Disclosed Corporate Site Visits: Audit Adjustments and the Quality of Financial Statements and Analyst Forecasts*

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Abstract

Using DiD regressions, we exploit the impact of mandatory and timely full disclosure of corporate site visits in China. We find robust evidence that the regulation improves the quality of auditing, financial statements, and analyst forecasts. First, after the full disclosure regulation, audit adjustments are significantly greater for a client firm with corporate site visits (i) when the client firm's auditors have more pressure to avoid audit failures, or (ii) when more forward-looking information and accounting and finance topics are discussed during the visits. These results support the idea that detailed public disclosure of corporate site visits on an open-access website can press auditors to examine company financial records with extra care and can also reveal useful information. Second, the disclosure also raises audit adjustments for client firms in the same industry audited by the affected auditor or her peer auditors working in the same audit firm. Finally, these affected client firms, conditioned on having audit adjustments, issue higher-quality financial reports than other client firms: they have less discretionary accruals and a lower likelihood of issuing restatement. It also follows that these client firms have lower average analyst forecast errors and dispersion after the issuance of higher-quality financial reports. Overall, the evidence supports the notion that full disclosure of site visits via a public digital platform improves the quality of financial reporting by improving auditing services and, thus, improves earnings forecasts.

Keywords: Corporate site visits; Audit adjustments; Spillover effects; the quality of financial statements; the quality of analyst forecasts
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1. Introduction

Credible and efficient information intermediation is crucial to an effective capital market. Corporate site visits play an important role; analysts or investment fund managers' direct access to management provides them with opportunities to obtain private information and improve their earnings forecast accuracy (Green, Jame, Markov, and Subasi 2014; Chen, Ma, Martin, and Michaely 2022) and helps investors make sound investment decisions (Cheng, Du, Wang, and Wang 2019). However, strategic selective disclosure, in which companies give material information only to a few, can create an uneven playing field, heighten investors' 'lemon' worry, and discourage their participation. Hence, the U.S. Securities and Exchange Commission (the SEC) adopted Regulation Fair Disclose (RFD) on Oct 23, 2000, intending to stop the practice of "selective disclosure."

The effect of such a policy has attracted much research effort. According to Allee, Bushee, Kleppe, and Pierce (2022), the extensive research comprises over 170 studies showing that RFD was initially effective in improving public disclosure and reducing the private information advantage of investors and analysts. However, while the 2000 RFD obliges client firms to release "material information as judged by management" to all analysts, many details could be missing.[1] Management may find ways to leak details unconstrained by fair disclosure regulations, e.g., allowing privileged investors to conduct gradual learning of unique information. A lawsuit involving the SEC and the Siebel System in 2005 led to less aggressive enforcement of RFD (see Fisch 2013). Allee et al. (2022) show that the more lax enforcement reversed the effect gained in the early stage of RFD.

However, effectively enforced open disclosure can discourage intermediary information discovery (Mohanram and Sunder 2006) and affect client firms' ability to attract targeted investment money (Agrawal, Chadha, and Chen 2006). Besides that less well-known client firms suffer more, the negative effect can harm the market's overall information environment, e.g., leading market-makers to raise the bid-ask spread in trading stocks (Sidhu, Smith, Whaley, and Willis 2008). It can also lead to less accurate

[1] Regulation Fair Disclosure (Oct 2000) imposed by the SEC in the US addresses the selective disclosure of information by publicly traded companies and other issuers. RFD provides that when an issuer discloses material nonpublic information to specific individuals or entities—generally, securities market professionals, such as stock analysts, or holders of the issuer's securities who may well trade based on the information—the issuer must also make public disclosure of that information. In this way, RFD aims to promote full and fair disclosure.

and more dispersed analyst earning forecasts; however, the evidence is still mixed (See, e.g., Bailey, Li, Mao, and Zhong, 2003; Heflin, Subramanyam, and Zhang, 2003; Francis, Nanda, and Wang 2006).

The effect of RFD deserves global attention because an efficient capital market builds on high-quality information intermediation. The U.S. experience leads us to think of various issues. First, the existing literature shows both the completeness of the disclosure requirement and its enforcement matter. Investigating the effect of more complete and better-enforced fair disclosure regulations in other countries is warranted.

Second, research on the U.S. RFD focuses on the consequences of regulatory efforts to reduce the market information gap. Besides asking whether RFD disadvantages small firms from getting attention, the research often focused on (i) how RFD changes intermediary professionals' sources of information and (ii) whether trading behavior reflects better information or the awareness of having inferior information. Expanding the scope and adopting additional focal measures will reveal valuable details on how RFD affects information intermediation, which involves interactive relationships between intermediary professionals, auditors, and investors.

The literature provides useful guidance. Early on, Jensen and Meckling 1976 (p. 306) point out that intermediary professionals, auditors, and investors all play essential roles interactively in information intermediation and, thus, collectively monitoring and controlling agency costs. Subsequently, a vast amount of literature has vividly shown fruitful private interactions between information intermediaries and auditors when doing their jobs. Examples are abundant: the work of Behn, Choi, and Kang (2008) and Defond, Fang, Lennox, and Luo (2024) together suggest that higher quality auditing and more accurate analyst forecasts feed into each other; indeed, auditing standards explicitly encourage auditors to incorporate the information intermediary professionals provide into their risk judgment in carrying out their jobs (e.g., AICPA 2002; PCAOB 2010).[2] Tong, Yang, Zhang, and Zhu (2023) suggest that auditors request adjustments in response to information from intermediary professionals, and Chan and Liu (2022) show that intermediary professionals scrutinize misreporting, especially when auditors are not under enough job pressure. (We chime in here that disclosing corporate site

[2] The PCAOB (2010, AS 2110) indicates that "the auditor should consider ... reading public information about the company ... {e.g.,} company-prepared presentation materials for analysts or investor groups, and analyst report. Additionally, the auditor should observe or read transcripts of earnings calls and, to the extent publicly available, other meetings." The AICPA (2002, AU-C 240.A75, SAS 99) instructs auditors to consider "the profitability or trend level expectations of investment analysts."

visits can raise auditors' job pressure.) Jensen and Meckling (1976) point out the importance of credible auditing and trustworthy financial statements. Gul, Kim, and Qiu (2010) and others also emphasize that auditors are responsible for ascertaining that companies' released comprehensive and reliable financial statements are as free of misstatements as possible.

Motivated by the broad literature, we opine that examining how an effectively enforced and comprehensive RFD affects the chain of interactive actions in information intermediation is fruitful. (i) Will the RFD affect auditing behavior? (ii) What is the boundary of the RFD's spillover impact on other auditors? (iii) Will the change in auditing behavior, if any, impact the quality of audited financial statements? (iv) Will the changes in the quality of financial statements, if any, affect the quality of analyst forecasts? This list of connected questions addresses whether timely and comprehensive disclosure can intensify users' ex-ante and ex-post triangulation checks of the reliability of rendered information services, leading to improvement in the information environment.

We utilize a setting in China to address the above issues. In 2012, China's Shenzhen Stock Exchange (SZSE) mandated its listed companies to disclose detailed information about corporate site visits on ***an open-access website within two trading days following the site visits***. (To the best of our knowledge, the U.S. and Europe do not mandate detailed release of corporate site visit information.) Visitors comprise analysts, investment fund managers, and occasionally bankers or reporters. The detailed information includes the names of visitors, the insider attendees, the questions discussed, and the managers' answers to these questions. During a corporate site visit, a visitor can inspect the client firm's production facilities and engage in face-to-face conversations about a client firm's operation with investor relations managers and employees. Through these opportunities, a visitor can obtain valuable information that is hard to acquire from alternative public sources such as financial reports and business newspapers.

Extant research shows that corporate site visits are significant information events and contain new content.[3] A detailed disclosure of a corporate site visit on an open-access website can influence auditing work for two reasons, grounded in possible

[3] For example, information obtained from corporate site visits increases price efficiency (Chen et al. 2022), is predictive of future performance (Cheng et al. 2019), and improves analysts' earnings forecast accuracy (Cheng, Du, Wang, and Wang 2016; Han, Kong, and Liu 2018; Chen et al. 2022).

stakeholder multiple triangulations. First, there is a fear of job failure view. Site visit information can enhance investors' understanding of a client firm's operations. Indeed, open access to the information allows triangulation by all stakeholders, raising the probability of detecting audit failure. Auditors will request necessary adjustments in their client firms' reported earnings and other information before public release to safeguard against job failure. Second, there is the information content view. Site visit reports potentially provide auditors with information related to financial performance that is difficult to access elsewhere. The released information prompts auditors to identify details they may have overlooked or to seek clarification on murky components in company-provided information. Site visit information, therefore, allows auditors to improve or reaffirm the precision of their private information.

Our first question is whether timely and detailed full disclosure improves affected auditors' job performance. Affected auditors are those whose client firms host and disclose corporate site visits. We measure the change in affected auditors' job performance by the shift in affected auditors' post-site-visit audit adjustments. Audit adjustments are auditor-requested (and client-agreed) adjustments to earnings. Using these unique audit adjustment data from China, Lennox, Wu, and Zhang (2014, 2016) and Lennox, Wang, and Wu (2018) show that audit adjustments cause earnings to become smoother and more persistent, resulting in higher-quality audited earnings.

For identification, we take advantage of the 2012 Chinese disclosure regulation that mandates public companies listed on the SZSE to fully disclose detailed information about corporate site visits on a web platform within two trading days following the site visits. Detailed information includes the names of visitors, the insider attendees, the questions discussed, and the managers' answers to these questions. Client firms that do not reveal corporate site visits, including those in the Shanghai Stock Exchange (SSE), serve as the base of our control. We form a group of matching client firms from the population of our potential control client firms based first on industry matching and then the nearest-neighbor propensity score matching. We run DiD regressions using the treatment and matched sample control client firms.

We find a robust, significant, and positive treatment effect. Audit adjustments for our treated client firms increase by 5.8% relative to the control client firms following corporate site visits. These results survive multiple robustness checks, which we report in detail in Section 4.3. First, we show that the treatment and control client firms have

parallel time trends in audit adjustments in the pre-disclosure period. Furthermore, our result remains significant, allowing for considerable deviations from the parallel trends based on the Rambachan-Roth (2023) approach. Second, our result is not affected by heterogeneous treatment effects and also holds when we correct for any potential heterogeneous treatment effect bias using the de Chaisemartin and d'Haultfoeuille (2020) approach. Third, we eliminate the possibility that our results are accidental due to time-specific industry effects, auditor and client-firm characteristics, or are caused by the presence of other client firm-level or auditor-intermediary communication channels. Fourth, our result remains when we adopt other reasonable definitions of the dependent variable or acceptable empirical specifications.

We further find evidence consistent with both the fear of job failure view and the information content view that explains heightened audit adjustments after corporate site visits. Evidence supporting the fear of job failure view includes that audit adjustments following corporate site visits are more pronounced among (i) client firms in stronger investor protection regions, (ii) non-SOE client firms, and (iii) client firms with more vociferous investors who participate more in a forum hosting investor public comments, and (iv) client firms with overseas listing. Furthermore, auditors above 50 who are close to retirement (and thus less concerned about career reputation) make fewer audit adjustments than younger auditors.[4] (In China, until 2024, the retirement age is 55 for females and 60 for males.) Evidence supporting the information content view includes that audit adjustments following corporate site visits to our treated client firms are more pronounced in the following situations: (i) when forward-looking information and accounting and finance topics are proportionately more discussed than nonfinancial and miscellaneous information (we use machine learning to capture the content of site visit discussions); (ii) when client firms give more relevant and targeted answers to visitors' questions (we employ the cosine similarity between question-answer text pairs to evaluate the answering relevance), (iii) when client firms have higher tangibility, (iv) when client firms with few analysts following, and (v) when client firms face less on-going informed risk arbitrage.

Our second empirical question is on information spillovers from RFD. In China,

[4] In China, the two signing auditors' signatures appear on the audit report, with the top signature from the review partner and the bottom signature from the engagement partner. Prior research shows that the engagement partner is more important in the audit fieldwork (Aobdia, Lin, and Petacchi 2015; PCAOB 2015). In this analysis, we focus on the engagement partner's age.

while auditors respect confidentiality in client firm-specific information, auditors in the same audit firm often share industry-level information. Indeed, industry-level knowledge is often an audit firm's pride; therefore, we are keen to examine if information spillovers exist. We designate an auditor who audits a client firm with a corporate site visit (affected client firm) as an affected auditor and her fellow auditors working in the same audit firm as peer auditors. We use China's Securities Regulatory Commission (CSRC) classification to designate firms into close-peer, distant-peer, and unrelated client firms. A close-peer client firm shares the same 2-digit classification as an affected client firm while a distant-peer client firm shares only the same 1-digit classification.[5] Our DiD regressions reveal that close-peer client firms without corporate site visits but audited by affected auditors or their peer auditors exhibit more extensive audit adjustments after the disclosure of a site visit. (We call them spillover-receiving client firms.) We do not find evidence of spillovers to distant-peer and unrelated client firms.

Our third empirical question is on the effects of disclosing corporate site visits on the quality of audit outcomes. Our DiD regressions reveal that, after the regulatory change, discretionary earnings management of both the treated client firms and the spillover-receiving client firms is more limited, but only in the presence of audit adjustments. The likelihood of these client firms' financial misstatements is also lowered. Another DiD regression shows further that the pre-audit accruals of the treated client firms and the spillover-receiving client firms behave the same before and after the disclosure requirement's imposition; the interpretation is that visitors' direct influence on company earnings management does not change after the regulatory change. These two results together support the idea that full disclosure of corporate site visits improves the quality of treated client firms' financial reports via the audit adjustment by affected auditors and their audit firms' fellow auditors.

Our final empirical question is on the impact of the heightened quality of financial statements via audit adjustments on the quality of analyst earning forecasts, which are the only publicly available third-party earnings forecasts. We compute the

[5] The China Security Regulation Commission (CSRC) industry classification is based on the "National Economic Industry Classification" standards. This classification sets up broad sector categories, represented by a single letter, such as 'A' for the agriculture, forestry, and fishing sectors, 'B' for the mining sector, etc. Furthermore, the CSRC provides a detailed sub-classification within each sectoral industry, like the SEC digit-by-digit sequential classification starting from A01.., A02.., .. C01.., … C11.., .. etc. We use this detailed classification system to identify each sector's peer and non-peer client firms.

mean and standard deviation of analyst forecast errors in year *t+1* for each client firm and examine if they have changed after the 2012 disclosure, especially if any changes are related to audit adjustments in year *t*. The treatment client firms comprise client firms with corporate site visits and information spillover-receiving client firms. The control group client firms are selected using propensity score-matched client firms in the same industries as treated client firms. Using DiD regressions, we show that in the full sample run, the mean and the standard deviation of analyst forecast errors did not change significantly after the 2012 disclosure requirement. However, among the sub-sample with audit adjustments after 2012, the treated group's mean and standard deviation of forecast errors dropped significantly after the regulatory changes.

This chain of results supports the idea that timely full disclosure induces improvements in the quality of auditing, the reliability of financial statements, and, finally, the quality of analyst earnings forecasts.

Our study makes several contributions to the literature. First, our results deepen our understanding of the impact of regulation fair disclosure in the information environment. Our results show that complete and detailed disclosure on an open-access platform benefits information intermediaries in fulfilling their information intermediation function. The disclosure allows open information triangulation that presses audits to do a conscientious job. It also emits relevant information content helpful for auditing. The outcome is higher-quality audited financial statements and more reliable analyst earnings forecasts. These improvements are particularly welcome in environments where equity trading is relatively uninformative (Morck, Yeung, and Yu 2000).

Second, our work adds to the literature on the impact of China's open disclosure requirement. The literature has mixed results. Ke, Kong, and Liu (2022) caution that the Shenzhen full disclosure regulation negatively affects the frequency of analysts' corporate visits, stock coverage, and research report issuance. Also, Bowen, Dutta, Tang, and Zhu (2024) find that the full disclosure regulation has reduced the information efficiency of SZSE-listed client firms. Still, Yang, Lu, and Xiang (2020) find that timely disclosure improves the fairness of information acquisition and decreases information asymmetry. Xiong and Yang (2023) find that effective mandatory disclosure increases market liquidity. Ru, Zheng, and Zou (2023) document that following the timely disclosure mandate, intermediaries spread their attention to non-visited client firms,

improving the information environment. Our results add the angle that timely, open, and full disclosure of corporate site visits has direct and spillover information effects ending in higher quality financial statements and more accurate analyst forecasts.

Third, our study contributes to financial market research on the usefulness of access to management. Prior research investigates whether such access helps investors make better decisions (e.g., Green et al. 2014; Cheng et al. 2019; Chen et al. 2022). Cao, Gong, Kim, Shi, and Wang (2024), using 2009-2018 data, find that China's policy of requiring companies to open doors to visitors leads to increased information flow and intensified monitoring and, thus, higher sensitivity of corporate investments to growth opportunities. Corporate site visits differ from other forms of management access activities such as conference calls, analyst/investor days, and broker-hosted investor conferences in that they allow visitors to observe client firms' production facilities and operations in a very close range (Green et al. 2014; Soltes 2014; Kirk and Markov 2016; Bushee, Gerakos, and Lee 2018; Mayew, Sethuraman, Venkatachalam 2020; Chen et al. 2022). We complement and extend this line of research by examining how releasing the contents during corporate site visits improves auditors' efficacy.

The remainder of the paper is structured as follows. Section 2 describes the institutional background of corporate site visits in China and develops the empirical hypotheses. Section 3 introduces the research design of our study. Section 4 presents the baseline regression results – the impact of open disclosure on audit adjustments and robustness checks. Section 5 discusses tests to examine the reasons for released corporate site visits to intensify audit adjustments. Section 6 reports empirical evidence that disclosing a client firm's corporate site visits affects some but not all other auditors' audit adjustments. Section 7 reports the empirical evidence that open disclosure raises the quality of audited financial statements via audit adjustments. Section 8 shows the empirical evidence that the disclosure feeds back positively onto analysts' earning forecasts: the mean and the variance of their forecast errors are smaller, but only in the presence of audit adjustments. Section 9 concludes.

2. Institutional Background and Baseline Empirical Proposition

2.1. Institutional Background

Corporate site visits are a unique form of access to management because they allow visitors to physically observe the client firms' production facilities and engage in face-

to-face discussions with management and employees about their operations.

In China, corporate site visits are usually initiated by sell-side analysts or investment managers rather than management. Sometimes, the requests come from a bank or a reporter. Mandated by the corporate site visit guidelines, visitors must formally apply for permission for a site visit, which client firms usually accommodate. A typical corporate site visit includes a pre-visit meeting, a physical visit of the client firm's production facilities and operation activities, and a post-visit Q&A session. In the pre-visit meeting, the management often briefly introduces the facilities, such as explaining how these facilities are used and maintained. After the pre-visit meeting, the visitor tours the production facilities and inventories. The visitor can examine details about specific production and employee activities in daily operations. During the post-visit Q&A session, visitors can post questions, and the management must respond to the visitors' concerns by answering their questions.

Corporate site visits take time and serious effort. Therefore, visitors would undertake such costly activities only if deemed beneficial; that is, they expect to acquire valuable information hard to obtain from companies' public releases and alternative public sources. Some observable activities often betoken client firms' development. For example, underutilized production facilities or overstocked inventories typically imply lower-than-expected sales. Then, managers must satisfactorily respond to visitors' concerns about any inconsistency between their physical inspection and prior opinions. Finally, visitors can question employees onsite and obtain information beyond management's diplomatic answers. These physical site inspections and live conversations with employees provide visitors with unique opportunities to acquire private client firm information. Brown, Call, Clement, and Sharp (2015) show that financial analysts rank corporate site visits as the second most useful way to obtain information about a client firm.

Regulation Fair Disclosure notwithstanding, to the best of our knowledge, detailed and complete disclosure of such site visit information is unavailable in both the United States and Europe. On the contrary, China's SZSE enacted the Fair Information Disclosure Guidelines ("the Guidelines") on August 10, 2006. Article 41 of the Guidelines states, ‘‘Listed companies should try to accommodate the request from investors, analysts, and fund managers to visit company headquarters and project sites

to the greatest extent."[6] In 2009, the SZSE issued a new disclosure rule mandating that all SZSE-listed client firms publicly disclose the occurrence of corporate site visits in their annual reports. However, it did not mandate the disclosure of information on site visit attendees, their questions, or the corresponding answers. Moreover, under this rule, the disclosure of corporate site visits and the issuance of annual audit reports are simultaneous, which means that the disclosure cannot affect audit reports.

Beginning in July 2012, SZSE mandated that listed client firms disclose the content of corporate site visits within two trading days following the site visits on SZSE's public platform, "Hu Dong Yi" (meaning "easy to interact").[7] However, SSE, regulated by the CSRC just as SZSE is, has no such requirement.[8] Typically disclosed corporate site visit information includes the types of visit activities, the names of the visitors, the insider attendees, the questions discussed, and the managers' answers to these questions. More importantly, such information disclosure is before auditors finalize their reports. We, therefore, expect the information to affect auditors' judgment and final reports. The efficacy of the 2012 disclosure regulation depends on SZSE's enforcement. We note that the SZSE has publicly denounced client firms that violate the 2012 disclosure regulation and issued attention letters to these client firms.[9]

Appendix A provides examples of annual report disclosures of corporate site visits under the 2009 and 2012 regulations. The disclosure format in the annual report in the pre-2012 and post-2012 regulation years are similar; both reports convey minimal details about site visits. In contrast, as shown in Panel C, the timely disclosure of corporate site visits in the "Hu Dong Yi" web portal under the 2012 regulation contains critical details on the content of formal and private meetings.

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[6] In the Guidelines, the SZSE also emphasizes, "Listed companies should arrange the site visits properly, so that visitors may better understand the companies' business and operational situations." Indeed, according to interviews documented in Cheng et al. (2016), "(client) firms usually do not reject site visit requests unless the requested visit occurs during a blackout or sensitive period (e.g., before major announcements)."

[7] The web portal is http://irm.cninfo.com.cn/szse/index_en.html.

[8] To facilitate communication between investors and client firms and protect the interests of investors, in June 2013, the Shanghai Stock Exchange (SSE) launched an official interactive platform, "E Hu Dong." This platform allows investors to directly communicate with client firms listed in SSE by posting inquiries on the Q&A section, see https://sns.sseinfo.com/. However, professional information intermediaries may choose not to conduct their corporate inquiry over a public platform. SSE did not require their listed client firms to disclose corporate site visit information.

[9] See http://www.szse.cn/disclosure/listed/bulletinDetail/index.html?38696426-eef8-4ff4-8600-02af18c6b553 for an example.

2.2. Baseline Empirical Proposition: RFD and Audit Adjustments

We expect open disclosure of corporate site visits to lead to increased audit adjustments based on two views.

The first one is the view of auditors' fear of job failure. The literature shows corporate site visits facilitate investors' information gathering (Cheng et al. 2019; Chen et al. 2022). The open-access nature of the disclosure further reduces all investors' costs in collecting information and enhances their understanding of client firms' operations. Stakeholders can triangulate their private information, the revealed information, and corporate statements. Accordingly, investors then find it easier to uncover managerial misreporting, which can implicate any auditors who have carelessly attested to a misstated financial reporting. Facing a higher probability of detection of audit failure, resultant penalties, and career damage, auditors undertake safeguard measures – carefully scrutinizing company statements and requesting needed audit adjustments to clients' statements and earnings.

The second one is the information content view. As the previous subsection describes, corporate site visits may provide hitherto private but relevant information, e.g., information related to client firm operations, production, and sales, which sheds light on cash flow trends and current fair value accounts. Auditors can combine the information revealed from corporate site visit releases and their own information to make better audit adjustment decisions. For instance, the information may help the auditor complete a 'mosaic' of information that, taken together, is material. Or, the information can validate an auditor's existing information or prompt her to recognize its incompleteness. The information triangulation sharpens an auditor's judgment on a client firm's past and future performance, leading to more extensive audit adjustments.

Thus, the empirical tests for our first research question are the following:[10]

Empirical Preposition I: The disclosure of corporate site visits before auditors have signed off on audited statements leads to increased audit adjustments in the financial statements.

[10] These empirical propositions may not necessarily hold for two possibilities. First, intermediary professionals are under pressure from various sources (Hong and Kubik 2003; Ljungqvist et al. 2009). In particular, when facing career pressures, they may engage in compromising behavior to facilitate their career advancement, pleasing managers instead of uncovering critical information and cooperating with management to pursue investment deals. These possibilities may result in skepticism regarding the usefulness of their site visits. Second, auditors may closely study company records and possess more client firm-specific information than visitors; the information obtained from others' corporate site visits may be stale (Gul et al. 2010).

Empirical Preposition II: The responding audit adjustments are more extensive if the auditor faces greater public scrutiny or higher career pressures.

Empirical Preposition III: The responding audit adjustments are more extensive if the disclosed corporate site visits include more material information.

3. Research Design

3.1. Data and Sample

We retrieve data on all corporate site visits from the company–investor interaction platform "Hu Dong Yi" and companies' annual reports. We obtain proprietary audit adjustment data from the Ministry of Finance (MOF) dataset in China and audit firm information from the yearly reports of Comprehensive Evaluations of Top 100 Chinese Audit Firms, published by the Chinese Institute of Certified Public Accountants (CICPA). Information on auditor age is from the CICPA. The stock forum data for listed client firms is from the Chinese Research Data Services (CNRDS) database. We obtain analysts' earnings forecasts, and client firms' financial and stock price information from the China Stock Market & Accounting Research (CSMAR) database.

Our sample comprises Chinese publicly listed client firms on the Main Board over the 2009–2011 period (the pre-period) and the 2013–2015 period (the post-period). We do not use the 2012 data because the disclosure regulation took effect in July 2012. Before the 2012 full disclosure regulation, corporate site visits disclosure was made simultaneously with the company's annual audit reports and could not impact audit adjustments.

A client firm is defined as a treatment client firm if it hosted at least one visitor before releasing audited company financial reports in the post-period (2013-2015).[11] By definition, a treatment client firm is from the SZSE.

We need to synchronize the release of corporate site visit reports and audit adjustments for our treatment client firms. A corporate site visit can affect audit

[11] A potential concern is the attraction of a client firm listed on the SZSE for site visits after the 2012 full disclosure regulation might change, rendering its status as a treated client firm or a control client firm questionable. Client firm fixed effects do not absorb away this time-varying change in a client firm. To further address this concern, we use three alternative requirements to define a client firm as a treatment client firm. Specifically, we define treatment client firms as those that (i) have at least one site visit in the pre-period and one site visit in the post-period; (ii) have a more significant number of site visits in the post-period than that in the pre-period; and (iii) have a smaller number of site visits in the post-period than that in the pre-period. The results (untabulated) in all cases are very similar to our baseline results, indicating that changes in firms' status are not a concern.

adjustments for fiscal year *t* if it occurs before the annual audit report announcement date for fiscal year *t* but after the most recent annual audit report announcement date for fiscal year *t-1*. In China, a company's fiscal year-end must be on December 31, and the announcement date of annual audited reports falls in the year after. Thus, we synchronize our data as follows: if a client firm's annual audit report announcement date is on date D in year *t+1* for fiscal year *t* and date E in year *t* for fiscal year *t-1*, we mark the corporate site visit reports issued after date E in year *t* and before D in year *t+1* to potentially impact the treatment client firm's audit adjustments for year *t*. Figure 1 illustrates the timeline we adopted for the synchronization.

[INSERT FIGURE 1 ABOUT HERE]

The control population comprises (1) SZSE-listed client firms that did not receive any visiting intermediary during the post-period and (2) SSE-listed client firms that are not required to release any intermediary site visit reports.

We exclude treatment client firms that went public after 2009 so that we have at least three years of data from before the regulation. Given the incomparability of audit adjustments among different auditors, we exclude treatment client firms with auditor changes after the regulation's enactment. We also exclude financial client firms.

We conduct a nearest-neighbor propensity score matching to mitigate the heterogeneity between the treatment and control client firms.[12] This involves two steps.

First, we estimate the probability of being treated by regressing $Treat_i$ on the following five sets of covariates in 2011 (2011 is the year immediately before the SZSE 2012 disclosure regulation's enactment): (1) client firm performance: profitability (*ROA*), measured as the ratio of net income to total assets, market-to-book ratio (*M.B.*), and stock returns (*Stock Return*); (2) market participants' informativeness: analyst coverage (*ANALYST*), measured as the natural logarithm of one plus the total number of analysts issuing earnings forecasts for the client firm, and institutional ownership (*INST*), measured as the ownership of institutional investors; (3) inclination to be transparent: client firm size (*Size*), measured as the natural logarithm of a client firm's total assets, market share (*MSHARE*), measured as the proportion of a client firm' sales in the total sales of all listed client firms that belong to the same CSRC industry code,

[12] See Geldof, Popovic, Van Damme, Huys, and Van Dyck (2020).

and leverage (*Leverage*), measured as the ratio of total debt to total assets;[13] (4) government involvement: an indicator variable for state-owned enterprises (*SOE*); and (5) the domiscile cite's economic status: city level GDP growth (*ΔGDP*), measured as the city's GDP in year *t* divided by the GDP in year *t-1*, minus 1, and the number of listed client firms (*NUM_Firms*) in the city (Cheng et al. 2019). Appendix B provides detailed definitions of the variables. Panel A of Appendix C reports the results of the logit regressions. The likelihood of being a treatment client firm is positively associated with profitability, analyst following, and city-level GDP growth, and it is negatively related to market-to-book ratio, client firm size, client firm leverage, and state ownership.

Second, we select a control client firm from the control population with the closest propensity score for each treatment client firm without replacement. We require the treatment client firm to be in the same CSRC official industry as the control client firm so that industry-level factors do not drive the results.[14] Panel B of Appendix C presents the effectiveness of our matching procedure. While many firm characteristics differ significantly between treatment client firms and client firms in the control population, none of the differences remain significant between the treatment and matched control client firms. Thus, the matched treatment and control client firms are comparable after our propensity score matching procedure.

Table 1 Panel A shows that, after matching, our sample consists of 440 treatment client firms and 440 matched control client firms, with 5,280 (= 440 × 2 × 6) client firm-year observations. We further discarded client firm-year observations in the treatment group without corporate site visits during 2013–2015 and their correspondingly matched control client firms. For example, if a treatment client firm has a corporate site visit in 2014 but not in 2013 and 2015, we delete its 2013 and 2015 observations and the corresponding matched control observations in our baseline regressions. After deleting observations with missing information for the main variables, our final sample comprises 4,291 client firm-year observations. Panel A of Table 1 presents the sample selection process. Panel B of Table 1 shows that our sample covers

[13] Prior studies show that large client firms usually have low information asymmetry (see, for example, Corwin 2003), and client firms having a higher demand for external financing have more significant incentives to be transparent (Leuz and Oberholzer-Gee 2006).

[14] The CSRC official industry classification is commonly used in China research (e.g., Giannetti, Liao, and Yu 2015). The results remain qualitatively unchanged if we require the treatment client firm to be in the same 2-digit industry as the control client firm.

a wide range of industries, of which the manufacturing industry accounts for the largest proportion. Panel C of Table 1 presents the sample distribution by year.

[INSERT TABLE 1 ABOUT HERE]

3.2. Empirical Model

We estimate the following DiD regression specification to examine the effect of disclosed intermediary corporate site visits on audit adjustments.

$$ADJMAG_{i,t} = \beta_0 + \beta_1 Treat_i \times Post_t + \sum \beta_n Controls + \lambda_t + \theta_i + \varepsilon_{i,t}, \quad (1)$$

where subscripts i and t refer to client firm i and year t, respectively, and λ_t and θ_i indicate client firm-fixed and year-fixed effects, respectively. *ADJMAG* is the absolute magnitude of the adjustment to earnings scaled by the absolute value of pre-audit earnings (i.e., |Audited earnings − Pre-audit earnings| / |Pre-audit earnings|) (Lennox et al. 2018). $Treat_i$ is an indicator variable that equals 1 for treatment client firms and 0 for control client firms. $Post_t$ is an indicator variable that equals 1 for the period after the adoption of the 2012 disclosure regulation (2013–2015) and 0 for the 2009–2011 period. The stand-alone $Treat_i$ and $Post_t$ indicators are subsumed by the client firm- and year-fixed effects. The DiD estimator is β_1 for $Treat_i \times Post_t$, which measures the differential change in audit adjustments for the treatment group relative to the control group after intermediary corporate site visits. Empirical Proposition I predicts a positive coefficient on $Treat_i \times Post_t$.

Following the literature (Lennox et al. 2014; Lennox et al. 2018; He, Kothari, Xiao, and Zuo 2018), we include two sets of control variables: client firm-specific and audit firm-specific characteristics.

Client firm-specific controls

We control for a client firm's size (*Size*), measured as the natural logarithm of a client firm's total assets; profitability (*ROA*), measured as the ratio of net income to total assets; client firm leverage (*Leverage*), measured as the ratio of total debt to total assets; client firm age (*Age*), measured as the natural logarithm of the client firm's age; and

market-to-book ratio (*M.B.*).

To control for a client firm's accounting complexity, we include whether the client firm engages in an M&A transaction (*M&A*) and the natural logarithm of one plus the number of subsidiaries (*Subsidiaries*), as M&A transactions and complicated accounting issues in large corporate groups can influence an auditor's judgment. To control for a client firm's reporting incentives, we include whether the client firm is a state-owned enterprise (*SOE*).

For controlling clients' corporate governance, we include the natural logarithm of the number of directors on the board (*Board Size*), the natural logarithm of the number of board meetings held by the client firm (*Board Meetings*), the proportion of independent directors on the board (*INDIR*), whether the CEO and chairman are the same person (*Duality*), and whether the client firm has an audit committee (*Audit Com*).

To control for analyst coverage of the client firm (*ANALYST*), we include the natural logarithm of one plus the total number of analysts issuing earnings forecasts for the client firm.

Audit firm-specific controls

To control for audit firm characteristics, we include a dummy indicating if a client firm's auditor is a Big 10 audit firm according to the annual reports of Comprehensive Evaluations of Top 100 Chinese Audit Firms by the CICPA (*Big10*).

We also control for the natural logarithm of an audit firm's tenure at the client firm (*Tenure*) and whether the sum of an auditor's audit fees for all of their clients is in the highest year-industry quartile (*Spec*).

Appendix B provides detailed definitions of all variables in our analyses. We winsorize all continuous variables at the highest and lowest 1% to eliminate outliers. Standard errors are clustered at the client firm level to account for within-client firm residual correlation.

3.3. Descriptive Statistics and Correlation Analysis

Panel A of Table 2 presents summary statistics for the main variables. The mean and standard deviation of *ADJMAG* are 0.088 and 0.252, respectively, suggesting a sizable cross-sectional dispersion. The mean of *ADJDUM* is 0.659, indicating that 65.9% of

the auditors adjust their clients' earnings. The average client firm size is about 3 billion (in RMB). The mean *ROA* is 0.041, and the mean *Leverage* is 0.468. The mean of *ANALYST* is 1.593, suggesting that 4 analysts follow an average client firm in our sample. The summary statistics for the main variables closely resemble those in prior studies (e.g., He et al. 2018; Cao et al. 2024).

[INSERT TABLE 2 ABOUT HERE]

Panel B of Table 2 presents the descriptive statistics of *ADJMAG*. The magnitude of audit adjustments is 0.076 for the treatment group and 0.087 for the control group before the SZSE 2012 disclosure regulation (the pre-period), and the difference between them (-0.011) is insignificant (t = -1.323). These observations suggest that audit adjustments do not differ between these two groups in the pre-period. In the post-regulation period (after the 2012 SZSE disclosure regulation), the magnitude of audit adjustments increases to 0.115 for the treatment group and declines to 0.075 for the control group. Their difference (0.040) is statistically significant (t = 2.980).[15] More importantly, the DiD in *ADJMAG* is 0.051 and statistically significant (t = 3.300), suggesting that corporate site visits are associated with increased audit adjustments.

Table 3 presents the Pearson correlation matrix of the main variables. Audit adjustments are positively correlated with client firm leverage, client firm age, market-to-book ratio, the proportion of independent directors, and Big 10 audit firms, while negatively correlated with client firm size, profitability, state ownership, the number of directors on the board, and analyst coverage. The highest variance inflation factor among these variables is 2.45, suggesting that multicollinearity is not a concern in our analyses (O'Brien 2007).

[INSERT TABLE 3 ABOUT HERE]

4. Regression Results

4.1. Baseline Results

Table 4 reports the baseline regression results of our DiD analysis. Column (1)

[15] The change in audit adjustments for the control group is -0.012 and not statistically significant (t = -1.204).

regression includes only the client firm- and year-fixed effects. The coefficient of $Treat_i \times Post_t$ is positive and significant at the 1% level (0.066, t = 3.68). The regression in Column (2) includes the full set of control variables and client firm and year fixed effects, and $Treat_i \times Post_t$ continues to load significantly positively (0.058, t = 3.03). In addition, our results are economically significant. Based on the coefficient estimates in Column (2), post-policy change, treatment client firms experience an increase in audit adjustments by 5.8% more than benchmark client firms following intermediary corporate site visits; the magnitude is comparable with others' findings.[16] These results support our empirical hypothesis that intermediaries' corporate site visits lead to an increase in audit adjustments. The results for the control variables are consistent with those reported in previous studies (e.g., He et al. 2018; Lennox, Wang, and Wu 2020): the magnitude of audit adjustments is significantly smaller for larger client firms and client firms with higher profitability.

[INSERT TABLE 4 ABOUT HERE]

4.2. Parallel Trends Assumption

We conduct several tests to provide evidence to support the parallel trends assumption for our DiD analysis. We first plot the average change in the dependent variable during our sample period (Figure 2) and find that the magnitude of audit adjustments exhibits parallel trends for the treatment and control groups before the onset of treatment (Roberts and Whited 2013).

[INSERT FIGURE 2 ABOUT HERE]

We then explore the dynamics of the effect of intermediary corporate site visits on audit adjustments. We replace $Post_t$ with the following dummies: *Year −3* and *Year −2* for the pre-period and with *Year + 1*, *Year + 2*, and *Year + 3* for the post-period; we use 2011 as the benchmark year. We interact these five timing dummies with $Treat_i$. Table 5 reports the analysis; the coefficients for both $Treat_i \times Year -3$ and $Treat_i \times Year$

[16] The magnitude is similar to the economic effect of mandatory audit partner rotation (6.5–9.3%) documented in Lennox et al. (2014), that of auditor early career experience (4.7%) documented in He et al. (2018), and that of auditor industry range (6.6–7.3%) documented in Dekeyser, He, Xiao, and Zuo (2024).

-2 are not statistically different from zero.

[INSERT TABLE 5 ABOUT HERE]

We further apply the Rambachan-Roth approach to examine the sensitivity of the main results to a potential violation of the parallel trends assumption (Rambachan and Roth 2023). This method relaxes the parallel trends assumption by allowing deviations from these trends and calculating robust confidence intervals (C.I.s) associated with these deviations for the parameters of interest. Following Rambachan and Roth (2023), we utilize an estimator type where the bias grows between two consecutive periods and is bounded by M. We define the boundaries for the relative magnitude of $\Delta^{RM}(\bar{M})$, with M presenting the extent of allowable violation post-treatment.

Figure 3 presents the estimated confidence intervals for the average effects across all three post-treatment periods. This figure contrasts the original confidence interval under our baseline assumptions (red caps) with robust confidence intervals calculated for values of M from 0 to 2 (blue caps). As the maximum deviation from the linear trend, M, increases, the confidence intervals around the estimates become wider, as expected. In the post-period, our coefficient of interest remains statistically significant at $\bar{M} = 1.4$, which allows a deviation of up to 1.4 times the maximum pre-treatment difference in audit adjustments.

[INSERT FIGURE 3 ABOUT HERE]

Figure 4 plots the tests of parallel trends assumption based on the estimation procedure documented in de Chaisemartin and d'Haultfoeuille (2020).

[INSERT FIGURE 4 ABOUT HERE]

To sum up, these analyses validate the assumption of parallel trends, and our results remain significant, allowing for considerable deviations from the parallel trends.

4.3. Robustness Checks

We conduct five types of robustness checks on our baseline results. In the following, we summarize them and report the statistical outcomes in the tables in Appendix D for readers' convenience.[17]

Robustness of Two-Way Fixed Effects DiD Estimates

First, we consider the potential estimation bias under the two-way fixed effects DiD design. de Chaisemartin and d'Haultfoeuille (2020) show that the estimator of two-way fixed effects specifications is a weighted sum of the average treatment effects in each group and period, with some possibly negative weights, which may bias the estimator of the two-way fixed effects regressions. To mitigate this concern, we first estimate the weights attached to our two-way fixed effects specifications and find only 0.9% of the weights are negative, and their sum is only -0.2%.[18] We then use an estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) and find that the very marginal negative weights do not bias our baseline result, as reported in Column (1) Table AD1.[19] Furthermore, Goodman-Bacon (2021) notes that coefficient estimates in generalized DiD models may be biased by heterogeneous treatment effects when an early treatment group serves as a control for a later treatment group. Accordingly, we conduct a diagnostic analysis following Ashraf and Sunder (2023) to mitigate this concern. We re-estimate Eq. (1) in multiple partitions, each including only a distinct treatment year in the same regression. Columns (2) – (5) in Table AD1 show the results of these runs. The baseline result, the coefficients for $Treat_i \times Post$, remains positive, statistically significant, and quantitatively consistent across all of these robustness check regressions.

Hidden Factors

Second, we examine whether hidden factors drive the baseline results in Table 4 and report the results in Table AD2. To mitigate the concerns that specific industries, provinces, or audit firms are driving our results, we control for industry × year, province of client firm headquarters × year, and audit firm × year fixed effects (Columns (1) –

[17] We welcome suggestions for additional tests to ascertain the robustness of our results.
[18] We use de Chaisemartin and d'Haultfoeuille's (2020) *twowayfeweights* Stata command to calculate the sum of negative weights.
[19] We use the Stata *did_multiplegt_dyn* command to compute de Chaisemartin and d'Haultfoeuille's (2020) estimator.

(3) of Table AD2). To control for potential omitted client firm characteristics, we input tertile dummies of client firm size × year fixed effects (Column (4) of Table AD2).[20] We also interact $Treat_i$ with each control variable to allow for changes in the control variables' influence on the dependent variable across the pre- and post-treatment periods (Column (5) of Table AD2). Finally, we double-cluster the standard errors by client firm and by year to mitigate concerns about within-year correlations (Column (6) of Table AD2). Columns (1) – (6) of Table AD2 report that our baseline results in Table 4 remain qualitatively identical in all these robust checks.

The result is spurious, reflecting auditor-client-firm characteristics and the presence of other information channels

Third, we conduct four robust checks on whether our baseline results are spurious due to corporate site visits reflecting notable client firms' and auditors' attributes and relations. Thus, our baseline results may reflect information acquisition and dissemination via channels other than public disclosures.

We first check whether the self-selection of clients and audit firms drives our results. Information intermediaries are likelier to pay attention to significant client firms (Francis, Hanna, and Philbrick 1998; Bushman, Piotroski, and Smith 2005), which tend to be audited by Big N audit firms (Francis, Maydew, and Sparks 1999). Also, auditors at Big N firms may be more competent than others (Gul et al. 2010); they may be better at detecting misreporting. To mitigate that special auditor or client firm self-selection driving our results, we divide the treatment client firms into two groups based on their audit firm size (large vs small) in the year before the 2012 disclosure regulation's enactment and investigate whether the treatment effect differs between these two treatment client firms.[21] Along the same considerations, we partition treatment clients into the above and below median client firm total assets sub-samples using 2011 data to examine whether the treatment effect differs for these two treatment clients. Columns (1) and (2) of Table AD3 show that our baseline result remains positive and statistically significant for large and small audit firms and for large and small client firms. While the impact of disclosure on audit adjustments is greater for smaller audit firms and for

[20] We rank client firm size in order from lowest to highest each year and construct a trichotomous variable that equals one when the observation is in the lowest tertile, two when the observation is in the middle tertile, and three when the observation is in the highest tertile.

[21] We use the total sales of listed clients to proxy for audit firm size. Our results continue to hold when we measure audit firm size based on the total audit fees of audit firms' listed clients.

larger client firms, these differences are insignificant. These results indicate that regulation fair disclosure impacts the auditing of client firms the same irrespective of the size of the audit firm and the client firm.

We then check whether our baseline results are spurious due to intermediaries and auditors coincidentally obtaining similar information via other information channels. For example, client firms may disclose pertinent information through social media or investor websites. To mitigate this concern, we control for information channels like investor relations, investor consulting, and the official Weibo and WeChat accounts channels.[22] We establish a *Web Relation* dummy if a client firm's website has an investor relation web, a *Web Consultant* dummy if a client firm's website has an investor consulting column, and a *Weibo&WeChat* dummy if a client firm has an official *Weibo* or *WeChat* account. Column (3) of Table AD3 shows that, in the presence of these additional controls, the coefficient of $Treat_i \times Post_t$ remains positive and significant at the 1% level, indicating our baseline result is robust. In addition, the results suggest that despite the substitution between traditional information and financial technology (Grennan and Michaely 2021; Coleman, Merkley, and Pacelli 2022), face-to-face interaction is still valuable and provides intermediaries with a comparative advantage.

Our third check is whether private information exchanges between an intermediary and auditor drive our baseline result rather than the public disclosure of corporate site visits. We can mitigate this possibility by excluding auditors and intermediaries who live close by. Previous literature shows that geographic distance limits information flows (e.g., Ivković and Weisbenner 2005; Kang and Kim 2008). Therefore, we repeat our analysis, excluding the subsample in which an auditor is located in the same city as a visiting intermediary's headquarters. Column (4) of Table AD3 reports the results. The coefficient on $Treat_i \times Post_t$, remains positive and significant, indicating that our baseline result is unlikely to be driven by private information communications between intermediaries and auditors.

Our final check is whether our baseline is spurious because, instead of public disclosure, common but significant events with widespread implications drives up both intermediaries' site visits and also auditors' audit adjustments. Our earlier industry ×

[22] Weibo and WeChat are two of the most popular social media platforms on which client firms issue information in China.

year fixed effects should absorb such effect. Still, it is useful to check if audit adjustments are raised when rare site visits happen. The intuition is that in an industry where corporate site visits are rare, the visits are more likely to be driven by client firm-specific matters. We use the ratio of the number of client firms with corporate site visits over the total number of client firms in an industry each year to define the likelihood of such a situation. We then exclude industries that have above the median number of site visits in a year and run our baseline DiD regression.[23] Column (5) of Table AD3 reports our baseline regression with the trimmed sample. The coefficient on $Treat_i \times Post_t$ is positive and significant, suggesting that our result can be driven by client firm-specific events and unlikely to be driven by common events with widespread impact on both intermediaries and auditors.[24]

Robustness in reasonable alternative specifications

Fourth, we examine if our baseline results in Table 4 remain the same in various reasonable empirical specifications. (i) We replicate our analysis using a Tobit regression because the dependent variable *ADJMAG* is truncated at zero. (ii) We use alternative measures of audit adjustments as the dependent variable, including using a dummy indicator for the presence of any audit adjustments, or the rank order of *ADJMAG*, or scaling the absolute value of audit adjustment by total assets instead of the absolute value of pre-audited earnings. (iii) Instead of the dummy, $Treat_i$, we input a treatment intensity measure to allow for variations in corporate site visits: the measure is $Treat\ \{Frequency\}_{i,}$ which is the natural logarithm of one plus the sum of site visits in each of the treatment years (2013–2015). We interact $Treat\ \{Frequency\}_i$ with $Post_t$ instead of $Treat_i$ with $Post_t$. Columns (1) – (5) of Table AD4 show that our baseline result remains.

[23] Operationally, we first calculate the ratio of the number of client firms with site visits over the total number of client firms for an industry in year t (t = 2013, 2014, 2015, the years after the SZSE 2012 regulation). Then, we mark industries in year t as more likely to have a common event if their ratio exceeds the sample median.

[24] We also consider the possibility that corporate site visits are just proxies for the social planner's specific interest in some particular industries. For example, intermediary professionals may think the real estate industry is more prone to enforcement actions if an industry leader fails. This is unlikely to occur because (1) we conduct our PSM procedure at the industry level, (2) our results are robust after we control for industry × year fixed effects, and (3) we have already excluded banks and financial institutions. To further address this concern, however, in an additional analysis (untabulated), we exclude the real estate industry and repeat our analysis. The inferences remain the same.

Auditor conservatism

Fifth, we examine if our result is driven by auditor conservatism; that is, we test whether only downward adjustments drive our results. By running split sample regressions, we allow corporate site visits to impact upward and downward audit adjustments differently. Columns (6) and (7) of Table AD4 report the DiD regressions results using only the positive audit adjustments and then only the negative adjustments client firms as the treated firms. The coefficients of $Treat_i \times Post_t$ in both columns are positive and statistically significant, and their difference is statistically insignificant.

5. The Drivers of Heightened Audit Adjustments

To strengthen interpreting our results as indicative of timely and full disclosure of corporate site visits heightens audit adjustments, we examine whether our results are consistent with the fear of job failure view (Empirical Preposition II) and the information content view (Empirical Proposition III).

5.1 Examine the Fear of Job Failure View

The fear of job failure view is that detailed disclosure of corporate site visits may increase the likelihood of finding auditor job failures, leading to penalties and reputational damage. Accordingly, auditors expected to face more career pressure will make more adjustments to avoid job failure.

In our empirical investigation, we split the treated client firms into $Treat_Greater_Pressure_i$ and $Treat_Less_Pressure_i$ based on five different measures of auditor job pressure: investor protection, state control, investor attention, overseas listing, and closeness to retirement. The first fourth measures external job pressure, while the last one measures internal job incentives.

First, a higher investor protection law index suggests more robust investor protection and, thus, more pressure on auditors to avoid job failure. We use 2011 data to sort auditors into higher (lower) audit-job pressure groups according to the investor protection law index (*PROTECT*) of the provinces where their client firms' headquarters are; a higher law index suggests stronger investor protection (Wang, Wong, and Xia 2008). Column (1) of Table 6 presents the results. *ADJMAG* increases by 0.098 for the client firms located in areas with stronger investor protection, while it increases by only 0.039 otherwise. The difference between the groups is significant at the 10%

level.

[INSERT TABLE 6 ABOUT HERE]

Second, we sort auditors into low (or high) audit-job pressure groups according to whether their client firm is a state-owned enterprise (SOEs) or a non-SOEs in 2011. SOEs typically have strong political connections and thus enjoy more business privileges, e.g., access to credits and business opportunities (Fisman 2001). Moreover, being connected with the government, SOEs can readily obtain capital support from the state when in financial distress (Faccio, Masulis, and McConnell 2006). The implication is that investors in non-SOEs are more conscious of company performance than their SOE counterparts. Therefore, we expect the positive effect of corporate site visits on audit adjustments to be more pronounced when clients are non-SOEs than SOEs. Column (2) of Table 6 shows that the effect on audit adjustments is more substantial when clients are non-SOEs: the coefficient of $Treat_non\text{-}SOE_i \times Post_t$ is positive and significant, while $Treat_SOE_i \times Post_t$ attracts an insignificant coefficient, and the difference between the two coefficients is statistically significantly.

Third, we use more investor comments to proxy for great investor attention. Specifically, we sort a treated client firm into the group with auditors facing more (less) pressure if the number of investor comments on the East Money stock forum (*COMMENT*) in 2011 is above (below) the sample median (Xu, Xuan, and Zheng 2021). Investor attention is typically associated with investors' monitoring activities and can place external pressure on auditors to avoid audit failures. Therefore, we expect that, following corporate site visit disclosure, audit adjustments in client firms will be greater among client firms with the above median investor comments. As shown in Column (3) of Table 6, *ADJMAG* increases by 0.090 when client firms have more investor comments, while it increases by only 0.022 otherwise. The difference between the groups is significant at the 5% level.

Fourth, we split the treated client firms into two groups based on whether they cross-listed their stock on international exchanges (*OVERSEAS LISTING*) in 2011. The literature shows that client firms with overseas listings have higher visibility (Baker, Nofsinger, and Weaver 2002) and thus obtain more attention from the market. Therefore, auditors of these client firms have more pressure to avoid job failure. Hence, we expect

audit adjustments to be more extensive when client firms cross-list their stock on international exchanges. As shown in Column (4) of Table 6, *ADJMAG* increases by 0.202 for client firms with overseas listing, while it increases by only 0.046 otherwise. The difference between the groups is significant at the 10% level.

Fifth, we partition the treatment client firms into the group of client firms whose auditors face greater (less) pressure to avoid job failures if the auditor age (*NONRETIREMENT*) is below (above) 50.[25] A younger auditor farther away from retirement typically has a greater concern about his/her career development and, consequently, faces more pressure to avoid job failures (Gibbons and Murphy 1992). In contrast, the implicit incentives are weaker for an auditor approaching retirement. Therefore, we expect our evidence to be stronger for auditors who are not approaching retirement. Column (5) of Table 6 presents the results. *ADJMAG* increases by 0.058 when an auditor is further away from retirement, while it declines by 0.005 otherwise. The difference between the groups is significant at the 10% level.

These results support Empirical Preposition II: given disclosure of site visits, auditors under more significant job pressure avoid job failures and adjust their clients' earnings more.

5.2 Examine the Information Content View

We conduct four tests on the empirical validity of the information content view. First, we check whether the extent to which intermediaries' corporate site visits influence audit adjustments varies with the type of information that intermediaries gather through site visits. We expect that a visit discussing forward-looking matters and financial and accounting topics will convey more immediately relevant information than one covering other information. The reason is that an auditor's objective is to examine the credibility of reported past performance and projections for the future.

We use machine learning to classify the questions posed during the observed site visits to explore these conjectures. Specifically, we manually classify the questions in the training data into four primary topic categories: ***Forward-Looking Information, Accounting and Finance Topics, Nonfinancial Information***, and ***Miscellaneous Information***. Following Cheng et al. (2019), the keywords related to Forward-Looking

[25] In China, the mandatory retirement age is 60 for men and 55 for women until 2024. Therefore, we use 50 as the cutoff in the analyses.

Information are "future strategy," "future development," "prospect," "vision," and "positioning;" the keywords related to Accounting and Finance topics are "performance," "gross margin," "forecast," "quarterly disclosure," and "annual disclosure," "seasoned offering," "bond issuance," "bank loan," "convertible debt," "private issuance," and "dividend"; the keywords related to nonfinancial information are "corporate social responsibility," "ESG," "environmental engagement," and "environmental governance"; the keywords related to miscellaneous information are "purpose of visits," "communication of visits," "introduction about the company," and "other information." We implement a gradient-boosted regression tree (GBRT) model developed by Friedman (2001) in Python that takes the training questions and four topic categories as inputs, trains and tests a model, and then uses the model to identify the topic category for each remaining question. Because the 2012 disclosure regulation took effect in July 2012, we use fiscal year 2012 as the training and validation period and the post-period (i.e., 2013-2015) as the test period.[26]

After classifying the questions into four topic categories, we count words for each answer to this question. *Visits_Forward-looking Information* is the word count for answers to questions covering forward-looking information; *Visits_Accounting and Finance Topics* is the word count for answers to questions covering accounting and finance topics; *Visits_Nonfinancial Information* is the word count for answers to questions covering nonfinancial information; we scale all word counts by the total word counts of disclosed corporate site visits in a given year *t*. We interact *Visits_Forward-looking Information*, *Visits_Accounting and Finance Topics*, and *Visits_Nonfinancial Information* with $Post_t$, respectively, and report the results in Column (1) of Table 7. By construction, (1 - *Visits_Forward-looking Information* - *Visits_Accounting and Finance Topics* - *Visits_Nonfinancial Information*) is the proportional word count for "miscellaneous information."

The coefficient of the interaction between *Visits_Forward-looking Information* and $Post_t$ is positive and significant at the 5% level (0.049, t = 2.37), suggesting that visits covering forward-looking information provide useful information to financial markets. The coefficient of the interaction between *Visits_Accounting and Finance Topics* and $Post_t$ is also positive and significant at the 5% level (0.236, t = 2.11), indicating that financial statement–related information helps auditors and markets

[26] We observe about 63 percent mean prediction accuracy.

update their prior opinions. The coefficients of the interactions between *Visits_Nonfinancial Information* and $Post_{t,}$ as well as between Miscellaneous information and $Post_t$, are both insignificant, suggesting that nonfinancial and miscellaneous information is unlikely to affect auditing.

[INSERT TABLE 7 ABOUT HERE]

Second, we examine whether the extent to which intermediaries' corporate site visits influence audit adjustments varies with the relevance between questions and answers in the site visit disclosure. We expect more relevant and targeted answers to be more informative, and if auditors indeed incorporate the disclosed information in their audit adjustments, the disclosure of these site visits will generate more audit adjustments.

Following prior literature (Bian, Chen, and Wang 2022), we employ the cosine similarity between question-answer text pairs to evaluate the answering relevance. Using textual similarity features in question-answering tasks to select relevant answers to questions is a common practice in the sense that a relevant answer to a question should be related to the topic and semantics of the question (Dai, Gong, Jackson, and Peng 2022).

Because similarities based on the "Bag of Words" model can only measure lexical similarity and fail to characterize the semantic relations, we use the word embeddings generated by the Word2Vec model (Mikolov, Chen, Corrado, and Dean 2013) to calculate the similarities. The similarity measure is Chinese word-based and implemented with the genism package in Python. The pairwise question-answer similarities are between (0, 1), and greater values imply a larger overlap between the two vectors of words (i.e., more similar question-answer text). We define the answer relevance measure of treated client firm i in year t as the average value of question-answer similarities in the year (t = 2013, 2014, 2015, the years after the SZSE 2012 regulation).

We then create a dummy, $High_Relevance_{i,t}$, which is equal to 1 if the answer relevance of treated client firm i in year t is greater than the sample median, and 0 otherwise. We re-estimate the Eq. (1) but add the interaction between $Treat_i \times Post_t$ and $High_Relevance_{i,t}$ in the model, and report the results in Column (2) of Table 7. The

coefficient on $Treat_i \times Post_t$ is insignificant, suggesting that low-quality disclosure does not increase audit adjustments. The coefficient on $Treat_i \times Post_t \times High_Relevance_{i,t}$ is positive and significant at the 1% level (0.058, t = 2.69), suggesting that only high-quality disclosure, namely high relevance between questions and answers, can provide incremental valuable information to auditors and markets.

Our third test is based on the expectations that disclosed corporate site visits will be more informative when involving client firms with higher asset tangibility because these client firms have more observable facilities and assets, which a visitor can inspect. We partition the treated client firms into higher and lower asset tangibility sub-groups using 2011 data and the median ratio of property, plant, and equipment over total assets (*ASSET TANGIBILITY*) as the divider (Cao et al. 2024). We label the treated client firms with higher and lower asset tangibility as $Treat_More_Information_i$ and $Treat_Less_Information_i$, respectively. The premise is that the former client firms' auditors are expected to receive more information from the release of corporate site visits than the latter client firms' auditors. Column (1) of Table 8 reports that *ADJMAG* increases by 0.087 (row 1) when client firms have more tangible assets; otherwise, it increases by only 0.029 (row 2). The difference across groups is significant at the 5% level.[27]

[INSERT TABLE 8 ABOUT HERE]

Fourth, we expect the disclosure of corporate site visits to convey more incremental information for client firms in poor information environments. We adopt two approaches to examine the possibility.

In our first approach, we split the treated client firms into the below and above median analysts following sub-samples, using 2011 data. Han et al. (2018) show that the public knows less (more) about those client firms with below-median (above-median) analysts following. Thus, corporate site visits emit more (less) information in the below (above) median analyst following group. Column (2) of Table 8 shows that *ADJMAG* increases by 0.106 among client firms followed by fewer analysts, while it

[27] In an untabulated analysis, we find that corporate site visits also lead to an increase in audit adjustments to total assets, and the results are stronger for client firms with higher asset tangibility. These results are consistent with the notion that the information content of full disclosure is arguably associated with visitors' physical inspection of tangible assets.

increases by only 0.010 among those more followed. This difference between the two groups is significant at the 1% level. The results continue to hold when we scale the number of analysts following by the natural logarithm of client firms' total assets or when we scale the number by total assets.

In our second approach, we split the treatment client firms into above and below-median stock price synchronicity subgroups using 2011 data. Morck et al. (2000) suggest that those in the above stock price synchronicity group have lower informed risk arbitrage. Morck, Yeung, and Yu (2013), elaborating on the subject matter, state that client firm-specific return volatility characterizes the intensity of the occurrence of client firm-specific events, which links with the ability of an informed stock market to finance creative destruction and which, in turn, elevates client firm-specific fundamental volatility and loops back to elevate client firm-specific return volatility. Thus, lower client firm-specific return volatility, i.e., higher return synchronicity, is characteristic of a client firm falling in a lower information content segment of the market and, therefore, can benefit more from information released from corporate site visits. Column (3) of Table 8 shows that the effect on audit adjustments is significantly stronger (0.085) when a client firm is inside the less informed segment of the market.

These results are consistent with Empirical Preposition III, which states that the disclosure of corporate site visits raises audit adjustments because the site visits provide auditors with incremental information.

6. Spillovers of Open Disclosure's Impact

Our results indicate that disclosing intermediaries' corporate site visits provides material information and presses auditors to be conscientious. This section examines our second key empirical question: Will open disclosure affect the non-directly affected client firms and their auditors? The intuition is that client firms in the same industry share similar industry-level fundamentals, though they may be competitors. Hence, information about one client firm, especially a significant client firm, could be material for peer client firms in the same or related industries (Hameed, Morck, Shen, and Yeung 2015; Merkley, Michaely, and Pacelli 2017).

To facilitate further communication, we call a client firm that discloses corporate site visits an "***affected client firm***," and its auditor an "***affected auditor***." An auditor in an affected auditor's audit firm is a "***peer auditor***;" the rest are "***non-peer***

auditors." Client firms in an affected client firm's sector and 2-digit industry are "***close-peer client firms,***" while those in the same sector and the same 1-digit (but not the same 2-digit industry) are "***distant-peer client firms***,"[28] Others are "***unrelated client firms***." (We also adopt these terms in the next section.)

Like in other countries, auditors keep privileged client firm-specific information confidential in China. However, auditors in the same audit company often can share industry-level information. Indeed, industry-level knowledge is usually an audit firm's pride. Therefore, intuitively, there are two information spillover avenues as follows. (i) The first avenue is directly through the affected auditor; the information spillovers may prompt her to raise audit adjustments in close-peer and distant-peer client firms that she audits. (ii) The second avenue is information spillovers from an affected auditor to her peer auditors; the spillovers may prompt peer auditors to raise audit adjustments for close-peer and distant-peer client firms they audit.

Following the specification in Eq. (1), we run four regressions to examine these potential information spillovers from a client firm, say *V*, with disclosed corporate site visits in year *t* after the SZSE 2012 regulation (*t* = 2013, 2014, 2015). First, we focus on the potential information spillovers from *V* via an affected auditor to (i) a close-peer client firm and (ii) a distant-peer client firm; both have hosted no corporate site visits and audited by the corresponding affected auditor in year *t*. Second, we focus on the potential information spillovers from *V* via a peer auditor to (iii) a close-peer client firm and (iv) a distant-peer client firm; both hosted no disclosed corporate site visits and audited by peer auditors in year *t*. All these client firms are "treated" and designated a $Treat_Spillover_i$ dummy in the regressions.

The control client firms in these four regressions are the $Treat_Spillover_i$ client firms' matching client firms drawn from our original control group (client firms with no corporate site visits and are audited by non-peer auditors). For treated client firms (i) and (ii), we use the nearest-neighbor propensity score matching procedure and 2011 data to pair each treated client firm with a control client firm closest in the propensity score and shares the same CSRC industry code. In addition, we exclude close-peer and distant-peer client firms audited by peer auditors from our potential control group. We

[28] We stricty apply the designation to client firms in the manufacturing sector, which is 64% of our sample. In the remaining sectors, like Agriculture, mining and real estates, the sectoral classification, e.g., A01, .. and Sector-1-digit classification are identifical, because firms in these sectors are not as refinely differentiated within a sector. For robustness, we repeat our reported regressions using only the manufacturing sector. The results are qualitatively identical.

do the same for treated client firms (iii) and (iv) but exclude close-peer and distant-peer client firms audited by the affected auditor from the potential control group.

Before we proceed further, we check whether these control firms' audit adjustments exhibit any time trends, especially in the treatment period, by running the regressions in Table 5 using 2011 as the benchmark year. The coefficients for the year dummies are all insignificantly different from zero. Thus, these control firms' audit adjustments exhibit no changes from the benchmark year in the before and after treatment periods, which means that the SSC 2012 Regulation Fair Disclosure did not impact the audit adjustments for the "unrelated" client firms.

Columns (1) – (4) of Table 9 present the results of these four regressions, respectively. Column (1) shows that the coefficient for $Treat_Spillover_i \times Post_t$ is positive and statistically significant (0.057, t = 2.05), while the corresponding coefficient in Column (2) is insignificant. Thus, the information spillovers from an affected client firm can raise the affected auditor's audit adjustments for close-peer client firms but not her audit adjustments for distant-peer client firms. Column (3) shows the coefficient of $Treat_Spillover_i \times Post_t$ is positive and significant at the 5% level (0.050, t = 2.14). These results indicate that the information spillovers from an affected client firm raise audit adjustments for close-peer client firms audited by peer auditors working in the same audit firm. Column (4) shows no spillovers from an affected client firm to peer auditors' audit adjustments for distant-peer client firms. (We designate the treated client firms in Columns (1) and (3) as spillover-receiving client firms.)

[INSERT TABLE 9 ABOUT HERE]

Additionally, we perform a robustness check to ascertain the integrity of our baseline results reported in Table 4. In that table, the control client firms could include client firms receiving information spillovers from disclosed site visits at affected client firms. We now include two groups of treatment client firms, each captured by a corresponding dummy: (i) the affected client firms and (ii) the spillover-receiving client firms (which are the treated firms in Columns (1) and (3)). The potential control group comprises client firms with no corporate site visits and do not receive information spillovers. We selected a matched-control client firm from the potential control group for each client firm in the two treated groups using the nearest-neighbor propensity

score matching procedure based on 2011 data. Also, we require a treated client firm and its matched control client firm to share the same CSRC industry code.

Column (5) of Table 9 reports the same DID baseline regression with the added dummy $Treat_Spillover_i \times Post_t$, indicating a client firm that may receive information spillovers from a treated client firm's corporate site visit. The coefficient for $Treat_Spillover_i \times Post_t$ is 0.054 and significant at the 5% level (t = 2.18), indicating that information spillover raises audit adjustments. The coefficient for $Treat_i \times Post_t$ is 0.129, significant at the 1% level (t = 4.12). A F-test indicates that the latter is significantly greater than the former.[29,30]

Our results suggest that information spillovers from disclosed corporate site visits are limited. The results in Columns (1) to (4) are consistent with the view that the full disclosure of a corporate site visit emits information relevant only to close-peer client firms audited by an affected auditor or by her peer auditors. Column (5) results indicate that the direct impact of disclosing corporate site visits on affected client firms is greater than the indirect spillover impact on peer client firms. The observation opens the question of the reach of even the most public information releases, even on an open-access website.

7. Effect of Disclosed Corporate Site Visits on the Quality of Financial Reports

We now turn to our third key empirical question: Does the 2012 full disclosure regulation improve the reliability of corporate financial statements? We capture reliability by low discretionary accruals and the probability of financial misstatements. The empirical focus is whether the changes in these measures from the pre- to post-2012 full disclosure regulation years differ between the treated and control client firms and whether the differences are attributable to audit adjustments.

We have two types of treated firms: $Treat_i$ indicates client firms with disclosed

[29] To further mitigate the concern that the spillover effect may introduce bias in our baseline estimator, we exclude the spillover-receiving control client firms from the control population and repeat our nearest-neighbor propensity score matching procedure using 2011 data, pairing each treatment client firm with a control client firm that is closest in propensity score and shares the same CSRC industry code, and then re-estimate the Eq. (1). As shown in Table AD5, our results continue to hold.

[30] We notice that the sample size in regression Column (5) of Table 9 (= 3,274) is smaller than that in regression Column (2) of Table 4 (= 4,291). We expect this situation because we have designated many control client firms from the original control group in Table 4 into the *Treat_Spillover* treated type. This reduces the control population's size and increases the size of the treated (*Treat* and *Treat_Spillover*) population. The outcome is fewer treated PSM pairs. To check the robustness of our results, we repeated the regression using all available firms in the control population. We obtained qualitative identical results, which are available upon request.

corporate site visits, and $Treat_Spillover_i$ indicates spillover-receiving client firms, which are the treated firms in Columns (1) and (3) in Table 9, respectively. (Table 9 reveals that, after the 2012 full disclosure regulation, these two types of treated client firms have statistically significantly more audit adjustments.) From the population of potential control client firms (client firms with no corporate site visits), we source a matched-control client firm for each of these two types of treated firms using the nearest-neighbor propensity score matching procedure based on 2011 data. Also, a treated client firm and its match control client firm must share the same CSRC industry code in 2011.

Our first step involves testing whether disclosed corporate site visits, the differentiator between targeted and control client firms, affect the pre-audit discretionary accruals. Using pre-audited company earnings statements, we compute discretionary accruals (*Pre-ABSDA*) based on the modified Jones model, following Dechow, Sloan, and Sweeney (1995). Following the literature (Gong, Li, Lin, and Wu 2016; He et al. 2018), we control for a battery of client firm-level client and auditor variables that may influence accrual quality. We control for client firm size (*Size*), profitability (*ROA*), leverage (*Leverage*), age (*Age*), market-to-book ratio (*M.B.*), audit firm's industrial specialization (*Spec*), audit firm's tenure (*Tenure*), state ownership (*SOE*), complexity (*InvRec*, measured as the sum of accounts receivable and inventory divided by total assets), current ratio (*Current Ratio*, measured as total current assets divided by total current liabilities), cash (*Cash*, which measured as the cash and cash equivalents divided by total assets), loss (*Loss*, an indicator variable that equals 1 if the client firm reports a loss, and 0 otherwise), analyst coverage (*ANALYST*), the percentage of ownership held by the largest shareholder (*Top1*), stock return (*Stock Return*, calculated as annual market-adjusted stock return), and return volatility (*Stock Vol*, calculated as the standard deviation of residuals from the market model estimated by daily return data).

Table 10 Column (1) reports the regression results using *Pre-ABSDA* as the dependent variable. $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are the cross terms between the post-regulation treatment dummy and the treated client firm dummy as well as the spillover receiving client firm dummy, respectively. The regression coefficients for both cross terms are statistically insignificant, indicating that intermediaries visiting sites do not affect the quality of the pre-audited financial

statements of the $Treat_i$ and $Treat_Spillover_i$ firms.

[INSERT TABLE 10 ABOUT HERE]

We turn to the effect of disclosing corporate site visits on the reliability of post-audit financial statements. Specifically, we use audited discretionary accruals (*Audited-ABSDA*) and financial misstatements (*RESTATE*) as the dependent variables[31]. *Audited-ABSDA* is again computed using the modified Jones model, following Dechow et al. (1995), and post-audit company earnings statements. *RESTATE* is an indicator variable that equals 1 if the client's financial statement is restated in a subsequent year due to accounting irregularities and 0 otherwise (Dekeyser et al. 2024). [32] Financial misstatements due to accounting irregularities are relatively rare events, accounting for 3.76% of client firm-year observations in our sample.[33] We include as control the variables mentioned above that may influence the accrual quality.

Table 10 Columns (2) and (3) report the regression results. Column (2) is based on the sub-sample of client firms without audit adjustments (i.e., *ADJDUM* = 0). Both $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are insignificant, indicating that treated client firms and spillover-receiving client firms without audit adjustments feature no visible changes in their post-audit discretionary accruals after corporate site visits are disclosed. Column (3) is based on the sub-sample of client firms with audit adjustments (i.e., *ADJDUM* = 1). The coefficient for $Treat_i \times Post_t$ (β_1) is -0.025 (t = -2.29) and the coefficient for $Treat_Spillover_i \times Post_t$ (β_2) is -0.017 (t = -2.06).[34] These results show that disclosing corporate site visits can lead to less audited discretionary accruals, but only in the presence of audit adjustments.

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[31] We estimate the regression model using an OLS method when the dependent variable is *RESTATE*. The following factor motivates our choice of a linear rather than nonlinear model. Nonlinear models can introduce biased estimates in panel data sets with a short time series and many fixed effects, leading to an incidental parameters problem and inconsistent estimates. The incidental parameters problem, discussed in Greene (2004), arises because the number of fixed effects increases without bounds; however, the amount of information available for their estimation is limited, particularly in large panel data sets with a short time series. As such, both fixed effect estimates and coefficients on other variables tend to be biased in this setting.

[32] We manually collect restatement data from the "Material Accounting Errors" section of the financial statement footnotes. We exclude restatements due to changes in accounting standards, mergers and acquisitions, stock splits, or other issues unrelated to accounting irregularities.

[33] The relatively low frequency of financial misstatements is consistent with prior China research (e.g., Dekeyser et al. 2024).

[34] Although β_1 is more negative than β_2, the Wald test shows that the difference is insignificant, indicating that corporate site visits' direct and information spillover effects on improving audited discretionary accruals are similar.

Columns (4) – (5) of Table 10 report regressions explaining the likelihood of a client firm issuing financial misstatements (*RESTATE*). Column (4) shows that for the sub-sample of client firms without audit adjustments, both $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are insignificant. Column (5) shows that the coefficient for $Treat_i \times Post_t$ (β_1) is -0.065 (t = -2.46) and the coefficient for $Treat_Spillover_i \times Post_t$ (β_2) is -0.052 (t = -2.19). Again, although β_1 is more negative than β_2, their difference is statistically insignificant. These results show that after the 2012 full disclosure regulations, both treated client firms and information spillover-receiving client firms have a reduced likelihood of issuing financial restatements but only in the presence of audit adjustments.

We check the robustness of our results in two ways. First, we undertake another approach to examine the role of audit adjustment in affecting the quality of financial statements. We use $Treat_i \times Post_t$ and $Treat_Spillover_i \times Post_t$ as instruments for audit adjustments and use the two-stage least squares (2SLS) approach to examine whether audit adjustments are the mechanism through which the disclosure of corporate site visits improves the quality of financial statements. We first regress $ADJMAG_{i,t}$ on $Treat_i \times Post_t$ and $Treat_Spillover_i \times Post_t$. The coefficients for both $Treat_i \times Post_t$ and $Treat_Spillover_i \times Post_t$ are significantly positive, with an F-statistic of 14.79, larger than the cutoff value of 10 for a weak instrument. In the second stage of the 2SLS regression, the dependent variables are audited discretionary accruals (*Audited-ABSDA*) and financial misstatements (*RESTATE*), respectively. The coefficient for the instrumentalized $ADJMAG_{i,t}$ (instrumented by $Treat_i \times Post_t$ and $Treat_Spillover_i \times Post_t$) is significantly negative, establishing that audit adjustments are the tool that auditors use to improve the quality of financial statements following disclosure of corporate site visits. These results are available upon request.

Second, as explained when we describe the result in Table 9 Column (5), having two types of treated client firms ($Treat_i$ and $Treat_Spillover_i$) raises the treated population and reduces the control population, reducing the number of treat-PSM pairs. For robustness, we utilize all available control client firms (i.e., without treatment-PSM pairing) and repeat all the regressions in Table 10. We obtain qualitative identical results, which are available upon request.

Thus, the results in Table 10 are robust and consistent with the notion that full disclosure of corporate site visits improves the quality of client firms' financial

reporting via increasing audit adjustments.

8. Effect of Disclosed Corporate Site Visits on Intermediaries' Research Quality

Our final research question is whether the higher-quality financial reports resulting from corporate site visit disclosure and audit adjustments improve investor corporate earnings forecasts.

The only publicly available earnings forecasts are from sell-side analysts. We retrieve their one-year-ahead earnings forecast. As we need to calculate the consensus forecast, we only include client firm-years with at least three analysts following. We estimate the quality of earnings forecasts following prior literature. We first calculate each analyst's one-year-ahead forecast error, measured as the absolute difference between the actual and an analyst's forecast of firm *i*'s earnings per share for year *t*, scaled by the client firm *i*'s stock price at the beginning of year *t*.[35] The first proxy for earnings forecast quality is $FERROR_{i,t+1}$, the mean of all forecast errors. The second proxy is $FDISP_{i,t+1}$, the standard deviation of all the forecast errors.

The sample period of both $FERROR_{i,t+1}$ and $FDISP_{i,t+1}$ spans from 2010–2012 (the pre-period) and 2014–2016 (the post-period). The gist of our empirical question is whether, after the imposition of fair regulation disclosure, the audit adjustments in year *t* affect $FERROR_{i,t+1}$ and $FDISP_{i,t+1}$. Thus, we cannot use $FERROR_{i,t+1}$ and $FDISP_{i,t+1}$ in 2013, as 2012 is the transition year when fair regulation disclosure was just adopted.

The two types of treated client firms and the control client firms are identical to those in Table 10. Again, we use the DiD approach (Eq. (1)), with earnings forecast errors ($FERROR_{i,t+1}$) and error dispersion ($FDISP_{i,t+1}$) as the dependent variables. We replace audit firm–specific controls in the specification with three variables that can affect analyst research quality. *INST* is the share of institutional investor ownership. *Return Vol* is the standard deviation of daily stock returns in the year. *ForHorizon* is the natural logarithm of the average number of days between the announcement of a client firm's financial statement and analyst forecasts.

The dependent variable in the DiD regressions in Columns (1) – (3) of Table 11 is $FERROR_{i,t+1}$. Column (1) is based on the full sample; the coefficients for both $Treat_i$

[35] We also use the absolute difference between an analyst's forecast for year *t* for a client firm *i*'s earnings per share and the actual earnings per share for the same year, scaled by the client firm *i*'s stock price one trading day before the forecast date, to measure each analyst's forecast error. The inferences remain the same (untabulated).

× $Post_t$ (β_1) and $Treat_Spillover_i$ × $Post_t$ (β_2) are insignificant, suggesting that, on average, treated client firms and the spillover-receiving client firms feature no visible change in analyst forecast accuracy post the disclosure regulation. Column (2) is based on the sub-sample of client firms without audit adjustments (i.e., *ADJDUM* = 0). The coefficients for both $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are insignificant, suggesting that both treated client firms and spillover-receiving client firms without audit adjustments feature no visible change in analyst forecast accuracy. Column (3) is based on the sub-sample of client firms with audit adjustments (i.e., *ADJDUM* = 1). The coefficient for $Treat_i \times Post_t$ (β_1) is -0.008 (t = -3.33) and the coefficient for $Treat_Spillover_i \times Post_t$ (β_2) is -0.006 (t = -1.74), indicating that the 2012 full disclosure regulations improve the analyst one year ahead forecast accuracy for both treated client firms and spillover-receiving client firms with audit adjustments. Although β_1 is more negative than β_2, the Wald test shows that the difference is insignificant.

[INSERT TABLE 11 ABOUT HERE]

Columns (4) – (6) of Table 11 report the results using $FDISP_{i,t+1}$ as the dependent variable. Column (4) is based on the full sample and the coefficients for both $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are insignificant, suggesting that, on average, the treated client firms and spillover-receiving client firms feature no visible change in analyst forecast dispersion. Column (5) is based on the sub-sample of client firms without audit adjustments (i.e., *ADJDUM* = 0). The coefficients for $Treat_i \times Post_t$ (β_1) and $Treat_Spillover_i \times Post_t$ (β_2) are insignificant, suggesting that, without audit adjustments, both treated client firms and the potential spillover receiving client firms feature no visible change in the dispersion of analyst forecast error. Column (6) is based on the sub-sample of client firms with audit adjustments (i.e., *ADJDUM* = 1). The coefficient for $Treat_i \times Post_t$ (β_1) is -0.005 (t = -2.47) and the coefficient for $Treat_Spillover_i \times Post_t$ (β_2) is -0.003 (t = -1.70), indicating that the 2012 full disclosure regulations decrease the dispersion of analyst one year ahead forecast error for both treated and spillover-receiving client firms with audit adjustments. The difference between β_1 and β_2 is insignificant.

We conduct robustness checks on Table 11's results as we check the robustness of Table 10's results. First, we use a 2SLS approach to examine the impact of

instrumentalized audit adjustments on $FERROR_{i,t+1}$ and $FDISP_{i,t+1}$. Again, the instruments are $Treat_i \times Post_t$ and $Treat_Spillover_i \times Post_t$. The coefficient for the instrumentalized $ADJMAG_{i,t}$ coefficient is significantly negative in explaining $FERROR_{i,t+1}$ and $FDISP_{i,t+1}$, indicating audit adjustments due to site visit disclosure reduce analyst one year ahead forecast errors' mean and standard deviation. These results are available upon request.

Second, as explained earlier, having two types of treated client firms ($Treat_i$ and $Treat_Spillover_i$) raises the treated population and reduces the control population, reducing the number of treat-PSM pairs. For robustness, we utilize all available control client firms (i.e., without treatment-PSM pairing) and repeat all the regressions in Table 11. We obtain qualitative identical results.

Thus, Table 11's results are robust and consistent with that analysts benefit from heightened audit adjustments and issue more accurate and less dispersed consensus forecasts. We interpret the strings of results from Section 4 to here support that: (i) fair disclosure regulations raise auditing quality reflected by higher audit adjustments, (ii) the higher audit adjustments contribute to higher quality financing statements, (iii) which in turn allow investors to make more reliable earnings forecasts.

9. Conclusions

By discovering and transmitting material information to investors, intermediary professionals help to monitor and discipline management. However, strategic revelations of private but material information from management to intermediaries can distort intermediaries' monitoring. Thus, regulators mandate disclosure of site visit information to mitigate the problem. Still, while such disclosure may reduce uneven transmission of information from managers to intermediaries and investors, it can have long-term negative repercussions as it may reduce intermediary professionals' incentives to dig up unique material information. This may also hinder less well-known client firms' efforts to draw investors' attention. Extent research on fair disclosure regulations reveals the intricacy of these trade-offs.

We advocate expanding the investigation into multiple regions because the impact of fair disclosure regulation depends on its completeness and effective enforcement, which could vary across regions. We also advocate expanding the investigation to how the regulation affects auditors' behavior, the quality of corporate

financial statements, and how these changes impact the quality of intermediaries' earnings forecasts. The expansion is important because intermediaries, auditors, and investors have interactive relations in monitoring and disciplining managers, as revealed in a vast amount of literature.

To do so, we exploit a 2012 regulatory change from limited to mandatory full disclosure of information on an open-access website regarding intermediary corporate site visits by the Shenzhen Stock Exchanges but not other exchanges in China. We apply a DiD research design to study the impact of open full disclosure on auditing behavior and the resultant quality of audited financial reporting. We obtain robust results on more extensive audit adjustments following full disclosure of corporate site visits. Consistent with a fear of job failure view, the resultant audit adjustments are more extensive for client firms located in areas with stronger investor protection, non-SOE client firms, and clients firms with more investor attention, with overseas listings, and when the auditors are not approaching retirement. Also consistent with an information content view, the results are more pronounced for visits where forward-looking information and accounting and finance topics are discussed during a site visit and when client firms give more relevant answers to intermediaries' questions during site visits.

Furthermore, open full disclosure of corporate site visits generates information spillovers impacting other auditors. We find that an intermediary's site visit to a client firm raises the audit adjustments for peer client firms in the same industry audited by the affected auditor or fellow auditors working in the same audit company.

We also find that open full disclosure of corporate site visits raises the quality of these client firms' financial reports. After the 2012 full disclosure regulation, corporate site visits are linked to client firms' lower audited earnings management and lower likelihood of financial misstatements. Information spillovers also do the same to client firms in the same industry audited by the affected auditor and her fellow auditors in the same auditing firm. However, these positive effects only appear in the affected client firms and spillover-receiving client firms with audit adjustments. These results imply that timely full disclosure of corporate site visits improves the quality of auditing and financial reporting via heightening audit adjustments.

Finally, these improvements linked with audit adjustments allow more reliable earning forecasts, proxied by analyst earnings per share forecasts: the forecast errors are lower on average and with a lower standard deviation. These results showcase that

open and full disclosure of corporate site visits enhances the complementary efforts between analysts and auditors in delivering high-quality financial reports and reliable earnings forecasts.

Our results shed light on fair disclosure regulations. Studies of fair disclosure regulation obtain mixed results, hinging on incomplete disclosure requirements and doubts about enforcement. The regulatory change in the Shenzhen Stock Exchange showcases that fair disclosure regulation requires full disclosure rules. We believe the enforcement is strict; however, this judgment deserves further scrutiny. In our opinion, the positive effect in our case is grounded on open internet-based disclosure – it facilitates ex-ante and ex-post checking of evidence by stakeholders, which generates a higher level of triangulation of the credibility of intermediary professionals.

Our findings have important policy implications regarding promoting management access activities and their disclosure. We show that direct human efforts remain instrumental, even when technology often replaces humans in many areas in collecting information. Technology, including machine learning, cannot easily acquire the detailed and interactive information generated via in-person interactions. However, direct human efforts and technology are complementary. The Shenzhen Stock Exchange's 2012 disclosure regulation provides an example: the public full disclosure of the results of corporate site visits on an open-access public digital platform (Hu Dong Yi) improves the efficacy of auditing, leading to higher-quality financial reporting and, in the end, more reliable analyst forecasts, which we use to proxy for intermediaries' earnings forecasts. This positive result of human-technology collaboration is particularly noteworthy in economies where equity prices are unreliable guidelines for resource allocation.

Finally, we note that our positive results on full disclosure regulations are conditioned on corporate site visits that have taken place. Because of this precondition, we cannot conclude definitively that full disclosure regulations improve overall market information. Strong disclosure regulations could backfire, like making intermediary professionals curtail information discovery activities governed by full disclosure regulations. Recent papers like Ke et al. (2022) showcase such happenstance. However, research also shows that full disclosure regulations positively impact market information, conditioned on the occurrence of site visits (e.g., Yang et al. 2020; Ru et al. 2023; Xiong and Yang 2023; Cao et al. 2024). Our paper shows that timely and

complete disclosure regulations enhance the value of such discovery efforts directly and via the spillover effects. Thus, timely and effective full disclosure would raise the reputational gains of an intermediary professional who first identifies important firm-level information. We believe that the jury on aggregate information discovery is still out, and further research is warranted.

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Appendix A
Site visit disclosure examples

Panel A: Examples of corporate site visits from the annual report of Qinghai SALT LAKE POTASH Co., Ltd. (stock code 000792.SZ) in the pre-period (2009-2011)

Time	Location	Visit Type	Visitor	Discussion
2009/06/27	Client firm Headquarters	Site visit	Guotai Junan Securities, ChangXin Fund Management Co., Ltd, First State Cinda Fund Management Co., Ltd, Great Wall Fund Management Co., Ltd	Site visit. Introduce the client firm situations. No written materials.
2009/11/16	Client firm Headquarters	Site visit	Industrial Securities	Site visit. Introduce the client firm situations. No written materials.
2010/07/05	Client firm Headquarters	Site visit	Citigroup Global Markets Asia Limited	Learn about the client firm's manufacturing process and production and operational situations. No written materials.
2010/08/03	Client firm Headquarters	Site visit	China Life Asset Management Co.,Ltd, Citic Securities Co.,Ltd, China Asset Management Co.,Ltd, etc.	Learn about the client firm's manufacturing process and production and operational situations. No written materials.
2011/05/25	Client firm Headquarters	Site visit	Cinda Securities Co.,Ltd	The situation regarding the project construction of Phase One and Phase Two of chemical engineering and the production of potash fertilizer business. No written materials.
2011/05/26	Client firm Headquarters	Site visit	Yinhua Fund Management Co.,Ltd, China International Fund Management Co., Ltd, the Goldman Sachs Securities, etc.	The situation regarding the project construction of Phase One and Phase Two of chemical engineering and the production of potash fertilizer business. No written materials.
2011/05/31	Client firm Headquarters	Site visit	HengTai Securities	The situation regarding the project construction of Phase One and Phase Two of chemical engineering and the production of potash fertilizer business. No written materials.

Panel B: Examples of corporate site visits from the annual report of Qinghai SALT LAKE POTASH Co., Ltd. (stock code 000792.SZ) in the post-period (2013-2015)

Time	Location	Visit Type	Visitor Type	Visitor	Discussion
2013/08/30	Client firm Production factories	Site visit	Institutions	Six Institutions including Shanghai Shenyin Wanguo Securities	Please refer to the investor relationship activities files on the SZSE's "Hu Dong Yi" platform.
2014/07/31	Client firm Production factories	Site visit	Institutions	Southwest Securities, ChangJiang Securities, China Galaxy Securities, etc.	Please refer to the investor relationship activities files on the SZSE's "Hu Dong Yi" platform.
2015/09/25	Client firm Headquarters	Site visit	Institutions	Citic Securities, Standard Life Assurance	Please refer to the investor relationship activities files on the SZSE's "Hu Dong Yi" platform.

Panel C: Information disclosure of corporate site visit on the SZSE's "Hu Dong Yi" platform in the post-period (2013-2015)

Category of investor relationship activities	☐Specific target visits ☐Analyst meeting ☐Media interview ☐Performance presentation ☐Press conference ☐Road show activity √ Site visit ☐Other
Names of the visitors	Shanghai Shenyin Wanguo Securities, Analyst, Peipei Ye and Assistant Analyst, Dong Wang. Changsheng Fund Management Co.,Ltd, Researcher, Bin Cai. Fullgoal Fund Management Co.,Ltd, Researcher, Lin Luo. Franklin Templeton Sealand Fund Management Co.,Ltd, investment manager, Feng Yang. PICC Asset Management Company Limited, Researcher, Bin Zhang. Nomura Asset Management Hong Kong Limited Shanghai Representative Office, Analyst, Ogura Benso.
Time	August 30, 2013
Place	Client firm Production factories
Names of client firm receptionist	Securities affairs representative Yong Li
Major Content of investor relationship activities	**1. What is the company's current production capacity of potassium fertilizer?** A: The company's current production capacity is 2.5 million tons per year. With the addition of a new 1 million tons/year potassium chloride project, the company's production capacity will reach 3.5 million tons per year. **2. Will the newly added 1 million tons/year potassium chloride project of the company be put into operation as scheduled?** A: Overall, the newly added 1 million tons/year potassium chloride project, with a total investment of CNY 2.72423 billion, is currently progressing smoothly. The construction of five project units, including mining ships, processing plants, public works, railway platforms, brine mining, and transportation, is being implemented according to the established objectives. The project is expected to undergo trial operation in October. **3. What was the reason for the decrease in the company's administrative expenses compared to the previous year in 2012?** A: The primary reason was the transformation of the company's subsidiary, which was engaged in potassium fertilizer development, into branch offices following the "Two-Lake Merger". Consequently, the managerial personnel expenses of this subsidiary were computed in the costs, and changes in accounting practices led to a decrease in administrative expenses. **4. What is the transportation cost per ton of the company's potassium fertilizer?** A: The transportation cost is included in the company's quoted price for potassium fertilizer. Currently, the railway transportation cost per ton is around CNY 240. This price enjoys preferential rates provided by the Chinese government for the transportation of agricultural materials. **5. What is the normal storage capacity of the company's potassium fertilizer?** A: In previous years, the typical storage capacity was around 1 million tons. Currently, the inventory level is somewhat lower.

	6. What are the reasons for reducing the company's potassium fertilizer inventory? A: Last year, the company sold over 2.8 million tons of potassium fertilizer, while the year before, sales amounted to over 2.4 million tons. The inventory reduction is due to sales volumes exceeding production levels. **7. What is the relationship between domestic and international potassium fertilizer prices?** A: There is a high correlation between domestic and international potassium fertilizer prices. Domestic potassium fertilizer prices are largely based on import prices of potassium fertilizer. **8. What is the production cost of the company's potassium fertilizer?** A: The company currently employs both direct flotation cold crystallization technology and reverse flotation cold crystallization technology for potassium fertilizer production. Currently, the production capacity of potassium fertilizer using direct flotation cold crystallization technology exceeds 600,000 tons, while the remainder is produced using reverse flotation cold crystallization technology. The production cost of potassium fertilizer using direct flotation cold crystallization technology is higher than that using reverse flotation cold crystallization technology. The production cost of the company's potassium fertilizer has been disclosed in the company's 2013 interim report. **9. Does the company intend to acquire equity in foreign potassium fertilizer production enterprises?** A: Currently, the company has not considered such an option. The company's current primary focus is the construction of the Salt Lake comprehensive utilization project. **10. Will the company's comprehensive utilization project achieve its designed capacity for Phase One and Phase Two this year?** A: By the end of last year, all process flows of Phase One of the comprehensive utilization project had been successfully established, producing qualified products such as acetylene gas, oxygen, nitrogen, potassium hydroxide, potassium carbonate, methanol, VCM, PVC, synthetic ammonia, and urea. Following equipment maintenance in June of the first half of this year, each unit's operation is relatively stable. For Phase two of the comprehensive utilization project, the pipeline purging work for each device has been completed, and some devices have entered into coordinated trial operations. The plan is to complete this year's overall process flow, fully preparing for gradual, continuous trial operation next year. **11. Given the production capacity of some products produced in Phase One and Phase Two of the comprehensive utilization projects is already saturated or even surplus in China, what other advantages does the company have in terms of product manufacturing?** A: The company's construction of Phase One and Phase Two of the comprehensive utilization projects fully capitalizes on the resource and energy advantages in the Qaidam Basin and facilitates the comprehensive development of Salt Lake resources. It directly utilizes potassium chloride or its by-products in terms of raw materials, engages in closed-loop production processes, and establishes a circular

	industrial chain to promote resource recycling. Its advantage lies in the raw materials for product production. Currently, some products produced in Phase One of the company's comprehensive utilization projects face relative oversupply in the domestic market. In addition, due to the price increases in certain raw materials used in product production, the cost advantage of the products is diminishing. **12. What are the funding sources, design capacity, and current situations of the company's Phase Three of the comprehensive utilization project (metal magnesium integration project)?** A: The total investment in the company's metal magnesium integration project is CNY 27.9 billion. The company has self-raised funds of approximately CNY 8.4 billion, with the remaining funds obtained through bank loans. Currently, the funding has been resolved. The design capacity of the metal magnesium integration project includes 100,000 tons/year of metal magnesium, 1 million tons/year of methanol, 1 million tons/year of MTO olefins production, 800,000 tons/year of PVC, 160,000 tons/year of polypropylene, 2.4 million tons/year of coke, 800,000 tons/year of calcium carbide, 1.2 million tons/year of soda ash, and supporting 2400T/h+320MW cogeneration projects. Currently, the metal magnesium integration project is progressing according to the planned schedule, with the completion of each device expected by the end of 2014. **13. Does the company currently have any plans for equity financing?** A: Currently, the company has no plans for equity financing. **14. How are the sales situations of chemical products produced in the company's comprehensive utilization project?** A: The sales situations of products such as potassium carbonate PVC and methanol, which were trial-produced in Phase One of the company's comprehensive utilization project, are relatively good. However, due to factors such as production volume, the production costs of these products remain high. **15. What is the situation of the company's lithium carbonate production? What is the market price of lithium carbonate?** A: The company's lithium carbonate project, with an annual production of 10,000 tons, completed the process technology transformation at the end of last year, establishing the process flow for extracting lithium carbonate from brine using the adsorption method. Due to the seasonal influence of winter, the production volume in the first half of the year was not good, producing 212 tons, but the concentration has now reached the industrial standard. Similar to potash fertilizer, lithium carbonate production is also subject to seasonal influence. The main reason for the low production volume is that the concentration of lithium ions in the brine of the salt field has not yet formed a gradient and is low. As exposure to sunshine increases, a concentration gradient of brine will gradually form, which will gradually increase the production volume. Currently, the price of lithium carbonate produced by the company is around CNY 38,000 per ton.
Attachment List (if any)	None
Date	August 30, 2013

Appendix B
Variable definitions

Panel A: Variable definitions for the propensity score matching analyses

Variables	Definitions
Dependent variable	
Treat	An indicator variable indicating treated client firms.
Independent variables	
ROA	The ratio of net income to total assets.
M.B.	Market-to-book ratio.
Stock Return	Annual market-adjusted stock return.
ANALYST	The natural logarithm of (one plus) the number of analysts that issue forecasts for the client firm.
INST	The ownership of institutional investors.
Size	The natural logarithm of client firm's total assets.
MSHARE	The proportion of a client firm' sales in the sum of sales of all listed client firms that belong to the same CSRC industry code.
Leverage	The ratio of total debt to total asset.
SOE	An indicator variable that equals 1 if the client firms are SOEs, and 0 otherwise.
ΔGDP	The city's GDP in year *t* divided by the GDP in year *t-1*, minus 1.
NUM_Firms	The number of publicly listed client firms in the city in which the client firm is located.

Panel B: Variables definitions for the main analyses

Variables	Definitions
Dependent variables	
ADJMAG	The absolute magnitude of the adjustment to earnings scaled by the absolute value of pre-audit earnings (i.e., |Audited earnings – Pre-audit earnings|/|Pre-audit earnings|).
ADJDUM	An indicator variable that equals 1 if the earnings are adjusted by auditors, and 0 otherwise.
ADJMAG1	The absolute magnitude of the adjustment to earnings scaled by total assets, multiplied by 100 (i.e., |Audited earnings – Pre-audit earnings|/|Assets| × 100).
Rank (*ADJMAG*)	The ranked value of *ADJMAG*. Specifically, when *ADJMAG* has N non-zero observations in year t, we first replace each observation with its corresponding rank (from j = 1, ..., N in ascending order) and the rank j assigned to each observation is then divided by N+1. When *ADJMAG* has zero-value observations, we code *Rank* (*ADJMAG*) as 0.
ADJ_ UP	*ADJ_UP* equals (|Audited earnings – Pre-audit earnings|)/|Pre-audit earnings | if Audited earnings > Pre-audit earnings, and 0 otherwise.
ADJ_ DOWN	*ADJ_DOWN* equals (|Audited earnings – Pre-audit earnings|)/|Pre-audit earnings| if Audited earnings < Pre-audit earnings, and 0 otherwise.
Pre-ABSDA	Discretionary accruals computed using the modified Jones model following Dechow et al. (1995). Pre-audit earnings are used in the model.
Audited- ABSDA	Discretionary accruals computed using the modified Jones model following Dechow et al. (1995). Audited earnings are used in the model.
RESTATE	An indicator variable that equals 1 if the client's financial statement is restated in a subsequent year due to accounting irregularities, and 0 otherwise.
FERROR	The mean value of forecast error among all analysts covering the client firm in the year. Forecast error is measured as the absolute difference between an analyst forecast for client firm in the year and client firm's actual earnings per share in the year, scaled by the client firm's stock price at the beginning of the year.
FDISP	The standard deviation of forecast error among all analysts covering the client firm in the year. Forecast error is measured as the absolute difference between an analyst forecast for client firm in the year and client firm's actual earnings per share in the year, scaled by the client firm's stock price at the beginning of the year.
Independent variables of interest	
Treat	An indicator variable indicating treated client firms.
Post	An indicator variable that equals 1 for the period after the SZSE 2012 Disclosure Regulation (2013–2015), and 0 for the period from 2009 to 2011.
Treat {Frequency}	The natural logarithm of (one plus) the sum of the frequency of intermediary corporate site visits during 2013-2015.
Treat_Spillover	An indicator variable indicating a client firm that may receive information spillovers from a treated client firm's

	intermediary corporate site visit. These client firms are the peer client firms of a treated client firm, audited either by the affected auditor or her peer auditors in the same audit firm.
Control variables	
Size	The natural logarithm of client firm's total assets.
ROA	The ratio of net income to total assets.
Leverage	The ratio of total debt to total asset.
Age	The natural logarithm of the client firm's age, which is the number of years since the client firm's establishment.
M.B.	Market-to-book ratio.
M&A	An indicator variable that equals 1 if the client firm has a merger or acquisition transaction that involves the purchase of more than 20 percent of the target client firm, and 0 otherwise.
Subsidiaries	The natural logarithm of (one plus) the number of subsidiaries.
SOE	An indicator variable that equals 1 if the client firms are SOEs, and 0 otherwise.
BD_Size	The natural logarithm of the number of client firm's board directors.
BD_Meetings	The natural logarithm of the number of board meetings held by the client firm.
INDIR	The proportion of independent directors on the board.
Duality	An indicator variable that equals 1 if client firm's CEO concurrently serves as chair of the board, and 0 otherwise.
Audit Com	An indicator variable that equals 1 if the client firm has an audit committee, and 0 otherwise.
ANALYST	The natural logarithm of (one plus) the number of analysts that issue forecasts for the client firm.
Big10	An indicator variable that equals 1 if the client firm is audited by a Big 10 audit firm according to the annual reports of Comprehensive Evaluations of Top 100 Chinese Audit Firms by CICPA, and 0 otherwise.
Tenure	The natural logarithm of audit firm tenure at the client firm.
Spec	An indicator variable that equals 1 if an auditor's sum of the total audit fees of all clients is in the highest year-industry quartile, and 0 otherwise.
Cross-sectional variables	
ASSET TANGIBILITY	The ratio of PP&E (property, plant and equipment) over total assets.
ANALYST	The natural logarithm of (one plus) the number of analysts that issue forecasts for the client firm.
SYN	Stock price synchronicity computed following Morck et al. (2000).
PROTECT	The law index of a province in which the client firm is located (Wang et al. 2008).
SOE	An indicator variable that equals 1 if the client firms are SOEs, and 0 otherwise.
COMMENT	The number of investor comments in the East Money stock forum.
NONRETIREMENT	An indicator variable that equals 1 if auditor age is below 50, and 0 otherwise.
OVERSEAS LISTING	An indicator variable that equals 1 if the client firm cross-

	lists the stock on international exchanges, and 0 otherwise.
Variables used in additional analyses	
Web Relation	An indicator variable that equals 1 if the client firm's website has an investor relations column, and 0 otherwise.
Web Consultant	An indicator variable that equals 1 if the client firm's website has an investor consulting column, and 0 otherwise.
Weibo&WeChat	An indicator variable that equals 1 if the client firm has had official *Weibo* or *WeChat* accounts, and 0 otherwise.
InvRec	The sum of total inventory and accounts receivables divided by total assets.
Current	Current assets divided by current liabilities.
Cash	Cash and cash equivalents divided by total assets.
Loss	An indicator variable that equals 1 if net income is negative, and 0 otherwise.
Top1	The percentage of ownership held by the largest shareholder.
Stock Return	Annual market-adjusted stock return.
Return Vol	The standard deviation of daily stock returns in the year.
INST	The ownership of institutional investors.
ForHorizon	The natural logarithm of the average number of days between the analyst's forecast date and the announcement date of the client firm's financial report.

Appendix C
The procedure to develop propensity-score-matched samples

Panel A: Logit model used to find propensity scores

Dependent Variable	*Treat*
	(1)
ROA	2.214*
	(1.71)
M.B.	-0.066**
	(-2.02)
Stock Return	-0.428
	(-1.19)
ANALYST	0.465***
	(6.10)
INST	-0.005
	(-1.57)
Size	-0.375***
	(-5.20)
MSHARE	-2.474
	(-0.82)
Leverage	-0.647*
	(-1.96)
SOE	-0.808***
	(-6.01)
ΔGDP	2.491**
	(2.08)
NUM_Firms	0.000
	(0.11)
Observations	1,408
Pseudo R^2	0.109

Panel B: Test of the effectiveness of the propensity score matches

	Treatment client firms	Full control client firms	Matched control client firms		
	(1) Mean	(2) Mean	(3) Mean	(1)−(2) Diff.	(1)−(3) Diff.
ROA	0.104	0.032	0.041	0.072**	0.063
M.B.	3.211	3.916	2.239	-0.705	0.972
Stock Return	-0.305	-0.283	-0.302	-0.022*	-0.003
ANALYST	1.766	1.487	1.649	0.279***	0.117
INST	47.488	51.985	48.384	-4.497***	-0.896
Size	21.694	22.133	21.789	-0.439***	-0.095
MSHARE	0.007	0.012	0.009	-0.005**	-0.002
Leverage	0.496	0.565	0.547	-0.069**	-0.051
SOE	0.389	0.649	0.439	-0.260***	-0.050
ΔGDP	0.196	0.201	0.193	-0.005	0.003
NUM_Firms	56.164	62.902	52.430	-6.738	3.734

Appendix D
Additional analyses

Table AD1
Robustness of two-way fixed effects DiD estimates

Dependent variable	*ADJMAG*				
		Treatment Event: 2013	Treatment Event: 2014	Treatment Event: 2015	Treatment Events: 2013 and 2014
	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.069***	0.054**	0.053**	0.061**	0.052**
	(3.01)	(2.17)	(2.02)	(2.24)	(2.52)
Size		-0.038**	-0.036**	-0.038**	-0.038***
		(-2.48)	(-2.26)	(-2.44)	(-2.76)
ROA		-0.367**	-0.510***	-0.297*	-0.445***
		(-2.45)	(-3.22)	(-1.70)	(-3.20)
Leverage		0.131**	0.027	0.119	0.070
		(2.03)	(0.42)	(1.59)	(1.08)
Age		0.180*	0.090	0.042	0.107*
		(1.90)	(1.50)	(0.69)	(1.79)
M.B.		-0.002	0.001	-0.000	0.000
		(-0.41)	(0.16)	(-0.09)	(0.09)
M&A		-0.023**	-0.019	-0.019	-0.016
		(-2.07)	(-1.33)	(-1.62)	(-1.33)
Subsidiaries		-0.003	0.008	0.004	0.007
		(-0.47)	(1.17)	(0.56)	(1.00)
SOE		0.007	-0.006	0.020	0.001
		(0.25)	(-0.14)	(0.73)	(0.05)
BD_Size		-0.008	-0.107*	-0.057	-0.069
		(-0.17)	(-1.91)	(-1.11)	(-1.48)
BD Meetings		0.036**	0.027	0.036**	0.023
		(2.35)	(1.51)	(2.21)	(1.48)
INDIR		0.026	0.123	0.045	0.112
		(0.18)	(0.61)	(0.27)	(0.70)
Duality		-0.007	-0.012	-0.037*	-0.011
		(-0.40)	(-0.85)	(-1.72)	(-0.85)
Audit Com		-0.002	-0.011	-0.022	0.001
		(-0.11)	(-0.86)	(-0.90)	(0.07)
ANALYST		0.011	0.016*	-0.010	0.018**
		(1.26)	(1.77)	(-0.94)	(2.22)
Big10		-0.015	0.003	0.009	0.007
		(-0.68)	(0.11)	(0.42)	(0.35)
Tenure		-0.004	-0.005	-0.010	-0.006
		(-0.40)	(-0.46)	(-0.82)	(-0.66)
Spec		0.029	0.027	0.025	0.019
		(0.83)	(0.44)	(0.18)	(0.47)
Client Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations		2,946	3,005	3,014	3,614
Adjusted R^2		0.138	0.105	0.092	0.116

This table presents the robustness of our main results (as in Table 4 in the main text) addressing the possible drawbacks of two-way fixed effects specifications. The dependent variable is *ADJMAG*. *Treat × Post* is a binary variable that equals 1 for treatment client firms after adopting the 2012 disclosure regulation (i.e., 2013–2015). Column (1) estimates Eq. (1) using the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) to address the issues of treatment effect heterogeneity and negative weights that may bias the DiD estimates. Columns (2), (3), and (4) estimate the effect of site visits on audit adjustments in a stepwise manner (i.e., we separately examine the treatment effect of each treatment event). Column (5) expands the analysis to cover the 2009–2011 and 2013-2014 period. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table AD2
Robustness: Alternative identification strategies

Dependent variable	*ADJMAG*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.056***	0.068***	0.055***	0.057***	0.091***	0.058***
	(2.94)	(3.40)	(2.78)	(2.96)	(2.83)	(4.06)
Size	-0.039***	-0.042***	-0.041***	-0.032*	-0.009	-0.040***
	(-3.10)	(-2.91)	(-2.95)	(-1.91)	(-0.68)	(-4.39)
ROA	-0.361**	-0.314**	-0.363**	-0.357**	-0.438**	-0.355**
	(-2.55)	(-2.22)	(-2.43)	(-2.51)	(-2.53)	(-2.83)
Leverage	0.091	0.095	0.121*	0.096	0.004	0.097
	(1.53)	(1.45)	(1.88)	(1.52)	(0.06)	(1.75)
Age	0.060	0.079	0.017	0.062	0.044	0.061
	(1.12)	(1.50)	(0.32)	(1.17)	(0.53)	(1.05)
M.B.	-0.000	-0.000	0.000	0.001	0.000	0.000
	(-0.02)	(-0.12)	(0.02)	(0.32)	(0.04)	(0.03)
M&A	-0.013	-0.010	-0.013	-0.012	-0.020*	-0.012
	(-1.16)	(-0.85)	(-1.12)	(-1.09)	(-1.68)	(-1.19)
Subsidiaries	0.009	0.009	0.009	0.008	-0.006	0.008
	(1.24)	(1.19)	(1.17)	(1.07)	(-0.45)	(1.02)
SOE	0.004	0.021	0.008	0.015	-0.009	0.017
	(0.17)	(0.80)	(0.26)	(0.61)	(-0.38)	(0.70)
BD_Size	-0.049	-0.029	-0.023	-0.044	-0.060	-0.043
	(-1.06)	(-0.61)	(-0.51)	(-0.98)	(-1.25)	(-0.87)
BD_Meetings	0.022	0.024	0.022	0.022	0.010	0.023
	(1.47)	(1.62)	(1.49)	(1.54)	(0.58)	(1.71)
INDIR	0.137	0.161	0.236	0.176	0.123	0.161
	(0.94)	(1.07)	(1.56)	(1.21)	(0.76)	(1.20)
Duality	-0.027*	-0.028*	-0.016	-0.022	-0.026*	-0.022
	(-1.85)	(-1.75)	(-0.96)	(-1.47)	(-1.82)	(-1.77)
Audit Com	0.018	-0.009	0.009	0.010	0.090**	0.012
	(0.84)	(-0.42)	(0.24)	(0.49)	(2.45)	(0.36)
ANALYST	0.006	0.002	0.008	0.007	-0.004	0.006
	(0.88)	(0.29)	(1.06)	(0.96)	(-0.39)	(0.58)
Big10	0.014	0.016	–	0.013	0.010	0.014
	(0.84)	(0.92)	–	(0.82)	(0.48)	(0.78)
Tenure	-0.007	-0.013	-0.006	-0.008	-0.006	-0.009
	(-0.81)	(-1.35)	(-0.54)	(-0.91)	(-0.59)	(-0.85)
Spec	0.026	0.013	0.001	0.017	0.095*	0.018
	(0.47)	(0.21)	(0.02)	(0.31)	(1.81)	(0.43)
Client Firm FE	YES	YES	YES	YES	YES	YES
Year FE	NO	NO	NO	NO	YES	YES
Industry × Year FE	YES	NO	NO	NO	NO	NO
Province × Year FE	NO	YES	NO	NO	NO	NO
Audit Firm × Year FE	NO	NO	YES	NO	NO	NO
Size × Year FE	NO	NO	NO	YES	NO	NO
Treat × Controls	NO	NO	NO	NO	YES	NO
Observations	4,291	4,291	4,291	4,291	4,291	4,291
Adjusted R^2	0.117	0.125	0.117	0.124	0.126	0.125

This table presents the robustness of our main results (as in Table 4 in the main text) using alternative specification strategies. The dependent variable is *ADJMAG*. *Treat × Post* is a binary variable that equals 1 for treatment client firms after adopting the 2012 disclosure regulation (i.e., 2013–2015). Columns (1), (2), (3), and (4) report results controlling for client firm and industry × year fixed effects, client firm and province-of-headquarters × year fixed effects, client firm and audit firm × year fixed effects, and client firm and terciles of client firm size × year fixed effects, respectively. Column (5) controls for the interactions of *Treat* and all control variables. Column (6) double cluster standard errors by client firm and by year. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table AD3
Robustness on if the baseline result is spurious

Dependent variable	*ADJMAG*				
	(1)	(2)	(3)	(4)	(5)
Treat_ SMALL_ Audit Firm × Post ($\beta 1$)	0.077***				
	(3.22)				
Treat_ BIG_ Audit Firm × Post ($\beta 2$)	0.058**				
	(2.44)				
Treat_ SMALL_ Client Firm × Post ($\beta 1$)		0.047*			
		(1.86)			
Treat_ BIG_ Client Firm × Post ($\beta 2$)		0.068***			
		(3.02)			
Treat × Post			0.062***	0.062**	0.074**
			(3.30)	(2.25)	(2.09)
Size	-0.040***	-0.039***	-0.041***	-0.023*	-0.038**
	(-3.07)	(-3.07)	(-3.15)	(-1.86)	(-2.09)
ROA	-0.351**	-0.349**	-0.351**	-0.357**	-0.179
	(-2.48)	(-2.45)	(-2.50)	(-2.28)	(-0.98)
Leverage	0.095	0.097	0.098	0.073	0.135
	(1.53)	(1.56)	(1.58)	(1.12)	(1.55)
Age	0.060	0.068	0.071	0.063	0.122
	(1.20)	(1.30)	(1.40)	(1.05)	(1.62)
M.B.	-0.000	0.000	-0.000	0.002	0.002
	(-0.01)	(0.09)	(-0.07)	(0.43)	(0.40)
M&A	-0.012	-0.012	-0.012	-0.021**	-0.015
	(-1.12)	(-1.12)	(-1.13)	(-2.08)	(-1.32)
Subsidiaries	0.008	0.008	0.008	0.003	0.003
	(1.06)	(1.11)	(1.14)	(0.48)	(0.37)
SOE	0.018	0.016	0.013	0.009	0.066
	(0.70)	(0.64)	(0.52)	(0.34)	(1.49)
BD_Size	-0.043	-0.042	-0.042	-0.060	-0.067
	(-0.97)	(-0.95)	(-0.96)	(-1.49)	(-1.41)
BD_Meetings	0.023	0.023	0.023	0.023	0.016
	(1.58)	(1.59)	(1.55)	(1.61)	(0.89)
INDIR	0.157	0.168	0.159	0.125	0.337
	(1.07)	(1.16)	(1.09)	(0.89)	(1.50)
Duality	-0.021	-0.022	-0.024	-0.026	-0.009
	(-1.40)	(-1.45)	(-1.56)	(-1.60)	(-0.35)
Audit Com	0.009	0.011	0.016	0.047**	-0.014
	(0.44)	(0.57)	(0.74)	(2.40)	(-0.77)
ANALYST	0.006	0.006	0.006	-0.007	0.005
	(0.83)	(0.89)	(0.88)	(-0.92)	(0.44)
Big10	0.012	0.014	0.013	0.024	-0.022
	(0.69)	(0.86)	(0.82)	(1.22)	(-0.91)
Tenure	-0.008	-0.008	-0.008	-0.004	-0.006
	(-0.92)	(-0.92)	(-0.91)	(-0.36)	(-0.50)
Spec	0.018	0.020	0.019	0.015	0.048
	(0.31)	(0.35)	(0.33)	(0.25)	(0.72)
Web Relation			-0.005		
			(-0.26)		
Web Consultant			-0.018		
			(-0.62)		
Weibo&WeChat			-0.026		
			(-1.13)		
Client Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	4,291	4,291	4,291	3,596	2,705
Adjusted R^2	0.127	0.126	0.126	0.127	0.123
Wald Test β_1- β_2	0.019	-0.021			
(p-value)	(0.506)	(0.458)			

This table presents the robustness checks on if the baseline result (as in Table 4 in the main text) is spurious. The dependent variable is *ADJMAG*. Column (1) classifies treatment client firms into two groups based on their audit firm size in 2011. Column (2) classifies treatment client firms into two groups

based on their total assets in 2011. Column (3) controls for other social media channels through which auditors can obtain similar information. Column (4) reports the analyses excluding the subsample of auditors who are located in the same city as visiting intermediaries. Column (5) reports the analyses excluding the subsample of industries in which the fraction of client firms with corporate site visits is above the median fraction. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table AD4
Robustness: Using alternative estimation methodology and measures of key variables

Dependent variable	*ADJMAG*	*ADJDUM*	*ADJMAG1*	*Rank* (*ADJMAG*)	*ADJMAG*	*ADJ_UP*	*ADJ_DOWN*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × Post	0.092***	0.064**	0.112**	0.077***		0.049*	0.045***
	(3.20)	(2.09)	(2.07)	(3.41)		(1.80)	(2.69)
Treat {Frequency} × Post					0.023**		
					(2.23)		
Size	-0.068***	-0.046*	-0.148***	-0.035**	-0.039***	-0.022	-0.023**
	(-3.19)	(-1.76)	(-3.52)	(-2.14)	(-3.08)	(-1.58)	(-1.99)
ROA	-0.539***	-0.344*	-1.070**	-0.677***	-0.358**	-0.036	-0.589***
	(-2.67)	(-1.90)	(-2.26)	(-5.28)	(-2.52)	(-0.30)	(-4.84)
Leverage	0.131	-0.013	0.243	0.052	0.097	-0.023	0.029
	(1.48)	(-0.15)	(1.39)	(0.87)	(1.55)	(-0.33)	(0.54)
Age	0.089	0.087	0.175	0.168**	0.062	0.075	0.054
	(1.15)	(0.88)	(1.11)	(2.34)	(1.20)	(0.84)	(1.03)
M.B.	-0.002	-0.004	0.013	-0.001	0.000	-0.003	0.001
	(-0.29)	(-0.57)	(0.80)	(-0.30)	(0.03)	(-1.07)	(0.18)
M&A	-0.026	-0.036*	-0.029	-0.013	-0.012	-0.028	-0.001
	(-1.62)	(-1.94)	(-0.92)	(-0.92)	(-1.08)	(-1.59)	(-0.10)
Subsidiaries	0.011	0.006	0.037	0.016	0.009	0.004	0.008
	(1.04)	(0.42)	(1.53)	(1.59)	(1.24)	(0.43)	(1.15)
SOE	0.031	0.003	-0.096	-0.029	0.014	0.006	0.014
	(0.69)	(0.04)	(-0.84)	(-0.60)	(0.57)	(0.20)	(0.53)
BD_Size	-0.039	0.049	-0.004	0.031	-0.047	0.038	-0.078*
	(-0.61)	(0.59)	(-0.03)	(0.56)	(-1.06)	(0.84)	(-1.77)
BD_Meetings	0.032	0.009	0.097**	0.029	0.023	0.049**	-0.009
	(1.52)	(0.38)	(2.15)	(1.59)	(1.57)	(2.24)	(-0.66)
INDIR	0.254	0.204	0.173	0.259	0.163	-0.013	0.186
	(1.21)	(0.93)	(0.39)	(1.48)	(1.12)	(-0.06)	(1.34)
Duality	-0.038*	-0.047*	-0.073	-0.033*	-0.022	-0.038	-0.013
	(-1.73)	(-1.83)	(-1.37)	(-1.87)	(-1.47)	(-1.64)	(-0.85)
Audit Com	0.013	0.016	0.016	0.011	0.011	0.006	0.046
	(0.33)	(0.18)	(0.14)	(0.20)	(0.52)	(0.39)	(1.30)
ANALYST	0.019*	0.034***	0.059**	0.004	0.006	0.012	0.002
	(1.81)	(2.86)	(2.49)	(0.46)	(0.80)	(1.38)	(0.24)
Big10	0.009	-0.018	0.032	-0.021	0.015	0.035	-0.000
	(0.37)	(-0.65)	(0.65)	(-1.07)	(0.92)	(1.49)	(-0.03)
Tenure	-0.005	0.014	-0.007	-0.001	-0.007	-0.003	-0.008
	(-0.42)	(0.98)	(-0.26)	(-0.14)	(-0.80)	(-0.28)	(-0.88)
Spec	0.028	-0.003	0.017	0.000	0.018	0.018	-0.066
	(0.31)	(-0.03)	(0.09)	(0.01)	(0.31)	(0.25)	(-1.43)
Client Firm FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Observations	4,291	4,291	4,291	4,291	4,291	2,332	3,421
Pseudo/Adjusted R^2	0.531	0.379	0.168	0.287	0.124	0.225	0.124
Diff. between *Treat × Post* (p-value)							0.004 (0.913)

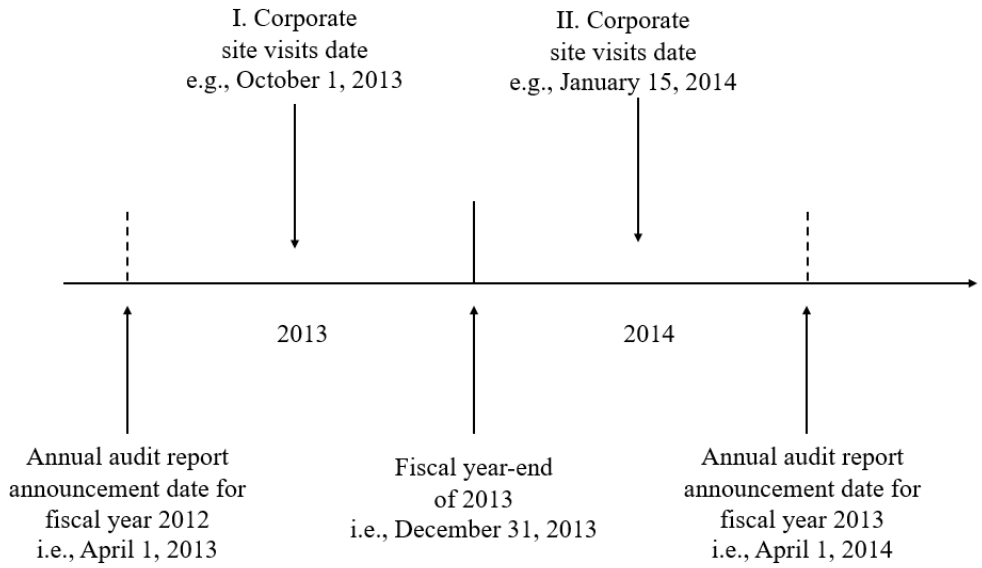
This table presents the robustness of our main results (as in Table 4 in the main text) using alternative estimation methodology and measures of key variables. Column (1) presents the analysis for a Tobit model as *ADJMAG* is truncated at 0. Columns (2), (3), and (4) display results using *ADJDUM*, *ADJMAG1,* and *Rank* (*ADJMAG*) as the dependent variable, respectively. Column (5) displays results using a continuous measure of treatment intensity (i.e., the frequency of corporate site visits). *Treat {Frequency}* is the natural logarithm of one plus the sum of the frequency of corporate site visits during 2013-2015. Columns (6) and (7) test the impact of corporate site visits on upward and downward audits adjustments. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table AD5
Robustness: Excluding the spillover-receiving client firms

Dependent variable	*ADJMAG*
	(1)
Treat × Post	0.095***
	(3.76)
Size	-0.026
	(-1.46)
ROA	-0.097
	(-0.54)
Leverage	0.140*
	(1.66)
Age	0.036
	(0.48)
M.B.	-0.002
	(-0.44)
M&A	-0.015
	(-1.05)
Subsidiaries	-0.016
	(-1.53)
SOE	-0.041
	(-0.72)
BD_Size	-0.010
	(-0.16)
BD_Meetings	0.002
	(0.11)
INDIR	0.239
	(1.24)
Duality	-0.001
	(-0.02)
Audit Com	-0.003
	(-0.21)
ANALYST	0.009
	(0.90)
Big10	0.011
	(0.44)
Tenure	-0.002
	(-0.18)
Spec	0.077
	(1.21)
Client Firm FE	YES
Year FE	YES
Observations	2,612
Adjusted R^2	0.085

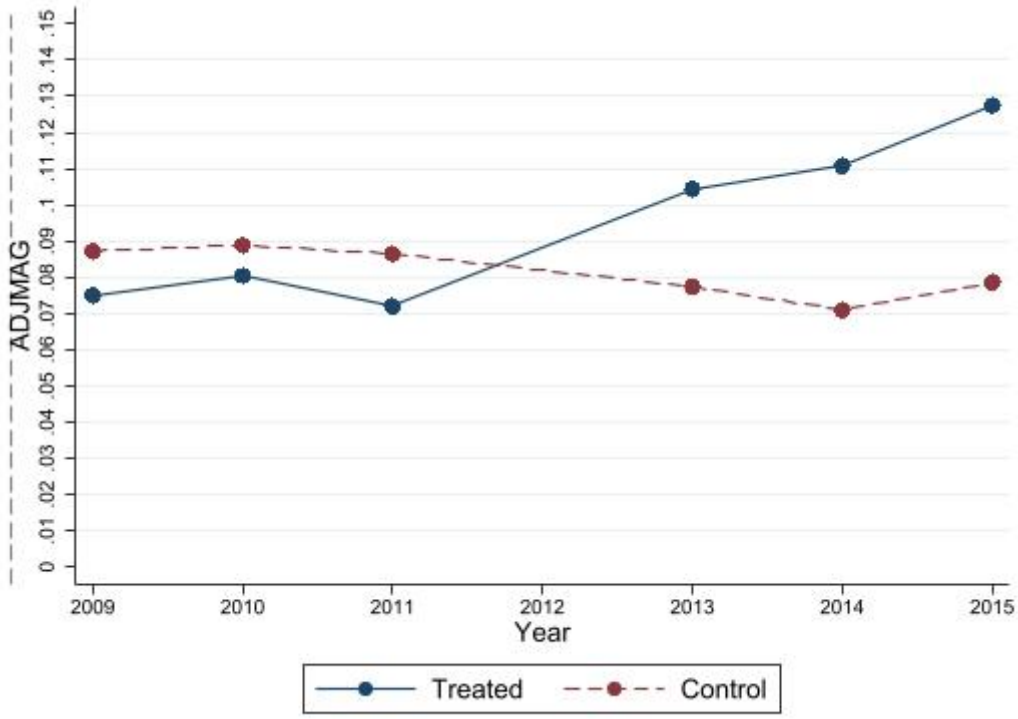
This table presents the robustness of our main results (as in Table 4 in the main text) excluding the spillover-receiving client firms. The dependent variable is *ADJMAG*. *Treat × Post* is a binary variable that equals 1 for treatment client firms after adopting the 2012 disclosure regulation (i.e., 2013–2015). All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively, based on two-tailed statistical tests. All variables are defined in Appendix B.

Figure 1
Timeline of corporate site visits and audit behavior



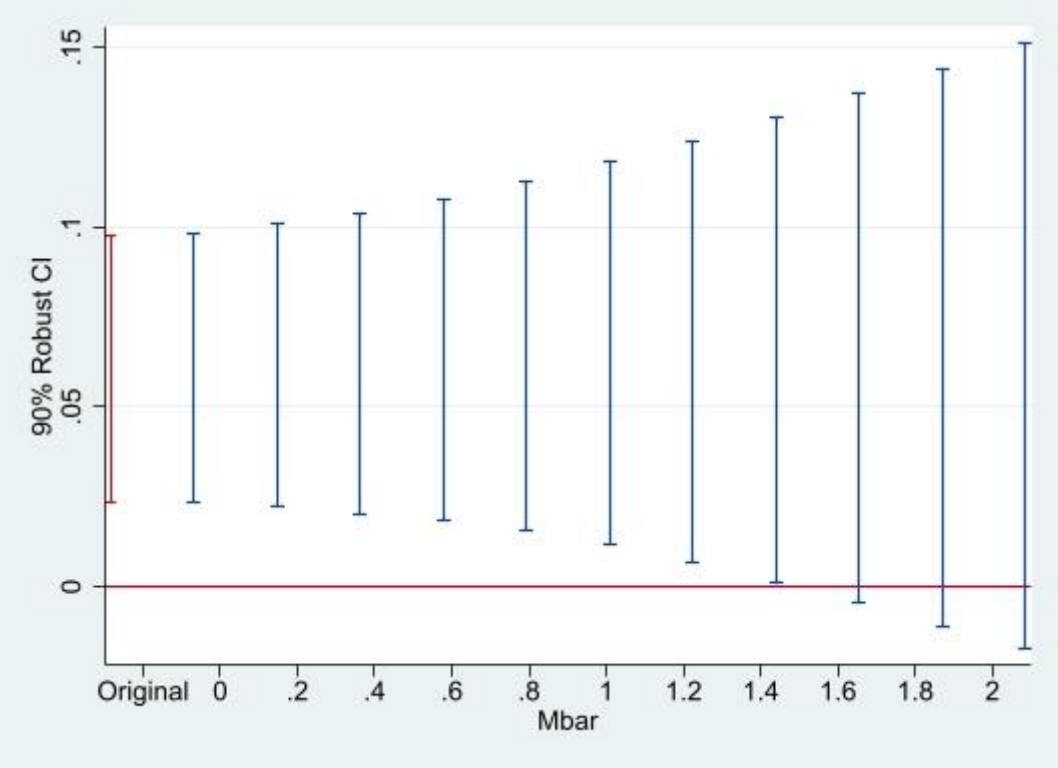
This figure plots the timeline we adopted for the synchronization of corporate site visits and audit behavior.

Figure 2
Yearly changes in audit adjustments



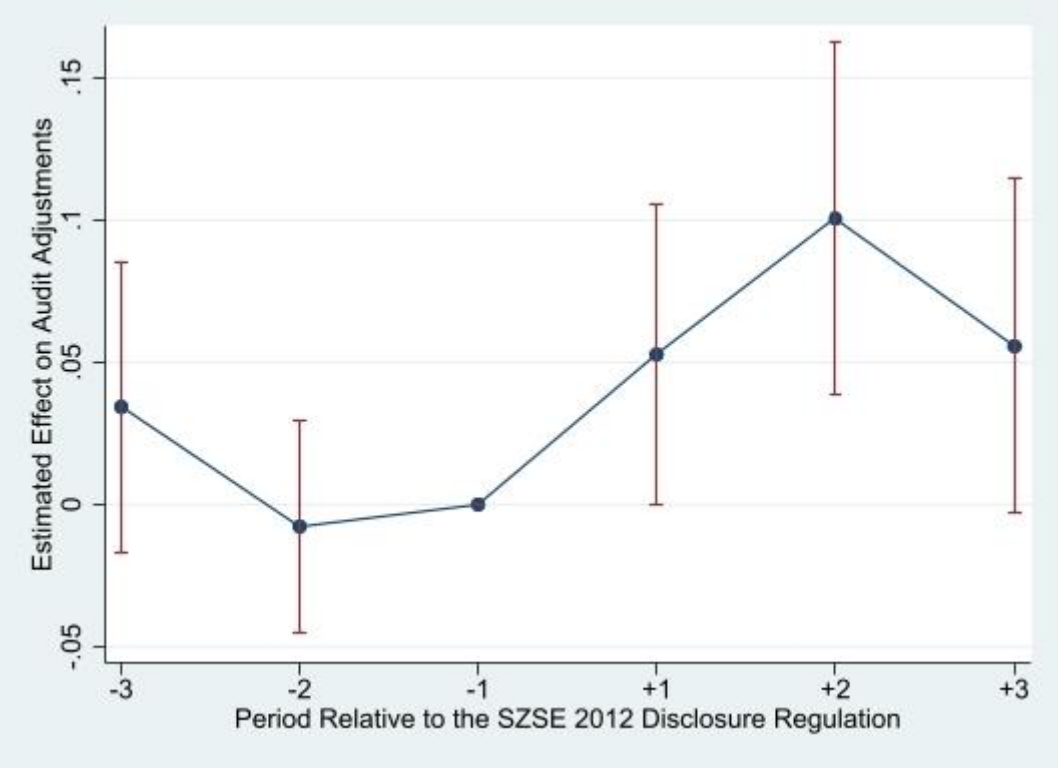
This figure plots the yearly changes in the dependent variable (*ADJMAG*) during our sample period. Treated refers to treated groups and Control refers to the control groups.

Figure 3
Sensitivity analysis based on the Rambachan-Roth (2023) approach



This figure employs bounding relative magnitudes $\Delta^{RM}(\bar{M})$ to construct robust confidence intervals (C.I.s) for the average effects across all three post-treatment periods. The red caps represent C.I.s under baseline identifying assumptions, while the blue caps illustrate C.I.s that account for potential violations of the parallel trend assumption. This is achieved by varying values of $\bar{M}$ from 0 to 2.

Figure 4
Tests of parallel trends assumption based on the de Chaisemartin and d'Haultfoeuille (2020) estimation procedure



This figure plots the tests of parallel trends assumption based on the estimation procedure documented in de Chaisemartin and d'Haultfoeuille (2020).

Table 1
Sample selection process and distribution

Panel A: Sample selection

Original treatment client firms listed before 2009	650		
Delete client firms with auditor changes after the 2012 disclosure regulation	(153)		
Delete client firms in the financial industries	(9)		
Delete client firms without properly matched control client firms	(48)		
Total treatment client firms	440		
Matched control client firms	440		
Total treatment and matched control client firms	880		
		Treat	Control
Client firm-year observations over the period 2009-2011 and 2013-2015		440×6= 2,640	440×6= 2,640
Delete observations without site visits during 2013-2015		(270)	(270)
Delete observations with missing information on main variables		(209)	(240)
Total treatment/control sample		2,161	2,130
Final sample		4,291	

Panel B: Distribution by industry

	N	% of sample
Agriculture, forestry and fishing	69	1.61
Mining	87	2.03
Food and apparel	283	6.60
Furniture and printing	782	18.22
Metal and equipment	1,649	38.43
Other manufacturing	70	1.63
Utilities	157	3.66
Construction	87	2.03
Wholesale and retail	221	5.15
Transportation	137	3.19
Accommodation and catering	35	0.82
Information and technology	221	5.15
Real estate	240	5.59
Leasing and commercial service	68	1.58
Scientific research and technical service	18	0.42
Water conservancy, environment and public facility management	86	2.00
Health and social work	33	0.77
Entertainment	22	0.51
Conglomerates	26	0.61
Total	4,291	100

Panel C: Distribution by year

	N	% of sample
2009	736	17.15
2010	785	18.29
2011	816	19.02
2013	609	14.19
2014	668	15.57
2015	677	15.78
Total	4,291	100

This table presents the sample selection process and distribution. Panel A presents the sample selection process. Panel B presents sample distribution by industry. Panel C presents sample distribution by year. The final sample includes 4,291 client-years during the 2009-2011 and 2013-2015 period.

Table 2
Descriptive statistics

Panel A: Summary statistics

Variables	N	Mean	Median	P25	P75	Std. Dev.
ADJMAG	4,291	0.088	0.008	0.000	0.059	0.252
ADJDUM	4,291	0.659	1.000	0.000	1.000	0.474
ADJMAG1	4,291	0.337	0.047	0.000	0.275	0.795
Rank (*ADJMAG*)	4,291	0.330	0.242	0.000	0.622	0.333
Size	4,291	21.848	21.787	21.014	22.600	1.192
ROA	4,291	0.041	0.037	0.014	0.069	0.060
Leverage	4,291	0.468	0.466	0.313	0.616	0.209
Age	4,291	2.628	2.708	2.398	2.890	0.423
M.B.	4,291	2.530	1.904	1.427	2.776	2.121
M&A	4,291	0.180	0.000	0.000	0.000	0.384
Subsidiaries	4,291	2.232	2.303	1.609	2.890	1.002
SOE	4,291	0.405	0.000	0.000	1.000	0.491
BD_ Size	4,291	2.154	2.197	2.079	2.197	0.197
BD_ Meetings	4,291	2.136	2.079	1.946	2.398	0.400
INDIR	4,291	0.370	0.333	0.333	0.400	0.052
Duality	4,291	0.215	0.000	0.000	0.000	0.411
Audit Com	4,291	0.996	1.000	1.000	1.000	0.061
ANALYST	4,291	1.593	1.609	0.693	2.565	1.143
Big10	4,291	0.481	0.000	0.000	1.000	0.500
Tenure	4,291	1.712	1.792	1.099	2.398	0.847
Spec	4,291	0.012	0.000	0.000	0.000	0.109

Panel B: Descriptive statistics of ADJMAG

			Diff: Post-2012 regulation versus Pre-2012 regulation	
	Pre-2012 regulation (1)	Post-2012 regulation (2)	Difference	t-stat.
Treat = 1	0.076 [n = 1,184]	0.115 [n = 977]	0.039***	(3.299)
Treat = 0	0.087 [n = 1,153]	0.075 [n = 977]	-0.012	(-1.204)
Diff: *Treat = 1* versus *Treat = 0*	-0.011	0.040***	0.051***	(3.300)
t-stat.	(-1.323)	(2.980)		

This table presents the descriptive statistics of the variables used in our main analyses. Panel A reports the summary statistics for the main client firm-year level variables included in the regression. Panel B presents the descriptive statistics of *ADJMAG*. All variables are defined in Appendix B.

Table 3
Correlation matrix

	1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*
1ADJMAG	1.000																	
2Size	**-0.087**	1.000																
3ROA	**-0.149**	**0.078**	1.000															
4Lev	**0.049**	**0.285**	**-0.361**	1.000														
5Age	*0.032*	**0.138**	**-0.143**	**0.229**	1.000													
6M.B.	**0.053**	**-0.429**	**0.050**	**-0.193**	**0.062**	1.000												
7M&A	-0.013	**0.065**	**0.041**	*0.033*	0.009	-0.011	1.000											
8Subsidiaries	-0.015	**0.523**	**0.048**	**0.178**	**0.192**	**-0.176**	**0.128**	1.000										
9SOE	**-0.062**	**0.224**	**-0.059**	**0.135**	**0.138**	**-0.108**	**-0.059**	0.000	1.000									
10BD_Size	**-0.059**	**0.243**	**0.065**	**0.070**	-0.013	**-0.146**	0.014	**0.067**	**0.200**	1.000								
11BD_Meetings	0.025	**0.235**	**-0.049**	**0.173**	**0.084**	**-0.056**	**0.195**	**0.274**	**-0.102**	-0.015	1.000							
12INDIR	**0.057**	-0.005	**-0.045**	0.016	-0.02	**0.071**	-0.026	**0.043**	**-0.068**	**-0.441**	*0.037*	1.000						
13Duality	-0.013	**-0.109**	0.026	**-0.082**	**-0.117**	**0.056**	-0.005	**-0.059**	**-0.219**	**-0.164**	-0.009	**0.119**	1.000					
14Audit Com	0.008	**-0.012**	-0.019	-0.004	0.013	-0.002	-0.001	**0.021**	0.012	-0.006	0.007	0.011	-0.005	1.000				
15ANALYST	**-0.114**	**0.455**	**0.441**	**-0.108**	**-0.228**	**-0.088**	**0.082**	**0.258**	**0.054**	**0.209**	**0.096**	*-0.030*	0.012	-0.011	1.000			
16Big10	*0.035*	**0.120**	0.005	-0.01	0.001	-0.016	-0.009	**0.112**	-0.011	-0.009	0.015	0.019	-0.003	0.005	**0.049**	1.000		
17Tenure	-0.009	**0.120**	**0.049**	**-0.050**	**0.149**	-0.007	-0.008	**0.102**	-0.019	0.000	0.000	-0.004	-0.021	0.008	**0.081**	**-0.087**	1.000	
18Spec	0.022	-0.008	0.008	-0.017	**0.041**	0.011	-0.008	-0.003	**0.069**	-0.014	-0.006	0.009	0.004	0.007	-0.025	**0.081**	-0.015	1.000

This table presents the Pearson correlations among the variables used in our main analyses. The correlation coefficients are highlighted in bold (*italic*) if they are significant at the 1% (5%) level. All variables are defined in Appendix B.

Table 4
The impact of disclosed corporate site visits on audit adjustments

Dependent variable	*ADJMAG*	*ADJMAG*
	(1)	(2)
Treat × Post	0.066***	0.058***
	(3.68)	(3.03)
Size		-0.040***
		(-3.10)
ROA		-0.355**
		(-2.51)
Leverage		0.097
		(1.56)
Age		0.061
		(1.20)
M.B.		0.000
		(0.03)
M&A		-0.012
		(-1.12)
Subsidiaries		0.008
		(1.12)
SOE		0.017
		(0.66)
BD_Size		-0.043
		(-0.99)
BD_Meetings		0.023
		(1.58)
INDIR		0.161
		(1.10)
Duality		-0.022
		(-1.45)
Audit Com		0.012
		(0.58)
ANALYST		0.006
		(0.85)
Big10		0.014
		(0.83)
Tenure		-0.009
		(-0.94)
Spec		0.018
		(0.32)
Client Firm FE	YES	YES
Year FE	YES	YES
Observations	4,291	4,291
Adjusted R^2	0.115	0.126

This table presents the baseline results of the impact of disclosed corporate site visits on audit adjustments. The dependent variable is *ADJMAG*. *Treat × Post* is a binary variable that equals 1 for treatment client firms after the adoption of the 2012 disclosure regulation (i.e., 2013–2015). Column (1) reports the result controlling for client firm and year-fixed effects but does not include control variables. Column (2) reports the result controlling for client firm and year-fixed effects and a full set of control variables. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 5
Tests of the parallel trends assumption

Dependent variable	*ADJMAG*
	(1)
Treat × YEAR -3	0.001
	(0.03)
Treat × YEAR -2	0.008
	(0.37)
Treat × YEAR +1	0.060**
	(2.23)
Treat × YEAR +2	0.057**
	(2.06)
Treat × YEAR +3	0.065**
	(2.25)
Size	-0.040***
	(-3.11)
ROA	-0.353**
	(-2.51)
Leverage	0.097
	(1.56)
Age	0.061
	(1.19)
M.B.	0.000
	(0.02)
M&A	-0.012
	(-1.13)
Subsidiaries	0.009
	(1.12)
SOE	0.017
	(0.65)
BD Size	-0.043
	(-0.98)
BD_Meetings	0.023
	(1.56)
INDIR	0.161
	(1.10)
Duality	-0.022
	(-1.46)
Audit Com	0.013
	(0.64)
ANALYST	0.006
	(0.84)
Big10	0.014
	(0.85)
Tenure	-0.009
	(-0.94)
Spec	0.018
	(0.31)
Client Firm FE	YES
Year FE	YES
Observations	4,291
Adjusted R^2	0.125

This table presents the results of the parallel trends assumption tests. The dependent variable is *ADJMAG*. Column (1) reports the dynamic effects of disclosed corporate site visits on audit adjustments. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B

Table 6
Cross-sectional analyses: fear of auditing failure

Dependent variable	*ADJMAG*				
	PROTECT	*NONSOE vs SOE*	*COMMENT*	*OVERSEAS LISTING*	*NONRETIREMENT*
	(1)	(2)	(3)	(4)	(5)
Treat_ Greater_ Pressure × Post (β_1)	0.098***	0.088***	0.090***	0.202**	0.058***
	(3.08)	(3.63)	(3.31)	(2.50)	(2.95)
Treat_ Less_ Pressure × Post (β_2)	0.039**	0.016	0.022	0.046**	-0.005
	(1.97)	(0.73)	(1.08)	(2.43)	(-0.16)
Size	-0.041***	-0.041***	-0.036***	-0.040***	-0.034***
	(-3.15)	(-3.20)	(-2.88)	(-3.11)	(-2.82)
ROA	-0.353**	-0.354**	-0.344**	-0.353**	-0.365**
	(-2.52)	(-2.52)	(-2.43)	(-2.49)	(-2.51)
Leverage	0.100	0.090	0.104*	0.094	0.088
	(1.60)	(1.45)	(1.68)	(1.51)	(1.36)
Age	0.056	0.022	0.098*	0.078	0.056
	(1.10)	(0.43)	(1.79)	(1.48)	(1.10)
M.B.	0.000	-0.000	0.001	0.001	0.001
	(0.03)	(-0.08)	(0.15)	(0.18)	(0.17)
M&A	-0.012	-0.012	-0.011	-0.013	-0.013
	(-1.10)	(-1.14)	(-1.01)	(-1.21)	(-1.23)
Subsidiaries	0.008	0.007	0.010	0.010	0.009
	(1.06)	(0.97)	(1.35)	(1.32)	(1.23)
SOE	0.017	0.009	0.017	0.020	0.006
	(0.68)	(0.34)	(0.65)	(0.76)	(0.25)
BD_Size	-0.040	-0.044	-0.036	-0.043	-0.037
	(-0.92)	(-1.02)	(-0.83)	(-1.01)	(-0.86)
BD Meetings	0.023	0.022	0.022	0.024*	0.026*
	(1.60)	(1.51)	(1.56)	(1.71)	(1.85)
INDIR	0.173	0.148	0.161	0.155	0.210
	(1.19)	(1.02)	(1.09)	(1.06)	(1.42)
Duality	-0.022	-0.023	-0.022	-0.020	-0.025
	(-1.46)	(-1.51)	(-1.44)	(-1.31)	(-1.63)
Audit Com	0.019	0.014	0.006	0.002	0.023
	(1.01)	(0.72)	(0.30)	(0.09)	(1.18)
ANALYST	0.006	0.005	0.006	0.006	0.005
	(0.81)	(0.67)	(0.83)	(0.82)	(0.74)
Big10	0.017	0.013	0.014	0.013	0.015
	(1.06)	(0.80)	(0.83)	(0.82)	(0.89)
Tenure	-0.010	-0.009	-0.008	-0.007	-0.005
	(-1.06)	(-0.97)	(-0.84)	(-0.84)	(-0.51)
Spec	0.021	0.016	0.017	0.021	0.017
	(0.37)	(0.28)	(0.31)	(0.37)	(0.30)
Client Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	4,288	4,291	4,291	4,291	4,164
Adjusted R^2	0.127	0.128	0.128	0.129	0.123
Wald Test β_1- β_2	0.059*	0.072**	0.068**	0.156*	0.063*
(p-value)	(0.070)	(0.010)	(0.023)	(0.058)	(0.057)

This table presents the cross-sectional analyses of fear of auditing failure view tests. *Treat_Greater_Pressure* is a binary variable that equals 1 for treatment client firms that are sorted into client firms located in areas with stronger investor protection, non-SOEs, client firms with more vociferous investors who participate more in East Money stock forum, client firms with overseas listing, or client firms audited by auditors who are not approaching retirement. *Treat_Less_Pressure* equals 1 minus *Treat_Greater_Pressure*. The table also reports the p-value of the Wald test on the difference of the coefficients of the *Treat_Greater_Pressure* and *Treat_Less_Pressure* groups. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 7
Cross-sectional analyses: information content

Dependent variable	*ADJMAG*	*ADJMAG*
	(1)	(2)
Visits_Forward-looking Information × Post	0.049**	
	(2.37)	
Visits_Accounting and Finance Topics × Post	0.236**	
	(2.11)	
Visits_Nonfinancial Information × Post	-0.169	
	(-1.21)	
(1- Visits_Forward-looking Information-Visits_Accounting and Finance Topics-Visits_Nonfinancial Information) × Post	-0.157	
	(-0.99)	
Treat × Post		0.029
		(1.44)
Treat × Post × High_Relevance		0.058***
		(2.69)
Size	-0.040***	-0.041***
	(-3.13)	(-3.16)
ROA	-0.342**	-0.361**
	(-2.41)	(-2.55)
Leverage	0.106*	0.095
	(1.71)	(1.52)
Age	0.068	0.068
	(1.33)	(1.35)
M.B.	0.000	0.000
	(0.07)	(0.05)
M&A	-0.012	-0.011
	(-1.16)	(-1.06)
Subsidiaries	0.008	0.008
	(1.14)	(1.08)
SOE	0.015	0.018
	(0.60)	(0.72)
BD_Size	-0.045	-0.046
	(-1.02)	(-1.05)
BD_Meetings	0.024*	0.023
	(1.70)	(1.58)
INDIR	0.168	0.170
	(1.15)	(1.18)
Duality	-0.022	-0.021
	(-1.47)	(-1.40)
Audit Com	0.011	0.015
	(0.57)	(0.76)
ANALYST	0.007	0.006
	(0.94)	(0.84)
Big10	0.012	0.013
	(0.74)	(0.83)
Tenure	-0.008	-0.009
	(-0.87)	(-0.96)
Spec	0.023	0.017
	(0.41)	(0.31)
Client Firm FE	YES	YES
Year FE	YES	YES
Observations	4,291	4,291
Adjusted R^2	0.128	0.128

This table presents the cross-sectional analyses of information content view tests. Column (1) displays the cross-sectional analysis of the type of information that intermediaries gather through site visits. *Visits_Forward-looking Information* is the word count for answers to questions covering forward-looking information, scaled by the total word counts of site visits in a given year *t*. *Visits_Accounting and Finance Topics* is the word count for answers to questions covering accounting and finance topics, scaled by the total word counts of site visits in a given year *t*. *Visits_ Nonfinancial Information* is the word count for answers to questions covering nonfinancial information, scaled by the total word counts of site visits in a given year *t*. Column (2) displays the cross-sectional analysis of relevance between questions and answers in the disclosure of intermediary corporate site visits. $High_Relevance_{i,t}$ is a dummy that equals

1 if the answer relevance of treated client firm *i* in year *t* is greater than the sample median, and 0 otherwise. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 8
Cross-sectional analyses: information content

Dependent variable	*ADJMAG*		
	ASSET TANGIBILITY	*ANALYST*	*SYN*
	(1)	(2)	(3)
Treat More Information × *Post* (β_1)	0.087***	0.106***	0.085***
	(3.64)	(3.64)	(3.12)
Treat_Less_Information × *Post* (β_2)	0.029	0.010	0.026
	(1.23)	(0.62)	(1.26)
Size	-0.039***	-0.037***	-0.039***
	(-3.00)	(-2.86)	(-2.95)
ROA	-0.353**	-0.376***	-0.369**
	(-2.50)	(-2.61)	(-2.48)
Leverage	0.100	0.095	0.116*
	(1.61)	(1.52)	(1.83)
Age	0.055	0.086*	0.078
	(1.09)	(1.70)	(1.47)
M.B.	0.000	-0.000	-0.001
	(0.13)	(-0.09)	(-0.16)
M&A	-0.013	-0.012	-0.015
	(-1.18)	(-1.15)	(-1.35)
Subsidiaries	0.009	0.008	0.009
	(1.19)	(1.15)	(1.25)
SOE	0.015	0.021	0.018
	(0.62)	(0.82)	(0.67)
BD_Size	-0.046	-0.036	-0.051
	(-1.06)	(-0.85)	(-1.15)
BD_Meetings	0.022	0.021	0.023
	(1.52)	(1.44)	(1.59)
INDIR	0.156	0.182	0.174
	(1.08)	(1.26)	(1.16)
Duality	-0.023	-0.025	-0.023
	(-1.51)	(-1.60)	(-1.54)
Audit Com	0.013	0.010	0.006
	(0.70)	(0.48)	(0.30)
ANALYST	0.006	0.003	0.007
	(0.83)	(0.44)	(0.91)
Big10	0.015	0.013	0.009
	(0.90)	(0.80)	(0.54)
Tenure	-0.008	-0.008	-0.008
	(-0.86)	(-0.89)	(-0.92)
Spec	0.021	0.012	0.020
	(0.36)	(0.21)	(0.35)
Client Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Observations	4,291	4,291	4,241
Adjusted R^2	0.127	0.130	0.128
Wald Test β_1- β_2	0.058**	0.096***	0.059**
(p-value)	(0.036)	(0.000)	(0.044)

This table presents the cross-sectional analyses of information content view tests. *Treat_More_Information* is a binary variable that equals 1 for treatment client firms sorted into client firms with higher asset tangibility, client firms covered by fewer analysts, or client firms with higher return synchronicity. *Treat_Less_Information* equals 1 minus *Treat_More_Information*. The table also reports the p-value of the Wald test on the difference of the coefficients of the *Treat_More_Information* and *Treat_Less_Information* groups. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 9
Information spillover effects of disclosed corporate site visits

Dependent variable	*ADJMAG*				
	(1)	(2)	(3)	(4)	(5)
Treat_Spillover × Post (β_1)	0.057**	0.031	0.050**	0.002	0.054**
	(2.05)	(0.49)	(2.14)	(0.07)	(2.18)
Treat × Post (β_2)					0.129***
					(4.12)
Size	0.040	0.003	0.019	0.062**	0.001
	(1.43)	(0.05)	(0.93)	(2.41)	(0.07)
ROA	-0.155	-0.259	-0.353	-0.062	-0.338*
	(-0.98)	(-0.66)	(-1.47)	(-0.33)	(-1.91)
Lev	-0.128	-0.162	0.043	-0.009	-0.017
	(-1.35)	(-0.75)	(0.58)	(-0.11)	(-0.24)
Age	-0.204	-0.374	-0.220	-0.024	-0.057
	(-0.87)	(-1.10)	(-1.51)	(-0.16)	(-0.66)
M.B.	0.016*	-0.020*	-0.005	-0.003	-0.004
	(1.80)	(-1.73)	(-1.61)	(-1.42)	(-1.55)
M&A	-0.015	0.044	0.003	0.031*	0.006
	(-0.85)	(0.96)	(0.24)	(1.69)	(0.41)
Subsidiaries	0.016	-0.070*	-0.041*	-0.081**	-0.036**
	(1.05)	(-1.67)	(-1.75)	(-2.34)	(-2.34)
SOE	-0.084***	0.144	-0.181**	-0.012	-0.067
	(-2.91)	(1.18)	(-2.57)	(-0.20)	(-1.16)
BD Size	-0.285***	-0.162	-0.092	0.033	-0.027
	(-3.39)	(-0.87)	(-1.28)	(0.49)	(-0.45)
BD_Meetings	0.024	0.035	0.009	-0.012	-0.009
	(1.02)	(0.51)	(0.43)	(-0.55)	(-0.44)
INDIR	0.289	0.600	-0.064	0.093	0.122
	(1.27)	(0.93)	(-0.34)	(0.53)	(0.83)
Duality	0.056	-0.026	-0.035	-0.021	-0.046**
	(1.04)	(-0.30)	(-1.18)	(-0.98)	(-1.98)
Audit Com	0.225***	-0.073	0.016	0.005	0.038
	(7.14)	(-1.47)	(0.86)	(0.35)	(0.89)
Analyst	-0.059***	0.001	-0.001	-0.009	-0.003
	(-3.55)	(0.05)	(-0.07)	(-0.90)	(-0.32)
Big10	-0.010	0.063	-0.018	0.007	-0.013
	(-0.35)	(0.88)	(-0.88)	(0.31)	(-0.55)
Tenure	-0.023**	-0.020	-0.013	-0.009	-0.007
	(-2.03)	(-0.67)	(-1.31)	(-0.71)	(-0.64)
Spec	–	–	0.168*	0.014	0.059
	–	–	(1.71)	(0.38)	(1.19)
Client Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	307	337	2,385	1,612	3,274
Adjusted R^2	0.202	0.057	0.139	0.144	0.092
Wald Test β_2- β_1					0.075**
(p-value)					(0.027)

This table presents the analyses of information spillover effects of disclosed corporate site visits. Columns (1)-(4) present the information spillovers from a site visit of client firm *V* in year *t* to (i) a close-peer client firm in the same year *t* with no site visits and audited by the corresponding affected auditor, (ii) a distant-peer client firm in the same year *t* with no site visits and audited by the affected auditor, (iii) a close-peer client firm in the same year *t* with no site visits and audited by peer auditors, (iv) a distant-peer client firm in the same year *t* with no site visits and audited by peer auditors, respectively. Column (5) presents a robustness check of the information spillover effect. The table also reports the p-value of the Wald test on the difference of the coefficients of the *Treat* and *Treat_Spillover* groups. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 10

Effect of disclosed corporate site visits on pre-audit accruals quality and audit outcomes

Dependent variable	*Pre-ABSDA*	*Audited-ABSDA*		*RESTATE*	
		ADJDUM=0	*ADJDUM=1*	*ADJDUM=0*	*ADJDUM=1*
	(1)	(2)	(3)	(4)	(5)
Treat × Post (β_1)	-0.002	0.017	-0.025**	0.002	-0.065**
	(-0.07)	(0.95)	(-2.29)	(0.10)	(-2.46)
Treat_Spillover × Post (β_2)	-0.014	0.006	-0.017**	-0.008	-0.052**
	(-0.48)	(0.68)	(-2.06)	(-0.19)	(-2.19)
Size	0.044*	-0.011	-0.008	0.035	0.026
	(1.79)	(-0.94)	(-0.87)	(1.05)	(1.23)
ROA	0.278	0.080	-0.030	0.458	-0.330
	(1.29)	(0.69)	(-0.34)	(1.42)	(-1.36)
Leverage	0.282***	0.041	0.045	0.111	-0.067
	(2.96)	(0.80)	(1.43)	(1.18)	(-1.05)
Age	-0.033	-0.076	0.115**	0.040	0.200*
	(-0.27)	(-1.39)	(2.22)	(0.61)	(1.93)
M.B.	0.003	0.001	0.001	0.002	0.001
	(0.65)	(0.33)	(0.76)	(0.50)	(0.27)
Spec	-0.024	0.021	0.002	0.030	0.101
	(-0.67)	(1.43)	(0.22)	(1.24)	(1.22)
Tenure	-0.006	-0.009*	-0.000	0.003	-0.000
	(-0.40)	(-1.66)	(-0.07)	(0.26)	(-0.02)
SOE	-0.119*	-0.027	-0.021	-0.053	-0.046
	(-1.70)	(-1.07)	(-1.50)	(-0.88)	(-0.69)
InvRec	0.186	-0.043	0.049	0.038	-0.069
	(1.46)	(-0.81)	(1.44)	(0.25)	(-0.79)
Current	0.009	0.016*	0.008*	0.001	-0.019*
	(0.76)	(1.93)	(1.86)	(0.12)	(-1.69)
Cash	0.241*	-0.027	0.005	-0.017	0.066
	(1.94)	(-0.43)	(0.13)	(-0.15)	(0.58)
Loss	0.013	0.012	-0.006	0.055	-0.017
	(0.48)	(1.18)	(-0.85)	(1.45)	(-0.65)
ANALYST	0.009	0.000	0.006*	-0.009	0.009
	(0.77)	(0.05)	(1.69)	(-0.57)	(0.93)
Top1	0.284**	-0.020	0.056	0.010	-0.174*
	(2.00)	(-0.33)	(1.33)	(0.13)	(-1.87)
Stock Return	-0.008	0.002	0.009	0.015	-0.004
	(-0.47)	(0.32)	(1.61)	(1.00)	(-0.42)
Return Vol	1.420	0.970	-0.493	0.270	-0.102
	(0.72)	(1.41)	(-0.99)	(0.18)	(-0.10)
ADJMAG			-0.015**		-0.031*
			(-2.53)		(-1.70)
Client Firm FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Observations	3,082	1,270	1,843	1,309	1,912
Adjusted R^2	0.124	0.218	0.226	0.244	0.115
Wald Test β_1- β_2			-0.008		-0.013
(p-value)			(0.423)		(0.602)

This table presents the impact of disclosed corporate site visits on pre-audit accruals quality and audit outcomes. Column (1) displays the results using *Pre-ABSDA* as the dependent variable. Columns (2) and (3) display results using *Audited-ABSDA* as the dependent variable in the absence of audit adjustments and in the presence of audit adjustments, respectively. Columns (4) and (5) display results using *RESTATE* as the dependent variable in the absence of audit adjustments and in the presence of audit adjustments, respectively. The table also reports the p-value of the Wald test on the difference of the coefficients of the *Treat* and *Treat_Spillover* groups. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.

Table 11
Effect of disclosed corporate site visits on analyst forecast quality

Dependent variable	*FERROR*			*FDISP*		
	Full Sample	*ADJDUM=0*	*ADJDUM=1*	*Full Sample*	*ADJDUM=0*	*ADJDUM=1*
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post (β_1)	-0.002	0.008	-0.008***	-0.002	0.003	-0.005**
	(-0.78)	(1.37)	(-3.33)	(-1.49)	(1.22)	(-2.47)
Treat_Spillover × Post (β_2)	0.002	0.005	-0.006*	0.001	0.003	-0.003*
	(0.65)	(0.96)	(-1.74)	(0.92)	(1.12)	(-1.70)
Size	0.001	0.002	0.001	0.000	-0.000	0.000
	(0.60)	(0.74)	(0.39)	(0.10)	(-0.23)	(0.07)
ROA	0.038*	0.048	0.017	0.006	0.004	-0.008
	(1.80)	(1.09)	(0.69)	(0.46)	(0.14)	(-0.48)
Leverage	0.012**	0.006	0.013*	0.007**	0.009	0.005
	(2.50)	(0.59)	(1.86)	(2.44)	(1.59)	(1.27)
Age	-0.008	-0.015*	0.001	-0.001	-0.002	-0.002
	(-1.24)	(-1.81)	(0.11)	(-0.33)	(-0.46)	(-0.38)
M.B.	0.000	0.000	0.000	-0.000	-0.000	0.000
	(0.06)	(0.24)	(0.68)	(-0.83)	(-0.24)	(0.29)
M&A	-0.000	-0.004**	0.001	-0.000	-0.001	0.001
	(-0.47)	(-2.23)	(0.78)	(-0.21)	(-1.16)	(0.66)
Subsidiaries	-0.000	-0.002**	0.000	0.000	-0.001	-0.000
	(-0.19)	(-2.02)	(0.54)	(0.01)	(-1.21)	(-0.16)
SOE	-0.006*	-0.005	-0.000	-0.006***	-0.007**	-0.001
	(-1.90)	(-0.77)	(-0.06)	(-2.91)	(-2.06)	(-0.25)
BD_Size	0.004	0.000	0.003	0.004	-0.001	0.002
	(0.89)	(0.04)	(0.53)	(1.58)	(-0.14)	(0.85)
BD_Meetings	-0.001	-0.003	0.001	-0.001	-0.002	-0.001
	(-0.63)	(-1.05)	(0.33)	(-1.15)	(-1.31)	(-0.55)
INDIR	0.013	0.031	-0.004	0.004	0.007	0.009
	(0.95)	(1.10)	(-0.23)	(0.58)	(0.48)	(0.96)
Duality	-0.002	-0.004	-0.001	0.000	-0.001	0.001
	(-0.96)	(-0.96)	(-0.62)	(0.22)	(-0.35)	(0.77)
Audit Com	0.003	0.002	0.004	0.007	-0.002	0.022
	(0.26)	(0.73)	(0.13)	(0.91)	(-1.19)	(1.36)
ANALYST	-0.001	-0.002	-0.001	0.000	-0.000	0.000
	(-1.19)	(-0.98)	(-1.15)	(0.07)	(-0.48)	(0.08)
INST	-0.000***	-0.000	-0.000*	-0.000**	-0.000*	-0.000
	(-2.86)	(-1.27)	(-1.88)	(-1.99)	(-1.75)	(-0.17)
Return Vol	-0.246***	-0.330**	-0.151	-0.184***	-0.203***	-0.123*
	(-2.68)	(-2.26)	(-1.32)	(-4.03)	(-2.83)	(-1.89)
ForHorizon	0.014***	0.019***	0.012***	0.007***	0.009***	0.006***
	(5.66)	(3.66)	(3.82)	(5.44)	(3.42)	(3.23)
Client Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	1,504	665	839	1,504	665	839
Adjusted R^2	0.378	0.253	0.485	0.279	0.278	0.203
Wald Test β_1- β_2			-0.002			-0.002
(p-value)			(0.554)			(0.406)

This table presents the impact of disclosed corporate site visits on analyst forecast error and forecast dispersion. Columns (1) – (3) display the results using *FERROR* as the dependent variable based on the full sample, client firms without audit adjustments, and client firms with audit adjustments, respectively. Columns (4) – (6) display the results using *FDISP* as the dependent variable based on the full sample, client firms without audit adjustments, and client firms with audit adjustments, respectively. The table also reports the p-value of the Wald test on the difference of the coefficients of the *Treat* and *Treat_Spillover* groups. All continuous variables are winsorized at the 1 percent and 99 percent levels. T-statistics shown in parentheses are adjusted for clustering by client firm. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels based on two-tailed statistical tests. All variables are defined in Appendix B.