.001	-0.000
Variables Used in Table 5:								
EPS Growth	83,136	-0.026	3.176	-2.767	-0.349	0.077	0.500	3.000
AF Disp	81,392	0.170	0.407	0.000	0.024	0.053	0.135	0.684
Flu^{Qtr}	83,136	0.016	0.012	0.004	0.008	0.014	0.021	0.039
Size	83,136	7.344	1.824	4.508	6.012	7.261	8.566	10.673
Leverage	83,136	0.193	0.195	0.000	0.016	0.159	0.301	0.542
MTB	83,136	1.675	1.428	0.286	0.841	1.275	2.038	4.342
R&D	83,136	0.010	0.020	0.000	0.000	0.000	0.013	0.046
EPSVolt	83,136	0.023	0.057	0.002	0.004	0.008	0.020	0.090
Variables Used in Tables 6 and 7:								
Lag^{High}	86,483	0.468	0.499	0.000	0.000	0.000	1.000	1.000
Lag_Deadline	86,483	-15.784	11.637	-39.000	-22.000	-14.000	-8.000	-1.000
$Lag\_Deadline^{High}$	86,483	0.470	0.499	0.000	0.000	0.000	1.000	1.000
$Accuracy^{Short}$	29,587	-0.006	0.026	-0.016	-0.003	-0.001	-0.000	-0.000
$Accuracy^{Long}$	27,816	-0.012	0.040	-0.043	-0.009	-0.003	-0.001	-0.000
Variables Used in Table 8:								
$Width^{Short}$	29,587	0.003	0.009	0.000	0.000	0.001	0.003	0.011
$Width^{Long}$	27,816	0.005	0.010	0.000	0.001	0.003	0.005	0.015
$AvgHorizon^{Long}$	27,816	5.453	0.341	4.990	5.075	5.505	5.762	6.033

This table reports the summary statistics of other variables used in Tables 4–8. See Appendix A for the variable definitions.

OA.2 The Effect of Mood and Unobserved Correlated Variable

As the first test, we further control for a battery of weather-related variables in Equation (1). Following previous studies, we first construct six weather condition variables around a firm's headquarters office—*Temperature*, *Wind Speed*, *Dew Point*, *Visibility*, *Sea Level Pressure*, and *Cloud*—each of which is equal to the average of the daily index of an underlying weather condition over the information production window for each firm-quarter.[1] Next, as these variables are highly correlated with our test variable, *Flu*, we construct a new variable as the proportion of variation in the flu activity that cannot be explained by these weather-related variables.[2] Specifically, we regress *Flu* on the abovementioned six weather condition variables and take the residuals, which we denote as $Flu^{Residual}$. We then re-estimate Equation (1) using $Flu^{Residual}$ as our independent variable of interest and further control for the six weather variables. Panel A of Table OA10 shows that the coefficient on $Flu^{Residual}$ is significant in both $Issue^{Short}$ and $Issue^{Long}$ specifications, suggesting that our main findings are not sensitive to considering weather conditions around headquarters offices.

We also assess the sensitivity of baseline results to an unobserved correlated variable by applying the procedure in Oster [2019]. Specifically, Oster [2019] evaluates the potential for the selection on unobservables by testing the sensitivity of the coefficient estimate (*Flu* in our case) to the inclusion of additional controls through measuring the extent of change in R^2 across regression models. Oster [2019] develops a test statistic (δ^*) for stability of the coefficient estimate under reasonable assumptions about the maximum attainable R^2, whereby the value of δ^* denotes the degree of selection on unobservables relative to observables that would be necessary to explain the estimated coefficient. We first regress $Issue^{Short}$ ($Issue^{Long}$) on *Flu*, absent of controls and fixed effects, and obtain the R^2 of baseline effect. We next regress $Issue^{Short}$ ($Issue^{Long}$) on *Flu*, control variables and fixed effects as Equation (1) and obtain the R^2 of controlled effect. Untabulated results show that the coefficient on *Flu* passes this test of coefficient stability ($\delta^* = 1.215$ and 1.344), suggesting that the relation between *Flu* and $Issue^{Short}$ ($Issue^{Long}$) in our baseline model is unlikely to be fully driven by omitted variable bias.[3]

Lastly, we specifically examine whether the flu is associated with forecast bias (relative to the corresponding actual outcome) and directional forecast news (relative to prior analyst consensus forecast). Panel B of Table OA10 shows that the coefficient on *Flu* is statistically insignificant across all specifications, suggesting that moods do not appear to have a significant effect in our setting of flu activity and forecast issuance.[4]

---

[1] We collect weather data covering all active weather stations in the US (7,610 weather stations) for the period 2003 to 2018 from the dataset provided by the National Oceanic and Atmospheric Administration (NOAA) (http://ftp.ncdc.noaa.gov/pub/data/gsod/). We calculate the geographic distance between each firm and each weather station and identify the five nearest weather stations within a 50-mile radius of each firm's headquarters. For each weather station, we obtain hourly data on (1) temperature, (2) wind speed, (3) dew point, (4) visibility, (5) sea level pressure and (6) sky cover. We then compute a daily index for each of these variables by averaging the hourly data of these variables between 6.00 am and midnight of each day.

[2] Untabulated results show that the correlations between the six weather condition variables and *Flu* range from -0.488 (*Temperature*) to 0.489 (*Sea Level Pressure*).

[3] Computing δ^* requires setting a value for R^2_{max}. Following prior studies (Heimer, Myrseth, and Schoenle [2019]; Oster [2019]), we set R^2_{max} as $1.3 \times R^2$ of controlled effect. An estimate of δ^* greater than 1 suggests that it is unlikely for the coefficient estimate to be confounded by selection on unobservables.

[4] Columns 1 and 2 of Table OA6 show that our findings in Table 3 are not affected when we exclude observations associated with extreme weather conditions. This suggests that our results are not affected by the potential change in the level of employee activities due to extreme weather (instead of flu) (e.g., deHaan et al. [2017]).

TABLE OA10

The Effect of Mood

Panel A. Controlling for firm-level weather-related variables		
	(1)	(2)
Dep. Var. =	$Issue^{Short}$	$Issue^{Long}$
$Flu^{Residual}$	-1.744***	1.211**
	(-3.27)	(2.37)
Temperature	0.001	-0.002*
	(0.64)	(-1.71)
Wind Speed	-0.003	0.002
	(-1.36)	(0.85)
Dew Point	0.002***	-0.000
	(2.79)	(-0.14)
Visibility	0.001	0.003
	(0.25)	(1.12)
Sea Level Pressure	-0.003***	0.001
	(-2.67)	(0.71)
Cloud	0.001	-0.011
	(0.06)	(-1.19)
Other Control Variables	Yes	Yes
Firm FE	Yes	Yes
State × YQ FE	Yes	Yes
Adj. R^2	0.516	0.476
N	78,240	78,240

TABLE OA10 (Cont'd)

Panel B. Forecasts bias

Dep. Var. =	(1) $MFBias^{Short}$	(2) $MFBias^{Long}$	(3) $MFNews^{Short}$	(4) $MFNews^{Long}$
Flu	-0.030	-0.016	0.001	-0.020
	(-1.47)	(-0.27)	(0.09)	(-0.96)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
State × YQ FE	Yes	Yes	Yes	Yes
Adj. R^2	0.231	0.510	0.327	0.203
N	29,587	27,816	28,807	27,176

This table presents analysis on alternative explanations of unconscious reactions. Panel A reports the results after controlling for the six weather condition variables: *Temperature*, *Wind Speed*, *Dew Point*, *Visibility*, *Sea Level Pressure*, and *Cloud*, which are the average of the daily index of each underlying weather condition over the information production window for each firm-quarter, respectively. The daily index for each weather variable is calculated by averaging the hourly data of each weather variable between 6 a.m. and midnight of a day, based on the data from the five nearest weather stations within a 50-mile radius of a firm's headquarters office. $Flu^{Residual}$ is the residual from regressing *Flu* on *Temperature*, *Wind Speed*, *Dew Point*, *Visibility*, *Sea Level Pressure*, and *Cloud*. We include but do not report control variables, firm fixed effects, and headquarters' state × year-quarter fixed effects. The untabulated other control variables are the same as those in Table 3 Panel A. See Appendix A for the detailed definitions of other variables. We estimate the first- and second-step models using OLS, and calculate corrected standard errors using a bootstrapping procedure with 1,000 replications and clusters drawn at the headquarters' state × year-quarter level (Chen, Hribar, and Melessa 2022; Stata code ***bootstep*** obtained from https://github.com/dveenman/bootstep). We define year-quarter as the calendar year-quarter of the EA date ($EA_{i,t}$ in Figure 1). *, **, and *** indicate that the estimated coefficient is statistically significant at the 10%, 5% and 1% levels, respectively.

Panel B reports the results of forecast bias and forecast news. $MFBias^{Short}$ ($MFBias^{Long}$) is the forecast bias of all the short-run (long-run) bundled EPS forecasts, where forecast bias is defined as the difference between the management forecast and corresponding actual value, scaled by the absolute value of the forecast. For the long-run forecasts, we take the average value of forecast biases. $MFNews^{Short}$ ($MFNews^{Long}$) is the forecast news of all the short-run (long-run) bundled EPS forecasts, where forecast news is defined as the difference between the management forecast and analysts' consensus forecast scaled by the absolute value of the management forecast. For the long-run forecasts, we take the average of forecast news. Column 1 includes 29,587 firm-quarters with short-run bundled EPS forecasts. Column 2 includes 27,816 firm-quarters with long-run bundled EPS forecasts. Column 3 includes 28,807 firm-quarters with short-run bundled EPS forecasts and analyst consensus data available. Column 4 includes 27,176 firm-quarters with at least one long-run bundled EPS forecast and analyst consensus data available. The untabulated control variables are the same as those in Table 3 Panel A. Robust *t*-statistics are reported in parentheses and are calculated using standard errors clustered at the headquarters' state × year-quarter level.

References to the Online Appendix

CHEN, W., HRIBAR, P. and MELESSA, S. 'Standard error biases when using generated regressors in accounting research.' Journal of Accounting Research, (2022): Forthcoming.

DEHAAN, E. 'Using and interpreting fixed effects models.' Working paper, University of Washington, 2021. Available at: https://ssrn.com/abstract=3699777.

DEHAAN, E., MADSEN, J., and PIOTROSKI, J. D. 'Do weather-induced moods affect the processing of earnings news?' Journal of Accounting Research, 55 (2017): 509–550.

HEIMER, R. Z., MYRSETH, K. O. R., and SCHOENLE, R. S. 'YOLO: Mortality beliefs and household finance puzzles.' The Journal of Finance, 74 (2019): 2957–2996.

OSTER, E. 'Unobservable selection and coefficient stability: Theory and evidence.' Journal of Business and Economic Statistics, 37 (2019): 187–204.