Public Disclosure of Private Meetings: Does Observing Peers' Information Acquisition Affect Analysts' Attention Allocation?

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Abstract

We investigate the impact of observing peers' information acquisition decisions on financial analysts' own allocation of attention. Using the timely disclosure mandate by the Shenzhen Stock Exchange as a setting, we find that, when analysts observe that a firm has been visited by other analysts, they pay less attention to that firm. This finding is consistent with the conjecture that the timely disclosure reveals the relative information advantage of visiting analysts, leading nonvisiting analysts to reallocate their limited attention and resources. We find that the reduction in nonvisiting analysts' attention not only occurs shortly after a visit but also persists over the long term. Further evidence suggests that the timely disclosure has positive externalities in the form of increased attention and improved information environment for nonvisited peer firms.

Keywords: analysts, attention allocation, information environment, externalities
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If you know the enemy and know yourself, you need not fear the result of a hundred battles. If you know yourself but not the enemy, for every victory gained you will also suffer a defeat.

– Sun Tzu, The Art of War

1. Introduction

Financial analysts acquire information from public and private sources and facilitate communication between firms and investors (Healy and Palepu, 2001; Kadan et al., 2009; Loh and Stulz, 2018). Research has shown that analysts must allocate their limited attention and resources across firms and that this allocation has important implications for investors, companies, and the capital market (deHaan et al., 2015; Harford et al., 2018; Driskill et al., 2020; Blankespoor et al., 2020). Due to competition among analysts (e.g., Hong and Kacperczyk, 2010; Merkley et al., 2017), analysts strive to provide new information to distinguish themselves from other analysts (Crawford et al., 2012). What analysts know about peers' information acquisition can thus significantly change their own information acquisition decisions and how they allocate attention across firms, about which we know little.

This study attempts to fill this gap by examining how observing peers' allocation of attention to information production affects analysts' own attention allocation. We employ a unique setting. Since 2012, the Shenzhen Stock Exchange (SZSE) has required listed firms to promptly disclose private meetings with corporate management, which allows analysts to observe information acquisition by their peers. Private meetings, such as corporate site visits, enable analysts to acquire information by talking with managers and observing operations and facilities (Brown et al., 2015; Chen et al., 2022; Choy and Hope, 2022). However, nonvisiting analysts typically do not observe *when* and *with whom* meetings occur (Soltes, 2014; Solomon and Soltes, 2015). The timely disclosure mandate by SZSE allows nonvisiting analysts to be aware of their

peers' visits and enables them to respond. This is especially true given that site visits are short-duration and high-frequency (Kirk and Markov, 2016) and can be adjusted quickly.

Studies document that analysts who visit a firm and privately meet with managers gain an information advantage and issue more accurate forecasts (Cheng et al., 2016; Han et al., 2018; Choy and Hope, 2022). When a firm discloses analysts' visits promptly, investors and nonvisiting analysts thus recognize that the visiting analysts have gained an information advantage vis-à-vis the visited firm. Investors interested in that firm are more likely to obtain information from visiting analysts, who issue earnings forecasts or privately communicate with investors. With respect to the visited firm, nonvisiting analysts likely anticipate a reduction in both investors' requests for information and their chances of discovering new information through their own visits. Thus observing other analysts' visits may decrease the expected benefits of visiting the same firm, leading nonvisiting analysts to reduce attention to that firm.[1]

Yet it is not obvious whether nonvisiting analysts will allocate less attention to a visited firm. Corporate site visits often occur when there is value-relevant information that is not yet well known or understood by the public (e.g., Cheng et al., 2019; Choy and Hope, 2022).[2] Timely disclosure of visits might point to the existence of value-relevant information. Other analysts may still want to conduct visits of their own because they can form different information mosaics (e.g., Brown et al., 2015; Solomon and Soltes, 2015).[3] Observing other analysts' site visits may thus increase the expected benefits of visiting the same firm, leading nonvisiting analysts to increase

[1] The theory and evidence of Van Nieuwerburgh and Veldkamp (2009) also suggest that market participants benefit more from knowing what others do not know.

[2] Van Nieuwerburgh and Veldkamp (2010) theoretically show that an investor with limited attention is more likely to pay attention to and learn about stocks she is uncertain about.

[3] Value-relevant information does not need to be material information itself. In fact, disclosing material information during private meetings contradicts the framework of Reg FD, which was adopted in China in 2006. However, visiting analysts can become informed by assessing nonmaterial information and forming an information "mosaic" using their expertise or information.

attention to that firm. Given this possibility, it is an open question as to how the timely disclosure of corporate site visits affects the allocation of financial analysts' attention.

To address this question, we use the timely disclosure mandate of corporate site visits by the SZSE in China in July 2012 as an empirical setting. In July 2012, SZSE-listed firms were required to disclose information about a corporate site visit within *two* trading days, including participants' identities, meeting dates, locations, and descriptions of meeting topics (hereafter referred to as the "timely disclosure mandate"). Whereas, before 2012, this information was only required to be disclosed in these firms' regular periodic reports (i.e., annual, semi-annual, and quarterly) and the lag between a site visit and its disclosure could be several months. In other words, the 2012 regulation requires firms to disclose information about corporate site visits more promptly. As a result, analysts can observe their peers' information acquisition sooner.[4]

We use a difference-in-differences research design, comparing changes in analysts' attention to firms that were visited by other analysts from 2009–2011 to 2013–2015, relative to firms that were not. We find that, following the timely disclosure mandate, when a firm is visited during a week, nonvisiting analysts are less likely to visit this firm afterward, compared to firms that were not visited in the same week. These results are consistent with the conjecture that the timely disclosure reveals visiting analysts' information advantage, leading nonvisiting analysts to reduce attention to visited firms. We also find that, prior to 2012, there were no differential trends in nonvisiting analysts' attention allocated to visited firms, compared to nonvisited ones, supporting the parallel trends assumption. We further document a larger reduction in attention

[4] One potential concern is that, before 2012, visiting analysts might have disclosed in their reports that their earnings forecasts and recommendations were based on the recent site visits. However, in our sample, only 19% of site visits are followed by visiting analysts' earnings forecasts in the month after their site visits. Thus, on average, nonvisiting analysts can observe visiting analysts' information acquisition in a timelier fashion after 2012.

allocated to visited firms, relative to nonvisited ones, if visiting analysts have stronger information advantages.

Motivated by our finding that nonvisiting analysts reduce attention to visited firms, we next explore whether those analysts increase their attention toward nonvisited peer firms. We find that, following the timely disclosure mandate, analysts increase attention to a nonvisited firm when more of its industry peers have hosted visits.[5] We then examine whether the change in analysts' attention is associated with consequences for firms' information environment. We document that, following the timely disclosure mandate, when more industry peers have hosted visits, a nonvisited firm experiences better quality of analyst coverage, as captured by an improvement in forecast accuracy around site visits (e.g., Cheng et al., 2016; Chen et al., 2021), and its stock price reflects greater firm-specific information, as revealed by a reduction in stock return synchronicity (Morck et al., 2000; Wurgler, 2000; Piotroski and Roulstone, 2004; Crawford et al., 2012). These results suggest that the timely disclosure mandate has positive externalities in the form of increased attention and an improved information environment for nonvisited peer firms.[6]

While the nature of the timely disclosure mandate implies a prompt response by the analysts shortly after a visit, it is not clear whether changes in analysts' short-term attention are transient or have long-term consequences. We thus examine an analyst's tendency to visit or cover a visited firm during the subsequent year after each visiting week. Our results suggest that nonvisiting analysts are more likely to reduce attention to and drop coverage of visited firms. We also find more dispersed analyst coverage and visits among all firms, as reflected in a reduction in

[5] The disclosure of a visit may reveal the existence of *industry-level* value-relevant information, which may induce analysts to increase their attention to the nonvisited peer firms from the same industry. However, as we discuss in Section 3.3, the increased attention does not vary based on industry-level information, as captured by the average stock returns of peer firms during the week, mitigating the concern.

[6] We do not find an obvious change in visited firms' information environment, despite some evidence suggesting that the visited firm's stock price might incorporate visiting analysts' information in a slower fashion shortly after the visit (within two weeks) following the timely disclosure mandate and the difference quickly disappears (within four weeks).

the tendency of analysts to visit and cover firms with certain characteristics that attract analyst attention. In other words, promptly disclosed visits seem to reduce redundant follow-up visits or repeated coverage.

We perform additional analyses to address potential alternative explanations. First, our findings may be attributed to firms' responses. While analysts initiate most visits (Solomon and Soltes, 2015; Cheng et al., 2016), firms can negotiate dates and combine visit requests from multiple analysts into one visit following the timely disclosure mandate, which allows them to report once and lower compliance costs. We do not find that firms experiencing a significant increase in the number of visiting analysts per visit or visits consisting of multiple analysts drive our main results. Relatedly, firms occasionally invite analysts to visit, and these visits typically occur in the month after major corporate events (e.g., earnings announcements) (Cheng et al., 2016, 2019). Our inferences are unchanged if we exclude these visits from our sample.

Next, although our proposed explanation relies on firms' timely disclosure of meeting times and visiting analysts' identities, meeting topics also must be disclosed, which could confound our results. To mitigate this concern, we examine whether nonvisiting analysts learn useful information from the timelier topic disclosures. We do not find any evidence suggesting an increase in nonvisiting analysts' forecast accuracy, even for firms that disclose relatively more information, following the mandate. These results suggest that nonvisiting analysts do not learn useful information from the timelier topic disclosures.[7]

Our paper contributes to the growing literature on how analysts allocate their limited attention and resources (Harford, 2018; Blankespoor et al., 2020; Driskill et al., 2020; Hsu et el.,

[7] Besides the aforementioned confounding factors, analysts may arrange site visits to facilitate access to firm management for investors. However, these corporate access events do not preclude analysts from gaining better information (Soltes, 2014; Brown et al., 2015). To mitigate this concern, we show that our results are similar when we exclude site visits conducted jointly by analysts and investors.

2021). According to rational inattention models, economic agents have limited information processing capacity, so they must decide how to allocate their attention across firms (e.g., Sims, 2003; Sims, 2010; Veldkamp, 2011).[8] Consistent with this argument, Driskill et al. (2020) find that analysts are less timely and thorough when firms within their coverage portfolios have contemporaneous earnings announcements. Harford et al. (2018) find that analysts strategically allocate more effort to portfolio firms that are relatively more important to their careers based on a firm's relative rank in market capitalization, trading volume, and institutional ownership. Rather than focusing on firm characteristics, we identify a new factor that affects analysts' attention allocation—information about peer analysts' information acquisition. In addition, to capture analyst attention and effort, we use the site visit which is an input-based measure that is typically not directly observable. In this way, our work differs from prior studies using output-based measures such as the timing of forecast or forecast accuracy. The use of an input-based measure helps provide a better understanding of how analysts fulfill their information intermediary role.

Our paper also contributes to the literature on the strategic interactions among analysts. Studies have documented herding (e.g., Clement and Tse, 2005; Jegadeesh and Kim, 2010) and social learning (e.g., Do and Zhang, 2020; Kumar et al., 2022) among analysts and have examined how competition among analysts shapes their earnings forecasts and stock recommendations (e.g., Hong and Kacperczyk, 2010; Merkley et al., 2017). In comparison, we show that the competition among analysts also affects their allocation of attention and effort across firms. Our paper relates closely to the work of Crawford et al. (2012), who study analysts' coverage initiation. They show that the analyst who first initiates coverage provides low-cost market and industry information,

[8] Limited attention can also arise for behavioral reasons (e.g., DellaVigna and Pollet, 2009; Hirshleifer et al., 2009). Our arguments are based on the limited information processing capacity of analysts, which is rooted in the rational inattention framework. See Blankespoor et al. (2020) for a review of the rational inattention models.

while subsequent analysts doing so for the *same* firm typically focus on firm-specific information to distinguish themselves. Our argument assumes the existence of a first-mover advantage for visiting analysts. Our results suggest that, when firm-specific information has been exploited by other analysts, analysts shift their focus to different firms to distinguish themselves.

Our paper also contributes to the literature on the externalities of firms' public disclosures (Dye, 1990; Admati and Pfleiderer, 2000). Leuz and Wysocki (2016) have called for more studies on such externalities. Evidence suggests that public disclosure of peer firms is associated with a firm's cost of capital and liquidity (e.g., Shroff et al., 2017; De George et al., 2019). Prior studies also document that peer disclosures affect a firm's own voluntary disclosures (e.g., Baginski and Hinson, 2016; Seo, 2021; Breuer et al., 2022). Our paper shows that strategic interactions among information intermediaries (such as analysts) play an important role in contributing to the externalities of public disclosure. In its focus on public disclosure of private meetings, our study also contributes to the call for more research to examine the consequences of requiring firms to disclose site visits (Lennox and Wu, 2022).

Although our setting is China, we believe the findings pertain to capital markets more broadly. As evidenced by anecdotes and surveys (Brown et al., 2015), private meeting with firm managers is an important source of information for analysts. This is confirmed by empirical evidence in Choy and Hope (2022), who use New York City's daily taxi trip records to proxy for private meetings. Prior studies using the U.S. setting (e.g., Hong and Kacperczyk, 2010; Crawford et al., 2012; Merkley et al., 2017) also suggest that competition among analysts affects their behavior. Meanwhile, there have been discussions about whether the U.S. Securities and Exchange Commission (SEC) should increase the transparency of these meetings (e.g., Bengtzen, 2017). Our

results may contribute to this policy debate by providing evidence on the consequences of mandated timely disclosure of private meetings.[9]

2. Institutional Background, Research Design, and Sample Description

2.1 Institutional Background

Aiming to prevent public companies from selectively disclosing important news, in August 2006, the SZSE proposed Information Fair Disclosure Guidelines, which resemble the 2000 U.S. Regulation Fair Disclosure (Reg FD) rules. The guidelines recommended that public companies publicly disclose the identities of private-meeting participants in regular periodic reports (e.g., in annual, semi-annual, and quarterly reports). On July 2, 2007, the SZSE required public companies listed on the mainboard to record private meetings in a standard format beginning with the 2007 semi-annual reporting period.[10] The records were brief and typically included meeting dates and locations, outside participants' affiliations (sometimes their names), and a short summary of meeting topics. Importantly, the information provided in these periodic reports was not timely.

In July 2012, the SZSE additionally required *all* publicly listed firms to disclose investor relation activities in a standard format *within two trading days* on the SZSE online investor service platform: *HudongYi*.[11] These reports had to include meeting dates and locations, names of

[9] The importance of distinguishing from peers was also verbally confirmed by informal conversations we had with some analysts in major U.S. brokerages. Nevertheless, we acknowledge that, despite that the competition among analysts is general in any financial market, the impact may materialize in different ways across different regions and markets.

[10] See the Guidelines on Improving 2007 Semi-annual Reporting issued by SZSE on July 2, 2007. This mandatory public disclosure requirement did not apply to firms listed on the Small and Medium-sized Enterprises (SME) Board, but some firms listed on the SME Board voluntarily disclosed private meeings. In 2009, firms listed on the Growth Enterprises Market (GEM) Board started trading publicly. Since then, most firms listed on SZSE began reporting private meetings in their regular periodic reports and we use 2009 as the first year of our pre-period. Also, because firms were not required to disclose corporate site visits before 2007, we cannot observe site visits so it is impossible to compare analysts' visiting patterns before and after the disclosure guidance in 2007.

[11] The *HudongYi* platform operates on the website (http://www.cninfo.com.cn/new/index) designated by the China Securities Regulatory Commission. The official website of the SZSE's *HudongYi* platform is http://irm.cninfo.com.cn/szse/.

participating institutions and individuals, names of hosting personnel, and descriptions of meeting topics. Appendix 1 provides an example. In panel A, the firm recorded and disclosed site visits in the periodic reports before 2012. Panel B shows that, after 2012, the firm provides a much timelier disclosure of visits on the *HudongYi* platform. Panel C presents detailed records of site visits after 2012. Notably, the disclosure became more detailed after 2012. For example, before 2012, most firms disclosed only the names of institutions but not analysts' names in periodic reports, while after 2012, analysts' names were typically disclosed.[12]

2.2 Research Design

Our identification strategy exploits the nature of the SZSE's timely disclosure mandate, which triggers timely public disclosure when a corporate site visit occurs after 2012. Specifically, we examine the change in analysts' attention allocated to a visited firm, relative to nonvisited firms, after the mandate is implemented (as only visited firms are treated by the timely disclosure mandate). In other words, in 2009–2011 (the pre-period), visited firms disclosed information about site visits in periodic reports, while nonvisited firms did not. In 2013–2015 (the post-period), visited firms disclosed information about site visits within *two* trading days after each visit, while nonvisited firms still disclosed nothing. As a result, analysts could not promptly observe other analysts' site visits in the pre-period, but they could in the post-period.

We focus on firm-week-analyst-level analyses to capture analysts' short-term responses to the timely disclosure of site visits. In particular, we examine how each analyst responds shortly after observing other analysts' site visits each week. Using one week as the unit of time helps avoid

[12] Besides the SZSE, the Shanghai Stock Exchanges (SHSE) is another important stock market exchange in China. The SHSE introduced a similar online investor service platform SHSE-*eHudong* in July 2013 to direct firms to adopt similar timely disclosure practice but did not mandate disclosure, as SZSE did. The official website of the SHSE-*eHudong* platform is http://sns.sseinfo.com/. The SHSE also did not require firms to disclose private meetings in their periodic reports before 2013. Without data for corporate site visits before the SHSE's 2013 timely disclosure guideline, it is impossible to examine changes in the visiting patterns following the disclosure guideline. Hence we only use the SZSE's 2012 timely disclosure mandate as our setting.

misclassifying visiting analysts as nonvisiting ones because some site visits fall on adjacent dates (Cheng et al., 2016). We follow prior studies (e.g., Gu et al., 2019; Harford et al., 2019; Chen et al., 2022) and focus on analysts who issued at least one forecast during the most recent year (i.e., the past 52 weeks) for a specific firm, which allows us to examine how analysts strategically allocate their attention among firms within their portfolios. By imposing this restriction, we also further mitigate the endogeneity concern that arises from analyst coverage decisions. For every firm *i* during each week *t*, we first identify every nonvisiting analyst *j* who has provided coverage for firm *i* during the past 52 weeks and classify firm *i* as a (non-)visited firm for analyst *j* in week *t* if firm *i* was (not) visited by analysts other than *j* in week *t*. We then compare changes in analyst *j*'s attention to a visited firm, relative to a nonvisited one, subsequent to week *t* before and after the timely disclosure mandate. We adopt the following difference-in-differences design using data from 2009–2011 and 2013–2015.

$$Attention_{i,j,t} = \alpha + \beta_1 Dvisit_{i,-j,t} + \beta_2 Post_{i,j,t} + \beta_3 Dvisit_{i,-j,t} \times Post_{i,j,t} + \beta_4 Other\ Controls + \varepsilon_{i,j,t}. \quad (1)$$

$Dvisit_{i,-j,t}$ is an indicator variable that equals one if at least one of analyst *j*'s peers visits firm *i* in week *t* and zero otherwise. $Post_{i,j,t}$ is an indicator variable that equals one if week *t* is in year 2013–2015 and zero if week *t* is in year 2009–2011.[13] $Attention_{i,j,t}$ captures the attention allocated by analyst *j* to firm *i* after week *t*. To capture subsequent attention, we examine whether analysts tend to visit firm *i* during subsequent weeks. We measure $DvisitF2_{i,j,t}$ (where the letter *F* indicates *Future*) as an indicator variable that equals one if analyst *j* visits the firm during the subsequent two weeks and zero otherwise. As some firms might delay the disclosure or the nonvisiting analysts

[13] We use a linear probability model, rather than a probit or logit model, because the marginal effects for interaction terms do not have a clear interpretation in nonlinear models (e.g., Ai and Norton, 2003). In addition, with the inclusion of fixed effects, these models may impose a potential bias or inconsistency on the coefficients and standard errors (Greene, 2010).

might not timely schedule the subsequent visit, we also use $DvisitF4_{i,j,t}$ as an indicator variable that equals one if analyst *j* visits the firm during the subsequent four weeks and zero otherwise.[14] In addition, firms might combine several visit requests that originally had different requested dates, and the concern of combining visits is lower given a longer window, that is, during the next four weeks rather than two weeks. We also conduct the analysis using attention during the subsequent one or three weeks, and our main inference remains the same. Our main variable of interest, $Dvisit_{i.-j,t} \times Post_{i,j,t}$, captures how analysts change their attention allocated to firms that were visited by other analysts, relative to firms that were not, from the pre-period (when visited firms are not required to make timely disclosures in 2009–2011) to the post-period (when visited firms must make timely disclosures in 2013–2015). Figure 1 provides an illustration of the research design. We report and discuss the results in Section 3.1.

To understand better the impact on analysts' attention, we next examine whether nonvisiting analysts increase attention to nonvisited firms whose industry peer firms are visited by other analysts. We estimate the following regression equation using the sample of nonvisited firm-weeks.

$$Attention\_nonvisit_{i,j,t} = \alpha + \beta_1 Peervisit_{i,-j,t} + \beta_2 Post_{i,j,t} + \beta_3 Peervisit_{i,-j,t} \times Post_{i,j,t} + \beta_4 Other\ Controls + \varepsilon_{i,t}. \quad (2)$$

$Peervisit_{i.-j,t}$ captures the proportion of peer firms listed on SZSE within firm *i*'s industry that are visited by analysts other than *j* during week *t*.[15] $Post_{i,j,t,}$ is an indicator variable that equals one if week *t* is in years 2013–2015 and zero if week *t* is in years 2009–2011. $Attention\_nonvisit_{i,j,t}$

[14] Before 2012, site visits that occurred close to the periodic reporting dates might have been disclosed promptly. This works against finding any results. The results are similar if we exclude site visits before 2012 that occurred and were disclosed within two weeks in the periodic reports.

[15] We include only SZSE-listed firms, as there is no data for corporate site visits of SHSE-listed firms before the SHSE's 2013 timely disclosure mandate. For this test, we use firms that are not visited by analysts during the week. We also exclude those analysts who visited peer firms within firm *i*'s industry during week *t*.

captures the subsequent attention allocated to a nonvisited firm *i* by analyst *j* who did not visit any peer firms in week *t*. To capture subsequent attention, we examine whether analyst *j* visits firm *i* during subsequent weeks. Specifically, $DvisitF2_{i,j,t}$ ($DvisitF4_{i,j,t}$), is an indicator variable that equals one if analyst *j* visits firm *i* in the subsequent two weeks (four weeks) and zero otherwise. Our main variable of interest, $Peervisit_{i,-j,t} \times Post_{i,j,t}$, captures how a nonvisiting analyst changes attention allocated to a nonvisited firm when more of its industry peers host visits from the pre-period (2009–2011) to the post-period (2013–2015).

We also examine the impact of the timely disclosure mandate on the information environment of visited and nonvisited firms. We follow the literature and use two measures of the information environment. One is the change in forecast accuracy around visits, which measures the extent to which analysts incorporate *new* information from site visits and thus the quality of analyst coverage (e.g., Cheng et al., 2016; Chen et al., 2021). The second is stock return synchronicity, which measures the extent to which stock prices reflect *firm-specific* information (e.g., Durnev et al., 2003; Crawford et al., 2012).[16] We use the difference-in-differences research design at the firm-week level and employ the following regressions.

$$\Delta AccuracyF_{i,t} \text{ or } SynchF_{i,t} = \alpha + \beta_1 Dvisit_{i,t} + \beta_2 Post_{i,t} + \beta_3 Dvisit_{i,t} \times Post_{i,t} + \beta_4 Other\ Controls + \varepsilon_{i,t}; \quad (3)$$

$$\Delta AccuracyF_{i,t} \text{ or } SynchF_{i,t} = \alpha + \beta_1 Peervisit_{i,t} + \beta_2 Post_{i,t} + \beta_3 Peervisit_{i,t} \times Post_{i,t} + \beta_4 Other\ Controls + \varepsilon_{i,t}, \quad (4)$$

where $Dvisit_{i,t}$ is an indicator variable that equals one if at least one analyst visits firm *i* in week *t* and zero otherwise, and $Peervisit_{i,t}$ captures the proportion of peer firms listed on SZSE within

[16] By construction, the stock return synchronicity measure can also measure the extent to which stock prices reflect market- and industry-level information. Considering the limitation, we use change in forecast accuracy around visits as another proxy for information environment.

firm *i*'s industry that are visited by analysts during week *t*. Following prior literature (e.g., Cheng et al., 2016; Chen et al., 2021), $\Delta AccuracyF_{i,t}$ is the change in forecast accuracy around week *t*, and $SynchF_{i,t}$ is stock return synchronicity for firm *i* in the subsequent weeks. $\Delta AccuracyF2_{i,t}$ ($\Delta AccuracyF4_{i,t}$) captures the additive inverse of the change in the average of the absolute forecast errors from forecasts made by all analysts for firm *i* up until 26 weeks before week *t* to the latest forecast made by each analyst for firm *i* in two (four) weeks after week *t*. $SynchF2_{i,t}$ ($SynchF4_{i,t}$) captures the stock return synchronicity in the subsequent two (four) weeks. To estimate the stock return synchronicity for each firm-week observation, we regress daily stock returns on the current day's market return and the current day's industry return and obtain the adjusted coefficient of determination (adjusted R^2).

$$RET_{i,d} = \alpha + \beta_1 MKTRET_d + \beta_2 INDRET_d + \varepsilon_{i,t}, \quad (5)$$

where $RET_{i,d}$, $MKTRET_d$, and $INDRET_d$ are the daily stock-, market-, and industry-level returns on day *d*, respectively, during the two (four) weeks after week *t*. Stock return synchronicity is calculated as the natural logarithm of $\frac{R^2_{i,t}}{1-R^2_{i,t}}$, following the literature (Piotroski and Roulstone, 2004; Crawford et al. 2012).[17]

Following the analyst literature, we include several firm-level control variables: firm size (*Size*), leverage (*Leverage*), return on assets (*ROA*), market-to-book ratio (*MB*), large blockholder ownership (*Top1*), state-ownership status (*SOE*), trading volume (*TV*), market-adjusted stock return over the prior four weeks (*Return*), stock return volatility (*STD*), and the number of following analysts (*Analyst*). Following Gu et al. (2019) and Li et al. (2020), we also include analyst-level control variables: the number of analysts within the analyst's brokerage (*Brokersize*),

[17] As a robustness test, we also calculate synchronicity by regressing daily stock returns on the current and prior day's market return and the current and prior day's industry return and obtain adjusted R^2. The results are reported in Table A1 of the online appendix.

the number of firms covered by the analyst (*Companies*), and an indicator variable equal to one if the analyst is a star analyst (*Star*). To capture the relationship between the analyst and the firm, we also include analyst-firm level controls including the length of the analyst's coverage history (*Firmexperience)* and an indicator variable equal to one if the analyst is connected to at least one top executive of the firm (*Connected*).[18] We include firm and year-week fixed effects to control for differences in analysts' attention allocation in different year-weeks and across different firms. We also include analyst fixed effects to identify the effect within an analyst. In all the tables reported, the coefficients on *Post* are omitted because of the year-week fixed effects. To alleviate concerns about residual serial correlation and adjust for heteroscedasticity, we two-way cluster standard errors at the firm level and year-week level. The results are qualitatively similar if we cluster standard errors at the firm level, analyst level, or analyst and year-week level.

2.3 Sample Selection and Descriptive Statistics

Our sample consists of SZSE-listed firms in the 2009–2011 pre-period and the 2013–2015 post-period. We begin our sample selection from 2009 when most firms listed on SZSE report private meetings in their regular periodic reports. (Han et al., 2018). We exclude observations in 2012 in case some firms delayed the adoption of the timely disclosure mandate. We also exclude firms that had no or poor disclosures about site visits before 2012.[19] We follow the literature (e.g., Gu et al., 2019; Chen et al., 2022) and identify analysts who issued at least one forecast during the

[18] Most firms do not report the names of the visiting analysts before 2012, so we assume that the analyst in a brokerage who has covered the firm during the most recent year is the visiting analyst, as a brokerage typically has one analyst covering a specific firm (Cheng et al., 2019). We follow Li et al. (2020) and define *Connected* analysts as those who have close connections with firm managers via school or business ties or geographic proximity. We use the attendance at the same colleges and universities and investment banking relationships to identify school and business ties, respectively. We identify an analyst as being geographically proximate to a firm if the brokerage is located in the same city as the firm's headquarters.

[19] Firms listed on the SME and GEM boards were not mandated to disclose private meetings before 2012. Therefore, in the pre-period, no disclosure by a firm might be due to the fact that it does not have private meetings or that it hosts private meetings but does not disclose them. We drop these observations. We also drop the observations when the firm did not disclose the visiting dates in the pre-period.

most recent year (52 weeks) for a firm-week.[20] We further exclude the analysts who issue no reports for any firm in the current year to ensure that the remaining analysts in our sample are active.[21] We likewise exclude firm-weeks without any market trading (within that week or during one of the preceding or following four weeks) to avoid confounding factors, such as public holidays and trading suspensions. We retain firms with observations available in both the pre- and post-periods. The timely disclosure mandate applies to all investor relations activities, including site visits, conference calls, and media and telephone interviews. We retain only site visits conducted by analysts, the strongest setting, because they demand significant time and resources (e.g., Cheng et al., 2016; Han et al., 2018; Cheng et al., 2019; So et al., 2020).

Following the literature (Dong et al., 2020; So et al., 2020; Chen et al., 2022), we obtain the data on investor relations activities after July 2012 from the China Listed Firm's Investor Relations Database under the China Stock Market & Accounting Research Database (CSMAR). CSMAR collects the records of site visits from the *HudongYi* platform.[22] For investor relations activities before July 2012, we use the data from the Corporate Site Visit Database (CSVD), developed by Datago Technology Limited, which collects the records of site visits disclosed in firms' periodic reports. Analyst forecast data are obtained primarily from three databases: CSMAR, Chinese Research Data Services (CNRDS), and RESSET. The other variables are all from the CSMAR database. We winsorize continuous variables at the 1% and 99% levels.

[20] In our full sample, 65.2% of analysts who visit a certain firm during a certain week do not have recent forecasting history with the visited firm, and so, by imposing this restriction, we do not include those analysts in the firm-week-analyst-level test. We examine whether our main results are robust to including analysts who did not cover the firm in the most recent 52 weeks in Section 7.1.

[21] As robustness test, we further require that a brokerage conducted at least one site visit to a sample firm during 2009-2015, exclude firms or analysts without any site visits for each year or only keep firm-analysts with at least one observation for both periods before and after 2012. The results are qualitatively similar.

[22] As our arguments are based on the timely disclosure of corporate site visits on the *HudongYi* platform, we use the data from CSMAR that collects the records of site visits from the platform. After 2012, firms still summarize this information in their periodic reports. However, after 2012, site visit disclosures in periodic reports occasionally miss key information, such as the participants. We rely on CSMAR data after 2012.

The resulting final sample includes 1,498,893 firm-week-analysts, 138,996 firm-weeks, 4,104 distinct analysts, and 791 distinct firms. Table 1 reports the descriptive statistics. In Panel A, we report the time trend for corporate site visits of our sample firms. The numbers of visits per broker (column (3)) and brokers per visit (column (4)) are higher in the post-period than the pre-period, while the number of visits per firm (column (2)) is lower. This trend may reflect the growing sizes of both listed firms on the SZSE and analysts per broker during the examined period.[23] More importantly, in column (2), the variation of the number of visits for each firm is lower in the post-period, suggesting that analyst visits are more dispersed among all firms. We find a similar pattern when using all firms that are listed on SZSE instead of our sample firms.

Panel B reports the summary statistics of our variables. We find that, on average, 1.7% of analysts visit the firm two weeks after each visit week, and 3.3% of them visit four weeks after each visit week. For each analyst during each week, 12.5% of the analyst's covered firms are visited by other analysts, 56.7% of the sample falls in the post-period, 13.1% of analysts are star analysts, and 7.2% of analysts have connections with the firm. The descriptive statistics also suggest that our sample firms are comparable to those in prior studies.

[Insert Table 1 Here]

3. Main Results

3.1 Attention Allocated to Visited Firms

Our main analysis compares the change in analysts' attention to firms visited by other analysts (visited firms), relative to those that were not (nonvisited firms), following the timely disclosure mandate in 2012. We estimate Eq. (1) and report the results in Table 2.

[Insert Table 2 Here]

[23] Our documented trend is consistent with the work of Chen et al. (2021). This trend might also be driven by firms combining multiple visiting requests into one visit, which we explore as one of alternative explanations in Section 6.1.1.

Columns (1)–(2) report the change in nonvisiting analysts' attention to visited firms, relative to nonvisited ones, during the subsequent two (*DvisitF2*) and four weeks (*DvisitF4*), respectively. The coefficients on *Dvisit* are positive across the two columns (coef. = 0.007; t-stat. = 7.7 and coef. = 0.010; t-stat. = 6.95, respectively), indicating that, on average, during the pre-period, when a firm hosts site visits during a week, the analyst is more likely to visit the firm subsequently. In column (1), the coefficient on the variable of interest, *Dvisit*×*Post*, is negative and statistically significant at the 5% level (coef. = -0.004; t-stat. = -2.21). In column (2), the coefficient on the variable of interest, *Dvisit*×*Post*, is negative and statistically significant at the 1% level (coef. = -0.008; t-stat. = -3.29). The results suggest that, following the timely disclosure mandate, nonvisiting analysts decrease the propensity to visit firms that were visited by other analysts, relative to those that were not. The results are consistent with the conjecture that, following the timely disclosure mandate, nonvisiting analysts tend to reduce attention allocated to visited firms relative to nonvisited firms. The effects we document are economically significant. For example, compared to a nonvisited firm during the same week, a nonvisiting analyst decreases his or her propensity to visit a firm that was visited by other analysts during the next four weeks by 0.8%, which equals approximately 24% of the sample average.

Our key identifying assumption is that the parallel trends assumption is satisfied. That is, absent treatment (that is, the 2012 timely disclosure mandate), nonvisiting analysts' attention to visited (treated) and nonvisited (control) firm-weeks should exhibit parallel trends in the outcome variable, *Attention*. While the parallel trends assumption is not directly testable (since the trend in *Attention*, absent the 2012 timely disclosure mandate, is not observable), following other difference-in-differences studies, we examine the trends in *Attention prior to* the event of interest (Roberts and Whited, 2013). In Figure 2, we plot the difference between *DvisitF4* for each year

between 2009 and 2015 for visited and nonvisited firm-weeks. To do so, we re-run Eq. (1) modified to include separate indicators to capture each year in 2009–2011 and 2013–2015, respectively (interacted with *Dvisit*, similar to the indicator *Post*). We use 2011 as the benchmark year, so each point on the graph shows the difference between nonvisiting analysts' attention allocated to visited and nonvisited firm-weeks, *relative to* the difference in 2011. In Figure 2, there is no evidence showing that nonvisiting analysts' attention allocated to visited and nonvisited firm-weeks have different trends preceding 2012, whereas the tendency of their subsequent visits to visited firm-weeks appears to decrease, relative to nonvisited firm-weeks, after 2012. When we use *DvisitF2* to plot the parallel trends, the inference remains the same. Overall the parallel trends assumption seems to be satisfied.

3.2 The Role of Information Advantage

In this section, we examine whether the documented differential change in attention varies with the degree of visiting analysts' information advantage. We expect a larger reduction in nonvisiting analysts' attention to visited firms, relative to nonvisited ones, if visiting analysts have a stronger information advantage. Lacking a direct measure of information advantage, we use two proxies for the extent to which visiting analysts have the expertise and can learn from visits. If an analyst has visited a company before, we expect that the analyst can better assemble an information mosaic from another visit (Soltes, 2014; Cheng et al., 2016). If an analyst is rated as a star analyst, we likewise expect that analyst to be better able to analyze information from a visit. Therefore, we predict the main results to be more pronounced if, during the most recent year, at least one visiting analyst has (i) visited the visited firm and (ii) been rated as a star analyst.

To test the predictions regarding visiting analysts' information advantage, we estimate two regressions as follows.

$$Attention_{i,j,t} = \alpha + \beta_1 Dvisit\_RecentVisit_{i,-j,t} + \beta_2 Dvisit\_NoRecentVisit_{i,-j,t} + \beta_3 Post_{i,j,t} + \beta_4 Dvisit\_RecentVisit_{i,j,t} \times Post_{i,j,t} + \beta_5 Dvisit\_NoRecentVisit_{i,j,t} \times Post_{i,j,t} + \beta_6 Other\ Controls + \varepsilon_{i,j,t}, \quad (6)$$

where *Dvisit_RecentVisit*$_{i,-j,t}$ equals one if at least one visiting analyst has visited firm *i* during the most recent 52 weeks and zero otherwise, and *Dvisit_NoRecentVisit*$_{i,-j,t}$ equals one if none of the visiting analysts have visited the firm during the most recent 52 weeks and zero otherwise. For nonvisited firms, *Dvisit_RecentVisit*$_{i,-j,t}$ and *Dvisit_NoRecentVisit*$_{i,-j,t}$, equal zero.

$$Attention_{i,j,t} = \alpha + \beta_1 Dvisit\_Star_{i,-j,t} + \beta_2 Dvisit\_NoStar_{i,-j,t} + \beta_3 Post_{i,j,t} + \beta_4 Dvisit\_Star_{i,-j,t} \times Post_{i,j,t} + \beta_5 Dvisit\_NoStar_{i,-j,t} \times Post_{i,j,t} + \beta_6 Other\ Controls + \varepsilon_{i,j,t}, \quad (7)$$

where *Dvisit_Star*$_{i,-j,t}$ equals one if at least one visiting analyst has been rated as a star analyst during the most recent 52 weeks and zero otherwise and *Dvisit_NoStar*$_{i,-j,t}$ equals one if none of the visiting analysts has been rated as a star analyst during the most recent 52 weeks and zero otherwise. A star analyst is someone who has been rated as such by *New Fortune Magazine*.

The results are reported in Table 3. Panels A and B report the results based on whether at least one visiting analyst has visited the firm or been rated as a star analyst during the most recent year, respectively. In Panel A, in columns (1)–(2), the coefficients on *Dvisit_RecentVisit*×*Post* are negative and significant at the 1% level, indicating that, following the timely disclosure mandate, nonvisiting analysts reduce their attention allocated to a firm visited by other analysts that had visiting experience with the firm. Furthermore, the two coefficients on *Dvisit_RecentVisit*×*Post* and *Dvisit_NoRecentVisit*×*Post* are significantly different at the 1% and 5% levels (*p*-values = 0.000 and 0.029, respectively). The results suggest that our main results are driven by visited firms that involved analysts who have visited those firms recently. In Panel B, in columns (1)–(2), the

coefficients on *Dvisit_Star×Post* are negative and significant at the 1% level, indicating that, following the timely disclosure mandate, nonvisiting analysts reduce their attention allocated to firms visited by star analysts. However, the two coefficients on *Dvisit_Star×Post* and *Dvisit_NoStar×Post* are significantly different at the 1% level (*p*-values = 0.000 and 0.001, respectively). The results suggest that our main results are more pronounced for visited firms that involved star analysts. Overall, our findings suggest a larger reduction in nonvisiting analysts' attention to visited firms, relative to nonvisited ones, if visiting analysts have a stronger information advantage.

[Insert Table 3 Here]

3.3 Externalities on Attention Allocation for Nonvisited Firms

As nonvisiting analysts reduce attention to visited firms, the next question is whether the reduced attention to visited firms implies an overall reduction of analysts' attention or enhanced attention at some of the nonvisited firms. We thus examine whether nonvisited peer firms experience positive externalities in the form of increased attention. Once the information advantage is revealed, following the timely disclosure mandate, nonvisiting analysts may visit nonvisited firms in the same industry, for which they are more likely to retain an information advantage. To examine this question, we estimate Eq. (2), using the sample of firm-weeks that were not visited by analysts, and report the results in Panel A of Table 4. We add the average abnormal returns of peer firms, *INFO_IND*, as an additional control variable to mitigate the concern that the disclosed site visits might reveal the existence of industry-level value-relevant information, as studies have found that information disclosure by one firm's industry peers can be useful for valuing itself (e.g., Foster, 1981; Bushee and Leuz, 2005; Hong et al., 2007; Hao et al.,

2011; Shroff et al., 2017).[24]

The coefficients on the variable of interest, *Peervisit*×*Post*, are positive and statistically significant at the 1% level (coef.= 0.026; t-stat.= 3.37 and coef.= 0.032; t-stat.= 2.85, respectively), suggesting that, following the timely disclosure mandate, nonvisiting analysts increase attention to a nonvisited firm during subsequent weeks when more of its industry peers hosted visits.

We further address the concern that the disclosure of a visit might reveal the existence of industry-level value-relevant information, which might attract nonvisiting analysts' attention to the nonvisited peer firms in the post-period. If this explanation drives our results, then we would expect the results to be stronger if it is more likely that visits to peer firms reveal industry-level information, which would be reflected in the abnormal returns of peer firms. Thus we examine whether the results in Panel A of Table 4 vary based on the average abnormal returns of peer firms each week. We report the results in Panel B of Table 4. The coefficients on *Peervisit*×*Post*×*INFO_IND* are insignificant (coef.= 0.140; t-stat.= 0.61 and coef.= -0.105; t-stat.= -0.39, respectively), suggesting that the extent to which nonvisiting analysts increase attention to a nonvisited firm does not vary based on industry-level information. Untabulated results also suggest that the inference is unchanged if we use the absolute value of abnormal returns of peer firms to capture the industry-level information.[25]

[Insert Table 4 Here]

[24] Once the visits are disclosed, the revealed existence of industry-level information might attract nonvisiting analysts' attention to these nonvisited firms. However, it is unclear how the existence of industry-level information could explain the reduction in attention allocated to *visited* firms, as shown in Table 2, as nonvisiting analysts should also increase attention paid to visited firms. Untabulated results also suggest that the main results in Table 2 do not vary based on the extent to which a visit might reflect the existence of industry-level information, as captured by the average abnormal returns of peer firms and their absolute values.

[25] To the extent that the industry-related information might not be fully reflected in the returns during the week, we conduct analyses using the abnormal returns of peer firms and their absolute values during the next four weeks to proxy for the industry-level information. Our inferences are unchanged.

4. Information Environment of Firms

Next we examine the effects of the timely disclosure on firms' information environment as a result of analysts' attention reallocation. We use two measures of the information environment. The first one is the change in analyst forecast accuracy around site visits, as this reflects the extent to which analysts incorporate new information after site visits. The second is stock return synchronicity, as this reflects the extent to which stock prices reflect firm-specific information.

4.1 Information Environment of Visited Firms

We first examine the effects on the information environment of firms that were visited by analysts. The aggregate attention and effort that a firm receives from all analysts contribute to its information environment. On the one hand, visited firms may suffer a deterioration in their information environment as nonvisiting analysts reduce the attention allocated to visited firms. On the other hand, the information environment of visited firms may not change as nonvisiting analysts are more likely to reduce attention to firms that were visited by analysts with information advantages who are more likely to contribute to price discovery. Therefore, it is an empirical question whether visited firms experience a change in the information environment.

To investigate this question, we estimate Eq. (3), using change in forecast accuracy around visits and stock return synchronicity as the dependent variable, respectively, and report the results in Panels A and B of Table 5. We do not find the coefficient on the variable of interest, *Dvisit* ×*Post*, to be significant in Panel A, while in Panel B we find it becomes positive and statistically significant at the 10% level when we examine return synchronicity during the next two weeks (coef.= 0.053; t-stat.= 1.73) and then insignificant when we examine return synchronicity during the next four weeks. Combing the results in both Panels A and B does not suggest an overall deterioration of the information environment of the visited firm, despite some evidence suggesting that, following the timely disclosure mandate, the stock price probably does not incorporate new

information as quickly in the first two weeks after the visit. One potential explanation is that visiting analysts process and disseminate information in a slower fashion because they face less competition as nonvisiting analysts are less likely to make follow-up visits.

[Insert Table 5 Here]

4.2 Information Environment of Nonvisited Firms

We next examine the effects on the information environment of firms that were not visited by analysts. The results in Section 3.3 suggest that, following the timely disclosure mandate, peer firms experience positive externalities in the form of increased attention, which may improve the information environment of nonvisited firms. We estimate Eq. (4) and report the results in Table 6. In Panel A, we report the results using the change in forecast accuracy around visits as the dependent variable. The coefficients on the variable of interest, *Peervisit*×*Post*, are positive and statistically significant at the 1% and 10% levels (coef.= 0.056; t-stat.= 2.92 and coef.= 0.059; t-stat.= 1.71, respectively), suggesting that, following the timely disclosure mandate, as more of the peer firms host site visits, the nonvisited firm experiences a larger improvement in analyst forecast accuracy. In Panel B, we report the results using return synchronicity as the dependent variable. The coefficient on the variable of interest, *Peervisit*×*Post*, is negative and statistically significant at the 10% and 1% levels when we examine return synchronicity during the next two and four weeks (coef. = -0.295; t-stat. = -1.76 and coef. = -0.452; t-stat. = -2.75, respectively), suggesting that, following the timely disclosure mandate, as more of the peer firms host site visits, the nonvisited firm experiences a larger reduction in return synchronicity.

[Insert Table 6 Here]

In summary, following the timely disclosure mandate, the information environment of a nonvisited firm improves as more of its industry peers host visits. By influencing the allocation of

analysts' attention, the timely disclosure of corporate site visits has positive externalities in the form of an improved information environment.

5. Long-term Effects

Thus far we have focused on analysts' attention shortly after a site visit within four weeks, but it is pertinent to examine longer-term consequences, too.

5.1 Long-term Attention Allocation

Our results suggest that, when analysts can observe that a firm is visited by other analysts following the timely disclosure mandate, they reduce attention to the visited firm shortly after the visit. It is natural to ask whether the short-term effects are transient or have consequences over the entire year. Even if there is a reduction in analyst attention shortly after a visit, overall analyst attention in the long term may not change if the short-term reduction is balanced by a future increase. We thus examine an analyst's tendency to visit a firm during the next 52 weeks after each week. We further examine coverage during the next 52 weeks, as any change in analyst attention might also affect coverage decisions. We re-run regression Eq. (1) but use the tendency to visit the firm (*DvisitF52*) and the tendency to cover the firm (*DcoverF52*) during the next 52 weeks as the dependent variable, respectively. The results are reported in Panel A of Table 7.

[Insert Table 7 Here]

In column (1), the coefficient on *Dvisit*×*Post* is negative and significant at the 5% level (coef.= -0.021; t-stat.= -2.52), indicating that, following the timely disclosure mandate, in the subsequent year, nonvisiting analysts reduce the tendency to visit the firms that were visited by peer analysts, relative to those that were not. In column (2), the coefficient on *Dvisit*×*Post* is negative and significant at the 10% level (coef.= -0.008; t-stat.= -1.89), indicating that, following the timely disclosure mandate, in the subsequent year, nonvisiting analysts are more likely to drop

coverage for firms that were visited by peer analysts, relative to those that were not. Overall, the results suggest that the reduction in nonvisiting analysts' attention persists over the long term.[26]

5.2 Long-term Information Environment of Visited Firms

We next examine the information environment of visited firms during the next year. We do not find consistent evidence suggesting an immediate reduction in the information environment in Section 4.1. To investigate the impact on visited firms' information environment over a longer term, we re-run regression Eq. (3) but use change forecast accuracy (∆*AccuracyF52*) and stock return synchronicity (*SynchF52*) during the next 52 weeks as the dependent variable, respectively. The results are reported in Panel B of Table 7. In both columns (1) and (2), the coefficients on *Dvisit*×*Post* are insignificant. The results suggest that, despite a reduction in nonvisiting analysts' visits and coverage, there is no adverse effect on visited firms' information environment.[27]

5.3 Changes in Analysts' Long-term Visiting and Coverage Patterns

Lastly, we examine whether the timely disclosure mandate changes how analysts choose to follow and conduct site visits from a more general perspective. If nonvisiting analysts reduce attention to firms that were visited by other analysts following the timely disclosure mandate as suggested in Section 5.1, we would expect a reduction in the tendency of visiting and covering firms with certain characteristics that attracted more analyst attention, that is, more dispersed

[26] To complement the analyses in Section 5.1, we conduct an analysis by using firm-year-analyst as the unit of observations. Specifically, we regress the analyst tendency to visit the firm during a year (*DvisitYear1*) and cover the firm during a year (*DcoverYear1*), respectively, on the frequency of which the firm was visited by peer analysts during the previous year (*AnnualVisit*) and its interaction with *Post*. We find the coefficients on *AnnualVisit*×*Post* to be negative and significant at the 5% level, corroborating with our results in Section 5.1. The results are reported in Table A2 of the online appendix.

[27] To complement the analyses in Section 5.2, we conduct an analysis by using firm-year as the unit of observations. Specifically, we regress analyst forecast accuracy during a year (*AccuracyYear1*) and stock return synchronicity during a year (*SynchYear1*), respectively, on the frequency of which the firm was visited during a year (*AnnualVisit*) and its interaction with *Post*. The results suggest that there is no deterioration in the information environment of visited firms over the following year. The results are reported in Table A3 of the online appendix.

analyst coverage and visits among all firms. To test this conjecture, we examine whether analysts tend to visit and cover firms with certain characteristics, and how the patterns changed after the timely disclosure mandate. We estimate the following regression equation at the firm-year-analyst level.

$$DvisitYear1 \ or \ DcoverYear1$$
$$= \alpha + \beta_1 Analyst\ Charateristics_{j,t} + \beta_2 Analyst\text{-}Firm\ Characteristics_{i,j,t}$$
$$+ \beta_3 Firm\ Charateristics_{i,t} + \beta_4 Analyst\ Characteristic_{j,t} \times Post_{i,j,t}$$
$$+ \beta_5 Analyst\text{-}Firm\ Characteristics_{i,j,t} \times Post_{i,j,t}$$
$$+ \beta_6 Firm\ Characteristics_{i,t} \times Post_{i,j,t} + \varepsilon_{i,j,t}, \quad (8)$$

where *Analyst Characteristics*$_{j,t}$ (including *Brokersize*, *Companies*, and *Star*), *Analyst-Firm Characteristics*$_{i,j,t}$ (including *Firmexperience* and *Connected*), and *Firm Characteristics*$_{i,t}$ (including *Size*, *Leverage*, *ROA*, *MB*, *Top1*, *SOE*, *TV*, *Return*, *STD*, and *Analyst*) are introduced and defined in Section 2.2 with details in Appendix 2. *DvisitYear1* (*DcoverYear1*) is an indicator variable equal to one if the analyst visits (covers) the firm during the next year.

We report the results in Panel C of Table 7. In column (1), we report changes in analysts' tendency to *visit* a firm during a year. The coefficient on *Analyst* is positive and significant at the 1% level, while the coefficient on *Analyst*×*Post* is negative and significant at the 1% level, indicating that analysts are more likely to visit firms with higher analyst coverage in the pre-period but the tendency weakens in the post-period. We do not find any evidence suggesting an increase in the tendency of analysts visiting firms with characteristics that received more attention.

Column (2) reveals interesting patterns in analyst coverage decisions. The coefficients on *Connected* and *Analyst* are positive and significant at the 1% level, and the coefficients on *Connected*×*Post* and *Analyst*×*Post* are negative and significant at the 10% and 1% levels,

indicating analyst coverage to focus less on firms they are connected with and those with higher analyst coverage. The coefficients on *STD* and *Companies* are negative and significant at the 5% and 1% levels, and the coefficients on *STD*×*Post* and *Companies*×*Post* are positive and significant at the 1% and 5% levels, indicating a weakened tendency of analysts covering firms with low volatility and a trend of brokerages covering more firms. The coefficients on *Star* and *Star*×*Post* are positive and significant at the 5% and 10% levels respectively, indicating that firms are more likely to receive coverage from star analysts. This result on star analysts suggests that more firms have the opportunity to receive high quality of coverage from star analysts after the mandate. Overall, our results suggest more dispersed analyst coverage among all firms as the timely disclosure mandate promptly reveals analysts' advantages in information access and production.

6. Alternative Explanations

6.1 Alternative Explanation: Firms' Impact on Site Visits

6.1.1 Firms Combining Visits

Although most site visits are initiated by analysts or investors and firms are required to accommodate their requests, as suggested in the "Guidelines of Investor Relations Management" issued by the SZSE, firms may negotiate the visiting date (Cheng et al., 2019). In particular, firms may combine several visit requests that originally had different requested dates into a single visit, following the timely disclosure mandate. In this way, instead of having to disclose several times (possibly over several weeks), firms would only need to do so once, reducing compliance costs. This scenario might explain our results, although it is unclear whether analysts would be willing to accommodate firms' visit combination requests and whether the compliance costs are significant enough to drive firms to do so. We attempt to empirically mitigate this concern in several ways. First, if the number of analysts per visit increases significantly, following the timely disclosure

mandate, it is more likely that the firm has combined visits. We therefore re-run Eq. (1) and exclude firms that experience a significant increase in the number of analysts per visit. Second, during a week when only one analyst visits a firm, it is unlikely that the firm combines visits to reduce its compliance burden. We therefore re-run Eq. (1) and exclude observations that involve multiple visiting analysts.

We report the results in Panel A of Table 8. In columns (1) and (2), we report the results after we remove firms that experience a significant increase in the number of analysts per visit. We classify such firms as those that experience an increase in the average number of analysts per visit, and the increase is above the median. The coefficients on *Dvisit*×*Post* are both negative and significant at the 5% and 1% level (coef.= -0.005; t-stat.= -2.38 and coef.= -0.010; t-stat.= -3.61, respectively). After we remove firm-week-analyst observations that involve multiple visiting analysts, while the coefficient on *Dvisit*×*Post* is not significant in column (3), it is negative and significant at the 5% level in column (4) (coef. = -0.005; t-stat. = -2.10), indicating that, following the timely disclosure mandate, nonvisiting analysts reduce their attention to firms that are visited by a single analyst. It is worth noting that our main effects might be stronger for site visits that involve multiple visiting analysts as more analysts may have gained information advantage. The expected benefits of visiting the same firm decrease to a greater extent, leading nonvisiting analysts to more likely decrease attention to that firm.

[Insert Table 8 Here]

In summary, our main results are not likely driven by the conjecture that firms tend to combine visits after the timely disclosure mandate.

6.1.2 Firms Inviting Analysts

Although most site visits are initiated by analysts or investors, firms occasionally invite analysts and investors to visit. Cheng et al. (2016, 2019) suggest that site visits that occur in the month after major corporate events (i.e., earnings announcements, M&As, seasoned equity offerings) are likely initiated by firms. We therefore examine whether our conclusions hold after excluding these visits. We re-run regression Eq. (1) and report the results in Panel B of Table 8. The coefficients on *Dvisit*×*Post* are still negative and significant at the 5% and 1% levels (coef.= -0.005; t-stat.= -2.18 and coef.= -0.009; t-stat.= -3.30, respectively), consistent with our main conclusions.

6.2 Alternative Explanation: Connected Analysts

To capture the potential effects of connected analysts, we include an indicator variable, *Connected*, that indicates whether an analyst and a firm's management are connected as one of our control variables. However, in Panel C of Table 7, we find a reduction in coverage by connected analysts after the timely disclosure mandate. Although the pattern could result from short-term attention reallocation, it might be driven by the timely disclosure mandate for other reasons. A reduction in coverage by connected analysts could naturally lead to a reduction in visits by these analysts. There could then be fewer follow-up visits by unconnected analysts for those firms, which could be an alternative explanation for our main result. While on average we do not find a reduction in visits by connected analysts in Panel C of Table 7, we examine whether our results hold for firms that did not experience a reduction in coverage or visits by connected analysts. The results are reported in Panel C of Table 8. In columns (1)-(2), we report the results when we exclude firms that experienced a drop in coverage by connected analysts. The coefficients on *Dvisit*×*Post* are negative and significant at the 5% and 1% levels (coef.= -0.006; t-stat.= -2.22 and

coef.= -0.010; t-stat.= -2.91, respectively). In columns (3)-(4), we report the results when we exclude firms that experienced a drop in the percentage of visits by connected analysts. The coefficients on *Dvisit*×*Post* are negative and significant at the 1% level (coef.= -0.006; t-stat.= -2.64 and coef.= -0.010; t-stat.= -3.51, respectively). Overall, our results are not driven by changes in connected analysts' visiting and coverage patterns.

6.3 Alternative Explanation: Timely Disclosure of Meeting Topics

Although our proposed explanation relies on firms' timely disclosure of meeting dates and visiting analysts' identities, meeting topics also must be disclosed promptly, which could confound our results. To mitigate the concern, we examine whether nonvisiting analysts learn useful information from the timelier topic disclosures.

First, we run the following regression to examine whether nonvisiting analysts issue more accurate forecasts after the timely disclosure mandate.

$$\Delta Accuracy\_nonvisitF_{i,j,t} = \alpha + \beta_1 Dvisit_{i,-j,t} + \beta_2 Post_{i,j,t} + \beta_3 Dvisit_{i,-j,t} \times Post_{i,j,t} + \beta_4 Other\ Controls + \varepsilon_{i,j,t}, \quad (9)$$

where $\Delta Accuracy\_nonvisitF_{i,j,t}$ captures the change in the absolute forecast error of nonvisiting analysts *j* for firm *i* around week *t*. $\Delta Accuracy\_nonvisitF2_{i,j,t}$ ($\Delta Accuracy\_nonvisitF4_{i,j,t}$) captures the change in forecast accuracy from 26 weeks before to two (four) weeks after week *t*. We report the results in columns (1) and (2) in Panel D of Table 8. The coefficients on *Dvisit*×*Post* are not statistically significant, suggesting nonvisiting analysts do not learn useful information from the timelier topic disclosures.

We further examine whether nonvisiting analysts issue more accurate forecasts for firms whose disclosures likely contain more information after the timely disclosure mandate.

$$\Delta Accuracy\_nonvisitF_{i,j,t} = \alpha + \beta_1 Dvisit_{i,-j,t} + \beta_2 Dvisit\_Post\_InfoH_{i,-j,t} + \beta_3 Dvisit\_Post\_InfoL_{i,-j,t} + \beta_4 Other\ Controls + \varepsilon_{i,t}, \quad (10)$$

where, for firm *i* that was visited by analyst *j*'s peers during week *t* in the post-period, *Dvisit_Post_InfoH*$_{i,-j,t}$ equals one if the number of words of the disclosure (columns (3) and (4)) is above the sample median during week *t* and zero otherwise, and *Dvisit_Post_InfoL*$_{i,-j,t}$ equals one if the number of words of the disclosure is below the sample median during week *t* and zero otherwise. In columns (3)–(4) in Panel D of Table 8, the coefficients on *Dvisit_Post_InfoH* and *Dvisit_Post_InfoL* are both insignificant and not significantly different from each other (*p*-values = 0.315 and 0.477, respectively). The results confirm that nonvisiting analysts do not learn useful information from the timelier topic disclosures, regardless of the information content of the disclosures.

In summary, our results suggest that nonvisiting analysts do not learn useful information from the timelier topic disclosures, which thus may not likely affect their visits.

6.4 Alternative Explanation: Site Visits as Corporate Access Events

Analysts may arrange site visits to introduce their buy-side clients to management. However, these corporate access events do not preclude analysts from gaining better information (Soltes, 2014). We examine whether our results hold when we exclude visits conducted jointly by analysts and buy-side investors. We re-run regression Eq. (1) and report the results in Panel E of Table 8. The coefficients on *Dvisit*×*Post* are still both negative and significant at the 5% and 1% level (coef.= -0.004; t-stat.= -2.08 and coef.= -0.009; t-stat.= -3.20, respectively), consistent with our main conclusions.

7. Robustness Tests

7.1. Analyses Conducted at the Firm-week Level

Our main analyses are conducted at the firm-week-analyst level, and we identify analysts who have issued at least one forecast during the most recent 52 weeks as those who might visit the firm. In our full sample, 65.2% of analysts who visit a certain firm during a certain week do not have recent forecasting history with the visited firm, and by imposing the restriction, we do not include visits conducted by those analysts in the firm-week-analyst-level test. In addition, by imposing the restriction, we do not include firm-weeks that were not covered by any analysts in the past 52 weeks. In this section, we examine whether our main results are subject to sample selection bias and can be generalized to the omitted analysts and firm-weeks.

As there are 7,716 analysts and 1,329 firms, it is infeasible for us to conduct the regression at the firm-week-analyst level. We thus conduct the analyses and estimate regression Eq. (1) at the firm-week level. In other words, we estimate the following regression equation.

$$Attention_{i,t} = \alpha + \beta_1 Dvisit_{i,t} + \beta_2 Post_{i,t} + \beta_3 Dvisit_{i,t} \times Post_{i,t} + \beta_4 Other\ Controls + \varepsilon_{i,t}. \quad (11)$$

$Dvisit_{i,t}$ is an indicator variable that equals one if firm i hosts at least one visit in week t and zero otherwise. $Post_{i,t}$ is an indicator variable that equals one if week t is in year 2013–2015 and zero if week t is in year 2009–2011. $Attention_{i,t}$ captures the attention allocated by analysts to firm i after week t. To capture the subsequent attention, we examine whether at least one nonvisiting analyst visits firm i during the subsequent two weeks ($DvisitF2_{i,t}$) and four weeks ($DvisitF4_{i,t}$), respectively.

First, we conduct the firm-week-level analyses using the same firm-weeks and analysts as those in the firm-week-analyst-level analyses. Next we conduct the analyses using the same firm-weeks but with analysts who did not cover the firm in the past 52 weeks and with all analysts,

regardless of the coverage status, respectively. Finally, we conduct the analysis using the full firm-week sample including those that were not covered by any analysts in the past 52 weeks. We report the results in Panel A of Table 9. For brevity, we only report the results using *DvisitF4* as the dependent variable. The results using *DvisitF2* as the dependent variable are qualitatively similar. The coefficients on *Dvisit*×*Post* are negative and statistically significant at the 1% level (coef. = -0.051; t-stat. = -4.14 and coef. = -0.047; t-stat. = -3.48 and coef. = -0.049; t-stat. = -3.44 and coef. = -0.061; t-stat. = -4.43, respectively). In terms of the economic magnitude, compared to a nonvisited firm during the same week, nonvisiting analysts decreased their propensity to visit a firm that was visited by other analysts during the next four weeks by 5.1%, 4.7%, 4.9%, and 6.1%, which equal approximately 33%, 20%, 17%, and 27% of the sample average (with respect to each sample). The economic magnitudes are comparable to those estimated at the firm-week-analyst level. The results suggest that our main inference can be generalized to the omitted sample.

[Insert Table 9 Here**]**

7.2 Controlling for Potential Changes in Analyst Visiting Patterns

In this section, we assess the robustness of our results to controlling for potential changes in analyst visiting patterns. Our results in Section 5.3 suggest analyst visits becoming more dispersed among all firms, which can be a consequence of nonvisiting analysts reducing attention to firms visited by other analysts. The timely disclosure mandate might change how analysts' own characteristics, firm characteristics, and their relationship with firms affect analysts' visiting decisions for other reasons. To mitigate this concern, we re-run regression Eq. (1) and control for each control variable interacted with *Post*. The results are reported in Panel B of Table 9. The coefficients on *Dvisit*×*Post* are negative and significant at the 5% and 1% levels (coef.= -0.004; t-

stat.= -2.13 and coef.= -0.008; t-stat.= -3.13, respectively), suggesting that our results are robust to controlling for potential changes in analyst visiting patterns due to other reasons.

7.3 Alternative Measures of Analysts' Attention

In this section, we use an alternative measure to proxy for analysts' attention. Specifically, we re-run Eqs. (1) and (2) but, instead of using the tendency to conduct subsequent site visits as a proxy for analysts' attention, we use the tendency to issue forecasts. We use the tendency of site visits to proxy for analysts' attention in our main results because site visits capture an explicit commitment of time and resources (So et al., 2020), while the tendency to issue forecasts is an implicit outcome of analysts' attention allocation. In addition, in our sample, only 19% of site visits are followed by visiting analysts' earnings forecasts in the month after their visits. The results are reported in Panel C of Table 9. In column (2), the coefficient on *Dvisit*×*Post* is negative and significant (insignificant in column (1)), suggesting that nonvisiting analysts reduce attention allocated to visited firms, following the timely disclosure mandate. In columns (3) and (4), the coefficients on *Peervisit*×*Post* are positive and significant at the 10% level, suggesting that, following the timely disclosure mandate, nonvisiting analysts increase attention to a nonvisited firm during subsequent weeks when more of its industry peers hosted visits. Therefore, by using the tendency of issuing forecasts as an alternative measure of analyst attention, we corroborate our main results.

8. Conclusion

We investigate whether observing information acquisition by peers affects financial analysts' attention allocation. Using the timely disclosure mandate of corporate site visits by the SZSE in 2012 as a setting, we find that nonvisiting analysts reduce attention to visited firms, relative to nonvisited ones. The reduction in nonvisiting analysts' attention allocated to visited firms not only occurs shortly after a visit (within a month) but also persists over the long term

(during the next year). We also document that nonvisited firms experience increased analyst attention and improved information environments when more of their industry peers host visits, suggesting that timely disclosure has positive externalities. By documenting these results, our paper contributes to the literature on the attention allocation of analysts, the strategic interactions among analysts, and the externalities of firms' public disclosures. Our study provides evidence on the consequences of timely mandated disclosure of private meetings, which also contributes to the policy debate regarding the transparency of private meetings between managers and analysts (Bengtzen, 2017) and the call for more research to examine the impact of requiring firms to disclose site visits (Lennox and Wu, 2022).

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Appendix 1 Disclosure of Corporate Site Visits by Tsinghua Unis Co., Ltd

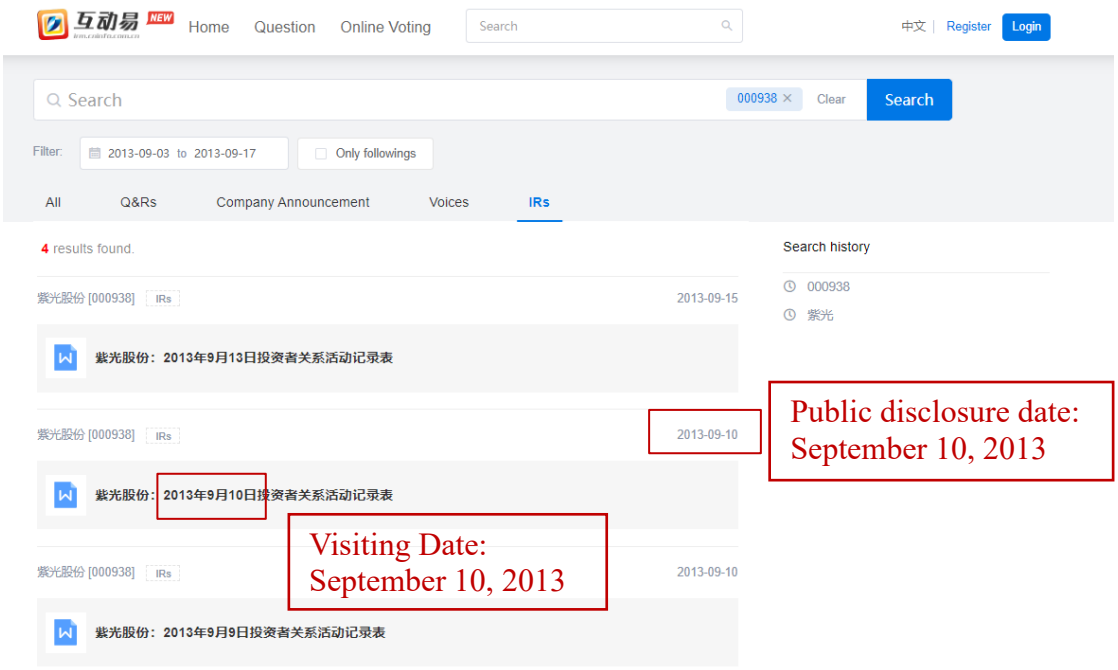
Panel A Disclosure of site visits during the pre-period

Disclosure Date: March 31, 2011

Visiting Date	Location	Format	Visitors	Topics
February 12, 2010	Planning Department	Site visit	Yinhe Securities client manager	Company basic operations and the direction of future development
March 1, 2010	Planning Department	Site visit	Xiangcai Securities analyst	Company basic operations and the direction of future development
October 12, 2010	Planning Department	Site visit	Huatai Securities analyst	Company basic operations and the direction of future development
October 13, 2010	Planning Department	Site visit	Hongyuan Securities analyst	Company basic operations and the direction of future development
November 10, 2010	Planning Department	Site visit	Fangzheng Securities analyst	Company basic operations and the direction of future development

Panel B Disclosure of site visits during the post-period

Disclosure on the *HudongYi* Platform (http://irm.cninfo.com.cn/szse/)



Panel C The Detailed Record of Site Visits by Tsinghua Unis Co., Ltd

Type of Investor Relation Activities	√Specific entity investigation □Analyst conference □Media interview □Performance conference □Press conference □Road show □Site visit □Other （Please explain）
Meeting Participants	Caifu Liang Securities, Huihui Xu, Lingtian Feng, Sijing Chen
Date	September 10, 2013
Location	The meeting room of Unis
Management in Attendance	Board Secretary, Wei Zhang Deputy Manager of Securities Department, Meng Ge
Main Meeting Topics	Company basic operations and direction of future development： 1. Basic Operations Our main business is divided into three categories: (1) own-brand information electronic products represented by digital imaging products; (2) IT services such as software and system integration; (3) value-added distribution business. In the field of self-owned brands, our company is constantly moving towards providing comprehensive industry solutions for digital input. With a foundation of the complete product lines of scanners and HD shooting products as the core digital imaging hardware products, we continue to improve the development and upgrade of digital imaging application software, and establish a rich industry application platform to meet customers' needs in image collection, data processing, classified storage, information extraction, data interaction, etc. In terms of industry applications, in 2012 our company launched a comprehensive management system for catering enterprises. This system helps comprehensively manage the business, procurement, inventory, financial management, employee management, etc. It has been promoted in the Beijing area. In the field of IT services, our company has many experiences and advantages in many fields such as civil affairs, education, transportation, public security, radio and television, and other government agencies and industries.

	While maintaining the stable development of the traditional business, our company has completed the research and development of the "Ziguang" cloud computing management platform, formed a regional e-government cloud and SME service cloud platform, and can provide big data cloud computing solutions. In the field of the value-added distribution business, our company cooperates with well-known domestic and foreign brands such as HP, Dell, Lenovo, BenQ, and Samsung. Our products cover mainstream IT products. We pay attention to the application of modern management methods and have established a perfect information management system. Our value-added distribution business is one of the top domestic distribution service providers. 2. Future Development after the M&As Our company will take this M&A as an opportunity, through business and resource integration, to gradually achieve the strategic goal of becoming a full-service provider in the construction, operation, and maintenance of modern information systems. We have extensive synergies with Nengtong Technology and Shenzhen Rongchuang Tianxia. The M&A can amplify the resources in customer, technology, marketing, and service networks, enabling our company to gain the first-mover advantages in cloud computing, IT operation and maintenance services, mobile internet applications, and big data processing, to further expand the opportunities to improve the smart city business.
Attachments（if any）	No
Date of record	September 10, 2013

Appendix 2 Variable Definitions

Variable	Data Source	Definition
$Dvisit_{i,-j,t}$	CSVD, CSMAR	An indicator variable that equals one if firm *i* is visited by analysts other than analyst *j* in week *t* (i.e., if firm *i* is a visited firm in week *t* for analyst *j*) and zero otherwise.
$Post_{i,t}$	CSVD, CSMAR	An indicator variable that equals one if week *t* is after 2012 and zero otherwise.
$Peervisit_{i,t}$	CSVD, CSMAR	The proportion of peer firms within firm *i*'s industry that are visited during week *t.*
$DvisitF_{i,j.t}$	CSVD, CSMAR	An indicator variable that equals one if the nonvisiting analyst *j* visits firm *i* during subsequent weeks and zero otherwise. $DvisitF2_{i,j,t}$ as an indicator variable that equals one if analyst *j* visits the firm during the subsequent two weeks and zero otherwise. $DvisitF4_{i,j,t}$ as an indicator variable that equals one if analyst *j* visits the firm during the subsequent four weeks and zero otherwise.
$Brokersize_{j,t}$	CSMAR	The total number of analysts hired by analyst *j*'s brokerage in year *t*.
$Firmexperience_{i,j,t}$	CSMAR	The natural log of one plus the number of years between analyst *j*'s initial year of issuing forecast for the firm and year *t*.
$Companies_{j,t}$	CSMAR	The natural log of one plus the number of stocks followed by analyst *j* in year *t*.
$Star_{j,t}$	CSMAR	An indicator variable that equals one if analyst *j* was awarded a star analyst rating by New Fortune Magazine in the previous year and zero otherwise.
$Connected_{i,j,t}$	CSMAR, analyst reports, web search	An indicator variable equal to one if analyst *j* and any of firm *i*' top executives attended the same school, or analyst *j*' brokerage firm served as the lead underwriter for any of the firm *i*'s equity offerings (IPOs or SEOs) within the last five years, or analyst *j*'s brokerage is located in the same city as firm *j*'s headquarters and zero otherwise.
$ROA_{i,t}$	CSMAR	The ratio of net income to total assets.
$Leverage_{i,t}$	CSMAR	The ratio of liability to total assets.
$Size_{i,t}$	CSMAR	The natural log of total assets.
$Analyst_{i,t}$	CSMAR, CNRDS, and RESSET	The natural log of one plus the number of analysts following the firm.
$MB_{i,t}$	CSMAR	The ratio of the market value of equity to the book value of equity.
$TV_{i,t}$	CSMAR	The ratio of trading volume to the number of shares outstanding.
$Return_{i,t}$	CSMAR	The buy-and-hold abnormal return over the prior four weeks.

$STD_{i,t}$	CSMAR	The standard deviation of stock returns in the prior four weeks.
$Top1_{i,t}$	CSMAR	Percentage of stock shares held by the largest shareholder.
$SOE_{i,t}$	CSMAR	An indicator variable that equals one if the firm is state-owned and zero otherwise.
$Dvisit\_RecentVisit_{i,-j,t}$	CSVD, CSMAR	An indicator variable that equals one if at least one visiting analyst has visited firm *i* during the most recent 52 weeks and zero otherwise.
$Dvisit\_NoRecentVisit_{i,-j,t}$	CSVD, CSMAR	An indicator variable that equals one if no visiting analyst has visited firm *i* during the most recent year and zero otherwise.
$Dvisit\_Star_{i,-j,t}$	CSVD, CSMAR CSMAR	An indicator variable that equals one if at least one visiting analyst has been rated as a star analyst during the most recent 52 weeks and zero otherwise.
$Dvisit\_NoStar_{i,t}$	CSVD, CSMAR CSMAR	An indicator variable that equals one if no visiting analyst has been rated as a star analyst and zero otherwise.
$INFO\_IND_{i,t}$	CSMAR	The value-weighted average abnormal return of peer firms of firm *i* during week *t*.
$\Delta AccuracyF_{i,t}$	CSMAR,CNRDS, and RESSET	-1 times the change in the absolute forecast error of all analysts for firm *i* from the 26 weeks before to two or four weeks after week *t*. We identify the most recent (latest) annual EPS forecast issued by each analyst in the 26 weeks prior to (two or four weeks after) week *t*, and calculate the absolute forecast error as the absolute value of the difference between the consensus EPS forecast and actual EPS, scaled by the stock price at the beginning of the year, expressed in percentage. For analysts who do not update their forecasts after week *t*, we assume their forecasts to be the same as their latest forecasts before week *t*. For an analyst who does not issue a forecast before week *t* (but does issue a forecast after week *t*), we use the consensus forecast of all the other analysts' forecasts before week *t* as such analyst's forecast before week *t*.
$SynchF_{i,t}$	CSMAR	The stock return synchronicity for firm *i* in the subsequent two weeks ($SynchF2_{i,t}$) or four weeks ($SynchF4_{i,t}$). $SynchF2_{i,t}$ ($SynchF4_{i,t}$) is calculated as $\log(\frac{R^2_{i,t}}{1-R^2_{i,t}})$, with $R^2_{i,t}$ from the firm-specific regression:$RET_{i,d} = \alpha + \beta_1 MKTRET_d + \beta_2 INDRET_d + \varepsilon_{i,t}$, where $RET_{i,d}$, $MKTRET_d$, and $INDRET_d$ are daily stock-, market-, and industry-level returns (excluding firm *i*'s return), respectively, during the two (four) weeks after week *t*.

$DvisitYear1_{i,j,t}$	CSVD, CSMAR	An indicator variable that equals one if analyst *j* visits firm *i* in year *t*+1 and zero otherwise.
$DcoverYear1_{i,j,t}$	CSVD, CSMAR	An indicator variable that equals one if analyst *j* covers firm *i* in year *t*+1 and zero otherwise.
$Dvisit\_Post\_InfoH_{i,-j,t}$	CSVD, CSMAR	For firm *i* that was visited by analyst *j*'s peers during week *t* in the post-period, $Dvisit\_Post\_InfoH_{i,-j,t}$ equals one if the number of words of the disclosure is above the sample median during week *t* and zero otherwise.
$Dvisit\_Post\_InfoL_{i,-j,t}$	CSVD, CSMAR	For firm *i* that was visited by analyst *j*'s peers during week *t* in the post-period, $Dvisit\_Post\_InfoL_{i,-j,t}$ equals one if the number of words of the disclosure is below the sample median during week *t* and zero otherwise.
$DforecastF_{i,j,t}$	CSMAR, CNRDS, and RESSET	An indicator variable that equals one if analyst *j* issues a forecast during the subsequent two weeks ($DforecastF2_{i,t}$) or four weeks ($DforecastF4_{i,t}$) and zero otherwise.

Figure 1 Research Design Timeline

This figure illustrates the timeline underlying the research design. We utilize the timely disclosure mandate in 2012 to design a difference-in-differences test. For every firm *i* during each week *t*, we first identify every nonvisiting analyst *j* who has provided coverage for firm *i* during the past 52 weeks and classify firm *i* as a (non-)visited firm for analyst *j* in week *t* if firm *i* was (not) visited by analysts other than *j* in week *t*. We then compare changes in analyst *j*'s attention to a visited firm, relative to a nonvisited one, subsequent to week *t*, before and after the timely disclosure mandate.

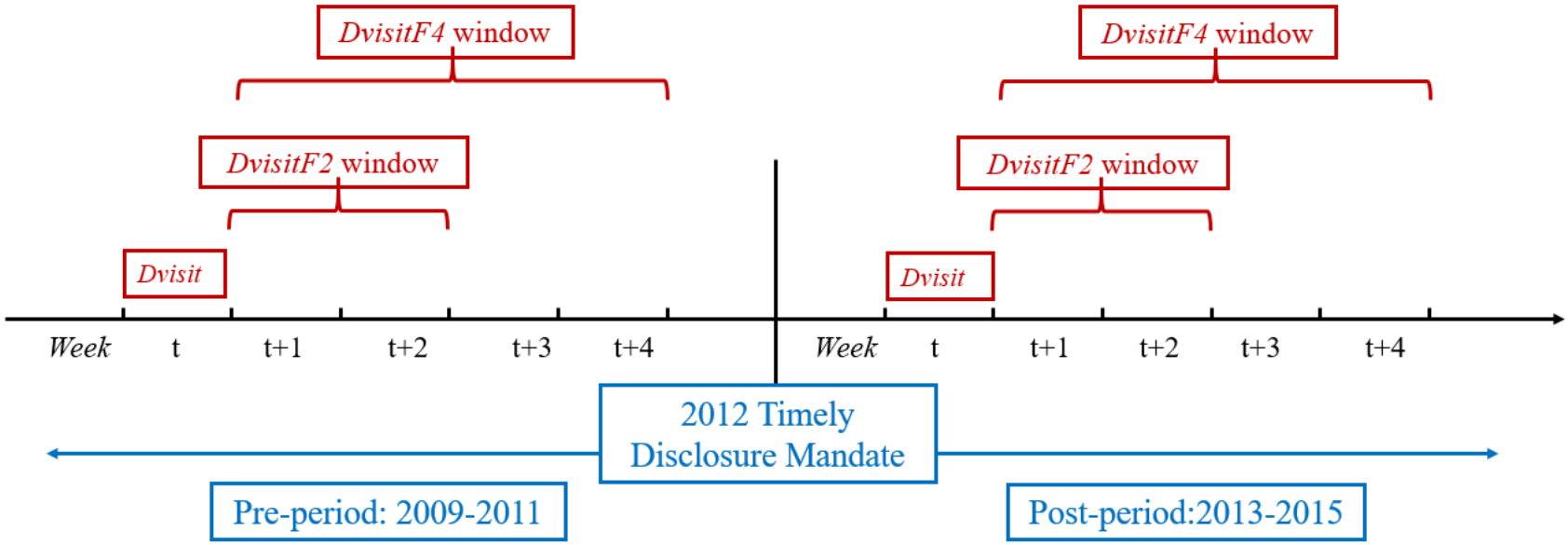


Figure 2 Parallel Trends

This figure presents trends of differences in *DvisitF4* between visited firm-week-analysts and nonvisited firm-week-analysts over six years. To construct the figure, we re-run Eq. (1) modified to include separate indicators to capture each year in 2009-2011 and 2013-2015, respectively (interacted with *Dvisit*, similar to the indicator *Post*). We use 2011 as the benchmark year, so each point on the graph shows the difference between analysts' attention to visited firms compared to nonvisited firms *relative to* 2011. *DvisitF4* equals one if the nonvisiting analyst visits the firm during the subsequent four weeks and zero otherwise. Each point estimate is accompanied by a 90 percent confidence interval which is calculated based on standard errors clustered at the firm and year-week level.

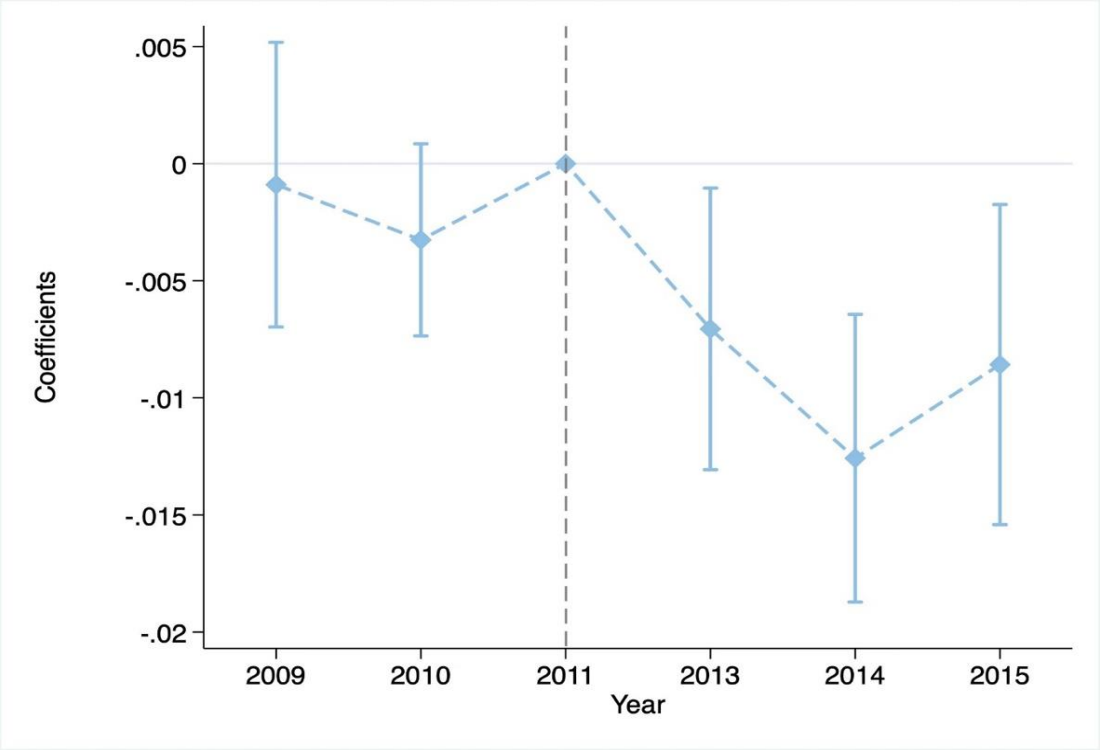


Table 1 Descriptive Statistics

This table presents the descriptive statistics. Panel A reports the time trend for corporate site visits for our sample firms during the sample period. Column (1) reports the total number of visits during each year. Columns (2)–(3) report for each year the average number and standard deviation of visits per firm and per broker, respectively. Column (4) reports for each year the average number and standard deviation of visiting brokers per visit for visited firm-weeks. In columns (2)–(4), the standard deviations are presented in the brackets. Panel B reports summary statistics for all the variables. All the variables are defined in Appendix 2. All the continuous variables are winsorized at the 1% and 99% levels.

Panel A. Time trend of corporate site visits

	(1)	(2)	(3)	(4)
Year	#Visits	# Visits per firm [STD. DEV.]	# Visits per broker [STD. DEV.]	# Brokers per visit [STD. DEV.]
2009	2,318	5.402 [6.662]	38.650 [42.423]	1.439 [0.916]
2010	3,967	6.785 [7.309]	66.393 [76.762]	1.549 [1.065]
2011	4,833	6.428 [6.710]	86.247 [90.717]	1.627 [1.233]
2013	3,112	3.880 [5.020]	73.867 [78.844]	2.203 [2.141]
2014	3,231	4.009 [4.275]	77.681 [84.429]	2.320 [2.279]
2015	2,635	3.303 [3.947]	63.473 [74.119]	2.308 [2.335]

Panel B. Summary statistics

Variables	Mean	STDEV	P25	Median	P75
DvisitF2	0.017	0.130	0.000	0.000	0.000
DvisitF4	0.033	0.179	0.000	0.000	0.000
Dvisit	0.125	0.330	0.000	0.000	0.000
Post	0.567	0.495	0.000	1.000	1.000
Brokersize	3.783	0.669	3.401	3.871	4.248
Firmexperience	0.620	0.545	0.000	0.693	1.099
Companies	2.941	0.866	2.398	2.996	3.526
Star	0.131	0.337	0.000	0.000	0.000
Connected	0.072	0.258	0.000	0.000	0.000
ROA	0.040	0.039	0.012	0.030	0.058
Leverage	0.414	0.217	0.232	0.401	0.592
Size	22.310	1.296	21.330	22.110	23.060
Analyst	1.963	0.842	1.386	2.079	2.639
MB	4.059	2.615	2.246	3.397	5.119
TV	1.356	1.151	0.547	0.999	1.800
Return	0.009	0.107	-0.058	-0.004	0.062
STD	0.024	0.014	0.014	0.020	0.030
SOE	0.423	0.494	0.000	0.000	1.000
Top1	0.362	0.148	0.237	0.357	0.466

Table 2 Attention Allocated to Visited Firms

This table presents the results from the estimation of Eq. (1). *Dvisit* is an indicator variable equal to one if at least one of the analyst's peers visits the firm during the week, and zero otherwise. *DvisitF2* (*DvisitF4*) is an indicator variable equal to one if the nonvisiting analyst visits the firm in the subsequent two (four) weeks, and zero otherwise. All the variables are defined in Appendix 2. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

VARIABLES	(1) *DvisitF2*	(2) *DvisitF4*
Dvisit	0.007***	0.010***
	(7.70)	(6.95)
Dvisit×Post	-0.004**	-0.008***
	(-2.21)	(-3.29)
Brokersize	0.002*	0.004**
	(1.81)	(2.09)
Firmexperience	-0.001**	-0.002**
	(-2.29)	(-2.42)
Companies	0.000	0.000
	(0.68)	(0.32)
Star	-0.002*	-0.003*
	(-1.90)	(-1.69)
Connected	0.010***	0.020***
	(9.50)	(9.39)
ROA	0.003	0.018
	(0.17)	(0.56)
Leverage	-0.003	-0.004
	(-0.59)	(-0.46)
Size	0.001	0.002
	(0.54)	(0.61)
Analyst	0.000	0.001
	(0.75)	(0.59)
MB	0.000	0.000
	(0.28)	(0.26)
TV	-0.001**	-0.002**
	(-2.19)	(-2.31)
Return	0.013***	0.021***
	(4.15)	(3.82)
STD	0.150***	0.213***
	(5.44)	(5.43)
SOE	-0.003	-0.005
	(-1.03)	(-0.90)
Top1	-0.016	-0.029
	(-1.40)	(-1.28)
Constant	-0.009	-0.022
	(-0.21)	(-0.27)
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,498,893	1,498,893
Adjusted R^2	0.021	0.040

Table 3 The Role of Visiting Analysts' Information Advantage

This table reports the results examining the role of the information advantage of visiting analysts. Panel A reports the results based on the recent visiting history of visiting analysts. *Dvisit_RecentVisit* equals one if at least one visiting analyst has visited the firm during the most recent year and zero otherwise. *Dvisit_NoRecentVisit* equals one if none of the visiting analysts have visited the firm during the most recent year and zero otherwise. Also reported are the *p*-values from testing the differences between the coefficients on *Dvisit_RecentVisit×Post* and *Dvisit_NoRecentVisit×Post.* Panel B reports the results based on the star status of visiting analysts. *Dvisit_Star* equals one if at least one visiting analyst has been rated as a star analyst during the most recent year and zero otherwise. *Dvisit_NoStar* equals one if none of the visiting analysts have been rated as a star analyst during the most recent year and zero otherwise. Also reported are the *p*-values from testing the differences between the coefficients on *Dvisit_Star×Post* and *Dvisit_NoStar×Post. DvisitF2* (*DvisitF4*) is an indicator variable equal to one if the nonvisiting analyst visits the firm in the subsequent two (four) weeks, and zero otherwise. All the variables are defined in Appendix 2. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

Panel A Information advantage based on visiting history of visiting analysts

VARIABLES	(1) *DvisitF2*	(2) *DvisitF4*
Dvisit_RecentVisit	0.008***	0.010***
	(6.11)	(4.60)
Dvisit_NoRecentVisit	0.006***	0.010***
	(6.56)	(7.09)
Dvisit_RecentVisit×Post	-0.008***	-0.011***
	(-3.32)	(-3.23)
Dvisit_NoRecentVisit×Post	0.001	-0.003
	(0.73)	(-1.29)
p-value for the difference between coefficients on *Dvisit_RecentVisit×Post* and *Dvisit_NoRecentVisit×Post*	0.000	0.029
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,498,893	1,498,893
Adjusted R^2	0.021	0.040

Panel B Information advantage based on the *Star* status

VARIABLES	(1) *DvisitF*2	(2) *DvisitF*4
Dvisit_Star	0.009***	0.010***
	(5.15)	(3.79)
Dvisit_NoStar	0.007***	0.010***
	(6.89)	(6.92)
Dvisit_Star×Post	-0.014***	-0.018***
	(-4.59)	(-4.50)
Dvisit_NoStar×Post	-0.001	-0.005*
	(-0.42)	(-1.76)
p-value for difference between coefficients on *Dvisit_Star×Post* and *Dvisit_NoStar×Post*	0.000	0.001
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,498,893	1,498,893
Adjusted R^2	0.021	0.040

Table 4 Externalities on Attention Allocation for Nonvisited Firms

This table reports the results of examining the externalities on attention allocation. *Peervisit* captures the proportion of peer firms within the firm's industry that are visited by the analyst's peers during the week. *INFO_IND* is the average abnormal returns of peer firms each week. *DvisitF2* (*DvisitF4*) equals one if the nonvisiting analyst (i.e., an analyst who does not visit any peer firms in the week) visits the firm in the subsequent two (four) weeks and zero otherwise. All the variables are defined in Appendix 2. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

Panel A Attention allocated to nonvisited firms

VARIABLES	(1) *DvisitF2*	(2) *DvisitF4*
Peervisit	-0.008*	-0.008
	(-1.86)	(-1.14)
Peervisit×Post	0.026***	0.032***
	(3.37)	(2.85)
INFO_IND	0.013	0.003
	(1.33)	(0.24)
Brokersize	0.002*	0.004*
	(1.92)	(1.92)
Firmexperience	-0.001**	-0.002***
	(-2.20)	(-2.61)
Companies	0.000	0.000
	(1.00)	(0.63)
Star	-0.002**	-0.003
	(-2.10)	(-1.60)
Connected	0.008***	0.016***
	(7.89)	(8.14)
ROA	-0.003	0.004
	(-0.17)	(0.12)
Leverage	0.000	0.001
	(0.05)	(0.06)
Size	0.001	0.004
	(0.87)	(1.07)
Analyst	0.001	0.001
	(1.35)	(1.15)
MB	0.000	0.000
	(0.31)	(0.30)
TV	-0.001**	-0.002**
	(-2.11)	(-2.31)
Return	0.012***	0.021***
	(3.96)	(3.60)
STD	0.130***	0.210***
	(4.51)	(4.94)
SOE	-0.004	-0.007
	(-1.59)	(-1.22)
Top1	-0.011	-0.022
	(-1.05)	(-0.97)
Constant	-0.024	-0.059
	(-0.63)	(-0.79)
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,120,868	1,120,868
Adjusted R^2	0.020	0.039

Panel B The role of industry-level information

VARIABLES	(1) *DvisitF2*	(2) *DvisitF4*
Peervisit	-0.008*	-0.009
	(-1.74)	(-1.20)
Peervisit×Post	0.026***	0.033***
	(3.19)	(2.83)
INFO_IND	0.026*	0.004
	(1.85)	(0.18)
Peervisit×INFO_IND	-0.088	0.129
	(-0.83)	(0.76)
INFO_IND×Post	-0.022	-0.008
	(-0.93)	(-0.27)
Peervisit×Post×INFO_IND	0.140	-0.105
	(0.61)	(-0.39)
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,120,868	1,120,868
Adjusted R^2	0.020	0.039

Table 5 Effects on Information Environment of Visited Firms

This table reports the results of examining the effects on visited firms' information environment. In Panel A, columns (1) and (2) report the change in the consensus analyst forecast accuracy around site visits. $\Delta AccuracyF2_{i,t}$ ($\Delta AccuracyF4_{i,t}$) captures the change in consensus analyst forecast accuracy from 26 weeks before to two (four) weeks after week *t*. In Panel B, columns (1) and (2) report the synchronicity in the subsequent two weeks (*SynchF2)* and four weeks (*SynchF4*), respectively. All the variables are defined in Appendix 2. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

Panel A Changes in forecast accuracy around site visits

	(1)	(2)
VARIABLES	∆*AccuracyF2*	∆*AccuracyF4*
Dvisit	0.000	0.003
	(0.01)	(0.54)
Dvisit×Post	0.001	-0.005
	(0.21)	(-0.81)
ROA	-0.142***	-0.372***
	(-2.76)	(-3.37)
Leverage	0.005	0.010
	(0.44)	(0.39)
Size	0.011**	0.023**
	(2.59)	(2.41)
Analyst	-0.006***	-0.009***
	(-4.14)	(-3.16)
MB	0.000	0.001
	(0.46)	(0.81)
TV	-0.003***	-0.006***
	(-3.19)	(-3.13)
Return	0.001	0.004
	(0.10)	(0.26)
STD	0.268***	0.415***
	(4.23)	(3.67)
SOE	0.000	-0.003
	(0.04)	(-0.18)
Top1	0.018	0.035
	(0.86)	(0.77)
Constant	-0.210**	-0.436**
	(-2.24)	(-2.06)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	126,929	126,929
Adjusted R^2	0.035	0.053

Panel B Stock return synchronicity

VARIABLES	(1) *SynchF2*	(2) *SynchF4*
Dvisit	-0.070***	-0.038**
	(-3.20)	(-2.15)
Dvisit×Post	0.053*	0.016
	(1.73)	(0.63)
ROA	1.254***	2.002***
	(3.59)	(5.40)
Leverage	-0.167	-0.195
	(-1.40)	(-1.43)
Size	0.175***	0.212***
	(4.81)	(5.36)
Analyst	0.001	0.005
	(0.07)	(0.40)
MB	-0.033***	-0.038***
	(-4.89)	(-5.43)
TV	0.051***	0.060***
	(5.10)	(5.60)
Return	-0.760***	-0.722***
	(-9.17)	(-9.87)
STD	-3.656***	-2.079***
	(-4.11)	(-3.44)
SOE	0.024	0.047
	(0.30)	(0.58)
Top1	-0.231	-0.175
	(-1.23)	(-0.86)
Constant	-3.469***	-4.670***
	(-4.32)	(-5.44)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	109,662	120,512
Adjusted R^2	0.227	0.313

Table 6 Effects on Information Environment of Nonvisited Firms

This table reports the results of examining the effects on nonvisited firms' information environment. In Panel A, columns (1) and (2) report the change in the consensus analyst forecast accuracy around site visits. $\Delta AccuracyF2_{i,t}$ ($\Delta AccuracyF4_{i,t}$) captures the change in consensus analyst forecast accuracy from 26 weeks before to two (four) weeks after week *t*. In Panel B, columns (1) and (2) report the synchronicity in the subsequent two weeks (*SynchF2)* and four weeks (*SynchF4*), respectively. All the variables are defined in Appendix 2. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

Panel A Changes in forecast accuracy around site visits

	(1)	(2)
VARIABLES	$\Delta AccuracyF2$	$\Delta AccuracyF4$
Peervisit	-0.025*	-0.024
	(-1.76)	(-0.92)
Peervisit×Post	0.056***	0.059*
	(2.92)	(1.71)
INFO_IND	-0.013	-0.010
	(-0.44)	(-0.20)
ROA	-0.133**	-0.360***
	(-2.49)	(-3.17)
Leverage	0.004	0.007
	(0.33)	(0.28)
Size	0.013***	0.027***
	(2.96)	(2.83)
Analyst	-0.005***	-0.008**
	(-3.29)	(-2.54)
MB	0.001	0.002
	(0.85)	(1.35)
TV	-0.003***	-0.005***
	(-2.76)	(-2.72)
Return	0.002	0.004
	(0.20)	(0.28)
STD	0.227***	0.317***
	(3.48)	(2.70)
SOE	0.000	-0.007
	(0.02)	(-0.42)
Top1	0.020	0.044
	(1.01)	(0.98)
Constant	-0.246***	-0.525**
	(-2.66)	(-2.50)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	113,603	113,603
Adjusted R^2	0.037	0.054

Panel B Stock return synchronicity

VARIABLES	(1) *SynchF2*	(2) *SynchF4*
Peervisit	0.307**	0.420***
	(2.32)	(3.31)
Peervisit×Post	-0.295*	-0.452***
	(-1.76)	(-2.75)
INFO_IND	-0.845***	-0.618**
	(-2.63)	(-2.48)
ROA	1.270***	2.106***
	(3.46)	(5.53)
Leverage	-0.148	-0.170
	(-1.21)	(-1.22)
Size	0.176***	0.210***
	(4.67)	(5.23)
Analyst	0.005	0.007
	(0.44)	(0.53)
MB	-0.036***	-0.040***
	(-5.13)	(-5.68)
TV	0.052***	0.059***
	(5.18)	(5.42)
Return	-0.755***	-0.705***
	(-9.00)	(-9.52)
STD	-3.450***	-1.873***
	(-3.71)	(-3.03)
SOE	0.033	0.035
	(0.41)	(0.43)
Top1	-0.215	-0.203
	(-1.15)	(-0.99)
Constant	-3.516***	-4.623***
	(-4.23)	(-5.31)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	98,848	108,623
Adjusted R^2	0.227	0.313

Table 7 Long-term Effects

This table reports the results of examining the long-term effects of the timely disclosure mandate. In Panel A, *DvisitF52* (*DcoverF52*) is an indicator variable equal to one if the nonvisiting analyst visits (covers) the firm in the subsequent 52 weeks, and zero otherwise. In Panel B, Δ*AccuracyF52* (*SynchF52*) is the change in consensus analyst forecast (stock return synchronicity) of the firm in the subsequent 52 weeks. In Panel C, *DvisitYear1* (*DcoverYear1*) is an indicator variable equal to one if the analyst visits (covers) the firm during the next year, and zero otherwise. All the variables are defined in Appendix 2. *t*-statistics are presented below the coefficient estimates. In Panels A and B, standard errors are clustered at the firm and year-week levels. In Panel C, standard errors are clustered at the firm level.

Panel A The tendency to visit and cover the firm during the next 52 weeks

VARIABLES	(1) *DvisitF52*	(2) *DcoverF52*
Dvisit	0.040***	0.008***
	(7.16)	(3.42)
Dvisit×Post	-0.021**	-0.008*
	(-2.52)	(-1.89)
Brokersize	0.025**	-0.042***
	(2.58)	(-3.90)
Firmexperience	-0.009*	0.025***
	(-1.83)	(4.39)
Companies	-0.003	0.115***
	(-0.95)	(19.51)
Star	-0.017*	0.037***
	(-1.71)	(4.14)
Connected	0.112***	0.146***
	(11.65)	(14.95)
ROA	0.098	0.213**
	(0.65)	(2.41)
Leverage	-0.006	0.072**
	(-0.12)	(2.15)
Size	0.008	-0.009
	(0.37)	(-0.72)
Analyst	0.010**	0.031***
	(2.37)	(10.30)
MB	0.003	0.009***
	(1.09)	(5.69)
TV	-0.003	-0.008***
	(-0.91)	(-4.25)
Return	0.038***	0.080***
	(3.51)	(10.34)
STD	0.247***	0.396***
	(2.74)	(5.88)
SOE	0.031	-0.007
	(0.64)	(-0.32)
Top1	-0.132	-0.062
	(-1.03)	(-1.17)
Constant	0.009	0.230
	(0.02)	(0.87)
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,498,893	1,498,893
Adjusted R^2	0.250	0.341

Panel B Information environment of visited firms during the next 52 weeks

VARIABLES	(1) $\Delta AccuracyF52$	(2) *SynchF52*
Dvisit	0.010	0.001
	(0.47)	(0.07)
Dvisit×Post	-0.020	0.023
	(-0.61)	(1.23)
ROA	-0.478	1.706***
	(-0.83)	(5.38)
Leverage	0.484**	-0.425***
	(2.37)	(-3.41)
Size	0.044	0.255***
	(0.70)	(6.25)
Analyst	-0.045**	0.053***
	(-2.41)	(4.83)
MB	-0.008	-0.002
	(-0.93)	(-0.28)
TV	-0.026**	0.045***
	(-2.19)	(5.05)
Return	-0.080	-0.273***
	(-1.37)	(-8.20)
STD	2.510***	0.505
	(5.38)	(1.48)
SOE	-0.023	0.121
	(-0.23)	(1.37)
Top1	-0.259	0.150
	(-0.68)	(0.65)
Constant	-0.432	-5.922***
	(-0.32)	(-6.67)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	126,824	128,334
Adjusted R^2	0.244	0.617

Panel C Changes in analysts' long-term visiting and coverage patterns

VARIABLES	(1) *DvisitYear1*	(2) *DcoverYear1*
Brokersize	0.013	0.007
	(1.06)	(0.64)
Brokersize×Post	0.009	-0.053***
	(0.83)	(-4.87)
Firmexperience	-0.008	-0.004
	(-1.14)	(-0.45)
Firmexperience×Post	-0.014	-0.046***
	(-1.39)	(-4.66)
Companies	-0.003	-0.015***
	(-0.52)	(-3.23)
Companies×Post	-0.003	0.013**
	(-0.53)	(2.25)
Star	-0.015	0.038**
	(-0.98)	(2.52)
Star×Post	0.005	0.033*
	(0.25)	(1.74)
Connected	0.098***	0.094***
	(6.39)	(8.25)
Connected×Post	-0.013	-0.026*
	(-0.60)	(-1.69)
ROA	0.490**	0.534***
	(2.20)	(4.27)
ROA×Post	0.165	-0.177
	(0.65)	(-1.17)
Leverage	0.073	0.090**
	(1.06)	(2.16)
Leverage×Post	-0.061	-0.062
	(-0.89)	(-1.64)
Size	-0.007	-0.002
	(-0.31)	(-0.19)
Size×Post	0.011	0.016**
	(0.71)	(2.32)
Analyst	0.036***	0.046***
	(3.62)	(6.91)
Analyst×Post	-0.047***	-0.030***
	(-3.85)	(-4.01)
MB	-0.005	0.003
	(-1.09)	(0.98)
MB×Post	-0.004	-0.002
	(-0.71)	(-0.70)
TV	-0.005*	-0.002
	(-1.92)	(-0.99)

TV×Post	0.004	-0.000
	(1.16)	(-0.14)
Return	0.024	0.016*
	(1.61)	(1.69)
Return×Post	-0.001	0.011
	(-0.05)	(0.88)
STD	0.178	-1.535**
	(0.18)	(-2.46)
STD×Post	0.365	2.867***
	(0.21)	(2.83)
SOE	0.030	0.012
	(0.62)	(0.69)
SOE×Post	-0.024	-0.005
	(-1.10)	(-0.49)
Top1	0.118	-0.055
	(0.84)	(-0.92)
Top1×Post	-0.172***	0.008
	(-2.88)	(0.26)
Constant	0.141	0.097
	(0.30)	(0.35)
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year FE	Yes	Yes
Observations	53,361	53,361
Adjusted R^2	0.182	0.294

Table 8 Alternative Explanations

This table reports the results of our analyses to mitigate the concerns related to alternative explanations. Panel A reports the results from the estimation of Eq. (1) excluding firms with a significant increase in the number of analysts per visit during the post-period (columns (1) and (2)) and excluding firm-weeks with multiple visiting analysts (columns (3) and (4)). Panel B reports the results from the estimation of Eq. (1) by excluding visits that occur in the month after major corporate events. Panel C reports the results from the estimation of Eq. (1) by excluding firms that are affected by connected analysts. Panel D reports the results examining changes in forecast accuracy of nonvisiting analysts. Δ*Accuracy_nonvisitF2* (Δ*Accuracy_nonvisitF4*) captures the change in the forecast accuracy of nonvisiting analysts from 26 weeks before to two (four) weeks after week *t*. Also reported are the *p*-values from testing the differences between the coefficients on *Dvisit_Post_InfoH* and *Dvisit_Post_InfoL*. Panel E reports the results from the estimation of Eq. (1) excluding site visits conducted jointly by analysts and buy-side investors. *DvisitF2* (*DvisitF4*) is an indicator variable equal to one if the nonvisiting analyst visits the firm in the subsequent two (four) weeks, and zero otherwise. All the variables are defined in Appendix 2. *t*-statistics based on robust standard errors clustered by firm and year-week are presented below the coefficient estimates.

Panel A Excluding firms that might combine visits

	(1)	(2)	(3)	(4)
	Excluding firms with a significant increase in the number of analysts per visit		Excluding firm-weeks with multiple visiting analysts	
VARIABLES	*DvisitF2*	*DvisitF4*	*DvisitF2*	*DvisitF4*
Dvisit	0.008***	0.012***	0.006***	0.010***
	(7.46)	(6.85)	(5.92)	(6.35)
Dvisit×Post	-0.005**	-0.010***	-0.000	-0.005**
	(-2.38)	(-3.61)	(-0.04)	(-2.10)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes
Observations	1,170,229	1,170,229	1,411,119	1,411,119
Adjusted R^2	0.022	0.042	0.021	0.040

Panel B Excluding visits that occur in the month after major corporate events

	(1)	(2)
VARIABLES	*DvisitF2*	*DvisitF4*
Dvisit	0.007***	0.010***
	(7.08)	(6.31)
Dvisit×Post	-0.005**	-0.009***
	(-2.18)	(-3.30)
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,426,219	1,426,219
Adjusted R^2	0.021	0.040

Panel C Excluding firms that are affected by connected analysts

	Exclude firms that experienced a drop in coverage by connected analysts		Exclude firms that experienced a drop in visits by connected analysts	
	(1)	(2)	(3)	(4)
VARIABLES	*DvisitF2*	*DvisitF4*	*DvisitF2*	*DvisitF4*
Dvisit	0.006***	0.007***	0.006***	0.008***
	(4.47)	(3.64)	(5.65)	(4.83)
Dvisit×Post	-0.006**	-0.010***	-0.006***	-0.010***
	(-2.22)	(-2.91)	(-2.64)	(-3.51)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes
Observations	786,559	786,559	1,060,207	1,060,207
Adjusted R^2	0.026	0.050	0.023	0.045

Panel D Forecast accuracy of nonvisiting analysts

	(1)	(2)	(3)	(4)
			Dvisit_Post_InfoH=1: Number of words > Median	
VARIABLES	Δ*Accuracy_ nonvisitF2*	Δ*Accuracy_ nonvisitF4*	Δ*Accuracy_ nonvisitF2*	Δ*Accuracy_ nonvisitF4*
Dvisit	-0.008***	-0.009**	-0.008***	-0.009**
	(-2.67)	(-2.08)	(-2.67)	(-2.08)
Dvisit×Post	-0.001	-0.006		
	(-0.21)	(-0.88)		
Dvisit_Post_InfoH			-0.003	-0.008
			(-0.70)	(-1.15)
Dvisit_Post_InfoL			0.001	-0.003
			(0.31)	(-0.41)
p-value for difference between coefficients on *Dvisit_Post_InfoH* and *Dvisit_Post_InfoL*			0.315	0.477
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes
Observations	126,859	126,859	126,859	126,859
Adjusted R^2	0.035	0.053	0.035	0.053

Panel E Excluding visits conducted jointly by analysts and buy-side investors

VARIABLES	(2) *DvisitF*2	(4) *DvisitF*4
Dvisit	0.007***	0.010***
	(5.23)	(5.46)
Dvisit×*Post*	-0.004**	-0.009***
	(-2.08)	(-3.20)
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,400,618	1,400,618
Adjusted R^2	0.021	0.040

Table 9 Robustness Tests

This table reports the results of three robustness tests. Panel A reports the results of the estimation of Eq. (11) at the firm-week level. *DvisitF2* (*DvisitF4*) is an indicator variable equal to one if *at least* one nonvisiting analyst visits the firm in the subsequent two (four) weeks, and zero otherwise. Columns (1)–(3) report the results based on the same firm-weeks as those in Table 2, but with analysts that covered the firm in the past 52 weeks, that did not cover the firm in the past 52 weeks, and all analysts, respectively. Column (4) reports the results using the full sample of firm-weeks and analysts. Panel B reports the results of the estimation of Eq. (1) after further controlling for each control variable interacted with *Post*. Panel C reports the results of the estimation of Eq. (1) using the tendency to issue forecasts as an alternative measure of analysts' attention. *DforecastF2* (*DforecastF4*) is an indicator variable equal to one if the nonvisiting analyst issues a forecast for the firm in the subsequent two (four) weeks, and zero otherwise. All the variables are defined in Appendix 2. All the specifications include firm fixed effects and year-week fixed effects. *t*-statistics, based on robust standard errors clustered by firm and year-week are presented below the coefficient estimates.

Panel A Analyses at the firm-week level

	Main Sample			Full sample
	(1)	(2)	(3)	(4)
VARIABLES	*DvisitF4_Cover*	*DvisitF4_NoCover*	*DvisitF4_All*	*DvisitF4_All*
Dvisit	0.072***	0.117***	0.131***	0.146***
	(8.32)	(12.05)	(12.88)	(14.44)
Dvisit×Post	-0.051***	-0.047***	-0.049***	-0.061***
	(-4.14)	(-3.48)	(-3.44)	(-4.43)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes
Observations	138,996	138,996	138,996	185,396
Adjusted R^2	0.169	0.156	0.195	0.220

Panel B Controlling for potential changes in analyst visiting patterns

VARIABLES	(1) *DvisitF2*	(2) *DvisitF4*
Dvisit	0.007***	0.009***
	(7.77)	(6.74)
Dvisit×Post	-0.004**	-0.008***
	(-2.13)	(-3.13)
Brokersize×Post	-0.000	-0.000
	(-0.09)	(-0.07)
Firmexperience×Post	-0.003***	-0.005**
	(-2.79)	(-2.42)
Companies×Post	0.001	0.001
	(0.91)	(0.70)
Star×Post	0.001	0.002
	(0.54)	(0.47)
Connected×Post	-0.003	-0.006
	(-1.37)	(-1.51)
ROA×Post	0.014	0.026
	(0.46)	(0.47)
Leverage×Post	-0.005	-0.009
	(-0.87)	(-0.78)
Size×Post	-0.000	-0.001
	(-0.12)	(-0.20)
Analyst×Post	-0.001	-0.003*
	(-1.43)	(-1.76)
MB×Post	0.000	0.000
	(0.40)	(0.49)
TV×Post	-0.001	-0.002
	(-1.03)	(-1.22)
Return×Post	-0.000	0.000
	(-0.00)	(0.05)
STD×Post	0.191***	0.371***
	(3.03)	(3.07)
SOE×Post	-0.002	-0.005
	(-0.99)	(-1.06)
Top1×Post	-0.017***	-0.032***
	(-3.12)	(-3.03)
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	1,497,510	1,497,510
Adjusted R^2	0.021	0.041

Panel C Alternative measure of analysts' attention

VARIABLES	(1) *DforecastF2*	(2) *DforecastF4*	(3) *DforecastF2*	(4) *DforecastF4*
Dvisit	-0.010***	-0.013***		
	(-4.06)	(-4.38)		
Dvisit×Post	-0.005	-0.008*		
	(-1.44)	(-1.76)		
Peervisit			-0.009	-0.010
			(-0.80)	(-0.68)
Peervisit×Post			0.028*	0.040*
			(1.82)	(1.95)
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Year-week FE	Yes	Yes	Yes	Yes
Observations	1,498,893	1,498,893	1,116,068	1,116,068
Adjusted R^2	0.092	0.144	0.095	0.149

Online Appendix

Public Disclosure of Private Meetings: Does Observing Peers' Information Acquisition Affect Analysts' Attention Allocation?

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In the online appendix, we provide detailed results of additional analyses.

In Table A1, to complement the analyses in Sections 4.1 and 4.2, we calculate stock return synchronicity by regressing daily stock returns on the current and prior day's market return and the current and prior day's industry return and obtain adjusted R^2. In Panel A, we report the effects on the stock return synchronicity of visited firms. We find that the coefficients on *Dvisit*×*Post* are insignificant, suggesting that the stock return synchronicity of visited firms does not change following the timely disclosure mandate. In Panel B, we report the effects on the stock return synchronicity of nonvisited firms. We find that the coefficient on *Peervisit*×*Post* is negative and significant at the 1% level when we use *SynchF4* as the dependent variable, suggesting that following the timely disclosure mandate, as more of the peer firms host site visits, the nonvisited firm experiences a larger reduction in return synchronicity during the next four weeks.

In Table A2, to complement the analyses in Section 5.1, we conduct an analysis by using firm-year-analyst as the unit of observations to assess the long-term effects on analysts' attention allocation. Specifically, we regress the analyst tendency to visit the firm during the next year (*DvisitYear1*) and cover the firm during a year (*DcoverYear1*), respectively, on the frequency with which the firm was visited by peer analysts during the year (*AnnualVisit*) and its interaction with *Post*. We find the coefficients on *AnnualVisit*×*Post* to be negative and significant at the 5% level, corroborating with our results in Section 5.1.

In Table A3, to complement the analyses in Section 5.2, we conduct an analysis by using firm-year as the unit of observations to assess the long-term effects on firms' information environment. Specifically, we regress analyst forecast accuracy during a year (*AccuracyYear1*) and stock return synchronicity during the next year (*SynchYear1*), respectively, on the frequency of which the firm was visited during a year (*AnnualVisit*) and its interaction with *Post*. We find that

analyst forecast accuracy increases and stock return synchronicity does not change as firms are visited more frequently in the previous year. The results suggest that there is no deterioration in the information environment of visited firms over the following year, despite fewer follow-up visits, as shown in Table 7 Panel A of the manuscript, possibly due to visiting analysts' high quality of coverage and advantages in information production for visited firms. This also explains the increase in analyst forecast accuracy in the post period as nonvisiting analysts with information disadvantages are more likely to promptly drop coverage after the timely disclosure mandate.

Table A1 Alternative Calculation of Stock Return Synchronicity

This table reports the results of examining the effects on firms' information environment, using an alternative measure of stock return synchronicity. *SynchF2*$_{i,t}$ (*SynchF4*$_{i,t}$) is calculated as $\log(\frac{R^2_{i,t}}{1-R^2_{i,t}})$, with $R^2_{i,t}$ from the firm-specific regression:$RET_{i,d} = \alpha + \beta_1 MKTRET_d + \beta_2 MKTRET_{d-1} + \beta_3 INDRET_d + \beta_4 INDRET_{d-1} + \varepsilon_{i,t}$, where $RET_{i,d}$, $MKTRET_d$, and $INDRET_d$ are daily stock-, market-, and industry-level returns, respectively, during the two (four) weeks after week *t*. In Panel A, we report the effects on stock return synchronicity of visited firms. In Panel B, we report the effects on stock return synchronicity of nonvisited firms. *Peervisit* captures the proportion of peer firms within the firm's industry that are visited during the week. All the variables are defined in Appendix 2. All the specifications include firm fixed effects and year-week fixed effects. *t*-statistics, based on robust standard errors clustered by firm and year-week, are presented below the coefficient estimates.

Panel A Stock return synchronicity of visited firm

	(1)	(2)
VARIABLES	*SynchF2*	*SynchF4*
Dvisit	-0.060**	-0.033*
	(-2.50)	(-1.82)
Dvisit×Post	0.009	0.020
	(0.29)	(0.73)
ROA	1.161***	1.977***
	(3.48)	(5.66)
Leverage	-0.127	-0.217*
	(-1.12)	(-1.68)
Size	0.157***	0.200***
	(4.66)	(4.89)
Analyst	-0.002	0.006
	(-0.21)	(0.52)
MB	-0.025***	-0.035***
	(-3.79)	(-5.22)
TV	0.046***	0.055***
	(4.69)	(5.35)
Return	-0.636***	-0.737***
	(-7.63)	(-10.13)
STD	-2.736***	-1.979***
	(-3.26)	(-3.34)
SOE	0.001	0.025
	(0.01)	(0.30)
Top1	-0.151	-0.205
	(-0.80)	(-1.03)
Constant	-2.878***	-4.314***
	(-3.89)	(-4.81)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	103,641	118,340
Adjusted R^2	0.177	0.296

Panel B Stock return synchronicity of nonvisited firms

VARIABLES	(1) *SynchF2*	(2) *SynchF4*
Peervisit	0.143	0.479***
	(1.00)	(3.68)
Peervisit×Post	-0.130	-0.569***
	(-0.69)	(-3.49)
INFO_IND	-0.894***	-0.414*
	(-2.66)	(-1.77)
ROA	1.244***	2.099***
	(3.57)	(5.88)
Leverage	-0.103	-0.194
	(-0.89)	(-1.47)
Size	0.165***	0.200***
	(4.82)	(4.77)
Analyst	0.001	0.005
	(0.05)	(0.43)
MB	-0.027***	-0.037***
	(-4.05)	(-5.36)
TV	0.049***	0.054***
	(5.03)	(5.27)
Return	-0.605***	-0.708***
	(-7.31)	(-9.74)
STD	-2.534***	-1.779***
	(-2.98)	(-2.98)
SOE	0.004	0.012
	(0.05)	(0.14)
Top1	-0.161	-0.216
	(-0.87)	(-1.07)
Constant	-3.071***	-4.315***
	(-4.10)	(-4.70)
Firm FE	Yes	Yes
Year-week FE	Yes	Yes
Observations	93,301	106,656
Adjusted R^2	0.177	0.295

Table A2 Attention Allocation at the Firm-year-analyst Level

This table reports the results of examining the long-term effects of the timely disclosure mandate. *DvisitYear1* (*DcoverYear1*) is an indicator variable equal to one if the analyst visits (covers) the firm during the next year. *AnnualVisit* is the frequency with which the firm was visited by peer analysts during the year. All the variables are measured at the annual level. *t*-statistics, based on robust standard errors clustered by firm, are presented below the coefficient estimates.

VARIABLES	(1) *DvisitYear1*	(2) *DcoverYear1*
Annualvisit	0.050***	0.008*
	(6.24)	(1.69)
Annualvisit×Post	-0.022**	-0.012**
	(-2.09)	(-2.05)
Brokersize	0.018*	-0.018*
	(1.72)	(-1.90)
Firmexperience	-0.018***	-0.029***
	(-3.79)	(-5.33)
Companies	-0.004	-0.008**
	(-1.13)	(-2.55)
Star	-0.013	0.060***
	(-1.07)	(6.05)
Connected	0.094***	0.079***
	(9.64)	(10.09)
ROA	0.572***	0.452***
	(3.20)	(4.67)
Leverage	0.043	0.082**
	(0.75)	(2.22)
Size	-0.008	0.004
	(-0.38)	(0.34)
Analyst	-0.006	0.027***
	(-0.85)	(5.47)
MB	-0.006*	0.004**
	(-1.66)	(1.97)
TV	-0.002	-0.001
	(-1.10)	(-1.30)
Return	0.020*	0.024***
	(1.81)	(3.96)
STD	0.489	-0.871*
	(0.60)	(-1.73)
SOE	0.016	0.005
	(0.37)	(0.29)
Top1	0.011	-0.083
	(0.09)	(-1.48)
Constant	0.270	0.159
	(0.60)	(0.59)
Firm FE	Yes	Yes
Analyst FE	Yes	Yes
Year FE	Yes	Yes
Observations	53,361	53,361
Adjusted R^2	0.184	0.292

Table A3 Effects on Information Environment at the Firm-year Level

This table reports the results of examining the long-term effects of the timely disclosure mandate on the information environment. *AccuracyYear1* (*SynchYear1*) is the analyst consensus forecast accuracy and stock return synchronicity during the next year. *AnnualVisit* is the frequency with which the firm was visited during the year. All the variables are measured at the annual level. *t*-statistics, based on robust standard errors clustered by firm, are presented below the coefficient estimates.

VARIABLES	(1) *AccuracyYear1*	(2) *SynchYear1*
Annualvisit	-0.001***	0.022
	(-3.05)	(1.13)
Annualvisit×Post	0.001**	0.014
	(2.16)	(0.62)
ROA	-0.020***	1.137***
	(-5.88)	(3.39)
Leverage	0.007***	-0.541***
	(4.79)	(-3.80)
Size	-0.002***	-0.152***
	(-10.08)	(-5.73)
Analyst	-0.000***	0.011
	(-4.03)	(1.45)
MB	-0.063***	9.860***
	(-2.95)	(3.46)
TV	0.000	0.021
	(0.40)	(0.21)
Return	-0.001*	0.223***
	(-1.69)	(5.38)
STD	-0.001***	0.067***
	(-5.53)	(3.24)
SOE	-0.006***	-0.544**
	(-3.50)	(-2.46)
Top1	-0.000***	0.019***
	(-5.34)	(4.24)
Constant	0.034***	-5.270***
	(4.39)	(-5.93)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	2,704	3,206
Adjusted R^2	0.791	0.568