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

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## Abstract
We construct a distraction measure based on extreme industry returns to gauge whether analysts' attention is away from certain stocks under coverage. We find that temporarily distracted analysts make less accurate forecasts, revise forecasts less frequently, and publish less informative forecast revisions, relative to undistracted 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 · Effort allocation · Analyst · Forecasts · Information environment

**JEL classification** G10 · G11 · G14 · G41

## 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 et al. 2017; Kothari et al. 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. Recent findings in behavioral finance and economics

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also underline how cognitive constraints, such as limited attention affect decision-making by economic agents (Falkinger 2008). In the analyst forecast setting, these cognitive constraints occur because the analysts' attention is a scarce resource. Therefore, how analysts allocate their limited attention to process information when forecasting will likely affect the properties of the forecasts. 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 et al. (2017), who focus on institutional investors, and motivated by Barber and Odean (2008) and Kacperczyk et al. (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 of 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 (stock A) belongs to an industry affected by extreme returns, while the others do not. In this case, we conjecture that, if attention is a limited resource, the analyst will shift attention away from the stocks in the unaffected industries and toward the attention-grabbing stock A. To capture this shift, our empirical approach defines a measure of analyst distraction at the analyst-firm-quarter level. For each stock under coverage, this measure 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 analyst distraction offers three advantages. First, it is plausibly exogenous to the economics of the stocks from which the analysts will be considered distracted; thus, it complements strategic factors, like the stock's importance to institutional investors, that have been shown to affect the analyst's effort allocation (Driskill et al. 2020; Harford et al. 2019). 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 toward 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, distracted analysts will be benchmarked against those of control analysts who follow the same stock but 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 our ability to define our distraction proxy. However, we aim to overcome this challenge by using broad industry returns, as analyst coverage universes are not always perfectly aligned with industry classification standards based on SIC or GICS codes.[1] Second, our proxy's ability 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 do 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 et al. 2019). In doing so, senior analysts will allocate their attention across the stocks under coverage and potentially resort to more heuristic behavior for those stocks not subject to attention-grabbing events.[2]

We predict that distracted analysts will issue less accurate forecasts for the stocks they are distracted from. To test this prediction, we rely on a sample of 1,110,420 street earnings forecasts spanning 128 quarters during the1985–2015 period. These 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 drawn from the I/B/E/S detail file, 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, that is, 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. To put this finding in perspective, the effect is larger than the average impact of being employed by a top-decile brokerage firm and it compares with the ones of other recently uncovered determinants of forecast accuracy using a similar setting (e.g., Bradley et al. 2017a; Harford et al. 2019; Fang and Hope 2021).[3] We strengthen this initial finding using a cross-sectional test that considers coverage universe size and find, broadly speaking, that analysts who are responsible for a larger coverage universe temporarily reallocate their effort toward 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

[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. We find that our results are unaffected by this choice.

[2] In untabulated tests, we find that our results hold if we restrict our sample to analysts we can identify by their last name on I/B/E/S and analysts who presumably are less likely to be part of a team.

[3] Bradley et al. (2017a) find that analysts with previous industry experience issue earnings forecasts that are on average 1.6% more accurate than forecasts issues by analysts lacking this experience. Harford et al. (2019) find that analysts issue earnings forecasts that are on average 1.9% more accurate for firms for which they have high career concerns versus other firms. Finally, Fang and Hope (2021) show that analyst teams generate more accurate earnings forecasts than individual analysts (with a low-bound estimate of 2.6%). In additional unreported analyses, we find that distraction also affects the *rank* of analysts in terms of forecast error for a given firm-quarter. Being distracted leads to a decrease in rank by one notch for 42% of the distribution.

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 one-quarter lead and lag analyst-firm distraction measures. Our results indicate that only contemporaneous distraction harms analyst forecast accuracy, which suggests that our main estimation obeys the parallel trends 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 whether analysts learn from distraction experiences. It builds on the literature showing that the first experience of an unusual event affects agents' decision-making more than subsequent experiences (e.g., Bourveau and Law 2021; Dessaint and Matray 2017). We find that that the effect of analyst distraction on forecast properties manifests itself only when the analyst experiences a 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 distractions.

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 their forecast revision frequency (e.g., Jacob et al. 1999; Groysberg et al. 2011; Harford et al. 2019; Merkley et al. 2020). We find that, on average, distracted analysts revise their forecasts significantly less often than nondistracted ones covering the same stock during the same quarter, consistent with limited attention affecting their allocation of effort.

Second, we investigate whether distracted analysts produce less informative forecasts than do nondistracted ones, 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 revisions being less informative. Overall, the results of this second additional analysis are consistent with the idea that limited attention affects the ability of analysts to gather and process information and release informative opinions.

After documenting the effects of distraction on analysts' effort allocation and forecast properties, we examine, in the second part of the paper, 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, studies have shown that the intensity of analyst coverage influences firms' information environments.[4] We therefore examine whether 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

[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 et al. (2014).

forecasts being worse, we find that firms covered by more distracted analysts experience larger earnings surprises. This finding suggests that the consensus for these stocks omitted information to process for investors at the earnings announcement (Core et al. 2006). Next, building on the link between analyst coverage and information asymmetry, we examine the relation between analyst distraction and information asymmetry in financial markets (e.g., Kelly and Ljungqvist 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 stocks that are covered by more distracted analysts during a given quarter. Importantly, this finding is consistent with the notion that limited analyst attention affects 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 research has considered factors related not only to analysts' strategic incentives but also to their behavioral biases.[5] Our paper contributes to the literature on 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 previously unexplored explanation for analyst forecasting performance.

Our paper closely relates to but is distinct from three recent studies that examine the role of limited analyst attention. Pisciotta (2021) finds that analysts involved in the underwriting 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 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, decision fatigue over the course of the day leads to a decrease in their forecast accuracy and an increase in reliance on heuristics in forecasting. Importantly, these three studies consider settings where analysts can anticipate an attention-allocation challenge induced by an increasing workload on a particular day. As a result, the limited attention these studies find occurs because analysts voluntarily

[5] The nonbehavioral factors considered in the literature include the analyst's forecasting experience (e.g., Clement 1999), the coverage portfolio complexity (e.g., Clement 1999), the prestige of the brokerage (e.g., Clement 1999), the geographical location (e.g., Malloy 2005; O'Brien and Tan 2015), the analyst's industry expertise (Bradley et al. 2017a), the analyst's career concerns (e.g., Hong and Kubik 2003; Harford et al. 2019), the analyst's cultural background (e.g., Du et al. 2017; Merkley et al. 2020), and the changing business model of sell-side research (Drake et al. 2020).

[6] Examples of this literature include a focus on attribution bias (Hilary and Menzly 2006), anchoring bias (Cen et al. 2013), seasonal affective disorder (Lo and Wu 2018), weather-induced inactivity (DeHaan et al. 2017), availability heuristic (Bourveau and Law 2021), and the affect heuristic (Antoniou et al. 2021). 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 et al. 2016) in permanently shaping analysts' future forecasting toward conservative forecasts.

and strategically choose to allocate their attention primarily to stocks that offer immediate potential for reward (Driskill et al. 2020; Hirshleifer et al. 2019).[7]

Since we examine a setting where analyst distraction follows from an exogenous attention-grabbing surprise, our paper also complements Dong and Heo (2014), who show that analysts have limited attention when the region where they live experiences flu epidemics, also an exogenous factor. However, our setup differs since 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 6 percent of our analyst-firm-quarter observations correspond to extreme attention-grabbing events. Our findings are consistent with those of Han et al. (2020), who show that, under conditions of climate disaster, analysts strategically allocate their scarce attention to firms of greater importance. However, our findings differ from theirs, since we find evidence that supports the role of resource constraints in our setting, which allows us to study analysts' behavior and performance during their normal course of work, rather than under special circumstances.

Second, we contribute more broadly to the literature on analysts' strategic effort allocation. Hong and Kubik (2003) and Harford et al. (2019), among others, find that analysts *permanently* provide more accurate, frequent, and informative earnings forecast revisions and issue stock recommendation changes with greater information content for firms deemed important for their careers. We complement these findings by illuminating a mechanism that explains how analysts *temporarily* allocate their effort across stocks in their coverage universe as a function of attention-grabbing events, thereby hampering the forecast properties of non-attention-grabbing stocks. Chiu et al. (2021) show that analysts issue more timely forecasts when abnormal institutional attention is high on the earnings announcement day. Unlike us, they do not document effects on forecast accuracy or on the informativeness of analyst forecasts. They focus on the consequences for the analyst's career, whereas we document the consequences of analyst distraction for firms' information environment.

Third, our paper contributes to the literature on the role of distraction in financial markets. Previous work documents the consequences of investors' distraction for managers' investment choices (Kempf et al. 2017), disclosure behavior (Abramova et al. 2020), the scheduling and timing of earnings announcements (deHaan et al. 2015), and earnings management (Garel et al. 2021). We complement these findings by documenting that distraction also affects analysts' forecast accuracy. Importantly, our results provide evidence on a learning mechanism in this 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. Studies document the effect of exogenous economic shocks on externalities in financial markets (e.g., Foucault et al. 2013). For example, Dessaint et al. (2018) find that noise (i.e., nonfundamental drops) in the stock price of product-market peers leads firms to suboptimally decrease their investment. Schneemeier (2018) shows that, if managers

[7] Hirshleifer et al. (2019) explicitly discuss the *nonrandom* ranking rule that analysts potentially use to allocate effort on days with multiple forecasts and conclude that it could be consistent with findings on decision fatigue.

exhibit both limited ability to filter out noise in prices and limited attention to stock prices, then nonfundamental shocks to a firm's stock price could also affect the investment of fundamentally unrelated firms. Our evidence suggests that exogenous economic shocks have an information spillover effect via analyst information production. Specifically, when analysts shift their attention away from stocks unaffected by attention-grabbing return events, the information environment of those stocks suffers.

## 2 Analyst distraction and analyst forecast performance

### 2.1 Measuring analyst distraction

We begin our empirical analysis by observing that financial analysts have limited attention, time, and resources. Thus, they must choose how to allocate their attention as they collect and analyze information across the firms in their coverage universe.[8] Some of the attention allocation will be guided by factors such as their involvement in the 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, 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 toward some firms under coverage and away from others, giving the latter a lower-than-optimal level of attention. That is, we introduce the possibility that sometimes 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*, which captures to what degree an analyst who follows 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. Specifically, for an analyst *(i)* following a firm *(f)* in quarter *(q),* we define analyst distraction as follows.

$$Analyst\ Distraction_{i,f,q} = \sum\nolimits_{IND \neq IND_f} \omega_{iq}^{IND} \times IS_q^{IND} \tag{1}$$

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 Eq. (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 $IND_f$.

[8] We focus on analysts' limited attention for two reasons. First, anecdotal evidence and recent academic work have suggested that analysts do not always exert the same effort for all of their stocks under coverage, and that this level of effort often relates to the market behavior of those stocks. Analysts realize that, when they publish their notes and forecasts, those pertaining to stocks that have recently exhibited noticeable market behavior in terms of returns or trading will typically receive most of the attention from their internal and external clients. Second, our focus on analyst distraction is a natural extension of recent work in finance that documents the role of distraction in the context of institutional investors.

Motivated by the work of 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 this choice, other papers identify extreme return periods as periods when learning about uncertainty can be particularly beneficial, leading analysts to pay more attention to firms experiencing extreme returns (e.g., Kacperczyk et al. 2016).

$\omega_{iq}^{IND}$ in Eq. (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 and 100 percent, and a higher number indicates that the analyst is more likely to shift attention away from firm $f$ toward 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 complement our continuous measure of analyst distraction, we also create an indicator variable, *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.[11] Thus, *Analyst Distraction* is a plausible proxy to identify exogenous shocks to analyst attention. Appendix Table 13 presents descriptive statistics on extreme quarterly returns across 12 Fama–French industries. Panel A in the Appendix Table 13 provides sample-wide information on both top and bottom extreme quarterly returns. However, this table hides significant time series variation in both measures. Therefore, Panels B and C in the Appendix Table 13 show, per quarter, the top and bottom industry returns and the average across the other industries. On average, the top performing quarterly returns are more than six times larger than the average return across the other eleven industries. This difference is sizeable and arguably large enough to distract the analyst.

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

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

[11] Our measure of analyst distraction is based on extreme industry-wide returns rather than on extreme returns for individual stocks. Therefore, while shared analyst coverage may create firm connections (e.g., Ali and Hirshleifer 2020), our measure of analyst distraction remains plausibly exogenous to the fundamentals of the stocks for which the analysts will be considered distracted.

### 2.2 Analyst forecast properties

Our empirical analyses compare the forecast performance of distracted analysts to that of nondistracted analysts. We use one-year-ahead so-called street earnings forecasts obtained from the I/B/E/S detail files to be consistent with recent analyst studies (e.g., Bradley et al. 2017a; Harford et al. 2019). We focus on one-year-ahead earnings for several reasons. First, from a data availability standpoint, we observe that the frequency of one-year-ahead EPS forecasts allows us to maximize the sample size and within-firm-quarter variations in forecast error. Second, conceptually, we believe that one-year-ahead EPS forecasts receive the most attention from analysts. Consistent with this assumption, a study by Bradshaw et al. (2012) finds that, on average, naïve extrapolation of one-year-ahead EPS forecasts outperforms two-year-ahead and three-year-ahead analysts' forecasts.[12]

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.

$$AFE_{ijt} = Absolute\left(Forecast\ EPS_{ijt} - Actual\ EPS_{ijt}\right) \tag{2}$$

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

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, thereby controlling for differences across companies, time, and industries (Ke and Yu 2006).[13]

We focus on earnings because anecdotal evidence shows that the analyst compensation is tied primarily to the accuracy of EPS forecasts (as opposed to non-earnings metrics). Moreover, based on survey evidence, one of analysts' primary motivations

[12] In unreported tests, we find that our main results are robust to using two-year-ahead EPS forecasts. Admittedly, they are more sensitive to the inclusion of fixed effects, presumably because our panel data become less balanced (~30% smaller). We cannot run a similar analysis for three-year-ahead and beyond EPS forecasts, because the number of unique analyst forecasts for the same firm-quarter becomes too small, making it hard to identify the effect of distraction.

[13] 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. The measure does become meaningless when analyst coverage of the firm is equal to one. Therefore, we exclude from the sample firms covered by fewer than two analysts in a given quarter.

for issuing accurate earnings forecasts is to use them as inputs to their own stock recommendations (Brown et al. 2015). We also observe that, across brokerages, EPS metrics feature prominently on the front pages of notes (while other metrics do not show up consistently).

We complement our baseline analyses by considering two alternative dependent variables.[14] 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; Groysberg et al. 2011; Healy and Palepu 2001; Harford et al. 2019). 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).[15] We follow the literature and restrict the sample to earnings forecasts with a horizon between one and 12 months (e.g., Clement 1999; Clement et al. 2007; Harford et al. 2019).[16] 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 nonmissing SIC code in Compustat. 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.[17]

Starting from this initial sample, we retain observations for which we have nonmissing 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 provides us with a baseline sample of 1,110,420 analyst forecasts spanning 128 quarters (the 1985–2015 period). These forecasts are issued by 11,622 unique analysts and correspond to 58,932 unique end-of-the-year earnings announcements for 8,496 unique firms listed on U.S. stock exchanges.[18]

[14] We focus on one-year-ahead earnings forecasts as opposed to multi-year forecasts not only to maximize our sample size but also because these forecasts likely receive the most attention from analysts. Consistent with this assumption, Bradshaw et al. (2012) find that, on average, naïve extrapolations of one-year-ahead EPS forecasts outperform two-year-ahead and three-year-ahead analysts' forecasts.

[15] We use several initial rules to drop observations from the sample: 1) observations for which the variable *cusip* is equal to "00,000,000" or missing; 2) observations with missing values for the variables *ticker* and *analys*; 3) observations for which the forecast date (*anndats*) is posterior to the 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*) is missing.

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

[17] We are grateful to Kenneth French for sharing this data on his website.

[18] Within the sample, we winsorize the forecast accuracy, the accounting, and the continuous market control variables at the 1$^{st}$ and 99$^{th}$ percentiles.

### 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 nondistracted ones. To formally test this prediction, we use a multivariate OLS regression model with *PMAFE* as the 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.

$$PMAFE_{i,j,t} = \beta_0 + \beta_1 \left( Analyst\ Distraction_{i,j,t}\ or\ Analyst\ Distraction\ dummy_{i,j,t} \right) + \beta' X_{i,j,t} + \gamma_i \times \theta_t + \varepsilon_{i,j,t} \tag{4}$$

$X_{i,j,t}$ 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 et al. 1997; Clement 1999; Clement and Tse 2003; Clement et al. 2003; Clement et al. 2007). Appendix Table 11 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. In particular, they absorb the effect of institutional investor distraction, ensuring that, while analysts may cater to institutional investors, any effect of analyst distraction cannot be driven by institutional investors being themselves distracted and paying less attention to some companies. Including firm-quarter fixed effects allows us to examine 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.[19] In all analyses, standard errors are doubled clustered at the firm and analyst level.[20]

Table 1 provides summary statistics for our main analyst and forecast variables. Distractions are rare, as only 6 percent of analyst-firm-quarter observations exhibit distraction levels above 20 percent; that is, more than 20 percent of firms in an analyst's portfolio are affected by attention-grabbing events in unrelated industries. The summary statistics for the analyst and forecast characteristics are in line with the literature (e.g., Clement and Tse 2005; Clement et al. 2007; De Franco and Zhou 2009; Bradley et al. 2017b; Harford et al. 2019). 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)

[19] An alternative approach to controlling for firm-year fixed effects is to adjust 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 this may produce inconsistent estimates and distort the results. They 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.

[20] Double clustering at the firm and analyst level follows the common approach in the literature (e.g., Bradley et al. 2017a; Harford et al. 2019). In unreported tests, we check that our results are robust if we implement different clustering of standard errors.

and covering the typical firm in our sample for about two years (seven 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 two-digit SIC code industries at a given quarter. Fifty-eight 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 nondistracted analysts.[21] Economically, the coefficient in Model 5 suggests that distracted analysts issue earnings forecasts that are on average 1.4 percent less accurate.[22] 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. This effect also compares with those of other recently uncovered determinants of forecast accuracy in similar settings (e.g., Bradley et al. 2017a; Harford et al. 2019; Fang and Hope 2021).

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 we control for analyst or analyst-quarter fixed effects, earnings forecasts issued by distracted analysts are less accurate than those issued by nondistracted ones. 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 fixed effects, since Cowen et al. (2006) find that analysts' forecast optimism varies across brokerages. Our findings remain unchanged, consistent with differences across brokerages are not driving the observed effect of analyst distraction on earnings forecast accuracy.[23]

[21] In untabulated tests, we find similar results when we exclude from our sample analysts covering stocks concentrated exclusively in one industry. In our main tests, these analysts serve as a benchmark only. Indeed, when they do experience extreme returns, they have, by definition, no stocks in their portfolio that would receive our "distraction treatment.".

[22] Following the recommendation of Mummolo and Peterson (2018) and DeHaan (2021), we also compute the standard deviation of analyst distraction shock after residualizing it with respect to firm-quarter fixed effects. The economic effect of the distraction shocks on the forecast accuracy of analysts becomes smaller (about 1%) when we consider the likelihood of finding distracted analysts within a given firm-quarter.

[23] In unreported analyses, we also estimate the models with a stricter fixed effect structure by including firm-quarter and firm-analyst fixed effects. Our results remain qualitatively similar, but the level of significance drops to 10% or 8% for the coefficients on the continuous and the dummy distraction variables. We do not prefer this stricter FE structure, as we want to avoid adopting a specification that creates a lot of zero within-unit variation. In his section 1.2., DeHaan (2021) cautions against creating so-called zero-variation firms, as these zero-variation firms do not contribute to the estimation of the coefficients of interest; therefore, only a subset of the entire sample maps into the coefficient estimation. There is a risk then that these contributing observations differ from the dropped observations.

We also observe that the coefficients on the control variables in Eq. (4) obtain their expected signs in line with the 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 who 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 the first, we examine whether the attention constraints are more binding and whether the effect of analyst distraction on forecast accuracy is larger when analysts cover larger universes. Intuitively, when analysts cover more firms, their attention will be more dispersed; therefore, attention to each stock under coverage potentially becomes 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 who cover more firms.[24]

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.[25] 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

[24] As an illustration, consider an analyst covering two firms, A and B, in a given quarter. If firm A is affected by an attention-grabbing shock during the quarter, by construction analyst distraction is equal to 50 percent for the analyst's forecasts for firm B. Intuitively, however, when an analyst covers two (or a low number of) firms, the attention-grabbing stock(s) will shift attention toward firm A, but the analyst is still likely to be able to dedicate enough time and resources to firm B.

[25] Untabulated statistics show that an industry experiences extreme returns over two consecutive quarters (quarters $q$ and $q+1$) only 10 percent of the time, and over three consecutive quarters (quarters $q$, $q+1$ and $q+2$) only 1 percent of the time. We thus expect the distraction shocks to vary significantly from one quarter to the other and affect the analyst information production in a specific quarter in a timely fashion.

**Table 1** Summary statistics

| Variables | No. 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 | 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 |

This table reports the descriptive statistics of the analyst and forecast variables. Appendix Table 11 provides the variable definitions

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

Our third additional analysis explores the effect of analyst learning 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 is repeatedly distracted. To implement this test, we create an indicator variable that equals one if the distraction event is the *first* significant distraction experienced by a particular analyst-firm pair during our sample period (i.e., *Analyst Distraction* is greater than or equal to 20 percent).[26]

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

**Table 2** Analyst distraction and forecast accuracy

| Relative Forecast Error | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Analyst Distraction | 3.290*** | 1.848** | 2.471** | 2.173** | | | | |
| | (0.944) | (0.873) | (1.129) | (0.863) | | | | |
| Analyst Distraction Dummy | | | | | 1.426*** | 0.779** | 1.497*** | 1.002*** |
| | | | | | (0.391) | (0.362) | (0.535) | (0.357) |
| Forecast Horizon | 0.401*** | 0.418*** | 0.437*** | 0.413*** | 0.401*** | 0.418*** | 0.436*** | 0.413*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Firm Experience | −0.068*** | −0.033*** | −0.040*** | −0.056*** | −0.073*** | −0.043*** | −0.048*** | −0.056*** |
| | (0.008) | (0.008) | (0.008) | (0.007) | (0.007) | (0.008) | (0.008) | (0.007) |
| General Experience | −0.019*** | −0.010 | −0.031*** | −0.001 | −0.019*** | 0.001 | −0.028*** | −0.001 |
| | (0.004) | (0.006) | (0.009) | (0.004) | (0.004) | (0.006) | (0.009) | (0.004) |
| Top 10 Brokerage | −1.148*** | −0.646*** | −1.140*** | −0.575*** | −1.060*** | −0.577*** | −1.288*** | −0.574*** |
| | (0.180) | (0.218) | (0.388) | (0.212) | (0.165) | (0.218) | (0.388) | (0.212) |
| Portfolio Size | 0.019 | 0.050*** | 0.104*** | 0.038*** | 0.022* | 0.045*** | 0.112*** | 0.039*** |
| | (0.013) | (0.014) | (0.024) | (0.013) | (0.012) | (0.014) | (0.024) | (0.013) |
| Nb. Different Industries | 0.933*** | 0.306*** | 0.362** | 0.500*** | 0.947*** | 0.327*** | 0.416*** | 0.501*** |
| | (0.068) | (0.064) | (0.141) | (0.063) | (0.063) | (0.064) | (0.141) | (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 |

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 the 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 in the analyst's portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than 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 fixed effects. In Columns 5 to 8, we repeat regressions 1 to 4 replacing *Analyst Distraction* with *Analyst Distraction Dummy*. Standard errors are robust to heteroscedasticity and doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

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

| Relative Forecast Error | (1) Below-median Portfolio Size | (2) Above-median Portfolio Size | (3) Below-median Portfolio Size | (4) Above-median Portfolio Size |
|---|---|---|---|---|
| Analyst Distraction | 3.015*** | 6.778*** | | |
| | (1.127) | (1.373) | | |
| Analyst Distraction Dummy | | | 1.320*** | 2.168*** |
| | | | (0.460) | (0.584) |
| Forecast Horizon | 0.398*** | 0.403*** | 0.398*** | 0.403*** |
| | (0.005) | (0.006) | (0.005) | (0.006) |
| Firm Experience | −0.057*** | −0.074*** | −0.057*** | −0.074*** |
| | (0.011) | (0.011) | (0.011) | (0.011) |
| General Experience | −0.024*** | −0.011** | −0.024*** | −0.011** |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| Top 10 Brokerage | −0.334 | −1.776*** | −0.334 | −1.793*** |
| | (0.234) | (0.253) | (0.234) | (0.253) |
| Portfolio Size | −0.152*** | 0.081*** | −0.150*** | 0.082*** |
| | (0.025) | (0.016) | (0.025) | (0.016) |
| Nb. Different Industries | 0.796*** | 1.059*** | 0.798*** | 1.073*** |
| | (0.103) | (0.086) | (0.103) | (0.086) |
| Observations | 566,193 | 544,227 | 566,193 | 544,227 |
| R-squared | 0.15 | 0.16 | 0.15 | 0.16 |
| Firm-quarter Fixed Effects | Yes | Yes | Yes | Yes |
| P-value of the Wald Test of Coefficient Equality | (1) vs (2): 0.016 | | (3) vs (4): 0.125 | |

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- 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 the 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 in the analyst's portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than 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 doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

Table 5 reports our results. As a benchmark, Model 1 repeats the earlier results 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), the results show that distraction affects the forecasts only the first time the analyst experiences distraction for a given stock. When analysts are distracted a second time (or more), their forecasts do not appear to

**Table 4** Timing of the effect of analyst distraction

| Relative Forecast Error | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Analyst Distraction Dummy | 1.426*** | 1.922*** | | |
| | (0.391) | (0.670) | | |
| Lagged Analyst Distraction Dummy | | 0.259 | | |
| | | (0.674) | | |
| Future Analyst Distraction Dummy | | −0.711 | | |
| | | (0.680) | | |
| Analyst Distraction | | | 3.290*** | 3.445** |
| | | | (0.941) | (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 |

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 the 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 in the analyst's portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than 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 an analyst distraction dummy variable (discrete variable) plus control variables and 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 doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

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.

The 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 economic agents. For example, Dessaint and Matray (2017) study managers' reaction to salient risks and find that managers of firms unaffected by 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, consistent with analysts learning from distractions and their underperformance relative to their peers for non-affected stocks.[27]

## 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, that is, 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 the 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 nondistracted ones who cover the same firm in the same quarter. The coefficient in Model 2 shows that distracted analysts update their forecasts five percent less often than nondistracted analysts. To put this magnitude in perspective, the effect is equivalent to a decrease in the analyst's coverage portfolio size by about nine firms.[28]

### 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 that of forecast revisions produced by nondistracted 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 analysts

[27] Descriptive statistics reported in Table 1 show that the fraction of forecasts made by analysts affected by first-distraction events is roughly equal to the one made by analysts affected by nonfirst-distraction events (about 3% in each case). Our results on first-time distraction are therefore unlikely to be driven by the relative scarcity of nonfirst distraction events.

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

**Table 5** First-time distraction and analyst forecast accuracy

| Relative Forecast Error | (1) | (2) | (3) |
|---|---|---|---|
| Analyst Distraction Dummy | 1.426*** | | |
| | (0.391) | | |
| First Distraction Event | | 1.714*** | 0.917** |
| | | (0.473) | (0.439) |
| Not-first Distraction Event | | 0.698 | 0.180 |
| | | (0.522) | (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 |

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 the same quarter. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than 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* identifies those cases where the analyst-firm pair experiences distraction for the first time during our sample period, and *Not-first Distraction Event* identifies the other cases. In Column 3, we add analyst fixed effects to the regression reported in Column 2. Standard errors are robust to heteroscedasticity and doubled clustered at the firm and analyst level. 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 test. Appendix Table 11 provides the variable definitions

issuing new forecasts. Next, we gauge the market reaction to forecasts provided by distracted and nondistracted analysts.

To carry out the first step, we create an indicator variable, *Self-Revision*, which takes the value of one if the forecast of analyst *i* is updated for a given firm in the absence of other analysts issuing forecasts since analyst *i*'s previous forecast. Our intuition is that, when analysts revise a forecast without waiting for other analysts to produce information (in the form of forecasts), this reflects their stock-specific effort of gathering and processing information. The results in 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 the notion that limited attention leads distracted analysts to temporarily allocate more effort to attention-grabbing stocks; therefore, they generate fewer new forecasts for non-attention-grabbing stocks than nondistracted analysts do.

To carry out the second step, we build on the 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 nondistracted

**Table 6** Analyst distraction and forecast revision frequency

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

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 the 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 in the analyst's portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than 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 and 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 and firm-quarter and analyst fixed effects. Standard errors are robust to heteroscedasticity and doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

analysts. To examine this prediction, we estimate a regression model, similar to the one used by Harford et al. (2019) and Bradley et al. (2017a).

$$\begin{aligned} Absolute\ CAR_{i,j,t} = &\beta_0 + \beta_1 \left( Analyst\ Distraction_{i,j,t} \times Absolute\ Forecast\ Revision_{i,j,t} \right) \\ &+ \beta_2 (Absolute\ Forecast\ Revision_{i,j,t}) \\ &+ \beta_3 (Analyst\ Distraction_{i,j,t}) + \beta' X_{i,j,t} + \gamma_i \times \theta_t + \varepsilon_{i,j,t} \end{aligned} \tag{5}$$

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].[29] 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).[30] We focus on the absolute value of the revision since we formulate no expectation about the market reaction as a 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 doubled clustered at the firm and analyst levels.

Table 8 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*×*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 finding that the market perceives forecast revisions issued by distracted analysts to be less informative holds across all specifications.

Taken together, the evidence in Tables 7 and 8 suggests that analysts issue fewer forecast revisions when they are distracted than when they are not and that the market perceives these forecast revisions to be less informative. Overall, these findings are consistent with the idea that limited attention reduces distracted analysts' ability to gather and process information and provide timely, informative forecast revisions to the market.[31]

## 3 The real effects of analyst distraction on firms' information environment

The results from Section 2 show how cognitive distraction harms analysts' outputs by leading distracted analysts to issue less accurate, less frequent, and less informative earnings forecasts. In this section, we explore whether these effects also lead to real consequences for the information environment of covered firms.

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

[30] Like 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.

[31] As mentioned, our findings relate to but differ from the results of Han et al. (2020), as our evidence suggests that resource constraints affect analysts' forecast revision frequency.

**Table 7** Analyst distraction and the likelihood of revising a forecast when other analysts have not produced new information

| Relative Self-Revision Frequency | (1) | (2) | (3) |
|---|---|---|---|
| Analyst Distraction | −28.381*** | | −15.320** |
| | (8.289) | | (7.645) |
| Analyst Distraction Dummy | | −10.932*** | |
| | | (3.412) | |
| Forecast Horizon | 0.023 | 0.023 | 0.101*** |
| | (0.043) | (0.043) | (0.037) |
| Firm Experience | 0.097 | 0.097 | 0.000 |
| | (0.101) | (0.101) | (0.081) |
| General Experience | −0.222*** | −0.222*** | −0.281*** |
| | (0.043) | (0.043) | (0.058) |
| Top 10 Brokerage | 9.264*** | 9.286*** | 3.971* |
| | (1.733) | (1.733) | (2.055) |
| Portfolio Size | 0.475*** | 0.467*** | 0.226* |
| | (0.117) | (0.117) | (0.119) |
| Nb. Different Industries | −3.334*** | −3.376*** | 0.354 |
| | (0.619) | (0.618) | (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 |

This table reports the results of regressions of the relative propensity for an analyst to issue a forecast revision 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 in the analyst's portfolio. *Analyst Distraction Dummy* is a dummy variable that takes the value one if *Analyst Distraction* is greater than or equal to 20% and zero otherwise. Standard errors are robust to heteroscedasticity and doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

### 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.

$$Avg.Analyst\ Distraction_{f,q} = \frac{1}{N_{f,q}} \sum\nolimits_{i=1}^{N_{f,q}} Analyst\ Distraction_{i,f,q} \tag{6}$$

**Table 8** Analyst distraction and the market reaction to forecast revisions

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Market Reaction | 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** | −0.613** | −2.594* | −2.481* | −0.608** | −0.605** |
| | (0.306) | (0.301) | (1.523) | (1.510) | (0.306) | (0.301) |
| Absolute Forecast Revision | 0.221*** | 0.225*** | 1.291*** | 1.280*** | 0.208*** | 0.211*** |
| | (0.042) | (0.041) | (0.086) | (0.085) | (0.042) | (0.041) |
| Analyst Distraction | −0.005 | −0.025 | −0.083 | −0.072 | −0.004 | −0.025 |
| | (0.043) | (0.042) | (0.068) | (0.068) | (0.042) | (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 Fixed Effects | No | No | No | No | Yes | Yes |

This table reports the regression of market reaction, measured as 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 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 Columns 3 and 4, we reproduce regressions 1 and 2, respectively, adding analyst fixed effects. In Columns 5 and 6, we reproduce regressions 1 and 2, respectively, adding day-of-the-week fixed effects. Standard errors are robust to heteroscedasticity and doubled clustered at the firm and analyst level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

$N_{f,q}$ 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. 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. As we did for *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 the literature and define two firm-level information asymmetry measures: absolute earnings surprise and Amihud's (2002) illiquidity measure (e.g., Harford et al. 2019; 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.[32] We adopt the absolute value of the earnings surprise in our main specification, as we focus 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 less than \$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.

$$Absolute\ Earnings\ Suprise_{j,t} = \beta_0 + \beta_1 Avg.Analyst\ Distraction_{j,t} + \beta' Z_{j,t} + \theta_t + \gamma_j + \epsilon_{j,t} \tag{7}$$

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 2 (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

[32] 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 forecasts made within a [−180, −4] day window prior to the earnings announcement date, rounded to the nearest cent (Caskey and Ozel 2017).

leverage, profitability, institutional ownership, and trading volume). Appendix Table 11 provides 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.

We report summary statistics for the firm-level sample over the period of 1985–2015 used in our empirical analysis in Appendix Table 12 Panel A.[33] 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 the findings in Section 2, analyst distraction is uncommon, with fewer than half of the firms in the sample experiencing distraction.

Table 9 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* has a positive and significant coefficient across all specifications. In other words, analyst distraction maps onto higher absolute earnings surprise.[34] Further, across specifications, *Ln(Analyst Coverage)* obtains a negative and significant coefficient, consistent with prior findings that analysts help improve a firm's information environment (e.g., Bradshaw et al. 2017). Overall, 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 for the average analyst characteristics at the firm level or 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(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.

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.

$$Amihud\ Illiquity_{j,t} = \beta_0 + \beta_1 Avg.Analyst\ Distraction_{j,t} + \beta' Z_{j,t} + \theta_t + \gamma_j + \epsilon_{j,t} \tag{8}$$

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

[33] We drop observations for firms with SIC codes 49 and 60–69. Our results remain qualitatively the same if we keep these observations.

[34] 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 insignificant, which further indicates that our effect precisely coincides with the distraction of the analysts covering a given stock.

**Table 9** Real effects of analyst distraction: earnings surprise

| Earnings Surprise | (1) Absolute earnings surprise | (2) Analyst 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*** | 0.073*** | 0.055** | 0.056** | 0.056* | 0.073*** | −0.132** |
| | (0.025) | (0.025) | (0.024) | (0.024) | (0.032) | (0.024) | (0.062) |
| Avg. Firm Experience | | 0.006*** | | 0.005*** | 0.00 | | |
| | | (0.001) | | (0.001 | (0.001) | | |
| Avg. General Experience | | −0.001*** | | −0.001*** | 0.00 | | |
| | | (0.00) | | (0.00) | (0.00) | | |
| Avg. Portfolio Size | | 0.001* | | 0.002** | −0.001 | | |
| | | (0.001) | | (0.001) | (0.001) | | |
| Avg. Nb. Different Industries | | −0.008* | | −0.007* | 0.002 | | |
| | | (0.004) | | (0.004) | (0.005) | | |
| Avg. Top 10 Brokerage | | 0.016 | | 0.02 | 0.001 | | |
| | | (0.014) | | (0.014) | (0.019) | | |
| Consensus Forecast Horizon | | −0.000*** | | −0.000*** | −0.000*** | | |
| | | (0.00) | | (0.00) | (0.00) | | |
| Ln(Analyst Coverage) | −0.179*** | −0.170*** | −0.112*** | −0.103*** | −0.092*** | −0.158*** | 0.254*** |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.02) | (0.009) | (0.023) |
| Size | | | −0.131*** | −0.132*** | | | |
| | | | (0.009) | (0.009) | | | |
| Market-to-book | | | −0.005*** | −0.004*** | | | |
| | | | (0.001) | (0.001) | | | |
| Book Leverage | | | 0.112*** | 0.107*** | | | |
| | | | (0.036) | (0.035) | | | |

**Table 9** (continued)

| Earnings Surprise | (1) Absolute earnings surprise | (2) Analyst 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 |
|---|---|---|---|---|---|---|---|
| Profitability | | | −0.524*** | −0.514*** | | | |
| | | | (0.052) | (0.052) | | | |
| Institutional Ownership | | | −0.366*** | −0.371*** | | | |
| | | | (0.032) | (0.032) | | | |
| Ln(Trading Volume) | | | 0.125*** | 0.124*** | | | |
| | | | (0.008) | (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 |

This table 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 at the firm level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05 and 0.01, respectively. Appendix Table 11 provides the variable definitions clustered at the firm level. Appendix Table 11 provides detailed definitions of all variables. Appendix Table 12 Panel B shows the summary statistics for the main variables in this analysis. Our focus on Amihud's measure restricts the sample for this empirical analysis to 45,043 firm-quarter observations.

Table 10 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* has 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 Ln(*Analyst Coverage*). Therefore, while firms covered by many 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 2 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. Importantly, since our results hold when we control for the extent of analyst coverage of the firm, our findings suggest that it is not only the number of analysts following the firm that influences a firm's information environment but also their level of attention to the firm at a given point.

## 4 Robustness analyses

We estimate a battery of (untabulated) robustness checks to validate our results and strengthen our conclusions. We start by addressing concerns about the validity of our research design and discuss numerous variations in the measurement of our key variables. Next, we discuss additional tests to rule out alternative explanations of our main findings.

In a first placebo test, we evaluate the validity of our empirical strategy to identify analyst distraction. Our strategy assumes that the analysts' exposure to attention-grabbing shocks (in the form of extreme industry returns) affects certain industries across their coverage portfolio. To validate our approach, we run a placebo test by randomly selecting attention-grabbing industries and re-estimating our core regressions, both at the analyst-firm level (Table 2, Model 1) and at the firm level (Table 9, Panel B, Model 4). We repeat this process 5,000 times and find that 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, thus giving us confidence that our main findings are not the product of randomness but rather follow from our identification of attention-grabbing industries.

Next, we use four alternative measures of analyst distraction to assess the robustness of our main findings. First, we examine whether our results are sensitive to the sign of the extreme returns. Specifically, we define analyst distraction based solely on positive or negative extreme returns and find that our results hold for both measures separately. Second, we create an alternative value-weighted measure of analyst distraction to incorporate the career concerns of analysts (*Analyst Distraction VW*) based on Harford et al. (2019). These authors argue that analysts strategically allocate effort among portfolio firms by devoting more effort to firms that are more important for their careers (e.g., large firms). We repeat our analysis using a measure of investor distraction weighted by market capitalization and find qualitatively similar results.

Third, we address the concern that some industries are more subject to extreme returns than others. Extreme negative or positive returns in a less volatile industry are more likely to divert an analyst's attention than extreme returns in a more volatile industry. We construct a measure of analyst distraction weighted by the inverse of the probability that an industry will experience extreme returns *(Analyst Distraction IERPW)*. Our results hold when we use this measure. Fourth, we verify the robustness of our results to our choice of using the Fama–French 12 industries classification to measure distraction. Specifically, 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.[35]

Focusing on alternative output variables and empirical specifications, we show in further analyses that our results hold when we differentiate between positive and negative forecast errors. This result rules out the possibility that our findings reflect a change in forecasting patterns related to other behavioral shortcuts, such as the affect and availability heuristics, that predict a directional change in analysts' forecasting errors (Antoniou et al. 2021; Bourveau and Law 2021).

Next, we find that our results continue to hold when we use the average forecast of an analyst-stock within a quarter rather than the latest issued forecasts, when we implement different clustering of the standard errors, when we demean the variables instead of including stock-quarter fixed effects, and when we restrict the sample to analysts with identifiable last names.

To address the additional concern that our empirical estimates capture an effect primarily driven by a change in outputs for firms in shocked industries, we exclude from our sample all firms that belong to the industries with extreme positive and negative returns. When we re-estimate our analyses at both the analyst level and the firm level, we find that our main results hold across all specifications. In a final robustness test, we examine the role of investor distraction in our setting. We build on the work of Kempf et al. (2017) to estimate a firm-level measure of institutional investors' distraction. When we add this measure as a covariate in our firm-level main specifications, we find that our results on the firm's information environment hold. This ensures that our results do not follow from a strong correlation between investors and analysts' distraction.

## 5 Conclusion

We identify a previously unexamined psychological mechanism whereby unexpected exogenous attention-grabbing events affect analysts' attention allocation. Specifically, we measure *cognitive distraction* at the analyst-firm-quarter level and establish two sets

[35] Research suggests that the Global Industry Classification Standard (GICS) provides the most accurate representation of how brokerages generally organize their analyst teams (e.g., Bhojraj et al. 2003; Boni and Womack 2006; Kadan et al. 2012).

**Table 10** Real effects of analyst distraction: Amihud's illiquidity

| Amihud Illiquidity | (1) Firm time-varying characteristics | (2) Analyst and firm time-varying characteristics |
|---|---|---|
| Avg. Analyst Distraction | 0.057** | 0.053** |
| | (0.024) | (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 | | −0.004 |
| | | (0.007) |
| Ln(Analyst Coverage) | −0.024*** | −0.024*** |
| | (0.005) | (0.005) |
| Market-to-book | 0.000 | 0.000 |
| | (0.000) | (0.000) |
| Size | −0.050*** | −0.049*** |
| | (0.004) | (0.004) |
| Book Leverage | 0.066*** | 0.066*** |
| | (0.016) | (0.016) |
| Institutional Ownership | −0.186*** | −0.186*** |
| | (0.016) | (0.015) |
| Ln(Trading Volume) | −0.035*** | −0.035*** |
| | (0.004) | (0.004) |
| Momentum | 0.018*** | 0.018*** |
| | (0.003) | (0.003) |
| Volatility | −0.186*** | −0.182*** |
| | (0.050) | (0.050) |
| Observations | 45,043 | 45,043 |
| R-squared | 0.768 | 0.769 |
| Year Fixed Effects | Yes | Yes |
| Firm Fixed Effects | Yes | Yes |

This table 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 firms with a stock price of less than \$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 at the firm level. Intercepts are not reported. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Appendix Table 11 provides the variable definitions

of results. Using our measure at the analyst level, we find that distracted analysts have lower forecast accuracy, revise forecasts less frequently, and publish less informative forecast revisions, relative to nondistracted ones. We add to a long literature that shows how behavioral biases as well as strategic incentives affect analyst forecast performance. Our findings emphasize not only how cognitive biases can temporarily affect analysts' forecasting 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, underscoring that the cognitive processes of market participants help determine how well capital markets function.

While our findings provide novel insights related to how analysts forecast earnings, arguably one of the most important outputs of the research process, they also prompt questions about whether and how distraction also affects other analyst output measures. One obvious additional output measure is price targets, because analysts have limited ability to forecast them accurately (Bradshaw et al. 2013), and earnings forecasts are an important input in price target calculation. Recent work by Dechow and You (2020) discusses why price targets exhibit large forecast errors. For example, could it be that distracted analysts also produce worse price targets? Similarly, Hand et al. (2021) draw attention to the paucity of research on a large battery of non-earnings non-KPI measures forecasted by analysts. Therefore, future research could examine how distraction affects different components of the forecast exercise differently.

Finally, our work speaks to the ongoing debate of man versus machine when it comes to processing information in capital markets (e.g., Blankespoor et al. 2018; Costello et al. 2020). Focusing on analyst output, Coleman et al. (2021) compare the recommendations of "robo-analysts" and human analysts and conclude that automation in the sell-side research industry can benefit investors. Our analysis similarly points to a behavioral cost of being a human analyst. However, while computers cannot be distracted, humans can better adapt to changing situations that require inventive and creative ways to make decisions, such as when traditional mechanical patterns in data are no longer valid. To illustrate, Cao et al. (2021) build an AI analyst that digests corporate financial information, qualitative disclosures, and macroeconomic indicators. They show that the AI analyst can beat most human analysts in stock price forecasts and generate excess returns, compared to human analysts. However, human analysts remain competitive when critical information requires institutional knowledge (such as the nature of intangible assets). The edge of AI over human analysts also declines over time when analysts gain access to alternative data and to in-house AI resources. This echoes our findings that human analysts quickly learn to correct their shortcomings, underlining the value of human information processing abilities. As Cao et al. (2021) argue, the promising way forward is to combine AI's computational power with the human art of understanding soft information (i.e., complement human analysts with robo-analysts instead of displacing the former).[36]

[36] In the same vein, Bochkay and Joos (2021) find that analysts weigh distinct types of information (soft versus hard) differently when forming risk forecasts as a function of changing underlying macroeconomic uncertainty at the time of the forecast. Abis (2022) compares investment decisions made by humans and machines: consistent with quantitative funds having more learning capacity but less flexibility to adapt to changing market conditions than discretionary funds, she finds that quantitative funds hold more stocks, specialize in stock picking, and engage in more overcrowded trades.

# Appendix 1

**Table 11** Variable definitions

| Variable | Definition | Source |
|---|---|---|
| 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's (2002) 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 1,000,000. We exclude firms with a stock price of less than $5 | CRSP |
| Analyst Coverage | The number of unique analysts issuing earnings forecasts 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 (i.e., extreme quarterly returns) in unrelated Fama–French 12 industries: an industry experiences an extreme return if it achieves the highest or the lowest return across all 12 Fama–French industries in a given quarter | I/B/E/S – Kenneth French's Website |
| Analyst Distraction Dummy | Dummy variable that takes the value one when *Analyst Distraction* is greater than or equal to five and zero otherwise | |
| 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 *others*). 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 that a given industry will experience extreme return events. For a given Fama–French 12 industry and quarter, we compute the probability of experiencing extreme return events as the number of quarters in which the industry experiences extreme returns over the last 20 quarters divided by 20 | I/B/E/S – Kenneth French's Website |

**Table 11** (continued)

| Variable | Definition | Source |
|---|---|---|
| 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 *Analyst Distraction* of the analysts covering firm $j$ at quarter $t$ | |
| Avg. Firm Experience | Average *Firm Experience* of the analysts covering firm $j$ at quarter $t$ | |
| Avg. General Experience | Average *General Experience* of the analysts covering firm $j$ at quarter $t$ | |
| Avg. Portfolio Size | Average *Portfolio Size* of the analysts covering firm $j$ at quarter $t$ | |
| Avg. Nb. Different Industries | Average *Nb. Different Industries* of the analysts covering firm $j$ at quarter $t$ | |
| Avg. Top 10 Brokerage House | Average *Top 10 Brokerage House* of the analysts covering firm $j$ at quarter $t$ | |
| At Least One Revision | Dummy variable that indicates whether the analyst updates his or her one-year-ahead EPS forecast for the same firm-quarter at least once | 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 | *First Distraction Event*, which is a dummy variable that takes the value one if it is the first time that an analyst experiences a significant distraction shock in a specific firm and zero 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 analyst $i$'s forecast for firm $j$'s earnings at quarter $t$ and analyst $i$'s last forecast for the same firm and earnings at quarter $t$ scaled by the last forecast | |
| Forecast Revision Frequency | The number of forecasts issued by 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 |

**Table 11** (continued)

| Variable | Definition | Source |
|---|---|---|
| Momentum | Buy-and-hold returns over the last 12 months prior to the fiscal year-end | CRSP |
| Nb. Different Industries | The number of two-digit SICs represented by firms followed by analyst $i$ in quarter $t$ | I/B/E/S |
| Not-first Distraction Event | *Not-first Distraction Event*, which is a dummy variable that takes the value one if it is not the first time that an analyst experiences a significant distraction shock in a specific firm and zero otherwise | |
| Portfolio Size | The number of unique firms followed by analyst $i$ in quarter $t$ | I/B/E/S |
| Profitability | Return on assets | 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 issues a forecast revision 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 | Indicator variable that is equal to one if analyst $i$ works at a top decile brokerage in quarter $t$ | I/B/E/S |
| Total Number of Distraction Events | The sum of the times an analyst is significantly distracted (*Analyst Distraction Dummy* = 1) | |
| Trading Volume | The annual trading volume (in thousand shares) for firm $j$ 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 2

**Table 12** Firm-level descriptive statistics

| Variables | Obs | Mean | S.D | 0.25 | Mdn | 0.75 |
|---|---|---|---|---|---|---|
| Panel A: Earnings surprise | | | | | | |
| 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 | 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 | | | | | | |
| 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 |

Panel A presents descriptive statistics for the firm-quarter level variables we use to examine the influence of analyst distraction on earnings surprise. Appendix Table 11 provides the variable definitions

Panel B presents descriptive statistics for the firm-year level data we use to examine the influence of analyst distraction on Amihud's illiquidity measure, our proxy for information asymmetry. Appendix Table 11 provides the variable definitions

# Appendix 3

**Table 13** Descriptive statistics on extreme industry returns

*Panel A: Top and bottom extreme returns*

| *Variables* | Mean | S.D. | 0.25 | Median | 0.75 |
|---|---|---|---|---|---|
| Top quarterly extreme returns | 12.28% | 10.10% | 6.90% | 11.65% | 18.02% |
| Bottom quarterly extreme returns | -6.25% | 9.65% | -10.27% | -3.67% | 0.22% |

*Panel B: Top industry performers*

Panel B reports the top performer Fama-French 12 industry on a quarterly basis, over the 1985–2015 period.

| y-q | Fama-French twelve industry | Quarterly return | Mean other Industries return |
|---|---|---|---|
| 1985-1 | Health | 15.54% | 10.15% |
| 1985-2 | Telecom | 12.42% | 7.42% |
| 1985-3 | Energy | -0.27% | -4.70% |
| 1985-4 | Finance | 21.62% | 16.68% |
| 1986-1 | Consumer Durables | 22.32% | 14.07% |
| 1986-2 | Consumer Nondurables | 16.14% | 6.04% |
| 1986-3 | Energy | 9.50% | -8.97% |
| 1986-4 | Chemicals | 9.21% | 4.38% |
| 1987-1 | Business Equipment | 31.15% | 20.58% |
| 1987-2 | Energy | 9.13% | 3.63% |
| 1987-3 | Telecom | 14.75% | 5.57% |
| 1987-4 | Utilities | -8.78% | -23.84% |
| 1988-1 | Shops | 15.59% | 7.19% |
| 1988-2 | Consumer Durables | 12.77% | 5.90% |
| 1988-3 | Consumer Nondurables | 7.98% | -0.35% |
| 1988-4 | Consumer Nondurables | 10.68% | 1.78% |
| 1989-1 | Telecom | 13.61% | 6.65% |
| 1989-2 | Telecom | 15.94% | 7.89% |
| 1989-3 | Health | 17.03% | 9.31% |
| 1989-4 | Energy | 10.53% | -0.34% |
| 1990-1 | Business Equipment | 7.31% | -3.52% |
| 1990-2 | Health | 18.11% | 4.65% |
| 1990-3 | Energy | 5.23% | -17.67% |
| 1990-4 | Chemicals | 15.89% | 8.17% |
| 1991-1 | Shops | 28.38% | 15.54% |
| 1991-2 | Consumer Durables | 7.03% | -0.42% |
| 1991-3 | Utilities | 11.68% | 5.02% |
| 1991-4 | Health | 21.04% | 6.25% |
| 1992-1 | Consumer Durables | 25.04% | -1.36% |
| 1992-2 | Energy | 9.04% | 0.26% |
| 1992-3 | Energy | 8.21% | 2.31% |
| 1992-4 | Finance | 14.56% | 6.64% |
| 1993-1 | Consumer Durables | 15.12% | 4.07% |
| 1993-2 | Consumer Durables | 6.84% | 0.78% |
| 1993-3 | Finance | 8.11% | 3.10% |
| 1993-4 | Consumer Durables | 13.30% | 2.30% |
| 1994-1 | Business Equipment | 2.38% | -4.52% |
| 1994-2 | Finance | 4.08% | -1.68% |
| 1994-3 | Health | 13.80% | 4.61% |
| 1994-4 | Business Equipment | 7.64% | -1.39% |
| 1995-1 | Finance | 12.19% | 8.04% |
| 1995-2 | Business Equipment | 22.61% | 7.25% |
| 1995-3 | Finance | 15.91% | 7.51% |
| 1995-4 | Health | 12.45% | 4.58% |
| 1996-1 | Chemicals | 11.81% | 5.46% |
| 1996-2 | Consumer Nondurables | 10.12% | 3.66% |
| 1996-3 | Finance | 7.36% | 1.20% |
| 1996-4 | Energy | 12.59% | 5.59% |
| 1997-1 | Consumer Nondurables | 4.27% | 0.31% |
| 1997-2 | Health | 23.59% | 14.66% |
| 1997-3 | Business Equipment | 18.44% | 8.28% |
| 1997-4 | Telecom | 19.13% | 1.89% |
| 1998-1 | Consumer Durables | 19.94% | 12.54% |
| 1998-2 | Consumer Durables | 10.21% | 1.48% |
| 1998-3 | Utilities | 4.51% | -12.68% |
| 1998-4 | Business Equipment | 36.93% | 17.19% |
| 1999-1 | Business Equipment | 10.68% | 0.75% |
| 1999-2 | Telecom | 15.85% | 7.28% |
| 1999-3 | Business Equipment | 3.44% | -8.41% |

**Table 13** (continued)

| | | | |
|---|---|---|---|
| 1999-4 | Business Equipment | 40.72% | 8.64% |
| 2000-1 | Business Equipment | 17.00% | -0.91% |
| 2000-2 | Health | 22.00% | -3.64% |
| 2000-3 | Utilities | 27.90% | 1.96% |
| 2000-4 | Chemicals | 17.92% | -2.56% |
| 2001-1 | Consumer Durables | 8.36% | -9.16% |
| 2001-2 | Business Equipment | 15.62% | 4.91% |
| 2001-3 | Consumer Nondurables | -0.31% | -14.64% |
| 2001-4 | Business Equipment | 33.35% | 9.33% |
| 2002-1 | Consumer Durables | 13.42% | 3.05% |
| 2002-2 | Consumer Nondurables | -1.89% | -11.82% |
| 2002-3 | Health | -7.66% | -17.71% |
| 2002-4 | Telecom | 23.33% | 5.87% |
| 2003-1 | Health | 1.17% | -4.71% |
| 2003-2 | Consumer Durables | 21.64% | 15.59% |
| 2003-3 | Business Equipment | 11.06% | 2.49% |
| 2003-4 | Consumer Durables | 23.84% | 12.58% |
| 2004-1 | Shops | 6.88% | 1.70% |
| 2004-2 | Energy | 8.56% | 1.97% |
| 2004-3 | Energy | 11.06% | -2.03% |
| 2004-4 | Business Equipment | 14.72% | 9.53% |
| 2005-1 | Energy | 18.61% | -2.78% |
| 2005-2 | Utilities | 9.62% | 0.48% |
| 2005-3 | Energy | 20.11% | 2.49% |
| 2005-4 | Finance | 7.42% | 0.08% |
| 2006-1 | Manufacturing | 12.29% | 5.01% |
| 2006-2 | Utilities | 5.89% | -0.58% |
| 2006-3 | Health | 8.44% | 3.50% |
| 2006-4 | Energy | 11.65% | 7.11% |
| 2007-1 | Utilities | 9.19% | 1.93% |
| 2007-2 | Energy | 14.14% | 5.96% |
| 2007-3 | Chemicals | 9.36% | 0.67% |
| 2007-4 | Utilities | 6.90% | -3.20% |
| 2008-1 | Chemicals | -3.44% | -8.99% |
| 2008-2 | Energy | 18.91% | -3.72% |
| 2008-3 | Health | 2.23% | -9.38% |
| 2008-4 | Health | -11.93% | -23.22% |
| 2009-1 | Business Equipment | 2.58% | -11.75% |
| 2009-2 | Consumer Durables | 43.75% | 15.62% |
| 2009-3 | Consumer Durables | 25.31% | 15.58% |
| 2009-4 | Consumer Durables | 14.72% | 6.32% |
| 2010-1 | Consumer Durables | 16.24% | 5.59% |
| 2010-2 | Utilities | -4.32% | -11.22% |
| 2010-3 | Consumer Durables | 18.31% | 12.35% |
| 2010-4 | Consumer Durables | 26.22% | 11.20% |
| 2011-1 | Energy | 17.31% | 5.08% |
| 2011-2 | Health | 6.47% | 0.36% |
| 2011-3 | Utilities | -1.84% | -16.40% |
| 2011-4 | Energy | 17.73% | 11.69% |
| 2012-1 | Finance | 21.97% | 10.72% |
| 2012-2 | Telecom | 7.63% | -4.10% |
| 2012-3 | Telecom | 10.24% | 5.86% |
| 2012-4 | Consumer Durables | 17.18% | 0.33% |
| 2013-1 | Health | 16.69% | 11.20% |
| 2013-2 | Consumer Durables | 13.27% | 2.53% |
| 2013-3 | Consumer Durables | 14.71% | 5.79% |
| 2013-4 | Finance | 12.53% | 8.82% |
| 2014-1 | Utilities | 8.47% | 1.33% |
| 2014-2 | Energy | 11.01% | 4.48% |
| 2014-3 | Health | 5.01% | -1.71% |
| 2014-4 | Shops | 12.48% | 4.32% |
| 2015-1 | Health | 6.85% | 0.67% |
| 2015-2 | Finance | 5.24% | -0.85% |
| 2015-3 | Consumer Nondurables | -0.21% | -8.86% |
| 2015-4 | Chemicals | 10.80% | 4.65% |

**Table 13** (continued)

*Panel C: Bottom industry performers*

Panel C reports the bottom performer Fama-French 12 industry on a quarterly basis, over the 1985-2015 period.

| y-q | Fama-French 12 industry | Quarterly return | Mean other Industries return |
|---|---|---|---|
| 1985-1 | Consumer Durables | 2.40% | 11.35% |
| 1985-2 | Business Equipment | -1.71% | 8.70% |
| 1985-3 | Shops | -8.72% | -3.93% |
| 1985-4 | Energy | 3.30% | 18.34% |
| 1986-1 | Energy | -6.05% | 16.65% |
| 1986-2 | Consumer Durables | -1.74% | 7.67% |
| 1986-3 | Shops | -15.70% | -6.68% |
| 1986-4 | Finance | 0.20% | 5.20% |
| 1987-1 | Utilities | 5.17% | 22.94% |
| 1987-2 | Utilities | -2.92% | 4.72% |
| 1987-3 | Utilities | -0.39% | 6.95% |
| 1987-4 | Shops | -28.70% | -22.03% |
| 1988-1 | Business Equipment | -0.23% | 8.63% |
| 1988-2 | Health | 0.38% | 7.03% |
| 1988-3 | Business Equipment | -10.43% | 1.32% |
| 1988-4 | Finance | -2.95% | 3.02% |
| 1989-1 | Business Equipment | -2.62% | 8.13% |
| 1989-2 | Consumer Durables | 4.10% | 8.97% |
| 1989-3 | Business Equipment | 2.67% | 10.61% |
| 1989-4 | Consumer Durables | -8.16% | 1.35% |
| 1990-1 | Telecom | -10.09% | -1.94% |
| 1990-2 | Energy | 0.02% | 6.29% |
| 1990-3 | Consumer Durables | -26.01% | -14.83% |
| 1990-4 | Energy | -4.75% | 10.05% |
| 1991-1 | Utilities | 5.16% | 17.65% |
| 1991-2 | Business Equipment | -8.25% | 0.97% |
| 1991-3 | Consumer Durables | -5.15% | 6.55% |
| 1991-4 | Energy | -3.97% | 8.52% |
| 1992-1 | Health | -13.02% | 2.10% |
| 1992-2 | Health | -5.72% | 1.60% |
| 1992-3 | Consumer Durables | -8.81% | 3.86% |
| 1992-4 | Energy | -4.20% | 8.34% |
| 1993-1 | Health | -14.42% | 6.76% |
| 1993-2 | Consumer Nondurables | -7.52% | 2.08% |
| 1993-3 | Health | -2.22% | 4.04% |
| 1993-4 | Energy | -7.16% | 4.16% |
| 1994-1 | Health | -10.21% | -3.38% |
| 1994-2 | Consumer Durables | -6.53% | -0.71% |
| 1994-3 | Consumer Durables | -1.77% | 6.03% |
| 1994-4 | Shops | -5.01% | -0.24% |
| 1995-1 | Consumer Durables | 0.36% | 9.11% |
| 1995-2 | Energy | 3.22% | 9.02% |
| 1995-3 | Energy | 1.97% | 8.78% |
| 1995-4 | Business Equipment | -3.49% | 6.03% |
| 1996-1 | Telecom | -1.79% | 6.69% |
| 1996-2 | Chemicals | -0.46% | 4.62% |
| 1996-3 | Telecom | -8.00% | 2.60% |
| 1996-4 | Shops | -2.67% | 6.98% |
| 1997-1 | Business Equipment | -4.32% | 1.09% |
| 1997-2 | Utilities | 5.46% | 16.31% |
| 1997-3 | Chemicals | 1.64% | 9.80% |
| 1997-4 | Business Equipment | -10.96% | 4.63% |
| 1998-1 | Utilities | 4.81% | 13.91% |
| 1998-2 | Manufacturing | -3.56% | 2.73% |
| 1998-3 | Finance | -21.28% | -10.34% |
| 1998-4 | Energy | 0.56% | 20.50% |
| 1999-1 | Utilities | -11.33% | 2.75% |
| 1999-2 | Health | -3.32% | 9.02% |
| 1999-3 | Finance | -15.40% | -6.70% |
| 1999-4 | Utilities | -7.65% | 13.04% |

**Table 13** (continued)

| | | | |
|---|---|---|---|
| 2000-1 | Chemicals | -20.82% | 2.53% |
| 2000-2 | Consumer Durables | -14.16% | -0.35% |
| 2000-3 | Telecom | -12.03% | 5.59% |
| 2000-4 | Business Equipment | -34.65% | 2.22% |
| 2001-1 | Business Equipment | -25.47% | -6.09% |
| 2001-2 | Telecom | -1.84% | 6.50% |
| 2001-3 | Business Equipment | -34.57% | -11.53% |
| 2001-4 | Telecom | -1.24% | 12.47% |
| 2002-1 | Telecom | -9.08% | 5.09% |
| 2002-2 | Business Equipment | -25.46% | -9.68% |
| 2002-3 | Business Equipment | -25.45% | -16.09% |
| 2002-4 | Shops | 0.26% | 7.97% |
| 2003-1 | Consumer Durables | -10.65% | -3.64% |
| 2003-2 | Chemicals | 5.52% | 17.06% |
| 2003-3 | Telecom | -5.96% | 4.04% |
| 2003-4 | Shops | 7.33% | 14.08% |
| 2004-1 | Consumer Durables | -2.36% | 2.54% |
| 2004-2 | Telecom | -2.90% | 3.02% |
| 2004-3 | Business Equipment | -9.70% | -0.15% |
| 2004-4 | Health | 3.79% | 10.52% |
| 2005-1 | Consumer Durables | -14.00% | 0.18% |
| 2005-2 | Chemicals | -5.55% | 1.86% |
| 2005-3 | Consumer Durables | -2.57% | 4.55% |
| 2005-4 | Energy | -8.11% | 1.49% |
| 2006-1 | Utilities | -0.47% | 6.17% |
| 2006-2 | Business Equipment | -8.88% | 0.76% |
| 2006-3 | Energy | -2.59% | 4.50% |
| 2006-4 | Health | 1.78% | 8.01% |
| 2007-1 | Finance | -2.38% | 2.98% |
| 2007-2 | Utilities | 0.14% | 7.23% |
| 2007-3 | Consumer Durables | -6.10% | 2.08% |
| 2007-4 | Finance | -11.08% | -1.56% |
| 2008-1 | Consumer Durables | -15.16% | -7.92% |
| 2008-2 | Finance | -16.29% | -0.52% |
| 2008-3 | Energy | -26.32% | -6.78% |
| 2008-4 | Consumer Durables | -39.56% | -20.71% |
| 2009-1 | Finance | -22.80% | -9.44% |
| 2009-2 | Shops | 8.22% | 18.85% |
| 2009-3 | Utilities | 7.01% | 17.24% |
| 2009-4 | Finance | 0.61% | 7.60% |
| 2010-1 | Utilities | -1.96% | 7.24% |
| 2010-2 | Finance | -14.96% | -10.25% |
| 2010-3 | Finance | 4.97% | 13.56% |
| 2010-4 | Utilities | 3.76% | 13.25% |
| 2011-1 | Consumer Durables | 0.79% | 6.58% |
| 2011-2 | Energy | -5.73% | 1.47% |
| 2011-3 | Consumer Durables | -31.15% | -13.73% |
| 2011-4 | Business Equipment | 8.11% | 12.56% |
| 2012-1 | Utilities | -0.07% | 12.72% |
| 2012-2 | Consumer Durables | -19.27% | -1.65% |
| 2012-3 | Utilities | 2.44% | 6.57% |
| 2012-4 | Business Equipment | -4.31% | 2.28% |
| 2013-1 | Business Equipment | 5.69% | 12.20% |
| 2013-2 | Utilities | -1.52% | 3.87% |
| 2013-3 | Consumer Nondurables | 0.78% | 7.05% |
| 2013-4 | Utilities | 3.30% | 9.66% |
| 2014-1 | Shops | -2.04% | 2.28% |
| 2014-2 | Finance | 1.19% | 5.38% |
| 2014-3 | Energy | -9.30% | -0.41% |
| 2014-4 | Energy | -13.52% | 6.69% |
| 2015-1 | Utilities | -3.51% | 1.61% |
| 2015-2 | Utilities | -5.56% | 0.14% |
| 2015-3 | Energy | -17.37% | -7.30% |
| 2015-4 | Utilities | -3.78% | 5.97% |

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