



HAL
open science

When attention is away, analysts misplay: distraction and analyst forecast performance

T. Bourveau, A. Garel, P. Peter Joos, A. Petit-Romec

► **To cite this version:**

T. Bourveau, A. Garel, P. Peter Joos, A. Petit-Romec. When attention is away, analysts misplay: distraction and analyst forecast performance. *Review of Accounting Studies*, 2024, 29 (1), 10.1007/s11142-022-09733-w . hal-03844012v3

HAL Id: hal-03844012

<https://hal.science/hal-03844012v3>

Submitted on 26 Sep 2024

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

When Attention Is Away, Analysts Misplay: Distraction and Analyst Forecast Performance

This version: July 2020

Thomas Bourveau¹
Columbia University

Alexandre Garel²
Audencia Business School

Peter Joos³
INSEAD

Arthur Petit-Romec⁴
SKEMA Business School, Université Côte d'Azur

Abstract

We construct a measure of analyst-level distraction based on analysts' exposure to exogenous attention-grabbing events affecting firms under coverage. We find that temporarily distracted analysts achieve lower forecast accuracy, revise forecasts less frequently, and publish less informative forecast revisions relative to non-distracted analysts. Further, at the firm level, analyst distraction carries real negative externalities by increasing information asymmetry for stocks that suffer from a larger extent of analyst distraction during a given quarter. Our findings thus augment our understanding of the determinants and effects of analyst effort allocation, and broaden the literature on distraction and information spillover in financial markets.

Keywords: Limited Attention, Distraction, Analyst, Forecasts, Information Environment

JEL: G10, G11, G14, G41

Acknowledgements: We are grateful for the helpful comments from Bart Frijns, Trevor Harris, Alberto Manconi, Alireza Tourani-Rad, and Armin Schwienbacher.

¹ Thomas Bourveau, Columbia University, United States, tb2797@columbia.edu

² Alexandre Garel, Audencia Business School, France, agarel@audencia.com

³ Peter Joos, INSEAD, Singapore, peter.joos@insead.edu, corresponding author.

⁴ Arthur Petit-Romec, SKEMA Business School, Université Côte d'Azur, France, arthur.petitromec@skema.edu

1. INTRODUCTION

Financial analysts are preeminent information intermediaries whose output (e.g., forecasts, recommendations) is central to decision makers in capital markets (e.g., Bradshaw, Ertimur, and O'Brien 2017; Kothari, So, and Verdi 2016; Loh and Stulz 2019). Despite this key role, a vast body of research on analyst behavior concludes that strategic incentives or behavioral biases often preclude analysts from processing information in a rational and unbiased fashion as they produce their output. Recent findings in the behavioral finance and economics literature additionally underline how cognitive constraints such as limited attention affect decision making by economic agents (Falkinger 2008). In the analyst forecast setting, these cognitive constraints follow from the analysts' attention being the scarce resource. Therefore, how analysts allocate their limited attention to process relevant information when forecasting will likely affect the properties of the forecasts. In this paper, we investigate this role of attention allocation in the analyst forecast setting by introducing two innovations to this behavioral literature. First, we identify a specific *mechanism* of attention allocation, namely *cognitive distraction*, and examine its effects on analyst output properties. Second, we study whether the effects of cognitive distraction on analysts' forecast properties affect the covered firms' information environment.

In the first part of the paper, we identify the effect of attention allocation through *cognitive distraction* on analyst output properties. While we cannot observe cognitive distraction directly, we view analyst distraction as stemming from *exogenous* attention-grabbing factors that affect the coverage universe of the analyst. That is, we develop an identification strategy inspired by Kempf, Manconi, and Spalt (2017) who focus on institutional investors and motivated by Barber and Odean (2008) and Kacperczyk, Van Nieuwerburgh, and Veldkamp (2016). The approach uses extreme industry-returns to capture attention-grabbing events for analysts covering stocks in those industries to construct a measure of distraction in analysts' attention to the stocks under their coverage at a given point in time.

Simply put, assume that an analyst covers a universe of stocks across broad industry classifications and one of the stocks, say stock A, belongs to an industry affected by extreme returns, while the other stocks do not. In this case, we conjecture that, if attention is a limited resource for this analyst, she will shift attention away from the stocks in the *unaffected* industries and towards the attention-grabbing stock A. To capture this shift, our empirical approach defines a measure of analyst distraction at the *analyst-firm-quarter* level that for each stock under coverage captures the extent to which the analyst is distracted by attention-grabbing events related to other stocks under coverage in a given quarter.

Our measure of financial analyst distraction offers three advantages. First, it is plausibly exogenous to the economics of the stocks for which the analysts will be considered as distracted, thus complementing strategic factors, like the stock's importance to institutional investors, that have been shown to affect the analyst's effort allocation (Driskill, Kirk, and Trucker 2020; Harford, Jiang, Wang, and Xie 2018). Second, it allows a precise observation of the *timing* of the impact of limited attention on analyst forecast performance that will guide our empirical model. Indeed, limited attention should affect an analyst-firm-quarter forecast precisely during the quarter when the analyst's attention is pulled away rather than during preceding and subsequent quarters. Using the distraction measure, we can assess whether analysts *temporarily* allocate their attention towards those stocks affected by attention-grabbing events at the expense of other stocks in their portfolio. Third, our measure allows us to obtain within-firm-quarter estimates where, for a given quarter, the forecasts of our "treated" analysts distracted away from a particular stock will be benchmarked against those of our "control" analysts following the same stock and that are not distracted, holding all public information constant.

Despite its advantages, our measurement of the distraction variable also comes with empirical challenges. First, analysts often organize coverage by industry, and this works against us being able to define our distraction proxy. However, by using broad industry-returns we aim to overcome this

challenge as analyst coverage universes are not always perfectly aligned with industry classification standards based on SIC or GICS codes.¹ Second, the ability of our proxy to measure distraction could be affected when analysts work in teams that collectively do not suffer from attention constraints and can optimally cover all stocks under coverage at all points in time. However, even if analysts work in teams, the team-leader or senior analyst will need to review the work, sign off on the forecasts, and report and ‘market’ the output to the sales team (Hirshleifer, Levi, Lourie, and Teoh 2019). In doing so, the senior analyst will allocate her attention across the stocks under coverage and potentially resort to more heuristic behavior for those stocks that are not subject to attention-grabbing events.²

We predict that distracted analysts who divert attention away from some stocks in their portfolio at a given point in time will issue less accurate forecasts for these stocks. To test this prediction, we rely on a sample of 1,110,420 forecasts spanning 128 quarters (1985-2015 period). Those forecasts are issued by 11,622 unique analysts and correspond to 58,932 unique end-of-the-year earnings announcements for 8,496 unique U.S. listed firms. Using this sample, we estimate empirical models that include various sets of fixed effects to obtain a within firm/analyst/quarter estimate of the impact of limited attention on analysts’ forecasting characteristics and draw conclusions at the analyst and stock level.

Our first set of results shows that analysts’ limited attention significantly decreases the accuracy of their earnings forecasts. Specifically, the forecast accuracy of distracted analysts, i.e., those analysts whose attention is diverted away from a particular stock in a given quarter, is on average 1.4 percent lower than that of other analysts covering the same stock.³ To put this finding in perspective, this

¹ In our research design, we rely on different broad industry classification schemes, such as the Fama-French 12 and 17 industry classifications or GICS sectors to determine our distraction proxy and find that our results are unaffected by this choice.

² Our results hold if we restrict our sample to analysts that we can identify through their last name on I/B/E/S and who presumably are less likely to be part of a team (Table IA.7).

³ We repeat this test by splitting our sample between positive and negative forecast errors and we find that limited attention has an effect of similar magnitude on both directions (See Table IA.3 in the Internet Appendix). This finding

effect is larger than the average impact of being employed by a top-decile-brokerage firm. We strengthen this initial finding using a cross-sectional test that considers coverage universe size and find, broadly speaking, that analysts responsible for a larger coverage universe temporarily reallocate their effort towards some attention-grabbing stocks at the expense of other stocks in their portfolio.

In a second test, we directly examine the validity of our identification strategy. An important feature of our setting is that we should observe the effect of limited attention only for identified analyst-firm-quarters. Therefore, we examine the timeliness of the analyst distraction effect on analyst forecast properties by extending our baseline model with a one-quarter-lead and lag analyst-firm distraction measures. Our results indicate that only *contemporaneous* distraction affects analyst forecast accuracy negatively, consistent with our main estimation obeying the parallel trend assumption needed for the validity of the empirical research design. The result also underlines the temporary effect of the attention-grabbing event on analyst attention allocation.

Our third test examines if analysts *learn* from distraction experiences building on prior literature showing that the first experience of an unusual event affects agents' decision-making process more than subsequent experiences (e.g., Bourveau and Law 2020; Dessaint and Matray 2017). We find that the effect of analyst distraction on forecast properties manifests itself *only* when the analyst experiences her first attention-grabbing event of this sort. Therefore, analysts appear to learn from their first distraction experience and subsequently maintain a constant level of accuracy across their coverage universe when they experience subsequent distraction inducing circumstances.

We corroborate these three baseline results with two additional findings. First, we examine the impact of limited attention on a different measure of analyst performance, namely analysts' forecast revision frequency (e.g., Jacob, Lys, and Neale 1999; Groyberg, Healy, and Maber 2011; Harford et

rules out an alternative interpretation that our distraction measure captures some form of pessimism or availability heuristic that would have an asymmetric effect on forecast errors (e.g., Bourveau and Law 2020).

al. 2018; Merkley, Michaely, and Pacelli 2020). We find that, on average, distracted analysts revise their forecasts significantly less often relative to non-distracted analysts covering the same stock during the same quarter, consistent with limited attention affecting the allocation of effort by analysts.

Second, we investigate whether distracted analysts produce less informative forecasts relative to non-distracted analysts, building on the rationale that limited attention prevents analysts from gathering and processing the optimal amount of information. From a supply perspective, we observe that distracted analysts are significantly less likely to revise forecasts for non-attention-grabbing stocks when no other analyst has produced forecasts for those stocks. From a demand perspective, we find that the stock market reacts significantly less strongly to forecast revisions issued by distracted analysts, consistent with those forecast revisions being less informative. Overall, the results of this second additional analysis are consistent with limited attention affecting the ability of analysts to gather and process information and release informative opinions to stock market participants.

After documenting the effects of distraction on analysts' effort allocation and forecast properties, the second part of the paper examines whether these effects result in negative externalities for the information environment of the stocks covered by distracted analysts. Given the key role of sell-side analysts in financial markets, a number of prior studies have shown that the intensity of analyst coverage influences firms' information environments.⁴ We therefore examine if the overall informativeness of analyst consensus forecasts for a given stock in a given quarter is affected by forecasts issued by distracted analysts. Consistent with those forecasts being of lower quality, we find that firms covered by more distracted analysts experience larger earnings surprises, suggesting that consensus for these stocks 'left out' more information to process for investors at the earnings announcement (Core, Guay, and Rusticus 2006). Next, building on the link between analyst coverage

⁴ See the discussion in section 6 of the overview paper by Bradshaw et al. (2017). Other related studies are Brennan and Subrahmanyam (1995), Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012), Derrien and Kecskés (2013), and Balakrishnan, Billings, Kelly, and Ljungqvist (2014).

and information asymmetry, we examine the relation between analyst distraction and information asymmetry in financial markets (e.g., Kelly and Ljunqvist 2012). Using Amihud's (2002) measure of illiquidity as our proxy for information asymmetry, we find evidence consistent with an increase in information asymmetry for those stocks that are covered by more distracted analysts during a given quarter. Importantly, this finding is consistent with limited attention by analysts having a real effect on the information environment of stocks.

Our paper makes four contributions to the literature. First, we contribute to the literature on the determinants of analyst forecast accuracy. Since Clement (1999), this large body of academic research has considered factors related to analysts' strategic incentives but also their behavioral biases.⁵ Our paper contributes to the literature on the latter category, i.e., the role of behavioral biases, and in particular to a small but growing literature on how analysts' forecasting behavior is *temporarily* affected by cognitive biases.⁶ We show that limited attention following unexpected attention-grabbing events constitutes a new psychological mechanism that explains analyst forecasting performance.

Our paper closely relates to but is distinct from three recent studies in this literature that examine the role of limited analyst attention. Pisciotta (2018) finds that analysts involved in the underwriting process of an IPO are less accurate when they forecast earnings for other stocks in their portfolio during the underwriting process. Similarly, Driskill et al. (2020) find that when analysts face concurrent earnings announcements across their coverage universe on the same day, they limit their

⁵ The non-behavioral factors considered in the literature include (but are not limited to) the analyst's forecasting experience (e.g., Clement 1999), the coverage portfolio complexity (e.g., Clement 1999), the prestige of the brokerage house (e.g., Clement 1999), the geographical location (e.g., Malloy 2005; O'Brien and Tan 2015), the analyst's industry expertise (Bradley, Gokkaya, and Liu 2017a), the analyst's career concerns (e.g., Hong and Kubik 2003; Harford et al. 2018), the analyst's cultural background (e.g., Du, Yu, and Yu 2017; Merkley et al. 2020), and the changing business model of sell-side research (Drake, Joos, Pacelli, and Twedt 2019).

⁶ Examples of this literature include a focus on attribution bias (Hilary and Menzly 2006), anchoring bias (Cen, Hilary and Wei 2013), seasonal affective disorder (Lo and Wu 2018), weather-induced inactivity (deHaan, Madsen, and Piotroski 2017), availability heuristic (Bourveau and Law 2020), and the affect heuristic (Antoniou, Kumar, and Maligkris 2020). Other academic research has studied the role of characteristics such as the economic conditions when analysts grew up (Clement and Law 2018) and their political ideology (Jiang, Kumar, and Law 2016) in permanently shaping analysts' future forecasting behavior towards conservative forecasts.

attention to firms with rich information environments that present good business cases for the analysts and their brokerages. Finally, Hirshleifer et al. (2019) find that, on days when analysts issue multiple forecasts, increasing decision fatigue over the course of the day leads to a decrease in analysts' forecast accuracy and an increase in reliance on heuristics in the forecasting process. Importantly, these three studies consider settings where analysts can *anticipate* the attention allocation challenge that they will face induced by a changing, increasing workload. As a result, the limited attention findings in these studies follow from analysts voluntarily and strategically choosing to allocate their attention primarily towards stocks that offer immediate potential for reward (Driskill et al. 2020, Hirshleifer et al. 2019).⁷ What sets our study apart is that we examine a setting where analyst *distraction* follows from an *exogenous* attention-grabbing occurrence that analysts cannot anticipate and that is therefore unrelated to the existing strategic ranking of the analyst's portfolio.⁸

Second, we contribute more broadly to the literature on analysts' strategic effort allocation. Hong and Kubik (2003) and more recently Harford et al. (2018), among others, find that analysts *permanently* provide more accurate, frequent, and informative earnings forecast revisions and stock recommendation changes with greater information content for firms deemed to be important for their careers. We complement these findings by shedding light on a mechanism that explains how analysts *temporarily* allocate their effort across stocks in their coverage universe as a function of attention-grabbing events thus hampering forecast properties of non-attention-grabbing stocks.

Third, our paper contributes to the literature on the role of distraction in financial markets. Previous work documents the consequences of investors' distraction on managers' investment choices

⁷ Hirshleifer et al. (2019) explicitly discuss the *non-random* ranking rule that analysts potentially use to allocate effort on days with multiple forecasts and conclude that it is not inconsistent with decision fatigue findings.

⁸ In this regard, our work relates to Dong and Heo (2014) who show evidence consistent with analysts having limited attention when the region where they live experiences flu epidemics, also an exogenous factor. Our set-up is different in that we study the role of attention allocation and limited attention in circumstances that reflect a normal course of work, unaffected by exogenous environmental factors. In particular, only six percent of our analyst-stock-year observations correspond to extreme attention-grabbing events.

(Kempf et al. 2017), disclosure behavior (Abramova, Core, and Sutherland 2019), the scheduling and timing of earnings announcements (deHaan, Shevlin, and Thornock 2015), and earnings management (Garel, Martin-Flores, Petit-Romec, and Scott 2017). We complement these findings by documenting that distraction also affects analysts' forecast accuracy. Importantly, our results provide evidence on a *learning* mechanism in this analyst forecast setting whereby limited attention affects forecast properties only during the analysts' first distraction experience.

Fourth, we contribute to the literature on information spillovers in financial markets. Prior studies document the effect of exogenous economic shocks on externalities in financial markets (e.g., Foucault, Kadan, and Kandel 2013). For example, Dessaint et al. (2018) find that noise (i.e., non-fundamental drops) in the stock price of product-market peers leads firms to sub-optimally decrease their investment. Schneemeier (2018) shows that if managers exhibit both limited ability to filter out noise in prices and limited attention to stock prices, then non-fundamental shocks to a firm's stock price could also affect the investment of fundamentally unrelated firms. We provide evidence supportive of an information spillover effect of exogenous economic shocks through analyst information production. Specifically, a change in attention allocation by analysts away from certain stocks not affected by attention-grabbing return events maps into a lower quality information environment for those stocks.

2. ANALYST DISTRACTION AND ANALYST FORECAST PERFORMANCE

2.1 Measuring Analyst Distraction

Our empirical analysis starts by observing that financial analysts have limited attention, time, and resources. Given their attention constraints, analysts choose how to allocate attention and devote effort to collect and analyze information across the firms in their coverage universe. Some of the attention allocation will be guided by factors such as their involvement in activities of the investment bank division (e.g., IPOs or other securities' deals), or the pattern of information supplied to the

market by their coverage firms. However, in this paper, we investigate a different and additional mechanism of attention allocation, namely cognitive distraction. We introduce the possibility that attention-grabbing events push analysts to shift their attention towards some firms under coverage and away from others, leading to the latter receiving a lower than optimal level of attention. That is, we introduce the possibility that at some points in time and for some firms under coverage, analysts become distracted.

The main variable of interest in our research design is an analyst-firm level measure of distraction, *Analyst Distraction*, that captures how much an analyst following a given firm (f) is distracted in a given quarter. We define the variable such that higher values for a given analyst-firm pair imply that the analyst is more distracted with respect to that firm at that point in time. Specifically, for an analyst (i), following a firm (f) in quarter (q), we define analyst distraction as follows:

$$\text{Analyst Distraction}_{i,f,q} = \sum_{IND \neq IND_f} \omega_{iq}^{IND} \times IS_q^{IND} \quad (1)$$

Here, IND denotes a given Fama-French 12 industry, and IND_f denotes firm f 's Fama-French industry. We define IS_q^{IND} in equation (1) as an indicator variable that equals one if an industry achieves the highest or lowest return across all 12 Fama-French industries in a given quarter. In other words, the variable IS_q^{IND} captures the occurrence of an attention-grabbing event in an industry *other* than IND_f . Motivated by Barber and Odean (2008) and Kempf et al. (2017), we rely on the use of “extreme” industry returns (both positive and negative) to identify attention-grabbing events. In support of our choice of measure, other papers identify extreme return periods as periods when learning about uncertainty can be particularly beneficial, leading analysts to steer their attention towards firms experiencing extreme returns (e.g., Kacperczyk et al. 2016).

ω_{iq}^{IND} in equation (1) captures the importance of the attention-grabbing industries in the coverage universe of the analyst. We measure this variable as the number of firms in the analyst's portfolio belonging to an attention-grabbing industry divided by the total number of firms in the

analyst's coverage universe during quarter q . Intuitively, *Analyst Distraction* is a function of both the occurrence of attention-grabbing shocks in industries other than IND_f and the extent to which the analyst's coverage universe is exposed to these other industries.⁹

Numerically, *Analyst Distraction* lies between 0 percent and 100 percent and a higher number indicates that it is more likely that the analyst shifts attention away from firm f towards the coverage firms in industries experiencing extreme returns. By construction, *Analyst Distraction* is equal to 0 for all firms belonging to the industries experiencing extreme returns at quarter q . To help the interpretation of our findings and to complement our continuous measure of analyst distraction, we also create an indicator variable, labeled *Analyst Distraction Dummy*, which takes the value of one if an analyst is distracted above a certain threshold and zero otherwise. In our main analyses, we choose as our threshold *Analyst Distraction* ≥ 20 percent.¹⁰

An important advantage of our measure of *Analyst Distraction* is that the industry shocks embedded in its computation do not mechanically relate to the fundamentals of the firm of interest since its own industry is excluded. Thus, *Analyst Distraction* is a plausible proxy to identify exogenous shocks to analyst attention. Panels A and B of Table IA.1 in the Internet Appendix report the attention-grabbing industries, their quarterly returns, and the average quarterly returns across the twelve industries for each quarter over our sample period (for top and bottom performers respectively). On average, the quarterly returns in the top performing industry are more than six times larger than the average return across the other eleven industries. This difference is sizeable and arguably large enough to act as an attention-grabbing phenomenon for the analyst.

2.2 Analyst Forecast Properties

⁹ In additional robustness analyses, we also compute a value-weighted measure of analyst distraction. See section 4.

¹⁰ The average analyst in our sample has 13 stocks in portfolio. For a given stock-quarter, a value of our measure of analyst distraction greater or equal to 20 percent for this analyst implies that at least 3 of the 12 other stocks in her portfolio belong to attention-grabbing industries. Our results are qualitatively similar when we use alternative thresholds (e.g., 15 percent or 30 percent).

Our empirical analyses examine the forecast performance of distracted analysts compared to non-distracted analysts. Our main dependent variable of interest is *relative* earnings forecast accuracy, constructed as the proportional mean absolute forecast error developed by Clement (1999) and widely used in previous studies (e.g., Malloy 2005; De Franco and Zhou 2009; Green et al. 2014). Specifically, the proportional mean absolute forecast error ($PMAFE_{i,j,t}$) is the difference between the absolute forecast error ($AFE_{i,j,t}$) of analyst i for firm j in quarter t and the mean absolute forecast error for firm j in quarter t . We scale this difference by the mean absolute forecast error for firm j in quarter t to reduce heteroscedasticity (Clement 1999). Formally, we define AFE_{ijt} and $PMAFE_{i,j,t}$ as follows:

$$AFE_{ijt} = \text{Absolute (Forecast } EPS_{ijt} - \text{Actual } EPS_{ijt}) \quad (2)$$

$$PMAFE_{ijt} = (AFE_{ijt} - MAFE_{jt})/MAFE_{jt} \quad (3)$$

where AFE_{ijt} is the absolute forecast error for analyst i 's forecast of firm j for quarter t , and $MAFE_{jt}$ is the mean absolute forecast error for firm j for quarter t excluding analyst i 's forecast. As defined, lower values of $PMAFE_{i,j,t}$ correspond to more accurate forecasts. One advantage of the measure is that it is comparable across analysts (Clement 1999). The measure captures an analyst's forecast accuracy relative to all analysts covering a given firm, thus controlling for differences across companies, time, and industries (Ke and Yu 2006).¹¹

We complement our baseline analyses by considering two alternative dependent variables. Our first alternative variable is the relative frequency of earnings forecast revisions, building on studies that use this measure to ascertain the level of analyst effort (e.g., Jacob et al. 1999; Groyberg et al. 2011;

¹¹ Comparing the forecast accuracy of analysts using forecast errors expressed as nominal values or as a percentage of the actual values of the earnings is potentially misleading because of differences in scale. Note that the measure does become meaningless when analyst coverage of the firm is equal to 1. Therefore, we exclude from the sample firms covered by fewer than two analysts in a given quarter.

Healy and Palepu 2001; Harford et al. 2018). The second alternative variable is the informativeness of analyst forecast revisions. We discuss the empirical specifications of the alternative tests below.

2.3 Sample Construction

We construct our sample using the historical detailed I/B/E/S one-year-ahead earnings per share forecast file (1985-2015).¹² We follow prior literature and restrict the sample to earnings forecasts with a horizon between one and twelve months (e.g., Clement 1999; Clement, Koonce, and Lopez 2007; Harford et al. 2018).¹³ Next, we aggregate the observations at the analyst-firm-quarter level by retaining the most recent forecast of end-of-fiscal-year earnings for each analyst-firm-quarter. We further restrict our sample to forecasts issued for firms with a non-missing SIC code in the COMPUSTAT database. Finally, we use SIC codes to identify which of the 12 Fama-French industries each firm belongs to. For each industry, we obtain the time-series of monthly returns from Kenneth French's website to derive quarterly industry returns.¹⁴

Starting from this initial sample, we retain observations for which we have non-missing data for all key dependent and independent variables used in our baseline model. Finally, we drop earnings forecasts issued by analysts with less than five observations over the full sample period. We also drop analyst-quarter pairs that cover fewer than two firms and firm-quarter pairs for which less than two analysts issue a forecast. This sample construction process provides us with a baseline sample of 1,110,420 analyst forecasts spanning 128 quarters (1985-2015 time-period). Those forecasts are issued

¹² We use several initial rules to drop observations from the sample: 1) observations for which the variable *cusip* is equal to "00000000" or missing; 2) observations with missing values for the variables *ticker* and *analys*; 3) observations for which the forecast date (*anndats*) is posterior to announcement date of the earnings (*actdats*); 4) observations for which either the value for the forecast (*value*) or the value of the actual earnings (*actual*) are missing.

¹³ Our results are qualitatively unchanged when we do not exclude forecasts with a horizon shorter than 30 days.

¹⁴ We are grateful to Kenneth French for sharing this data on his website.

by 11,622 unique analysts and correspond to 58,932 unique end-of-the-year earnings announcements for 8,496 unique firms listed in U.S. stock exchanges.¹⁵

2.4 Analyst Distraction and Earnings Forecast Accuracy: Baseline Results

Our baseline analysis examines the prediction that forecasts issued by distracted analysts are less accurate than those issued by non-distracted analysts. To formally test this prediction, we use a multivariate OLS regression model with *PMAFE* as dependent variable. The primary variables of interest are *Analyst Distraction* or *Analyst Distraction Dummy*, defined earlier. Standard errors are robust to heteroscedasticity and double-clustered at the firm and analyst levels (Petersen, 2009). Formally, we use the following model:

$$PMAFE_{i,j,t} = \beta_0 + \beta_1(\text{Analyst Distraction}_{i,j,t} \text{ or } \text{Analyst Distraction dummy}_{i,j,t}) + \beta'X_{i,j,t} + \gamma_i \times \theta_t + \varepsilon_{i,j,t} \quad (4)$$

$X_{i,j,t}$ is a set of control variables that include several time-varying analyst characteristics and time-varying analyst-forecast characteristics identified by previous research as important explanatory factors for forecast accuracy (e.g., Mikhail, Walther, and Willis 1997; Clement 1999; Clement and Tse 2003; Clement, Rees, and Swanson 2003; Clement et al. 2007). Appendix A contains the definitions of all included variables. We also include firm-quarter fixed effects ($\gamma_i \times \theta_t$) to capture both unobservable and observable firm-level varying factors that could affect the analyst's forecast accuracy. The inclusion of firm-quarter fixed effects allows examining how, within a group of analysts forecasting earnings for the same firm in the same quarter, variations in analyst distraction relate to variations in forecast accuracy.¹⁶

¹⁵ Within the sample, we winsorize the forecast accuracy, the accounting and the continuous market control variables at the 1st and 99th percentiles.

¹⁶ An alternative approach to control for firm-year fixed effects consists in adjusting variables by their firm-year means (e.g., Clement 1999; Malloy 2005; Clement et al. 2007; Bradley et al. 2017a). Gormley and Matsa (2014) show that a potential concern with de-meaning variables is that it may produce inconsistent estimates and distort the results. They

Table 1 provides summary statistics for our main analyst and forecast variables. Distractions are rare as 6 percent of analyst-firm-quarter observations exhibit distraction levels above 20 percent, i.e., more than 20 percent of their portfolio's firms are affected by attention-grabbing events in unrelated industries. The summary statistics for the analyst and forecast characteristics are in line with prior literature (e.g., Clement et al. 2007; De Franco and Zhou 2009; Bradley et al. 2017b; Harford et al. 2018; Clement and Tse 2005). The median absolute forecast error is 0.09 and the mean frequency of forecast revisions within a quarter is 0.44. The median analyst in our sample has been issuing forecasts for 7.5 years (29 quarters) and covering the typical firm in our sample for about two years (7 quarters). The median number of days between earnings forecasts and the fiscal year end is 196. The median analyst covers 11 firms from two distinct 2-digit SIC code industries at a given quarter. 58 percent of the forecasts are issued by analysts working for a top-decile brokerage house based on the number of analysts employed by each broker.

Table 2 reports the baseline regression results. Models 1 and 5 show estimations of eq. (4) that include control variables and firm-quarter fixed effects. These specifications show a positive relation between analyst distraction and relative forecast error: the coefficients on *Analyst Distraction* in Model 1 and *Analyst Distraction Dummy* in Model 5 are both significantly positive, consistent with earnings forecasts issued by distracted analysts exhibiting larger relative forecast errors than those issued by non-distracted analysts. Economically, the coefficient in Model 5 suggests that distracted analysts issue earnings forecasts that are on average 1.4 percent less accurate. To put this in perspective, this effect is equivalent to the effect of five years (20 quarters) of firm-specific experience and it is greater than the effect of being employed by a top-decile-brokerage house.

suggest using the raw value of the variables and controlling for fixed effects. In robustness tests, we check that our results hold if we adjust variables by their firm-year means instead of controlling for firm-quarter effects.

Next, we augment our baseline specification with analyst fixed effects (Models 2 and 6), or analyst-quarter fixed effects (Models 3 and 7). Across these specifications, the magnitude of the coefficients on the distraction variables becomes lower, but the coefficients remain significantly positive. In other words, even after controlling for analyst or analyst-quarter fixed effects, earnings forecasts issued by distracted analysts are less accurate compared to those issued by non-distracted analysts. Hence, persistent or time-varying heterogeneity across analysts cannot explain the effect of analyst distraction on relative forecast accuracy. In Models 4 and 8, we augment the baseline specification with brokerage-house fixed effects since Cowen et al. (2006) find that analysts' forecast optimism varies across brokerage firms. Our findings remain unchanged, consistent with differences across brokerage houses not driving the observed effect of analyst distraction on earnings forecast accuracy.

We also observe that the coefficients on the control variables in eq. (4) obtain their expected signs in line with prior literature (e.g., Clement 1999; Malloy 2005; Clement et al. 2007; Bradley et al. 2017a). Longer forecast horizons map into larger forecast errors, while analyst experience, both general and firm-specific, results in more accurate forecasts. Analysts employed by top decile brokerage houses forecast more accurately, consistent with the view that these analysts have more resources available to them. Finally, analysts that cover more firms and different industries produce less accurate forecasts.

2.5 Analyst Distraction and Earnings Forecast Accuracy: Additional Analyses

To sharpen our baseline inferences, we carry out three additional analyses. In a first additional analysis, we examine whether the attention constraints are more binding and the effect of analyst distraction on forecast accuracy larger when analysts cover larger universes. Intuitively, when an analyst covers a greater number of firms, her attention will be allocated to more firms and therefore attention to each stock under coverage becomes potentially more sensitive to attention-grabbing

shocks to other stocks. Put differently, the attention constraints become more binding and we expect the effect of analyst distraction on forecast accuracy to be more pronounced for analysts covering a greater number of firms.¹⁷

We test this prediction by dividing our sample into two groups based on an analyst's portfolio size median value (eleven stocks) and by estimating our baseline regression in each subgroup. The results of this analysis in Columns 1 and 2 of Table 3 show that the positive and significant association between *Analyst Distraction* and relative forecast error is limited to the group of analysts with above-median portfolio size. We find no significant association between *Analyst Distraction* and relative forecast error in the below-median group and a Wald test of coefficient equality shows that the difference between coefficients is statistically significant. The analyses in Columns 3 and 4 using *Analyst Distraction Dummy* find the same result.

Our second additional analysis zooms in on the timing of the distraction event. By construction, our measure of analyst distraction enables us to identify the quarter during which analysts become distracted and shift their attention across firms under coverage.¹⁸ The effects of analyst distraction should therefore be limited to the quarter during which extreme industry returns affect some of the analyst's portfolio firms. To explore this, we augment our baseline regression by including the first lead and lag of analyst distraction as explanatory variables. The results in Table 4 show that *only* the contemporaneous analyst distraction variables obtain positive and significant coefficients in the specifications while the coefficients on leading and lagging analyst distraction are

¹⁷ As an illustration, consider an analyst covering two firms A and B in a given quarter. If one of these two firms (A for example) is affected by an attention-grabbing shock during the quarter, by construction analyst distraction is equal to 50 percent for the forecasts issued for B by this analyst. Intuitively, in the case of an analyst covering two (or a low number of) firms, the attention-grabbing stock(s) will shift attention towards A but the analyst will likely still be able to dedicate enough time and resources to B.

¹⁸ Table IA. 1 shows that an industry experiences extreme returns over two consecutive quarters (quarter q and $q+1$) only 10 percent of the time, and over three consecutive quarters (quarter q , $q+1$ and $q+2$) only 1 percent of the time. We thus expect the distraction shocks to vary importantly from one quarter to the other and affect the analyst information production in a specific quarter in a timely fashion.

neither statistically nor economically associated with forecast accuracy. In other words, these findings provide strong support for our identification strategy of the distraction effect.

Our third additional analysis explores the effect of learning by analysts by examining whether the effect of analyst distraction on forecast accuracy is more pronounced the first time an analyst is distracted. Our descriptive statistics in Table 1 indicate that attention-grabbing shocks (extreme returns) affecting a significant fraction of an analyst's portfolio are relatively rare events. We therefore test whether our findings of lower forecast accuracy in the baseline tests disappear or become less pronounced when an analyst experiences a repeated distraction event. To implement this test, we create an indicator variable that equals one if the distraction event is the *first* significant distraction event experienced by a particular analyst-firm pair during our sample period (i.e., *Analyst Distraction* greater or equal to 20 percent).¹⁹

Table 5 reports our results. As a benchmark Model 1 in the table repeats the earlier result from Model 4 in Table 2. When Model 2 decomposes *Analyst Distraction Dummy* into two components (*First Distraction Event* and *Not-first Distraction Event* depending on whether the analyst-firm pair experiences distraction for the first time or not), the results show that distraction *only* affects the analyst's forecasts the first time she experiences distraction for a given stock. When analysts are distracted a second time (or more), their forecasts do not appear to be affected, all else equal. When we include analyst fixed effects in Model 3, the coefficient on *First Distraction Event* attenuates but remains significant and positive, while the coefficient on *Not-first Distraction Event* remains insignificantly different from zero.

These results in Table 5 are consistent with findings from other studies showing that the relative saliency of (extreme) events determines the strength of their effect on decision making by

¹⁹ Some analysts have been working before the start of our sample period, which might create a bias against finding an effect of first distraction event. 5.36 percent of the total analysts in our sample were already active in 1985, the first year of our sample period. We find similar results if we exclude these analysts for this test.

economic agents. For example, Dessaint and Matray (2017) study managers' reaction to salient risks and find that managers of firms unaffected by the event of a hurricane in their proximity react by substantially increasing corporate cash holdings. However, this reaction is temporary and less pronounced when the event is repeated. Similarly, our findings in Table 5 show that a *repetition* of attention-grabbing events is seemingly less salient and does not affect forecast accuracy.

2.6 Analyst Distraction and Other Outcomes: Frequency and Informativeness of Analyst Forecast Revisions

2.6.1 Frequency of Analyst Forecast Revisions

As discussed, we complement our focus on earnings forecast accuracy with a test that adopts analyst forecast revision frequency as the variable of interest. We explore whether analysts allocate less effort, i.e., revise forecasts less often, to firms that do not belong to attention-grabbing industries. We test this relation by estimating the multivariate OLS regression model in eq. (4) using the relative frequency of earnings forecast updates as dependent variable. We measure this frequency as the difference between the number of forecasts made by analyst i for a firm j during quarter t with a minimum forecast horizon of 30 days and the average number of forecasts issued by all analysts for firm j at quarter t , scaled by the average number of forecasts.

Table 6 reports the results of this estimation and finds that regardless of whether we use *Analyst Distraction* in Model 1 or *Analyst Distraction Dummy* in Model 2, the coefficient on the distraction variable is negative and statistically significant, consistent with distracted analysts updating their earnings forecasts less frequently than non-distracted analysts covering the same firm in the same quarter. The coefficient in Model 2 shows that distracted analysts update their forecasts five percent

less often than non-distracted analysts. To put this magnitude in perspective, the effect is equivalent to a decrease in the analyst's coverage portfolio size by about 9 firms.²⁰

2.6.2 Informativeness of Analyst Forecast Revisions

Thus far, our findings for forecast accuracy and revision frequency are consistent with distraction having a negative effect on analyst forecast properties. However, since distracted analysts do produce forecast revisions, we next investigate whether the market perceives the informativeness of these revisions differently from forecast revisions produced by non-distracted analysts. The rationale behind the analysis is our intuition that limited attention prevents analysts from gathering and processing the optimal amount of information, consistent with their observed relative lower forecast accuracy. We therefore first examine the likelihood that a distracted analyst will produce a forecast revision in the absence of other covering analyst issuing new forecasts. Next, we gauge the market reaction to forecasts provided by distracted and non-distracted analysts.

To carry out the first step, we create an indicator variable, *Self-Revision*, that takes the value of one if analyst i updates her forecast for a given firm in the absence of other analysts issuing forecasts since analyst i 's previous forecast. Our intuition is that when an analyst revises a forecast without waiting for other analysts to produce information (in the form of forecasts), this reflects her stock-specific effort of gathering and processing of information. The results in Panel A of Table 7 show that distracted analysts are significantly less likely to revise forecasts for non-attention-grabbing stocks when no other analyst has produced forecasts for those stocks. This finding is consistent with limited attention leading distracted analysts to temporarily allocate more effort to attention-grabbing stocks and therefore generating fewer new forecasts for non-attention-grabbing stocks relative to non-distracted analysts.

²⁰ Similarly, in an unreported test, we find that the probability of revising at least once a forecast is significantly lower for distracted analysts.

To carry out the second step, we build on prior literature that adopts the market reaction to forecast revisions as a proxy for their informativeness (e.g., Loh and Stulz 2011; Green et al. 2014). We expect to observe a less pronounced market reaction to forecast revisions issued by distracted analysts if the market perceives these forecasts to be less informative than the forecasts produced by non-distracted analysts. To examine this prediction, we estimate a regression model, similar to the one used by Harford et al. (2018) and Bradley et al. (2017a):

$$\begin{aligned}
 \text{Absolute } CAR_{i,j,t} = & \beta_0 + \beta_1(\text{Analyst Distraction}_{i,j,t} \times \text{Absolute Forecast Revision}_{i,j,t}) + \\
 & \beta_2(\text{Absolute Forecast Revision}_{i,j,t}) + \beta_3(\text{Analyst Distraction}_{i,j,t}) + \beta'X_{i,j,t} + \gamma_i \times \theta_t + \\
 & \varepsilon_{i,j,t} \quad (5)
 \end{aligned}$$

The dependent variable in eq. (5) is the absolute value of the cumulative CRSP VW-Index adjusted abnormal return over the three-day event window [-1;1], centered around the day of the analyst's forecast revision. As an alternative dependent variable, we also use the cumulative abnormal return in excess of the CAPM market model over the same three-day event window [-1;1].²¹ We also define *Absolute Forecast Revision* as the absolute value of the difference between the new forecast and the old forecast, scaled by the absolute value of the old forecast (e.g., Ivković and Jegadeesh 2004).²² We focus on the absolute value of the revision since we formulate no expectation about the market reaction in function of the direction of the revision (Gleason and Lee 2003). Our primary variable of interest in eq. (5) is the interaction term of the absolute value of the forecast revision (*Absolute Forecast Revision*) with *Analyst Distraction*. All regressions also include firm-quarter fixed effects. Standard errors are robust to heteroscedasticity and double-clustered at the firm and analyst levels.

²¹ We drop observations for which there are several forecast revisions within the same day because in this case it is unclear to which forecast the market reacts. We also exclude absolute cumulative abnormal returns greater than 5 percent.

²² As in Ivković and Jegadeesh (2004), we set the denominator equal to 0.01 if the absolute value of the previous forecast is smaller. We also multiply values by 100 and truncate observations between 50 percent and -50 percent. Our results are robust to deflating the forecast revision by stock price instead.

Panel B of Table 7 presents the results. Using our two market reaction measures, Models 1 and 2 both show a positive and significant coefficient on *Absolute Forecast Revision*, consistent with larger absolute forecast revisions triggering greater stock price reactions. Importantly, both models also show that the coefficients on the interaction term *Absolute Forecast Revision* \times *Analyst Distraction* are significantly negative. Therefore, conditional on the magnitude of the forecast revisions, the stock market reaction is significantly weaker for forecast revisions issued by distracted analysts. Using the estimates in Model 1 and setting all variables to their mean value, we observe that an increase in analyst distraction of one standard deviation is associated with a decrease in the market reaction to forecast revisions of about 35 percent (from 0.20 to 0.13). Models 3 and 4 additionally include analyst fixed effects, while Models 5 and 6 further control for day-of-the-week fixed effects (e.g., Dellavigna and Pollet 2009). Our result that the market perceives forecast revisions issued by distracted analysts to be less informative holds across all specifications.

Taken together, the evidence in Table 7 suggests that distracted analysts issue fewer forecasts revisions than when they are not distracted and the market perceives these forecast revisions to be less informative. Overall, these findings are consistent with limited attention reducing the ability of distracted analysts to gather and process information and producing timely informative forecast revisions to the market.

3. THE REAL EFFECTS OF ANALYST DISTRACTION ON FIRMS' INFORMATION ENVIRONMENT

The results from section 2 provide evidence on how cognitive distraction is a mechanism of attention allocation that affects analysts' outputs negatively by leading distracted analysts to issue less accurate, less frequent, and less informative earnings forecasts. In this section, we explore whether these effects of limited attention on analyst forecast properties also lead to real consequences for the information environment of covered firms.

3.1 Measuring Analyst Distraction at the Firm Level

To assess the real effects of analyst distraction on the information environment of covered firms, we create a firm-level measure of analyst distraction to capture the degree of distraction by the firm's covering analysts at a given point in time. In other words, after considering distraction at the analyst-firm level in section 2, we now focus on firm-level variables of analyst distraction, defined as follows:

$$Avg. Analyst Distraction_{f,q} = \frac{1}{N_{f,q}} \sum_{i=1}^{N_{f,q}} Analyst Distraction_{i,f,q} \quad (6)$$

$N_{f,q}$ is the total number of analysts following firm f at quarter q and $Analyst Distraction_{i,f,q}$ is the level of distraction of analyst i for firm f at quarter q as defined in section 2.1. Our measure of analyst distraction at the firm-level is thus the average distraction level of the analysts following the firm during a given quarter. Similar to *Avg. Analyst Distraction*, we also compute firm-level averages of the other analyst characteristics used in section 2 and create the following variables: *Avg. General Experience*, *Avg. Firm Experience*, *Avg. Portfolio Size*, *Avg. Number of Different Industries*, and *Avg. Top 10 Brokerage House*.

3.2 Measuring the Firm's Information Environment

To examine the effect of analyst distraction on the firm's information environment, we follow previous literature and define two firm-level information asymmetry measures, namely absolute earnings surprises and Amihud's (2002) illiquidity measure (e.g., Harford et al. 2018; Bradley et al. 2017a). To measure the former, we use quarterly earnings forecasts and compute earnings surprise as I/B/E/S actual earnings per share minus the last mean analyst consensus forecast before the earnings-

announcement date, scaled by the stock price at the beginning of the fiscal quarter.²³ We adopt the absolute value of the earnings surprise in our main specification as our focus is on the magnitude of the surprise rather than its direction. In additional tests, we also repeat the analysis separately for positive and negative earnings surprises. Our second dependent variable is Amihud's (2002) illiquidity measure, computed as the natural logarithm of one plus the average daily ratio of absolute stock return to dollar volume over the last 250 trading days multiplied by 1,000,000. We exclude firms with a stock price inferior to 5\$ (Amihud 2002).

3.3 Results

We examine the relation between average analyst distraction and absolute earnings surprise using the following multivariate OLS regression model:

$$\text{Absolute Earnings Surprise}_{j,t} = \beta_0 + \beta_1 \text{Avg. Analyst Distraction}_{j,t} + \beta'Z_{j,t} + \theta_t + \gamma_j + \epsilon_{j,t} \quad (7)$$

The main variable of interest in eq. (7) is *Avg. Analyst Distraction*, defined earlier. $Z_{j,t}$ is a set of control variables that includes the average of the analyst characteristics used in the analyst-firm level tests in section II (i.e., *Average General Experience*, *Average Firm Experience*, *Average Portfolio Size*, *Average Number of Different Industries*, *Average Top 10 Brokerage House*, and *Consensus Forecast Horizon*) as well as additional control variables that capture time-varying influences on earnings surprise (e.g., analyst coverage, size, market-to-book ratio, book leverage, profitability, institutional ownership, and trading volume). Appendix A provides detailed definitions of all variables. Finally, we control for firm and time fixed effects in all regressions. Standard errors are robust to heteroscedasticity and clustered at the firm level.

²³ Our results are robust to alternative definitions of earnings surprises such as the difference between the actual earnings per share and the average of all analysts' latest forecast made within [-180, -4] day window prior to the earnings announcement date, rounded to the nearest cent used in Caskey and Ozel (2017).

We report summary statistics for the firm-level sample over the period 1985-2015 used in our empirical analysis in Appendix B, Panel A.²⁴ We observe that both *Earnings Surprise* and *Absolute Earnings Surprise* exhibit a large variation across the sample. Further, the descriptive statistics on *Avg. Analyst Distraction* show that, consistent with section II, analyst distraction is a selective phenomenon with fewer than half of the firms in the sample experiencing distraction.

Table 8, Panel A reports the results of estimating several specifications of eq. (7). Models 1 through 5 focus on absolute earnings surprises and show that *Avg. Analyst Distraction* obtains a positive and significant coefficient across all specifications. In other words, analyst distraction maps into higher absolute earnings surprise.²⁵ Further, across specifications $\ln(\textit{Analyst Coverage})$ obtains a negative and significant coefficient, consistent with findings in prior studies that analysts play an important role in improving a firm's information environment (e.g., Bradshaw et al. 2017). Together, this pattern of coefficients suggests that distraction diminishes the effect that the extent of analyst coverage has on earnings surprises. This result does not change when we control, respectively, for the average analyst characteristics at the firm level and for different firm characteristics, or when we insert firm-year fixed effects.

In Models 6 and 7, we separately regress positive and negative earnings surprises on the variables of interest. These specifications show that the coefficients on *Avg. Analyst Distraction* and $\ln(\textit{Analyst Coverage})$ remain significant as before, although they switch signs when negative earnings surprises is the dependent variable in Model 7. Overall, the findings in both models show that, regardless of the sign of the earnings surprise, average firm-level analyst distraction maps into higher earnings surprises.

²⁴ We drop observations for firms with SIC codes 49 and 60-69. Our results remain qualitatively the same if we keep these observations.

²⁵ Our results hold when we control for lagged average analyst distraction over the past quarter or the past two quarters. The coefficients on the lagged variables are not significant, which further indicates that our effect precisely coincides with the distraction of the analysts covering a given stock.

Next, we examine the relation between analyst distraction and Amihud's (2002) illiquidity measure. We conjecture that firms that exhibit higher firm-level distraction will have a higher Amihud's (2002) illiquidity measure, indicative of more information asymmetry. To test this prediction, we estimate the following multivariate OLS regression model:

$$Amihud\ Illiquidity_{j,t} = \beta_0 + \beta_1 Avg.\ Analyst\ Distraction_{j,t} + \beta' Z_{j,t} + \theta_t + \gamma_j + \epsilon_{j,t} \quad (8)$$

The main variable of interest in eq. (8) is again *Avg. Analyst Distraction*. We include several control variables to capture firm and stock characteristics that potentially influence the Amihud Illiquidity measure and we also include firm and time fixed effects in all regressions. Standard errors are robust to heteroscedasticity and clustered at the firm level. Appendix A provides detailed definitions of all variables. Appendix B, Panel B, shows the summary statistics for the main variables in this analysis. Our focus on Amihud's measure restricts the sample to 45,043 firm-quarter observations for this empirical analysis.

Table 8, Panel B reports the results of estimating different specifications of eq. (8). Model 1 presents a baseline specification while Model 2 augments this specification by adding analyst and firm-level characteristics used in earlier tests. Across both specifications, *Avg. Analyst Distraction* obtains a positive and significant coefficient, consistent with higher analyst distraction for a stock in a given quarter mapping into greater information asymmetry. As in Panel A, both specifications also show a negative and significant coefficient on $\text{Ln}(\textit{Analyst Coverage})$. Therefore, while firms covered by a larger number of analysts enjoy higher stock market liquidity, higher average firm-level analyst distraction moderates this effect.

Overall, these results complement our findings at the analyst-firm level in section II by showing that average *firm-level* analyst distraction affects the firm's information environment. Firms that exhibit higher analyst distraction experience larger earnings surprises and worse stock market

liquidity, consistent with a larger presence of distracted analysts being associated with increased information asymmetry surrounding the firm in the market. Importantly, since our results hold when we control for the extent of analyst coverage of the firm, our findings suggest that not only the number of analysts following the firm influences a firm's information environment, but also their level of attention to the firm at a given point in time.

4. ROBUSTNESS ANALYSES

We estimate a battery of robustness checks to validate and strengthen the conclusions we draw from our results. All these tests are reported in the Internet Appendix. In a first placebo test, we evaluate the validity of our empirical strategy to identify analyst distraction. Our strategy centers on the analysts' exposure to attention-grabbing shocks affecting certain industries across their coverage portfolio and thus the way we define attention-grabbing shocks is key. Motivated by Barber and Odean (2008) and Kempf et al. (2017), we rely on the use of "extreme" industry returns (both positive and negative) to identify attention-grabbing events. To validate our approach, we run a placebo test where we randomly select the attention-grabbing industries and re-estimate our core regressions at the analyst-firm level (Table 2, Model 1) and at the firm level (Table 9, Panel B, Model 4). We repeat the process 5,000 times. The distributions of the coefficients estimated with these placebo tests are reported in Figure IA.1 and IA.2. As one can see, the coefficient on *Analyst Distraction* in our analyst-level analysis and the coefficient on *Avg. Analyst Distraction* in our firm-level analysis both lie well to the right of the distributions of placebo coefficients giving us confidence that both results are not the product of randomness and rather are due to our ability to identify attention-grabbing industries.

Next, we assess the robustness of our main findings at the analyst-firm and firm levels to four alternative measures of analyst distraction. First, we examine whether our results are sensitive to the sign of the extreme returns and find that our results hold for measures of investor distraction based on positive extreme returns only (*Analyst Distraction Top Only*) and negative extreme returns only

(*Analyst Distraction Bot Only*). Second, we create an alternative value-weighted measure of analyst distraction to incorporate the career-concern of analyst (*Analyst Distraction VW*) based on Harford et al. (2018). These authors argue that analysts strategically allocate effort among portfolio firms by devoting more effort to firms that are relatively more important for their career and use market capitalization among other variables to capture the importance of the firm for an analyst's career. When we repeat our analysis using a measure of investor distraction weighted by market capitalization, we document qualitatively similar results (See Panel B of Table IA.2).

Third, we construct a measure of analyst distraction that is weighted by the inverse of the probability for an industry to experience extreme returns (*Analyst Distraction IERPW*) to address the concern that some industries are more subject to extreme returns. Extreme negative or positive returns in an industry that is used to experience stable returns (such as the manufacturing industry) are arguably more likely to divert an analyst's attention than in an industry used to extreme returns (such as the energy industry). Our results hold when we use an industry-probability-adjusted measure of distraction. Fourth, our main measure of investor distraction relies on the Fama-French twelve industries classification. To verify the robustness to this classification scheme, we re-estimate our results using the Fama-French 17 industry classification and the GICS sector classifications and find that our results hold in both cases (Table IA.2).²⁶

Further analyses show that our results hold when we differentiate between positive and negative forecast errors (Table IA.3), when we use the average forecast of an analyst-stock within a quarter rather than the latest issued forecasts (Table IA.4), when we implement different clustering of the standard errors (Table IA.5), when we demean the variables instead of including stock-quarter

²⁶ Prior research suggests that the Global Industry Classification Standard (GICS) provides the most accurate representation of how many brokerage houses organize their analyst teams (e.g., Bhojraj, Lee, and Oler 2003; Boni and Womack 2006; Kadan, Madureira, Wong, and Zach 2012).

fixed effects (Table IA.6), and when we restrict the sample to analysts with identifiable last names (Table IA.7).

Next, we seek to address the concern that the likelihood that an analyst is allocated to firms in industries that are prone to experience extreme returns, and therefore the likelihood that she experiences attention-grabbing shocks is not random. To mitigate this concern, we restrict the sample to analysts covering at least one firm belonging to industries with a higher propensity to experience extreme returns (e.g., consumer durables, business equipment, energy, utilities, health, and telecom). In Table IA.8, we find that analyst distraction is positively associated with relative forecast errors when analysts cover at least one firm belonging to industries with a higher propensity to experience extreme returns (e.g., consumer durables, business equipment, energy, utilities, health, and telecom).

Another potential source of concern is that our empirical estimates actually capture an effect primarily driven by a change in outputs for firms in shocked industries. To alleviate this concern, we repeat our main analyses both at the analyst-level (Table IA.9, Panel A) and the firm-level (Table IA.9, Panel B) and exclude from our sample all the firms that belong to the industries with extreme positive and negative returns. Our results for relative forecast error, absolute earnings surprise, and liquidity hold across all specifications.

In a final robustness test, we follow Kempf et al. (2017) and build a firm-level measure of institutional investors' distraction. We add this measure as a covariate in our firm-level main specifications (Table IA.10) to ensure that our results are not driven by a strong correlation between investors and analysts' distraction. Our results on absolute earnings surprise and liquidity remain economically and statistically similar.

5. CONCLUSION

In this paper, we identify a new psychological mechanism whereby unexpected *exogenous* attention-grabbing events affect the attention allocation of analysts. Specifically, we measure cognitive *distraction* at the *analyst-firm-quarter* level and establish two sets of results. Using our measure at the analyst level, we find that distracted analysts achieve lower forecast accuracy, revise forecasts less frequently, and publish less informative forecast revisions relative to non-distracted analysts. Adding to a long literature that shows how, in addition to strategic incentives, behavioral biases affect analyst forecast performance, our findings emphasize not only how cognitive biases can *temporarily* affect analysts' forecasting behavior and performance, but also that analysts *learn* from their distraction experience.

Next, at the firm level, we find that firm-level analyst distraction carries real negative externalities for the firm's information environment, in the form of increased information asymmetry. Importantly, these firm-level findings show that *involuntary* analyst distraction has real effects on the information environment of covered firms, thus underscoring that the cognitive processes of market participants play an important role in determining the well-functioning of capital markets.

References

- Abramova, I., J.E. Core, and A. Sutherland. 2019 Institutional investor attention and firm disclosure. *The Accounting Review*, forthcoming.
- Amihud, Y. 2002. Illiquidity and stock returns: cross-section and time-series effects. *Journal of Financial Markets* 5: 31-56.
- Antoniou, C., A. Kumar, and A. Maligkris. 2020 Terrorist attacks, analyst sentiment, and earnings forecasts. *Management Science*, forthcoming.
- Balakrishnan, K., M.B. Billings, B. Kelly, and A. Ljungqvist. 2014. Shaping liquidity: On the causal effects of voluntary disclosure. *The Journal of Finance* 69: 2237-2278.
- Barber, B., and T. Odean. 2008. All that glitters: The effect of attention and news on the buying behavior of individual and institutional investors. *Review of Financial Studies* 21: 785-818.
- Bhojraj, S., C. Lee, and D. Oler. 2003. What's my line? A comparison of industry classification schemes for capital markets research. *Journal of Accounting Research* 41: 745-774.
- Boni, L., and K. Womack. 2006. Analysts, Industries, and Price Momentum. *Journal of Financial and Quantitative Analysis* 41: 85-109.
- Bourveau, T., and K. Law. 2020. Do Disruptive Life Events Affect How Analysts Assess Risk? Evidence from Deadly Hurricanes. *The Accounting Review*, forthcoming.
- Bradley, D., S. Gokkaya, and X. Liu. 2017a. Before an analyst becomes an analyst: Does industry experience matter? *The Journal of Finance* 72: 751-792.
- Bradley, D., S. Gokkaya, X. Liu, and F. Xie. 2017b. Are all analysts created equal? Industry expertise and monitoring effectiveness of financial analysts. *Journal of Accounting and Economics* 63: 179-206.
- Bradshaw, M. T., Y. Ertimur, and P. O'Brien. 2017. Financial analysts and their contribution to well-functioning capital markets. *Foundations and Trends in Accounting* 11: 119-191
- Brennan, M. J., and A. Subrahmanyam. 1995. Investment analysis and price formation in securities markets. *Journal of Financial Economics* 38: 361-381.
- Caskey, J., and N. B. Ozel. 2017. Earnings expectations and employee safety. *Journal of Accounting and Economics* 63: 121-141.
- Cen, L., G. Hilary, and K.C. Wei. 2013. The role of anchoring bias in the equity market: Evidence from analysts' earnings forecasts and stock returns. *Journal of Financial and Quantitative Analysis* 48: 47-76.
- Clement, M. B. 1999. Analyst forecast accuracy: Do ability, resources, and portfolio complexity matter? *Journal of Accounting and Economics* 27: 285-303.

- Clement, M. B., L. Koonce, and T. J. Lopez. 2007. The roles of task-specific forecasting experience and innate ability in understanding analyst forecasting performance. *Journal of Accounting and Economics* 44: 378-398.
- Clement, M. B., and K. Law. 2018. *Labor market dynamics and analyst ability*. Unpublished Working Paper. University of Texas, Austin.
- Clement, M. B., L. Rees, and E. P. Swanson. 2003. The influence of culture and corporate governance on the characteristics that distinguish superior analysts. *Journal of Accounting, Auditing, and Finance* 18: 593-618.
- Clement, M. B., and S. Y. Tse. 2003. Do investors respond to analysts' forecast revisions as if forecast accuracy is all that matters?. *The Accounting Review* 78: 227-249.
- Clement, M. B., and S. Y. Tse. 2005. Financial analyst characteristics and herding behavior in forecasting. *The Journal of Finance* 60: 307-341.
- Core, J. E., W. R. Guay, and T. O. Rusticus. 2006. Does weak governance cause weak stock returns? An examination of firm operating performance and investors' expectations. *The Journal of Finance* 61: 655-687.
- Cowen, A., B. Groyberg, and P. Healy. 2006. Which types of analyst firms are more optimistic? *Journal of Accounting and Economics* 41: 119-146.
- De Franco, G., and Y. Zhou. 2009. The performance of analysts with a CFA® designation: The role of human-capital and signaling theories. *The Accounting Review* 84: 383-404.
- deHaan, E., J. Madsen, and J. D. Piotroski. 2017. Do weather-induced moods affect the processing of earnings news? *Journal of Accounting Research* 55: 509-550.
- deHaan, E., T. Shevlin, and J. Thornock. 2015. Market (in) attention and the strategic scheduling and timing of earnings announcements. *Journal of Accounting and Economics* 60: 36-55.
- DellaVigna, S., and J. M. Pollet. 2009. Investor inattention and Friday earnings announcements. *The Journal of Finance* 64: 709-749.
- Derrien, F., and A. Kecskés. 2013. The real effects of financial shocks: Evidence from exogenous changes in analyst coverage. *The Journal of Finance* 68: 1407-1440.
- Dessaint, O., and A. Matray. 2017. Do managers overreact to salient risks? Evidence from hurricane strikes. *Journal of Financial Economics* 126: 97-121.
- Dessaint, O., T. Foucault, L. Frésard, and A. Matray. 2018. Noisy stock prices and corporate investment. *The Review of Financial Studies* 32: 2625-2672.
- Dong, G.N., and Y. Heo. 2014. *Flu epidemic, limited attention and analyst forecast behavior*. Unpublished Working Paper, Columbia University.

- Drake, M., P. Joos, J. Pacelli, and B. Twedt. 2019. Analyst forecast bundling. *Management Science*, forthcoming.
- Driskill, M., M. Kirk, and J. W. Tucker. 2020. Concurrent earnings announcements and analysts' information production. *The Accounting Review* 95: 165-189.
- Du, Q., F. Yu, and X. Yu. 2017. Cultural proximity and the processing of financial information. *Journal of Financial and Quantitative Analysis* 52: 2703-2726.
- Falkinger, J. 2008. Limited attention as a scarce resource in information-rich economies. *The Economic Journal* 118: 1596-1620.
- Foucault, T., O. Kadan, and E. Kandel. 2013. Liquidity cycles and make/take fees in electronic markets. *The Journal of Finance* 68: 299-341.
- Garel, A., J. Martin-Flores, A. Petit-Romec, and A. Scott. 2017. *Distracted investors and earnings management*. Unpublished Working Paper, Auckland University of Technology.
- Gleason, C. A., and C.M. Lee. 2003. Analyst forecast revisions and market price discovery. *The Accounting Review* 78: 193-225.
- Gormley, T. A., and D.A. Matsa. 2014. Common errors: How to (and Not to) control for unobserved heterogeneity. *Review of Financial Studies* 27: 646-651.
- Green, T. C., R. Jame, S. Markov, and M. Subasi. 2014. Access to management and the informativeness of analyst research. *Journal of Financial Economics* 114: 239-255.
- Groysberg, B., P. M. Healy, and D.A. Maber. 2011. What drives sell-side analyst compensation at high-status investment banks? *Journal of Accounting Research* 49: 969-1000.
- Harford, J., F. Jiang, R. Wang, and F. Xie. 2018. Analyst career concerns, effort allocation, and firms' information environment. *The Review of Financial Studies* 32: 2179-2224.
- Healy, P. M., and K.G. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31: 405-440.
- Hilary, G., and L. Menzly. 2006. Does past success lead analysts to become overconfident? *Management Science* 52: 489-500.
- Hirshleifer, D., Y. Levi, B. Lourie, and S. H. Teoh. 2019. Decision fatigue and heuristic analyst forecasts. *Journal of Financial Economics* 133: 83-98.
- Hong, H., and M. Kacperczyk. 2010. Competition and bias. *The Quarterly Journal of Economics* 125: 1683-1725.
- Hong, H., and J.D. Kubik. 2003. Analyzing the analysts: Career concerns and biased earnings forecasts. *The Journal of Finance* 58: 313-351.

- Ivković, Z., and N. Jegadeesh. 2004. The timing and value of forecast and recommendation revisions. *Journal of Financial Economics* 73: 433-463.
- Jacob, J., T.Z. Lys, and M. A. Neale. 1999. Expertise in forecasting performance of security analysts. *Journal of Accounting and Economics* 28: 51-82.
- Jiang, D., A. Kumar, and K. Law. 2016. Political contributions and analyst behavior. *Review of Accounting Studies* 22: 37-88.
- Kacperczyk, M., S. Van Nieuwerburgh, and L. Veldkamp. 2016. A rational theory of mutual funds' attention allocation. *Econometrica* 84: 571-626.
- Kadan, O., L. Madureira, R. Wang, and T. Zach. 2012. Analysts' industry experience. *Journal of Accounting and Economics* 54: 95-120.
- Ke, B., and Y. Yu. 2006. The effect of issuing biased earnings forecasts on analysts' access to management and survival. *Journal of Accounting Research* 44: 965-999.
- Kelly, B., and A. Ljungqvist. 2012. Testing asymmetric-information asset pricing models. *Review of Financial Studies* 25: 1366-1413.
- Kempf, E., A. Manconi, and O. Spalt. 2017. Distracted shareholders and corporate actions. *Review of Financial Studies* 30: 1660-1695.
- Kothari, S. P., E. So, and R. Verdi. 2016. Analysts' forecasts and asset pricing: A survey. *Annual Review of Financial Economics* 8: 197-219.
- Lo, K., and S. S. Wu. 2018. The impact of seasonal affective disorder on financial analysts. *The Accounting Review* 93: 309-333.
- Loh, R., and R. Stulz. 2011. When are analyst recommendation changes influential? *Review of Financial Studies* 24: 593-627.
- Loh, R., and R. Stulz. 2019. Is sell-side research more valuable in bad times? *The Journal of Finance* 73: 959-1013.
- Malloy, C. J. 2005. The geography of equity analysis. *The Journal of Finance* 60: 719-755.
- Merkley, K. J., R. Michaely, and J. Pacelli. 2020. Cultural diversity on Wall Street: Evidence from sell-side analysts' forecasts. *Journal of Accounting and Economics*, forthcoming.
- Mikhail, M. B., B. R. Walther, and R.H. Willis. 1997. Do security analysts improve their performance with experience?. *Journal of Accounting Research* 35: 131-157.
- O'Brien, P., and H. Tan. 2015. Geographic proximity and analyst coverage decisions: Evidence from IPOs. *Journal of Accounting and Economics* 59: 41-59.

Petersen, M. A. 2009. Estimating standard errors in finance panel data sets: Comparing approaches. *The Review of Financial Studies* 22: 435-480.

Pisciotta, K. 2018. *Analyst workload and information production: Evidence from IPO involvement*. Unpublished Working Paper. University of Kansas.

Schneemeier, J. 2018. *Shock propagation through cross-learning in opaque networks*. Unpublished Working Paper. Kelley School of Business.

Table 1. Summary statistics

This table reports descriptive statistics of analyst and forecast variables. Appendix A provides the variable definitions.

Variables	Nb Obs.	Mean	S.D.	0.25	Mdn	0.75
Relative Forecast Error (%)	1,110,420	-1.28	58.68	-36.42	-3.70	21.68
Absolute Forecast Error	1,110,420	0.24	0.45	0.03	0.09	0.25
Relative Revision Frequency (%)	890,934	0.00	172.14	-100.00	-100.00	54.55
Revision Frequency	1,110,420	0.44	0.75	0.00	0.00	1.00
CAR excess (%)	499,185	0.00	6.82	-2.93	0.04	3.15
CAR market model (%)	499,185	-0.10	6.83	-2.98	-0.03	3.03
Analyst Distraction	1,110,420	0.03	0.11	0.00	0.00	0.00
Analyst Distraction Dummy	1,110,420	0.06	0.23	0.00	0.00	0.00
Forecast Revision	559,862	-0.01	0.13	-0.06	0.00	0.04
Forecast Horizon	1,110,420	204.21	90.44	116.00	196.00	281.00
Firm Experience	1,110,420	11.97	13.73	2.00	7.00	17.00
General Experience	1,110,420	35.01	27.43	13.00	29.00	51.00
Top 10 Brokerage House	1,110,420	0.58	0.49	0.00	1.00	1.00
Portfolio Size	1,110,420	12.66	8.51	8.00	11.00	16.00
Nb. Different Industries	1,110,420	2.24	1.46	1.00	2.00	3.00
First Distraction	1,110,420	0.03	0.16	0.00	0.00	0.00
Not-first Distraction	1,110,420	0.03	0.17	0.00	0.00	0.00

Table 2. Analyst distraction and forecast accuracy

This table reports the results of regressions of relative forecast error on analyst distraction plus control variables. *Relative Forecast Error* measures the absolute forecast error of an analyst relative to the absolute forecast error of all the analysts covering the same firm in a same quarter. *Analyst Distraction* is an analyst-firm-quarter measure that captures the percentage of an analyst's attention that is distracted by attention-grabbing events affecting the other firms of her portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. In Column 1, we include firm-quarter fixed effects. In Column 2, we include firm-quarter fixed effects and analyst fixed effects. In Column 3, we include firm-quarter fixed effects and analyst-quarter fixed effects. In Column 4, we include firm-quarter fixed effects and brokerage-house fixed effects. In Columns 5 to 8, we repeat regressions 1 to 4 replacing *Analyst Distraction* by *Analyst Distraction Dummy*. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Analyst Distraction	3.290*** (0.944)	1.848** (0.873)	2.471** (1.129)	2.173** (0.863)				
Analyst Distraction Dummy					1.426*** (0.391)	0.779** (0.362)	1.497*** (0.535)	1.002*** (0.357)
Forecast Horizon	0.401*** (0.004)	0.418*** (0.004)	0.437*** (0.004)	0.413*** (0.004)	0.401*** (0.004)	0.418*** (0.004)	0.436*** (0.004)	0.413*** (0.004)
Firm Experience	-0.068*** (0.008)	-0.033*** (0.008)	-0.040*** (0.008)	-0.056*** (0.007)	-0.073*** (0.007)	-0.043*** (0.008)	-0.048*** (0.008)	-0.056*** (0.007)
General Experience	-0.019*** (0.004)	-0.010 (0.006)	-0.031*** (0.009)	-0.001 (0.004)	-0.019*** (0.004)	0.001 (0.006)	-0.028*** (0.009)	-0.001 (0.004)
Top 10 Brokerage House	-1.148*** (0.180)	-0.646*** (0.218)	-1.140*** (0.388)	-0.575*** (0.212)	-1.060*** (0.165)	-0.577*** (0.218)	-1.288*** (0.388)	-0.574*** (0.212)
Portfolio Size	0.019 (0.013)	0.050*** (0.014)	0.104*** (0.024)	0.038*** (0.013)	0.022* (0.012)	0.045*** (0.014)	0.112*** (0.024)	0.039*** (0.013)
Nb. Different Industries	0.933*** (0.068)	0.306*** (0.064)	0.362** (0.141)	0.500*** (0.063)	0.947*** (0.063)	0.327*** (0.064)	0.416*** (0.141)	0.501*** (0.063)
Observations	1,110,420	1,110,420	1,110,420	1,110,420	1,110,420	1,110,420	1,110,420	1,110,420
R-squared	0.023	0.047	0.203	0.023	0.023	0.047	0.203	0.023
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst Fixed Effects	No	Yes	No	No	No	Yes	No	No
Analyst-quarter Fixed Effects	No	No	Yes	No	No	No	Yes	No
Brokerage-house Fixed Effects	No	No	No	Yes	No	No	No	Yes
Firm-analyst Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3. Effect of analyst distraction on forecast accuracy conditional on portfolio size

This table reports the results of the regression of relative forecast on analyst distraction plus control variables and firm-quarter fixed effects for two subsamples of analysts with below-median and above-median portfolio size (11 stocks). *Relative Forecast Error* measures the absolute forecast error of an analyst relative to the absolute forecast error of all the analysts covering the same firm in a same quarter. *Analyst Distraction* is an analyst-firm-quarter measure that captures the percentage of an analyst's attention that is distracted by attention-grabbing events affecting the other firms of her portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. Columns 1 and 2 report the results for *Analyst Distraction* and columns 3 and 4 report the results for *Analyst Distraction Dummy*. The last row of the table reports the p-value of a Wald-test of equality of the coefficients in both subsamples. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

	(1)	(2)	(3)	(4)
<i>Relative Forecast Error</i>	Below- median Portfolio Size	Above- median Portfolio Size	Below- median Portfolio Size	Above- median Portfolio Size
Analyst Distraction	3.015*** (1.127)	6.778*** (1.373)		
Analyst Distraction Dummy			1.320*** (0.460)	2.168*** (0.584)
Forecast Horizon	0.398*** (0.005)	0.403*** (0.006)	0.398*** (0.005)	0.403*** (0.006)
Firm Experience	-0.057*** (0.011)	-0.074*** (0.011)	-0.057*** (0.011)	-0.074*** (0.011)
General Experience	-0.024*** (0.005)	-0.011** (0.005)	-0.024*** (0.005)	-0.011** (0.005)
Top 10 Brokerage House	-0.334 (0.234)	-1.776*** (0.253)	-0.334 (0.234)	-1.793*** (0.253)
Portfolio Size	-0.152*** (0.025)	0.081*** (0.016)	-0.150*** (0.025)	0.082*** (0.016)
Nb. Different Industries	0.796*** (0.103)	1.059*** (0.086)	0.798*** (0.103)	1.073*** (0.086)
Observations	485,173	432,700	485,173	432,700
R-squared	0.15	0.16	0.15	0.16
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes
Firm-analyst Clusters	Yes	Yes	Yes	Yes
P-value of the Wald test of coefficient equality	(1) vs (2): 0.016		(3) vs (4): 0.125	

Table 4. Timing of the effect of analyst distraction

This table reports the results of regressions of relative forecast error on contemporaneous analyst distraction plus control variables and lagged and future analyst distraction. *Relative Forecast Error* measures the absolute forecast error of an analyst relative to the absolute forecast error of all the analysts covering the same firm in a same quarter. *Analyst Distraction* is an analyst-firm-quarter measure that captures the percentage of an analyst's attention that is distracted by attention-grabbing events affecting the other firms of her portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. *Lagged Analyst Distraction* is the value for *Analyst Distraction* of analyst *i* in firm *j* at quarter *t-1*. *Future Analyst Distraction* is the value for *Analyst Distraction* of analyst *i* in firm *j* at quarter *t+1*. For brevity, the coefficients on the control variables are not reported. Column 1 (Column 3) reports the results of the regression of relative forecast error on analyst distraction dummy variable (discrete variable) plus control variables, firm-quarter and analyst fixed effects. Column 2 (Column 4) reports the results of the same regression augmented with lagged and future analyst distraction. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)	(3)	(4)
Analyst Distraction Dummy	1.426*** (0.391)	1.922*** (0.670)		
Lagged Analyst Distraction Dummy		0.259 (0.674)		
Future Analyst Distraction Dummy		-0.711 (0.680)		
Analyst Distraction			3.290*** (0.941)	3.445** (1.620)
Lagged Analyst Distraction				2.000 (1.627)
Future Analyst Distraction				-0.094 (1.625)
Baseline variables (Table 2)	Yes	Yes	Yes	Yes
Observations	1,110,420	456,575	1,110,420	456,575
R-squared	0.023	0.165	0.023	0.165
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes
Firm-analyst Clusters	Yes	Yes	Yes	Yes

Table 5. First-time distraction and analyst forecast accuracy

This table reports the results of regressions of relative forecast error on analyst distraction shocks plus control variables. *Relative Forecast Error* measures the absolute forecast error of an analyst relative to the absolute forecast error of all the analysts covering the same firm in a same quarter. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. For brevity, the coefficients on the control variables are not reported. Column 1 presents the results of Table 2, Column 3. In Column 2, *Analyst Distraction Dummy* is partitioned into *First Distraction Event* and *Not-first Distraction Event*. *First Distraction Event* identifying those cases where the analyst-firm pair experiences distraction for the first time during our sample period, and *Not-first Distraction Event* identifying the other cases. In Column 3, we add analyst fixed effect to the regression reported in Column 2. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Panel A of Table 4 provides detailed definitions of the additional variables we use for this specific test. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)	(3)
Analyst Distraction Dummy	1.426*** (0.391)		
First Distraction Event		1.714*** (0.473)	0.917** (0.439)
Not-first Distraction Event		0.698 (0.522)	0.180 (0.480)
Baseline variables (Table 2)	Yes	Yes	Yes
Observations	1,110,420	1,110,420	1,110,420
R-squared	0.023	0.023	0.047
Firm-quarter Fixed Effects	Yes	Yes	Yes
Analyst Fixed Effects	No	No	Yes
Firm-analyst Clusters	Yes	Yes	Yes

Table 6. Analyst distraction and forecast revision frequency

This table reports the results of regressions of relative revision frequency on analyst distraction plus control variables, firm-quarter fixed effects, and analyst fixed effects. *Relative Revision Frequency* measures the revision frequency of an analyst relative to the revision frequency of all the analysts covering the same firm in a same quarter. *Analyst Distraction* is an analyst-firm-quarter measure that captures the percentage of an analyst's attention that is distracted by attention-grabbing events affecting the other firms of her portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. Column 1 reports the results of the regression of *Relative Revision Frequency* on our discrete measure of analyst distraction plus control variables, firm-quarter and analyst fixed effects. Column 2 reports the results of the regression of *Relative Revision Frequency* on our binary measure of analyst distraction plus control variables, firm-quarter and analyst fixed effects. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Revision Frequency</i>	(1)	(2)
Analyst Distraction	-11.555*** (2.858)	
Analyst Distraction Dummy		-4.948*** (1.194)
Forecast Horizon	-0.110*** (0.012)	-0.110*** (0.012)
Firm Experience	0.049** (0.022)	0.049** (0.022)
General Experience	-0.409*** (0.019)	-0.409*** (0.019)
Top 10 Brokerage House	3.252*** (0.699)	3.252*** (0.699)
Portfolio Size	0.581*** (0.045)	0.576*** (0.045)
Nb. Different Industries	1.482*** (0.207)	1.475*** (0.207)
Observations	890,934	890,934
R-squared	0.070	0.070
Firm-Quarter Fixed Effects	Yes	Yes
Analyst Fixed Effects	Yes	Yes
Firm-analyst Clusters	Yes	Yes

Table 7. Analyst distraction and information production*Panel A: Likelihood of revising a forecast when other analysts have not produced new information*

Panel A reports the results of regressions of the relative propensity for an analyst to revise her forecast when other analysts are not producing information (*Relative Self-Revision Frequency*) on analyst distraction plus control variables, firm-quarter fixed effects, and analyst fixed effects. *Analyst Distraction* is an analyst-firm-quarter measure that captures the percentage of an analyst's attention that is distracted by attention-grabbing events affecting the other firms of her portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater or equal to 20% and zero otherwise. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Self-Revision Frequency</i>	(1)	(2)	(3)
Analyst Distraction	-28.381*** (8.289)		-15.320** (7.645)
Analyst Distraction Dummy		-10.932*** (3.412)	
Forecast Horizon	0.023 (0.043)	0.023 (0.043)	0.101*** (0.037)
Firm Experience	0.097 (0.101)	0.097 (0.101)	0.000 (0.081)
General Experience	-0.222*** (0.043)	-0.222*** (0.043)	-0.281*** (0.058)
Top 10 Brokerage House	9.264*** (1.733)	9.286*** (1.733)	3.971* (2.055)
Portfolio Size	0.475*** (0.117)	0.467*** (0.117)	0.226* (0.119)
Nb. Different Industries	-3.334*** (0.619)	-3.376*** (0.618)	0.354 (0.606)
Observations	287,491	287,491	287,491
R-squared	0.001	0.001	0.092
Firm-quarter Fixed Effects	Yes	Yes	Yes
Analyst Fixed Effects	No	No	Yes
Firm-analyst Clusters	Yes	Yes	Yes

Panel B: Analyst distraction and the market reaction to forecast revisions

Panel B reports the regression of market reaction, measured by the absolute cumulative abnormal returns over the three days surrounding the forecast revision announcement, on the absolute change in the forecast (*Absolute Forecast Revision*), analyst distraction, the usual control variables from Table 2, firm-quarter fixed effects and an interaction term between *Absolute Forecast Revision* and *Analyst Distraction*. In Column 1, we measure the market reaction as the cumulative abnormal returns in excess of the market return over the three days surrounding the forecast revision. In Column 2, we measure the market reaction as the cumulative abnormal returns in excess of the CAPM market model over the three days surrounding the forecast revision. In Column 3 and 4, we reproduce regressions 1 and 2 respectively, adding analyst fixed effects. In Column 5 and 6, we reproduce the regressions 1 and 2 respectively, adding day-of-the-week fixed effects. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix A provides the variable definitions.

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Market Reaction</i>	Absolute CAR in excess of market return	Absolute CAR in excess of CAPM market model	Absolute CAR in excess of market return	Absolute CAR in excess of CAPM market model	Absolute CAR in excess of market return	Absolute CAR in excess of CAPM market model
Absolute Forecast Revision X Analyst Distraction	-0.624** (0.306)	-0.613** (0.301)	-2.594* (1.523)	-2.481* (1.510)	-0.608** (0.306)	-0.605** (0.301)
Absolute Forecast Revision	0.221*** (0.042)	0.225*** (0.041)	1.291*** (0.086)	1.280*** (0.085)	0.208*** (0.042)	0.211*** (0.041)
Analyst Distraction	-0.005 (0.043)	-0.025 (0.042)	-0.083 (0.068)	-0.072 (0.068)	-0.004 (0.042)	-0.025 (0.042)
Observations	334,059	334,059	334,059	334,059	334,059	334,059
Baseline variables (Table 2)	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.51	0.51	0.60	0.60	0.51	0.51
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Analyst Fixed Effects	No	No	Yes	Yes	No	No
Day-of-the-week Dummies	No	No	No	No	Yes	Yes
Firm-Analyst Clusters	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. Real effects of analyst distraction*Panel A: Earnings surprise*

Panel A reports the results of regressions of earnings surprises on firm-level aggregate analyst distraction plus control variables and firm and year-quarter fixed effects. *Earnings Surprise* is calculated as earnings per share minus the last mean analyst consensus forecast before the earnings-announcement date, scaled by the stock price at the beginning of the fiscal quarter. *Avg. Analyst Distraction* is the average *Analyst Distraction* of analysts covering firm *i* in quarter *t*. Standard errors are robust to heteroscedasticity and clustered by firm. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Earnings Surprise</i>	(1) Absolute earnings surprise	(2) Analysts time-varying characteristics	(3) Firm time-varying characteristics	(4) Analyst and firm time-varying characteristics	(5) Firm-year fixed effects	(6) Positive earnings surprise only	(7) Negative earnings surprise only
Avg. Analyst Distraction	0.071*** (0.025)	0.073*** (0.025)	0.055** (0.024)	0.056** (0.024)	0.056* (0.032)	0.073*** (0.024)	-0.132** (0.062)
Avg. Firm Experience		0.006*** (0.001)		0.005*** (0.001)	0.00 (0.001)		
Avg. General Experience		-0.001*** (0.00)		-0.001*** (0.00)	0.00 (0.00)		
Avg. Portfolio Size		0.001* (0.001)		0.002** (0.001)	-0.001 (0.001)		
Avg. Nb. Different Industries		-0.008* (0.004)		-0.007* (0.004)	0.002 (0.005)		
Avg. Top 10 Brokerage House		0.016 (0.014)		0.02 (0.014)	0.001 (0.019)		
Consensus Forecast Horizon		-0.000*** (0.00)		-0.000*** (0.00)	-0.000*** (0.00)		
Ln(Analyst Coverage)	-0.179*** (0.011)	-0.170*** (0.011)	-0.112*** (0.011)	-0.103*** (0.011)	-0.092*** (0.02)	-0.158*** (0.009)	0.254*** (0.023)
Size			-0.131*** (0.009)	-0.132*** (0.009)			
Market-to-book			-0.005*** (0.001)	-0.004*** (0.001)			
Book Leverage			0.112*** (0.036)	0.107*** (0.035)			
Profitability			-0.524*** (0.052)	-0.514*** (0.052)			
Institutional Ownership			-0.366*** (0.032)	-0.371*** (0.032)			
Ln(Trading Volume)			0.125*** (0.008)	0.124*** (0.008)			
Observations	110,578	110,578	110,578	110,578	110,578	59,918	39,550
R-squared	0.376	0.378	0.395	0.396	0.749	0.482	0.466
Year-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	No	Yes	Yes
Firm-year Fixed Effects	No	No	No	No	Yes	No	No
Firm Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: *Amihud's illiquidity*

Panel B reports the results of regressions of Amihud's measure of illiquidity on aggregate analyst distraction plus control variables and firm and year fixed effects. *Amihud Illiquidity* is computed as the natural logarithm of one plus the average daily ratio of absolute stock return to dollar volume over the last 250 trading days multiplied by 1,000,000. We exclude firm with a stock price inferior to 5\$. *Avg. Analyst Distraction* is the average *Analyst Distraction* of analysts covering firm *i* in quarter *t*. Column 2 reports regression 1 augmented with extra aggregate analyst control variables. Standard errors are robust to heteroscedasticity and clustered by firm. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Amihud Illiquidity</i>	(1) Firm time-varying characteristics	(2) Analyst and firm time-varying characteristics
Avg. Analyst Distraction	0.057** (0.024)	0.053** (0.024)
Avg. General Experience		-0.000 (0.000)
Avg. Portfolio Size		0.001** (0.001)
Avg. Nb Different Industries		0.005 (0.003)
Avg. Top Brokerage House		-0.004 (0.007)
Ln(Analyst Coverage)	-0.024*** (0.005)	-0.024*** (0.005)
Market-to-book	0.000 (0.000)	0.000 (0.000)
Size	-0.050*** (0.004)	-0.049*** (0.004)
Book Leverage	0.066*** (0.016)	0.066*** (0.016)
Institutional Ownership	-0.186*** (0.016)	-0.186*** (0.015)
Ln(Trading Volume)	-0.035*** (0.004)	-0.035*** (0.004)
Momentum	0.018*** (0.003)	0.018*** (0.003)
Volatility	-0.186*** (0.050)	-0.182*** (0.050)
Observations	45,043	45,043
R-squared	0.768	0.769
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Firm Clusters	Yes	Yes

Appendix A: Variable definitions

Variable	Definitions	Sources
Absolute CAR Market	Three-day CRSP value-weighted market-adjusted cumulative abnormal return. Values are multiplied by 100.	CRSP
Absolute CAR CAPM	Three-day CRSP value-weighted CAPM-market-model-adjusted cumulative abnormal return. Values are multiplied by 100.	CRSP
Absolute Forecast Error	The absolute forecast error of analyst i for firm j , calculated as the absolute value of the difference between analyst i 's earnings forecast for firm j and the actual earnings reported by firm j .	I/B/E/S
Amihud Illiquidity	Amihud (2002)'s measure of illiquidity computed as the natural logarithm of one plus the average daily ratio of absolute stock return to dollar volume over the last 250 trading days multiplied by 1000000. We exclude firm with a stock price inferior to 5\$.	CRSP
Analyst Coverage	The number of unique analysts issuing earnings forecast for firm j in fiscal year t .	I/B/E/S
Analyst Distraction	Percentage of an analyst-firm-quarter portfolio exposed to firms experiencing attention-grabbing shocks in unrelated Fama-French twelve industries, whereby we defined attention-grabbing shocks as extreme quarterly returns.	I/B/E/S Kenneth French's Website
Analyst Distraction Dummy	Dummy variable that takes the value one when <i>Analyst Distraction</i> is greater or equal to 5 and 0 otherwise.	I/B/E/S Kenneth French's Website
Analyst Distraction 17	Same as Analyst Distraction but using the Fama-French 17 industry classification	I/B/E/S Kenneth French's Website
Analyst Distraction GICS	Same as Analyst Distraction but using the GICS eleven sectors (including <i>others</i>) The GICS sector classification is retrieved from Compustat.	I/B/E/S COMPUSTAT
Analyst Distraction Bot Only	Same as Analyst Distraction but using only negative extreme returns.	I/B/E/S Kenneth French's Website
Analyst Distraction IERP	Same as Analyst Distraction but weighting the attention-grabbing shocks by the inverse of the probability for a given industry to experience extreme return events. For a given Fama-French twelve industry and quarter, we compute the probability to experience extreme return events as the number of quarters the industry experiences extreme returns over the last 20 quarters and divide it by 20.	I/B/E/S Kenneth French's Website
Analyst Distraction Top Only	Same as Analyst Distraction but using only positive extreme returns.	I/B/E/S Kenneth French's Website
Analyst Distraction VW	Same as Analyst Distraction but weighting the attention-grabbing shocks by the market capitalization of the analyst's portfolio firms.	I/B/E/S Kenneth French's Website
Avg. Analyst Distraction	Average <i>Analyst Distraction</i> of the analysts covering firm j at quarter t .	
Avg. Firm Experience	Average <i>Firm Experience</i> of the analysts covering firm j at quarter t .	
Avg. General Experience	Average <i>General Experience</i> of the analysts covering firm j at quarter t .	
Avg. Portfolio Size	Average <i>Portfolio Size</i> of the analysts covering firm j at quarter t .	

Avg. Nb Different Industries	Average <i>Nb. Different Industries</i> of the analysts covering firm j at quarter t .		
Avg. Top 10 Brokerage House	Average <i>Top 10 Brokerage House</i> of the analysts covering firm j at quarter t .		
At least one revision	Dummy variable that indicates whether the analyst updates at least once her one-year-ahead EPS forecast for a same firm-quarter	I/B/E/S	
Book-to-market	Book value of equity divided by the current market value of equity at the beginning of the fiscal year.	COMPUSTAT	
Consensus Horizon Forecast	Number of days between the last consensus forecast and the earnings announcement date.	I/B/E/S	
Earnings Surprise	Quarterly earnings surprise calculated as I/B/E/S actual earnings per share minus the last mean analyst consensus forecast before the earnings-announcement date, scaled by the stock price at the beginning of the fiscal quarter.	I/B/E/S	
First Distraction Event	<i>First Distraction</i> , which is a dummy variable that takes the value 1 if it is the first time that an analyst experiences a significant distraction shock in a specific firm and 0 otherwise.		
Forecast Horizon	The number of days between analyst i 's forecast for firm j and the firm fiscal year end.	I/B/E/S	
Forecast Revision	The difference between the analyst i 's forecast for the firm j 's earnings at quarter t and the last analyst i 's forecast for the same firm and earnings at quarter t scaled by the last forecast.		
Forecast Revision Frequency	The number of forecasts issued by an analyst i for firm j 's earnings at quarter t , minus one.	I/B/E/S	
Firm Experience	The number of quarters since analyst i 's first earnings forecasts for firm j at quarter t .	I/B/E/S	
General Experience	The total number of quarters that analyst i appeared in I/B/E/S at quarter t .	I/B/E/S	
Institutional Ownership	The percentage of a firm's equity held by all institutions at the end of fiscal year $t-1$.	13F	Thomson database
Momentum	Buy-and-hold returns over the last twelve months prior to the fiscal year end.	CRSP	
Nb. Different Industries	The number of 2-digit SICs represented by firms followed by analyst i in quarter t .	I/B/E/S	
Not-first Distraction Event	<i>Not-first Distraction</i> , which is a dummy variable that takes the value 1 if it is not the first time that an analyst experiences a significant distraction shock in a specific firm and 0 otherwise.		
Portfolio Size	The number of unique firms followed by analyst i in quarter t .	I/B/E/S	
Profitability	Return on asset.	COMPUSTAT	
Relative Forecast Error	The difference between the absolute forecast error for analyst i and firm j in quarter t and the mean absolute forecast error for firm j in quarter t scaled by the mean absolute forecast error for firm j in quarter t .	I/B/E/S	
Relative Revision Frequency	The difference between the forecast revision frequency for analyst i and firm j in quarter t and the mean forecast revision frequency for firm j in quarter t scaled by the mean forecast revision frequency for firm j in quarter t .	I/B/E/S	
Relative Self-Revision Frequency	The difference between the forecast self-revision frequency for analyst i and firm j in quarter t and the mean forecast self-revision frequency for firm j in quarter t scaled by the mean forecast revision frequency for firm j in quarter t .	I/B/E/S	

Revision	Dummy variable that takes the value one if an analyst revises her forecast for a given firm's end-of-the-fiscal-year earnings and zero otherwise.	I/B/E/S
Size	Natural logarithm of market capitalization of the covered firm (in \$thousands) at the end of fiscal year t-1.	COMPUSTAT
Top 10 Brokerage House	Indicator variable that is equal to one if an analyst <i>i</i> works at a top decile brokerage house in quarter <i>t</i> .	I/B/E/S
Total Number of Distraction Events	<i>Total Number of Distraction Events</i> , which is the sum of the times an analyst is significantly distracted (Analyst Distraction Dummy = 1)	
Trading Volume	The annual trading volume (in thousand shares) for a firm <i>j</i> in year t-1.	CRSP
Volatility	Standard deviation of the monthly stock returns over the last 36 months preceding the fiscal year end.	CRSP

Appendix B: Firm-level descriptive statistics

Panel A: Earnings surprise

Panel A presents descriptive statistics for the firm-quarter level variables we use to examine the influence of analyst distraction on earnings surprise. Appendix A provides the variable definitions.

Variables	Obs.	Mean	S.D.	0.25	Mdn.	0.75
Earnings Surprise	110,578	-0.05	0.84	-0.08	0.02	0.14
Absolute Earnings Surprise	110,578	0.38	0.76	0.03	0.12	0.35
Avg. Analyst Distraction	110,578	0.03	0.10	0.00	0.00	0.02
Consensus Forecast Horizon	110,578	44.59	12.80	35.00	42.00	49.00
Avg. Firm Experience	110,578	11.22	8.00	5.00	9.57	15.86
Avg. General Experience	110,578	33.46	15.30	22.67	33.00	43.93
Avg. Portfolio Size	110,578	12.50	5.10	9.50	12.00	14.67
Avg. Nb. Different Industries	110,578	2.26	0.91	1.60	2.08	2.75
Avg. Top 10 Brokerage House	110,578	0.55	0.27	0.40	0.57	0.75
Analyst Coverage	110,578	2.11	0.63	1.61	2.08	2.56
Size	110,578	7.11	1.68	5.91	6.99	8.18
Market-to-book	110,578	3.17	3.62	1.41	2.19	3.69
Book Leverage	110,578	0.21	0.19	0.04	0.17	0.32
Profitability	110,578	0.03	0.12	0.01	0.04	0.08
Institutional Ownership	110,578	0.62	0.23	0.46	0.64	0.80
Trading Volume	110,578	13.47	1.65	12.34	13.47	14.58

Panel B: Firm-level descriptive statistics

Panel B presents descriptive statistics for firm-year level data we use to examine the influence of analyst distraction on Amihud's illiquidity measure, our proxy for information asymmetry. Appendix A provides the variable definitions.

Variables	Obs.	Mean	S.D.	0.25	Mdn	0.75
Amihud Illiquidity	45,043	0.11	0.21	0.00	0.00	0.46
Avg. Analyst Distraction	45,043	0.04	0.07	0.00	0.01	0.05
Analyst Coverage	45,043	1.98	0.78	1.39	1.95	2.56
Market-to-book	45,043	2.81	2.93	1.33	1.99	3.23
Size	45,043	6.60	1.78	5.31	6.46	7.74
Book Leverage	45,043	0.21	0.20	0.04	0.17	0.32
Institutional Ownership	45,043	0.55	0.25	0.36	0.57	0.75
Ln(Trading Volume)	45,043	12.65	1.94	11.25	12.67	14.00
Momentum	45,043	0.21	0.50	-0.08	0.13	0.39
Volatility	45,043	0.11	0.06	0.07	0.10	0.14

**Internet Appendix to
“When attention is away, analysts misplay:
Distraction and analyst forecast performance”**

This Internet Appendix discusses additional tests to check the robustness of our results.

Figure IA.1: Placebo-test based on pseudo industry shocks: Relative forecast error

This figure reports the coefficients from 5,000 placebo tests for baseline regression reported in Column 1 of Table 2. Each time, we replace the *Analyst Distraction* by *Pseudo Analyst Distraction* variable. To derive this variable, instead of utilizing the actual attention-grabbing industries, each quarter, we randomly select two industries and use these pseudo attention-grabbing industries to compute our measure of *Pseudo Analyst Distraction*. The true coefficient for *Analyst Distraction* (Column 1, Table 2) is 3.290.

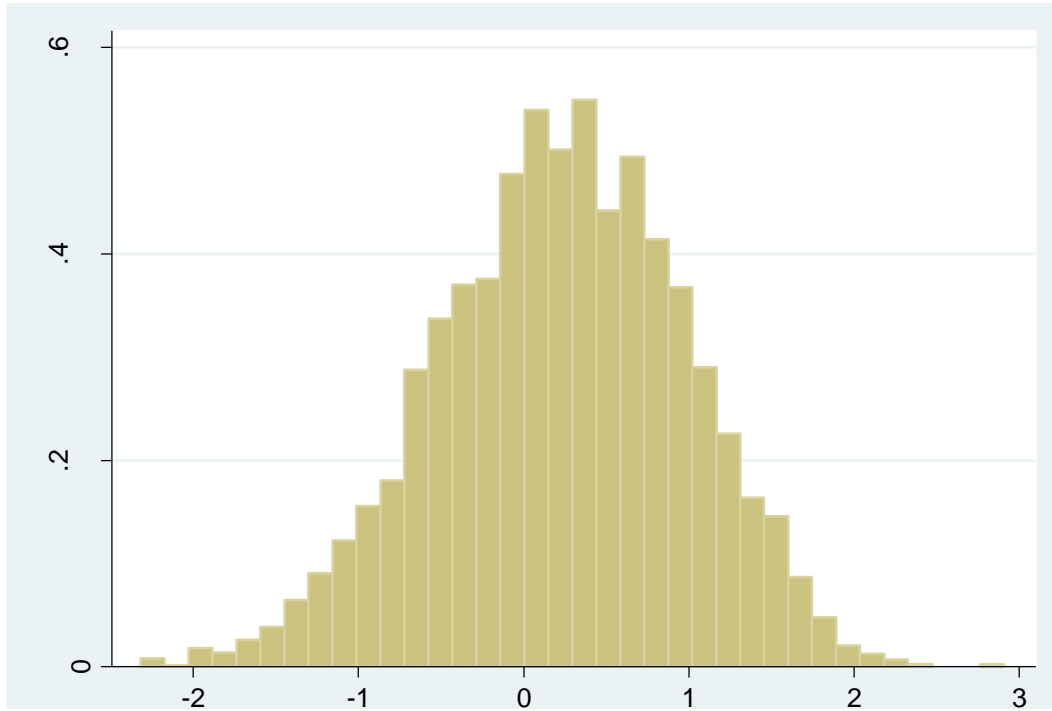


Figure IA.2: Placebo-test based on pseudo industry shocks: Earnings surprise

This figure reports the coefficients from 5,000 placebo tests for baseline regression reported in Column 4, Panel A, of Table 8. Each time, we replace *Analyst Distraction* by a *Pseudo Analyst Distraction* variable. Instead of utilizing the actual attention-grabbing industries, each quarter, we randomly select two industries and use these pseudo attention-grabbing industries to compute our measure of *Pseudo Analyst Distraction*. At the firm level, each quarter, we aggregate the *Pseudo Analyst Distraction* of all the analysts covering a firm to derive a pseudo measure of aggregate analyst distraction (*Avg. Pseudo Analyst Distraction*). The true coefficient for *Avg. Analyst Distraction* (Column 4, Panel A, Table 8) is 0.056.

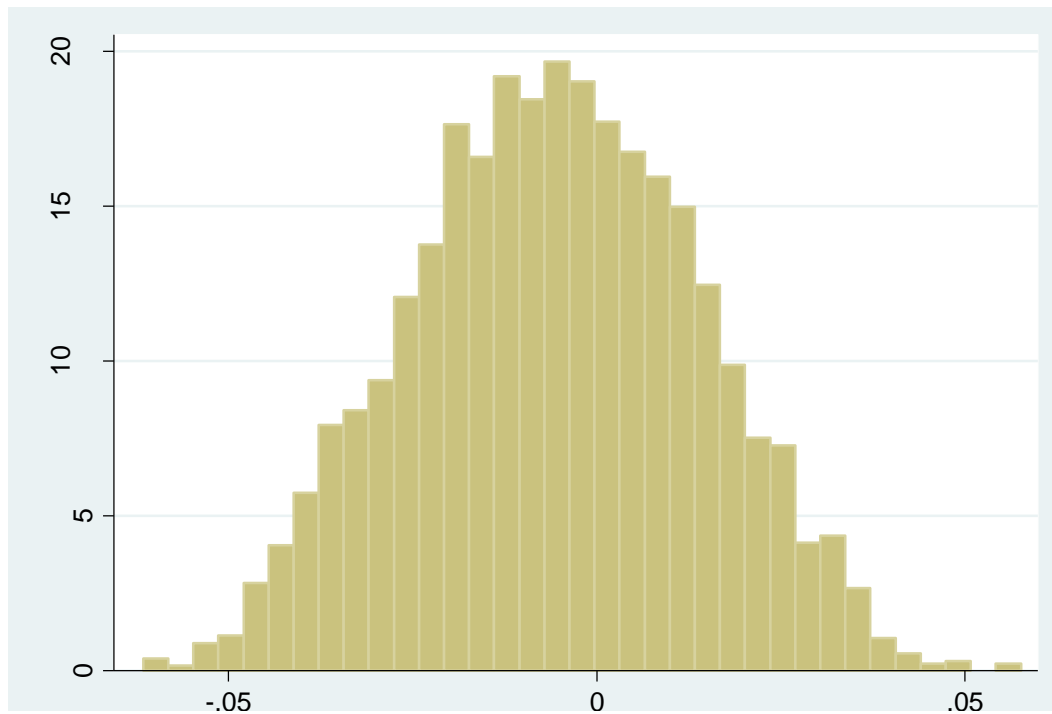


Table IA. 1: Quarterly returns of top and bottom industry performers

Panel A: Top industry performers

Panel A reports the top performer Fama-French twelve industry on a quarterly basis, over the 1985-2015 period.

y-q	Fama-French twelve industry	Quarterly return	Mean other Industries return				
				1996-2	Consumer Non-Durables	10.12%	3.66%
				1996-3	Finance	7.36%	1.20%
				1996-4	Energy	12.59%	5.59%
1985-1	Health	15.54%	10.15%	1997-1	Consumer Non-Durables	4.27%	0.31%
1985-2	Telecom	12.42%	7.42%	1997-2	Health	23.59%	14.66%
1985-3	Energy	-0.27%	-4.70%	1997-3	Business Equipment	18.44%	8.28%
1985-4	Finance	21.62%	16.68%	1997-4	Telecom	19.13%	1.89%
1986-1	Consumer Durables	22.32%	14.07%	1998-1	Consumer Durables	19.94%	12.54%
1986-2	Consumer Non-Durables	16.14%	6.04%	1998-2	Consumer Durables	10.21%	1.48%
1986-3	Energy	9.50%	-8.97%	1998-3	Utilities	4.51%	-12.68%
1986-4	Chemicals	9.21%	4.38%	1998-4	Business Equipment	36.93%	17.19%
1987-1	Business Equipment	31.15%	20.58%	1999-1	Business Equipment	10.68%	0.75%
1987-2	Energy	9.13%	3.63%	1999-2	Telecom	15.85%	7.28%
1987-3	Telecom	14.75%	5.57%	1999-3	Business Equipment	3.44%	-8.41%
1987-4	Utilities	-8.78%	-23.84%	1999-4	Business Equipment	40.72%	8.64%
1988-1	Shops	15.59%	7.19%	2000-1	Business Equipment	17.00%	-0.91%
1988-2	Consumer Durables	12.77%	5.90%	2000-2	Health	22.00%	-3.64%
1988-3	Consumer Non-Durables	7.98%	-0.35%	2000-3	Utilities	27.90%	1.96%
1988-4	Consumer Non-Durables	10.68%	1.78%	2000-4	Chemicals	17.92%	-2.56%
1989-1	Telecom	13.61%	6.65%	2001-1	Consumer Durables	8.36%	-9.16%
1989-2	Telecom	15.94%	7.89%	2001-2	Business Equipment	15.62%	4.91%
1989-3	Health	17.03%	9.31%	2001-3	Consumer Non-Durables	-0.31%	-14.64%
1989-4	Energy	10.53%	-0.34%	2001-4	Business Equipment	33.35%	9.33%
1990-1	Business Equipment	7.31%	-3.52%	2002-1	Consumer Durables	13.42%	3.05%
1990-2	Health	18.11%	4.65%	2002-2	Consumer Non-Durables	-1.89%	-11.82%
1990-3	Energy	5.23%	-17.67%	2002-3	Health	-7.66%	-17.71%
1990-4	Chemicals	15.89%	8.17%	2002-4	Telecom	23.33%	5.87%
1991-1	Shops	28.38%	15.54%	2003-1	Health	1.17%	-4.71%
1991-2	Consumer Durables	7.03%	-0.42%	2003-2	Consumer Durables	21.64%	15.59%
1991-3	Utilities	11.68%	5.02%	2003-3	Business Equipment	11.06%	2.49%
1991-4	Health	21.04%	6.25%	2003-4	Consumer Durables	23.84%	12.58%
1992-1	Consumer Durables	25.04%	-1.36%	2004-1	Shops	6.88%	1.70%
1992-2	Energy	9.04%	0.26%	2004-2	Energy	8.56%	1.97%
1992-3	Energy	8.21%	2.31%	2004-3	Energy	11.06%	-2.03%
1992-4	Finance	14.56%	6.64%	2004-4	Business Equipment	14.72%	9.53%
1993-1	Consumer Durables	15.12%	4.07%	2005-1	Energy	18.61%	-2.78%
1993-2	Consumer Durables	6.84%	0.78%	2005-2	Utilities	9.62%	0.48%
1993-3	Finance	8.11%	3.10%	2005-3	Energy	20.11%	2.49%
1993-4	Consumer Durables	13.30%	2.30%	2005-4	Finance	7.42%	0.08%
1994-1	Business Equipment	2.38%	-4.52%	2006-1	Manufacturing	12.29%	5.01%
1994-2	Finance	4.08%	-1.68%	2006-2	Utilities	5.89%	-0.58%
1994-3	Health	13.80%	4.61%	2006-3	Health	8.44%	3.50%
1994-4	Business Equipment	7.64%	-1.39%	2006-4	Energy	11.65%	7.11%
1995-1	Finance	12.19%	8.04%	2007-1	Utilities	9.19%	1.93%
1995-2	Business Equipment	22.61%	7.25%	2007-2	Energy	14.14%	5.96%
1995-3	Finance	15.91%	7.51%	2007-3	Chemicals	9.36%	0.67%
1995-4	Health	12.45%	4.58%	2007-4	Utilities	6.90%	-3.20%
1996-1	Chemicals	11.81%	5.46%	2008-1	Chemicals	-3.44%	-8.99%

2008-2	Energy	18.91%	-3.72%	2012-2	Telecom	7.63%	-4.10%
2008-3	Health	2.23%	-9.38%	2012-3	Telecom	10.24%	5.86%
2008-4	Health	-11.93%	-23.22%	2012-4	Consumer Durables	17.18%	0.33%
2009-1	Business Equipment	2.58%	-11.75%	2013-1	Health	16.69%	11.20%
2009-2	Consumer Durables	43.75%	15.62%	2013-2	Consumer Durables	13.27%	2.53%
2009-3	Consumer Durables	25.31%	15.58%	2013-3	Consumer Durables	14.71%	5.79%
2009-4	Consumer Durables	14.72%	6.32%	2013-4	Finance	12.53%	8.82%
2010-1	Consumer Durables	16.24%	5.59%	2014-1	Utilities	8.47%	1.33%
2010-2	Utilities	-4.32%	-11.22%	2014-2	Energy	11.01%	4.48%
2010-3	Consumer Durables	18.31%	12.35%	2014-3	Health	5.01%	-1.71%
2010-4	Consumer Durables	26.22%	11.20%	2014-4	Shops	12.48%	4.32%
2011-1	Energy	17.31%	5.08%	2015-1	Health	6.85%	0.67%
2011-2	Health	6.47%	0.36%	2015-2	Finance	5.24%	-0.85%
2011-3	Utilities	-1.84%	-16.40%	2015-3	Consumer Non-Durables	-0.21%	-8.86%
2011-4	Energy	17.73%	11.69%	2015-4	Chemicals	10.80%	4.65%
2012-1	Finance	21.97%	10.72%				

Panel B: Bottom industry performers

Panel B reports the bottom performer Fama-French twelve industry on a quarterly basis, over the 1985-2015 period.

y-q	Fama-French twelve industry	Quarterly return	Mean other Industries return				
				1995-3	Energy	1.97%	8.78%
				1995-4	Business Equipment	-3.49%	6.03%
				1996-1	Telecom	-1.79%	6.69%
1985-1	Consumer Durables	2.40%	11.35%	1996-2	Chemicals	-0.46%	4.62%
1985-2	Business Equipment	-1.71%	8.70%	1996-3	Telecom	-8.00%	2.60%
1985-3	Shops	-8.72%	-3.93%	1996-4	Shops	-2.67%	6.98%
1985-4	Energy	3.30%	18.34%	1997-1	Business Equipment	-4.32%	1.09%
1986-1	Energy	-6.05%	16.65%	1997-2	Utilities	5.46%	16.31%
1986-2	Consumer Durables	-1.74%	7.67%	1997-3	Chemicals	1.64%	9.80%
1986-3	Shops	-15.70%	-6.68%	1997-4	Business Equipment	-10.96%	4.63%
1986-4	Finance	0.20%	5.20%	1998-1	Utilities	4.81%	13.91%
1987-1	Utilities	5.17%	22.94%	1998-2	Manufacturing	-3.56%	2.73%
1987-2	Utilities	-2.92%	4.72%	1998-3	Finance	-21.28%	-10.34%
1987-3	Utilities	-0.39%	6.95%	1998-4	Energy	0.56%	20.50%
1987-4	Shops	-28.70%	-22.03%	1999-1	Utilities	-11.33%	2.75%
1988-1	Business Equipment	-0.23%	8.63%	1999-2	Health	-3.32%	9.02%
1988-2	Health	0.38%	7.03%	1999-3	Finance	-15.40%	-6.70%
1988-3	Business Equipment	-10.43%	1.32%	1999-4	Utilities	-7.65%	13.04%
1988-4	Finance	-2.95%	3.02%	2000-1	Chemicals	-20.82%	2.53%
1989-1	Business Equipment	-2.62%	8.13%	2000-2	Consumer Durables	-14.16%	-0.35%
1989-2	Consumer Durables	4.10%	8.97%	2000-3	Telecom	-12.03%	5.59%
1989-3	Business Equipment	2.67%	10.61%	2000-4	Business Equipment	-34.65%	2.22%
1989-4	Consumer Durables	-8.16%	1.35%	2001-1	Business Equipment	-25.47%	-6.09%
1990-1	Telecom	-10.09%	-1.94%	2001-2	Telecom	-1.84%	6.50%
1990-2	Energy	0.02%	6.29%	2001-3	Business Equipment	-34.57%	-11.53%
1990-3	Consumer Durables	-26.01%	-14.83%	2001-4	Telecom	-1.24%	12.47%
1990-4	Energy	-4.75%	10.05%	2002-1	Telecom	-9.08%	5.09%
1991-1	Utilities	5.16%	17.65%	2002-2	Business Equipment	-25.46%	-9.68%
1991-2	Business Equipment	-8.25%	0.97%	2002-3	Business Equipment	-25.45%	-16.09%
1991-3	Consumer Durables	-5.15%	6.55%	2002-4	Shops	0.26%	7.97%
1991-4	Energy	-3.97%	8.52%	2003-1	Consumer Durables	-10.65%	-3.64%
1992-1	Health	-13.02%	2.10%	2003-2	Chemicals	5.52%	17.06%
1992-2	Health	-5.72%	1.60%	2003-3	Telecom	-5.96%	4.04%
1992-3	Consumer Durables	-8.81%	3.86%	2003-4	Shops	7.33%	14.08%
1992-4	Energy	-4.20%	8.34%	2004-1	Consumer Durables	-2.36%	2.54%
1993-1	Health	-14.42%	6.76%	2004-2	Telecom	-2.90%	3.02%
1993-2	Consumer Non-Durables	-7.52%	2.08%	2004-3	Business Equipment	-9.70%	-0.15%
1993-3	Health	-2.22%	4.04%	2004-4	Health	3.79%	10.52%
1993-4	Energy	-7.16%	4.16%	2005-1	Consumer Durables	-14.00%	0.18%
1994-1	Health	-10.21%	-3.38%	2005-2	Chemicals	-5.55%	1.86%
1994-2	Consumer Durables	-6.53%	-0.71%	2005-3	Consumer Durables	-2.57%	4.55%
1994-3	Consumer Durables	-1.77%	6.03%	2005-4	Energy	-8.11%	1.49%
1994-4	Shops	-5.01%	-0.24%	2006-1	Utilities	-0.47%	6.17%
1995-1	Consumer Durables	0.36%	9.11%	2006-2	Business Equipment	-8.88%	0.76%
1995-2	Energy	3.22%	9.02%				

2006-3	Energy	-2.59%	4.50%	2011-2	Energy	-5.73%	1.47%
2006-4	Health	1.78%	8.01%	2011-3	Consumer Durables	-31.15%	-13.73%
2007-1	Finance	-2.38%	2.98%	2011-4	Business Equipment	8.11%	12.56%
2007-2	Utilities	0.14%	7.23%	2012-1	Utilities	-0.07%	12.72%
2007-3	Consumer Durables	-6.10%	2.08%	2012-2	Consumer Durables	-19.27%	-1.65%
2007-4	Finance	-11.08%	-1.56%	2012-3	Utilities	2.44%	6.57%
2008-1	Consumer Durables	-15.16%	-7.92%	2012-4	Business Equipment	-4.31%	2.28%
2008-2	Finance	-16.29%	-0.52%	2013-1	Business Equipment	5.69%	12.20%
2008-3	Energy	-26.32%	-6.78%	2013-2	Utilities	-1.52%	3.87%
2008-4	Consumer Durables	-39.56%	-20.71%	2013-3	Consumer Non-Durables	0.78%	7.05%
2009-1	Finance	-22.80%	-9.44%	2013-4	Utilities	3.30%	9.66%
2009-2	Shops	8.22%	18.85%	2014-1	Shops	-2.04%	2.28%
2009-3	Utilities	7.01%	17.24%	2014-2	Finance	1.19%	5.38%
2009-4	Finance	0.61%	7.60%	2014-3	Energy	-9.30%	-0.41%
2010-1	Utilities	-1.96%	7.24%	2014-4	Energy	-13.52%	6.69%
2010-2	Finance	-14.96%	-10.25%	2015-1	Utilities	-3.51%	1.61%
2010-3	Finance	4.97%	13.56%	2015-2	Utilities	-5.56%	0.14%
2010-4	Utilities	3.76%	13.25%	2015-3	Energy	-17.37%	-7.30%
2011-1	Consumer Durables	0.79%	6.58%	2015-4	Utilities	-3.78%	5.97%

Table IA.2: Alternative measures of analyst distraction*Panel A: Analyst level – summary statistics*

Appendix A provides the variable definitions. *Analyst Distraction Top Only* is an alternative measure of analyst distraction that only considers as attention-grabbing the positive extreme return events. *Analyst Distraction Bot Only* is an alternative measure of analyst distraction that only considers as attention-grabbing the negative extreme return events. *Analyst Distraction VW (Value Weighted)* is an alternative measure of analyst distraction for which the effect of attention-grabbing firms on analyst attention is weighted by firm’s capitalization. *Analyst Distraction IERPW (Industry Extreme Return Probability Weighted)* is an alternative measure of analyst distraction for which the effect of attention-grabbing firms on analyst attention is weighted by the inverse of the probability for the firm’s industry to experience extreme returns (attention grabbing events). *Analyst Distraction 17* is based on the Fama-French 17 industry classification instead of the Fama-French 12 industry one. *Analyst Distraction GICS* relies on GICS sector classification instead of the Fama-French 12 industry one.

Variables	Obs.	Mean	Std.	P25	P50	P75
Analyst Distraction	1,110,420	0.03	0.11	0.00	0.00	0.00
Analyst Distraction 17	1,110,420	0.04	0.13	0.00	0.00	0.00
Analyst Distraction GICS	1,110,420	0.02	0.08	0.00	0.00	0.00
Analyst Distraction VW	1,110,420	0.03	0.13	0.00	0.00	0.00
Analyst Distraction IERPW	1,045,723	0.05	0.17	0.00	0.00	0.00
Analyst Distraction Top Only	1,110,420	0.02	0.08	0.00	0.00	0.00
Analyst Distraction Bot Only	1,110,420	0.02	0.08	0.00	0.00	0.00

Panel B: Analyst level – analyst distraction and relative forecast error

Panel B report the results of the regression reported in Table 2, Column 1, using alternative measures of analyst distraction detailed in Panel A. Regressions include firm-quarter fixed effects. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. For brevity, the coefficients on the control variables are not reported. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Analyst Distraction	3.290*** (0.944)						
Analyst Distraction Top Only		4.388*** (1.283)					
Analyst Distraction Bot Only			2.673** (1.239)				
Analyst Distraction VW				2.315*** (0.756)			
Analyst Distraction IERPW					1.672*** (0.627)		
Analyst Distraction 17						1.523* (0.882)	
Analyst Distraction GICS							1.850** (0.910)
Observations	1,110,420	1,110,420	1,110,420	1,110,420	1,045,723	1,110,420	1,110,420
Control Variables of Table 2	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.023	0.023	0.023	0.023	0.031	0.023	0.023
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Analyst Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Firm level – summary statistics

Alternative measures of analyst distraction presented in Panel A that we average at the firm-quarter level. Appendix A provides the variable definitions.

Variables	Obs.	Mean	Std.	p25	p50	p75
Avr. Analyst Distraction	110,578	0.03	0.10	0.00	0.00	0.02
Avr. Analyst Distraction Top Only	110,578	0.02	0.07	0.00	0.00	0.00
Avr. Analyst Distraction Bot Only	110,578	0.02	0.07	0.00	0.00	0.00
Avr. Analyst Distraction VW	110,578	0.04	0.11	0.00	0.00	0.01
Avr. Analyst Distraction IERPW	109,139	0.06	0.16	0.00	0.00	0.02
Avr. Analyst Distraction 17	110,578	0.05	0.11	0.00	0.00	0.06
Avr. Analyst Distraction GICS	110,577	0.02	0.07	0.00	0.00	0.00

Panel D: Firm level – earnings surprises

Panel D report the results of the regression of Table 8, Panel A, Column 4, using alternative measures of average analyst distraction at the firm level. In Column 1, we present the result of the regression as reported in Column 4, Panel A, of Table 8. In Column 2, 3, 4, 5, 6, and 7, we use aggregate measure of analyst distraction based on the *Analyst Distraction Top Only*, *Analyst Distraction Bot Only*, *Analyst Distraction VW*, *Analyst Distraction IERPW*, *Analyst Distraction 17*, and *Analyst Distraction GICS* respectively. Regressions include firm and year-quarter fixed effects. Standard errors are robust to heteroscedasticity and clustered by firms. For brevity, the coefficients on the control variables are not reported. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

Absolute Earnings Surprise	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Avr. Analyst Distraction	0.056** (0.024)						
Avr. Analyst Distraction Top Only		0.073*** (0.030)					
Avr. Analyst Distraction Bot Only			0.053* (0.032)				
Avr. Analyst Distraction VW				0.051** (0.022)			
Avr. Analyst Distraction IERPW					0.040** (0.015)		
Avr. Analyst Distraction 17						0.053** (0.021)	
Avr. Analyst Distraction GICS							0.060* (0.032)
Observations	110,578	110,578	110,578	110,578	109,139	110,578	110,578
Control Variables of Table 9	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.396	0.396	0.396	0.396	0.397	0.397	0.397
Quarter-year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table IA.3: Analyst distraction and signed relative forecast error

This table reports the results of regressions of relative forecast error on analyst distraction plus control variables for a subsample of positive forecast errors and for a subsample of negative forecast errors. Our baseline model is the one of Table 2, Column 1. *Forecast Error* is measured as the difference between the analyst's forecast and the actual one-year-ahead earnings per share value. Column 1 reports the results for the subsample of positive forecast errors. Column 2 reports the results for the subsample of negative forecast errors. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1) Forecast Error >0	(2) Forecast Error <0
Analyst Distraction	2.607** (1.217)	2.376** (1.174)
Forecast Horizon	0.302*** (0.005)	0.487*** (0.005)
Firm Experience	-0.083*** (0.010)	-0.045*** (0.010)
General Experience	-0.022*** (0.005)	-0.016*** (0.005)
Top 10 Brokerage House	0.955*** (0.240)	-3.555*** (0.221)
Portfolio Size	-0.004 (0.018)	0.043*** (0.015)
Nb. Different Industries	0.995*** (0.091)	0.812*** (0.085)
Observations	565,841	502,520
R-squared	0.194	0.243
Firm quarter Fixed Effects	Yes	Yes
Analyst Fixed Effects	Yes	Yes
Firm-analyst Clusters	Yes	Yes

Table IA.4: Quarterly average relative forecast accuracy instead of last forecast within a quarter

This table reports the results of regressions of relative forecast error on analyst distraction plus our usual control variables when we consider all the forecasts issued by an analyst for a given firm and quarter instead of the most recent one within the quarter. Column 1 reports the results of the regression including firm-quarter fixed effects. Column 2 reports the results of the regression including firm-quarter fixed effects and analyst fixed effects. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)
Analyst Distraction	3.953*** (1.043)	1.777** (0.870)
Forecast Horizon	0.371*** (0.002)	0.371*** (0.001)
Firm Experience	-0.025*** (0.007)	-0.002 (0.006)
General Experience	-0.009*** (0.003)	0.009* (0.005)
Top 10 Brokerage House	-1.275*** (0.156)	-0.721*** (0.189)
Portfolio Size	0.104*** (0.011)	0.125*** (0.011)
Nb. Different Industries	0.269*** (0.051)	-0.275*** (0.043)
Observations	1,110,420	1,110,420
R-squared	0.129	0.155
Firm-quarter Fixed Effects	Yes	Yes
Analyst Fixed Effects	No	Yes
Firm-analyst Clusters	Yes	Yes

Table IA.5: Alternative clusters

This table reports the results of regressions of relative forecast error on analyst distraction plus our usual control variables for different standard-error clustering specifications. Column 1 reports our baseline result as reported in Table 2, Column 1 with standard errors clustered by firm and analyst. Columns 2, 3, 4, and 5 report the results of regression where standard errors are clustered by firm, firm-quarter, analyst, and analyst-quarter, respectively. In all the regressions, standard errors are robust to heteroscedasticity. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides the variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)	(3)	(4)	(5)
Analyst Distraction	3.290*** (0.944)	3.290*** (1.055)	3.290*** (1.017)	3.290*** (1.007)	3.290*** (0.942)
Forecast Horizon	0.401*** (0.004)	0.401*** (0.006)	0.401*** (0.005)	0.401*** (0.005)	0.401*** (0.004)
Firm Experience	-0.068*** (0.008)	-0.068*** (0.009)	-0.068*** (0.006)	-0.068*** (0.010)	-0.068*** (0.006)
General Experience	-0.019*** (0.004)	-0.019*** (0.004)	-0.019*** (0.003)	-0.019*** (0.006)	-0.019*** (0.003)
Top 10 Brokerage House	-1.148*** (0.180)	-1.148*** (0.234)	-1.148*** (0.153)	-1.148*** (0.307)	-1.148*** (0.159)
Portfolio Size	0.019 (0.013)	0.019 (0.015)	0.019 (0.012)	0.019 (0.019)	0.019 (0.013)
Nb. Different Industries	0.933*** (0.068)	0.933*** (0.075)	0.933*** (0.069)	0.933*** (0.113)	0.933*** (0.089)
Observations	1,110,420	1,110,420	1,110,420	1,110,420	1,110,420
R-squared	0.023	0.023	0.023	0.023	0.023
Firm-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm-analyst Clusters	Yes	No	No	No	Yes
Firm Clusters	No	Yes	No	No	No
Firm-quarter Clusters	No	No	Yes	No	No
Analyst Clusters	No	No	No	Yes	No
Analyst-quarter Clusters	No	No	No	No	Yes

Table IA.6: Firm-quarter fixed effects vs. demeaning by firm-quarter mean values

This table reports the result our baseline regression (Table 2, Column 1), which include firm-quarter fixed effects in Column 1 and, in Column 2, the same regression but demeaning all the right-hand side variables by their firm-quarter mean value instead of including firm-quarter fixed effects. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

<i>Relative Forecast Error</i>	(1) Firm-quarter fixed effects	(2) Demeaned variables
Analyst Distraction	3.290*** (0.944)	3.290*** (0.854)
Forecast Horizon	0.401*** (0.004)	0.401*** (0.004)
Firm Experience	-0.068*** (0.008)	-0.068*** (0.007)
General Experience	-0.019*** (0.004)	-0.019*** (0.004)
Top 10 Brokerage House	-1.148*** (0.180)	-1.148*** (0.165)
Portfolio Size	0.019 (0.013)	0.019 (0.012)
Nb. Different Industries	0.933*** (0.068)	0.933*** (0.063)
Observations	1,110,420	1,110,420
R-squared	0.129	0.155
Firm-quarter Fixed Effects	Yes	No
Firm-analyst Clusters	Yes	Yes

Table IA.7: Baseline regression using only analysts with identifiable last names in I/B/E/S

This table reports our baseline regression (Table 3, Column 1) restricting the pool of analysts to analysts for which I/B/E/S references a last name in the I/B/E/S detail recommendation file. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

<i>Relative Forecast Error</i>	Sample restricted to individual analysts as identified by their last names on I/B/E/S
Analyst Distraction	2.901*** (1.029)
Forecast Horizon	0.407*** (0.004)
Firm Experience	-0.067*** (0.008)
General Experience	-0.015*** (0.004)
Top 10 Brokerage House	-1.211*** (0.198)
Portfolio Size	-0.024 (0.018)
Nb. Different Industries	0.903*** (0.077)
Observations	974,210
R-squared	0.048
Firm-quarter Fixed Effects	Yes
Firm-analyst Clusters	Yes

Table IA.8: Controlling for differences in return volatility across industries

Panel A: Distribution of attention-grabbing shocks per Fama-French twelve industry

This panel reports the proportion of quarters with attention-grabbing events by industry (Fama-French twelve industry classification), whereby an attention-grabbing event is defined as an extreme positive or negative quarterly return with respect to the other industries.

Fama-French twelve Industries	% quarters with attention-grabbing events (top or bottom performers)
Business Equipment	0.213
Chemicals	0.082
Consumer Durables	0.313
Consumer Non-Durables	0.058
Energy	0.293
Finance	0.118
Health	0.221
Manufacturing	0.046
Other	0.113
Shops	0.151
Telecom	0.187
Utilities	0.203

Panel B: Controlling for industry likelihood to experience extreme returns

This panel reports our baseline regression (Table 2, Column 1) for a subsample (1) of analysts covering at least one firm belonging to industries with a relatively high likelihood to experience extreme returns (likelihood greater than 15% in Panel A) and for a subsample of analysts not covering such firms. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

<i>Relative Forecast Error</i>	Analysts covering at least one industry more likely to experience extreme returns (1)	Analysts not covering industries more likely to experience extreme returns (2)
Analyst Distraction	2.993*** (0.975)	19.885** (10.066)
Forecast Horizon	0.407*** (0.004)	0.334*** (0.010)
Firm Experience	-0.066*** (0.009)	-0.097*** (0.022)
General Experience	-0.016*** (0.004)	-0.029*** (0.010)
Top 10 Brokerage House	-1.037*** (0.201)	-1.911*** (0.490)
Portfolio Size	0.007 (0.014)	0.053 (0.040)
Nb. Different Industries	1.015*** (0.074)	0.678** (0.267)
Observations	935,388	175,032
R-squared	0.045	0.139
Firm-quarter Fixed Effects	Yes	Yes
Firm-analyst Clusters	Yes	Yes

Table IA.9: Dropping firms from shocked industries*Panel A: Analyst-level analysis*

This table reports our baseline regression (Table 2, columns 1 and 5) for a subsample excluding firms from shocked industries. Standard errors are robust to heteroscedasticity and clustered by firm and analyst. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

<i>Relative Forecast Error</i>	(1)	(2)
Analyst Distraction	4.417*** (0.947)	
Analyst Distraction Dummy		1.067*** (0.393)
Observations	917,873	917,873
Controls	Yes	Yes
R-squared	0.023	0.023
Firm-quarter Fixed Effects	Yes	Yes
Firm-analyst Clusters	Yes	Yes

Panel B: Firm-level analysis

This table reports our firm-level baseline regression (Table 8, Panel A, column 4 and Table 8, Panel B, column 2) for a subsample excluding firms from shocked industries. Standard errors are robust to heteroscedasticity and clustered by firm. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

	(1) Absolute earnings surprise	(2) Amihud Illiquidity
Avg. Analyst Distraction	0.098*** (0.028)	0.035** (0.017)
Observations	79,799	36,692
Analyst controls	Yes	Yes
Firm controls	Yes	Yes
R-squared	0.353	0.629
Year-quarter Fixed Effects	Yes	No
Year Fixed Effects	No	Yes
Firm Fixed Effects	Yes	Yes
Firm Clusters	Yes	Yes

Table IA.10: Controlling for institutional investor distraction

This table reports our firm-level baseline regression (Table 8, Panel A, column 4 and Table 8, Panel B, column 2) including as control variable institutional investor distraction, computed following Kempf et al. (2017). We exclude firms from shocked industries. The institutional investor distraction measure is computed quarterly for (1) and aggregated to a yearly level for (2). Standard errors are robust to heteroscedasticity and clustered by firm. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix A provides variable definitions.

	(1) Absolute earnings surprise	(2) Amihud Illiquidity
Avg. Analyst Distraction	0.084*** (0.027)	0.034** (0.017)
Institutional Investor Distraction	-1.509*** (0.207)	0.081** (0.035)
Observations	79,799	36,692
Analyst controls	Yes	Yes
Firm controls	Yes	Yes
R-squared	0.353	0.629
Year-quarter Fixed Effects	Yes	No
Year Fixed Effects	No	Yes
Firm Fixed Effects	Yes	Yes
Firm Clusters	Yes	Yes