Journal of Corporate Finance 68 (2021) 101931

Contents lists available at ScienceDirect

Journal of Corporate Finance

journal homepage: www.elsevier.com/locate/jcorpfin

Does an anti-corruption campaign increase analyst earnings forecast optimism?☆

Nian Li [a], Nianhang Xu [b], Rui Dong [c], Kam C. Chan [e], Xiaowei Lin [d,*]

[a] School of Economics and Management, Yanshan University, No. 438 West Hebei Avenue, Qinhuangdao City, Hebei Province, PR China
[b] School of Business, Renmin University of China, No.59, Zhongguancun St., Haidian District, Beijing, PR China
[c] School of Business, Nanjing University, 22 Hankou Road, Gulou District, Nanjing, Jiangsu Province, PR China
[d] School of Finance and Accounting, Fuzhou University of International Studies and Trade, PR China
[e] School of Accounting, Zhongnan University of Economics and Law, Wuhan, PR China

ARTICLE INFO

Keywords:
Anti-corruption
Analyst earnings forecast optimism
Local government pressure
Firm bad news hiding

ABSTRACT

We examine the impact of an anti-corruption campaign on analyst earnings forecast optimism. Using hand-collected site visits data by the Central Inspection Team (CIT) in China that began in 2013, we document higher analyst optimism during CIT visit periods than during non-CIT visit periods. The results are robust to matched samples, placebo tests, alternative fixed effect and clustering specifications, endogeneity of CIT site visits concern, and alternative samples. Additional analysis suggests that local government pressure and firm bad-news-hiding explain the findings but it is not consistent with the improved firm fundamentals interpretation. Moreover, we find that the effect of CIT visits on analyst optimism is more pronounced for star, non-affiliated, and experienced analysts, supporting the notion that, because of their greater influence, local governments focus on pressuring these analysts. More important, the impact of CIT visits on analyst optimism is more salient if a CIT leader had previous work experience or longer work experience in the inspected province. Interestingly, we document a reversion in analyst earnings forecast optimism 60 days after CIT site visits, especially among the non-state-owned firms, suggesting that, after the CIT investigation, analyst optimism is no longer needed.

1. Introduction

Bardhan et al. (2015) defines corruption as the abuse of power by government officials for private benefits. Previous studies suggest that corruption leads to misallocation of resources (Aidt et al., 2008). The literature on the economics of corruption focuses on two aspects. First, it focuses on the economic consequences of corruption (Fan et al., 2008; Cai et al., 2011). Second, it studies the impact of government-led anti-corruption campaigns on firm behavior, such as negative information release (Cao et al., 2018; Chen et al., 2018) and research and development investment (Gan and Xu, 2019). These studies firstly examine the internal responses of a firm when facing a government-led anti-corruption campaign. The literature is unclear how external firm stakeholders, especially financial

☆ We acknowledge the helpful comments from two anonymous reviewers, William L Megginson (the editor) and Xiaodan Gao. This project is supported by the Fundamental Research Funds for the Central Universities and the Research Funds of Renmin University of China (14XNJ019). The usual caveats apply.

* Corresponding author.
E-mail addresses: nhxu@ruc.edu.cn (N. Xu), johnny.chan@wku.edu (K.C. Chan), linxiaowei@fzfu.edu.cn (X. Lin).

https://doi.org/10.1016/j.jcorpfin.2021.101931
Received 1 July 2020; Received in revised form 1 February 2021; Accepted 17 March 2021
Available online 23 March 2021
0929-1199/

analysts, react to such campaigns.

The purpose of this study is to investigate how analyst earnings forecasts change when their followed firms' jurisdictions become subject to an anti-corruption investigation. Analysts are important external stakeholders of a firm. They engage in information production to help investors make investment decisions. In the process, they enhance a country's financial market development and improve the information environment through their analyses, forecasts, and recommendations (Chan and Hameed, 2006; Xu et al., 2013) and hence, they play a growing role in the emerging financial markets (Cao et al., 2019a). Thus, it is important to examine how an anti-corruption campaign affects analyst behavior in terms of their earnings forecasts. We conduct our analysis using Chinese data for two reasons. First, due to the perception of inequity and resources allocation concerns, Chinese political leaders have incentives to address corruption issues. In 2012, President Xi Jinping officially launched an anti-corruption campaign following the 18th Communist Party People's Congress. The Central Inspection Team (CIT) led this campaign and began to conduct site visits in 2013 (Ding et al., 2020). The Chinese government is determined to make a serious effort with this anti-corruption campaign, which provides a quasi-natural experiment (Cao et al., 2018; Ding et al., 2020) to examine the impact of such a campaign on analyst behavior. Second, the Chinese campaign has some good features that aid our analysis. The Central Commission for Discipline Inspection decides when to send a CIT to specific provinces. The specific time and sequence of CIT site visits are more likely to be exogenous to local government officials and their jurisdictions. In addition, CIT site visits are scattered across different provinces at different times, which allows us to use the provinces that are not inspected by CIT as a benchmark sample to examine analyst earnings forecast optimism for their followed firms during CIT site visits and non-visited periods (Cao et al., 2018). Furthermore, after the site visits, the CIT fully disclosed the exact dates of its site visits on its website. Hence, we can precisely match CIT site visit dates with analyst earnings forecasts release times, which allows us to use analyst-forecast-firm-year data to conduct the analysis. This method is an improvement over previous studies that attempt to determine the impact of anti-corruption campaigns that commonly use firm-year data (Chen et al., 2018).

We argue that there are three conceptual underpinnings for the impact of an anti-corruption campaign on analyst earnings forecasts: 1) local government pressure, 2) firm bad news hiding, and 3) improved firm fundamentals. The first underpinning suggests that local government officials pressure brokerages, encouraging their analysts to hype earnings forecasts. The second draws from Cao et al. (2018) and others who find that local government officials pressure their subordinates and firms in their jurisdiction to hide bad news during CIT site visits. The third one reveals that if the anti-corruption campaign shakes up local government officials' performance, leading to an improved external and internal operating environment for firms. All three perspectives indicate that analysts turn optimistic in their earnings forecasts.

Using hand-collected CIT data matched to analyst earnings forecasts, we document that analyst optimism increases during CIT site visits. The results from provincial-level CIT visits are robust to matched samples, placebo tests, alternative fixed effect and clustering specifications, endogeneity of CIT site visits concern, and alternative samples. Additional analyses suggest that local government pressure and firm bad news hiding variables mediate the impact of CIT visits on analyst optimism. In contrast, CIT site visits do not improve a firm's fundamentals. These findings are consistent with local government pressure and bad-news-hiding explanations but inconsistent with the improved firm fundamentals interpretation. Moreover, we find that the effect of CIT visits on analyst optimism is more pronounced for star, non-affiliated, and experienced analysts, supporting the notion that, because of their greater influence, local governments focus on pressuring these analysts. More importantly, we report that the impact of CIT visits on analyst optimism bias is more salient if a CIT leader had previous work experience or longer work experience in the inspected province. Interestingly, we document a reversion in analyst earnings forecast optimism 60 days after CIT site visits, especially among the non-state-owned firms, which corroborates the argument that local government officials pressure firms and after the CIT investigation, bad-news-hiding is no longer needed. Thus, the effect of the anti-corruption campaign on analyst earnings forecast optimism is transient.

We make four contributions to the literature. First, we advance the literature on the effect of government anti-corruption campaigns. Besides the impact of CIT site visits on the internal operation of a firm such as hiding information (Cao et al., 2018; Chen et al., 2018), shareholder value (Cao et al., 2018; Chen et al., 2018; Ke et al., 2018; Lin et al., 2016; Zeume, 2017) and financial reporting quality (Fan et al., 2014), we document that such anti-corruption efforts bring an unintended consequence of inflating analyst earnings forecasts. Hence, anti-corruption has material impact on a firm's external stakeholders in additional to a firm's internal response.

Second, we document that the effects of the anti-corruption campaign on analyst optimism bias is different with respect to (1) affiliated analysts vs. non-affiliated analysts, (2) star analysts vs. non-star analysts, and (3) experienced vs. less experienced analysts. Our findings echo a large branch of archival studies focuses on the divergence between affiliated analysts vs. non-affiliated analysts (Chan et al., 2020; Firth et al., 2013; Gu et al., 2013; Jiang et al., 2016) and star vs. non-star analysts (Xu et al., 2013). Essentially, analysts are not homogeneous for their response to political pressure.

Third, we document a new driver for analyst earnings forecast optimism. The literature suggests such optimism may arise from trading commissions (Jackson, 2005), personal reputation (Cowen et al., 2006), and favors to executives (Libby et al., 2008), among other reasons. We complement the literature by showing that anti-corruption campaigns contribute to analyst optimism.

Finally, we contribute to the broad literature for the impact of social networking on the effectiveness of government-led economic and political campaigns. Previous studies on social networking focus on the effect of firm-level political connections on various outcomes. For instance, Correia (2014) documents that politically connected firms are less likely to be investigated by the US Securities and Exchange Commission (SEC). Similarly, Fisman and Wang (2015) show that politically connected firms have two to three times the occupational mortality rate of non-politically connected firms. Chu et al. (2020) report that government auditors are less strict in their hometown audits. Interestingly, our findings imply that CIT inspectors are perceived to be stricter (leading to stronger analyst earnings forecast optimism) if they had previous or longer experience in their inspected provinces, suggesting that common social networking (in terms of political connection) is not perceived to be useful by local officials in the anti-corruption campaign setting. Our approach is novel by sharpening the links among regulatory enforcers, local officials, and firms by using analyst earnings forecast optimism in a social networking platform. Thus, we expand the scope of the anti-corruption literature in Lin et al. (2016), Cao et al. (2018), Chen et al. (2018), and Hope et al. (2020).

2. Background and hypothesis development

2.1. Background

The CIT is part of the Central Commission for Discipline Inspection and in 2013, was given full power to execute the government's anti-corruption campaign. Team members go to provinces to conduct thorough examinations, with the following four aims: 1) to root out corruption among provincial, city, or state-owned firms and their officials that involve personal integrity, personal gain, or bribery, 2) to refer officials for discipline if they violate the Chinese Communist Party's principles and policies, 3) to look for any violations of organizational discipline in terms of disunity, rules of employment, and the appointment of officials, and 4) to look for any violations of work and life disciplines or issues of formalism, bureaucracy, hedonism, or extravagance. CIT members have the authority to examine documents, summon officials, interview officials and citizens, and participate in officials' meetings, among other things. Local government officials must comply with the CIT's demands for information. If the CIT suspects that government officials are guilty, it will refer the cases to the Central Commission for Discipline Inspection for prosecution. By the end of July 2014, the CIT had completed regular inspections (Rounds 1–4) in 31 provinces within China. Then, during 2016 and 2017, CIT teams revisited 16 of these provinces for special inspections (Rounds 5–8). Overall, as of April 2017, CIT site visits discovered more than 140,000 violations related to approximately 1300 government officials.[1] Approximately 90% of identified cases led to government officials' being prosecuted, reprimanded, or penalized. In addition, the violations and prosecutions of local government officials were highly publicized. Thus, the deterrent effect of the anti-corruption campaign is real for local government officials, which allows us to study the possibility of various explanations for the impact of the campaign on analyst optimism. Anecdotal evidence suggests that the anti-corruption has effect on analysts. We list several cases that analysts integrate elements in the anti-corruption policy in their reports in Appendix E.

2.2. Hypothesis development

It is natural that provincial, city, and other local officials become nervous about CIT site visits. It is not surprising that some of these officials have been involved in corruption in the past, especially given that these officials have many interactions with rent-seeking firms in China's emerging markets. The rent-seeking literature suggests that a rent-seeking firm typically spends resources to bribe government officials (Lui, 1985; Beck and Maher, 1986; Aidt et al., 2008).

Given that the political careers of these officials are on the line, we expect that they will do their best to come up clean or even earn praise during CIT site visits. The baseline is to provide no signs of wrongdoing to the CIT (Cao et al., 2018) and perhaps even promote a positive personal image via complimentary media coverage to advance officials' careers (Watts and Zimmerman, 1990). Hence, local officials are motivated to apply political pressure to subordinates (local officials or state-owned firms) to minimize bad news and/or generate good news. In addition, the political advancement of government officials in China is a de facto tournament among peers and is primarily related to the economic performance of the units in their jurisdictions (Li and Zhou, 2005). Any bad news related to their jurisdictions is magnified during CIT site visits and thus, is not welcomed by local government officials.

Drawing from the findings in Cao et al. (2018), we extend their study of the impact of anti-corruption campaigns on firm bad news hiding to analyst earnings forecast optimism. We develop three possible mechanisms to explain how the anti-corruption campaigns impact analyst forecast optimism. The first explanation is local government pressure. Due to the significant personal interests at stake, local government officials apply political pressure to local or state-owned brokerages, which in turn pressure their analysts to hype earnings forecasts on firms located in the officials' jurisdictions during CIT site visit period. Then, analyst earnings forecast optimism increases. In this mechanism, analysts are not misled by firm bad news hiding, but rather exhibit excessive optimism due to the analysts' own decisions to raise earnings forecasts in response to indirect political pressure from officials on brokerage firms.

Second is the bad-news-hiding explanation, which is a straightforward extension of Cao et al. (2018). We expect that, during CIT site visit periods, local government officials will direct their subordinates to hide bad local news about firms (Chen et al., 2018). Our basic logic is that analysts are important information intermediaries in financial markets (Bradshaw, 2011). With less bad news at the local firm levels, analysts naturally provide more-optimistic earnings forecasts for their followed firms than scenarios without firm bad news hiding. Accordingly, analyst earnings forecast optimism is higher after being misled by firm bad news hiding.

Third, CIT site visits have a deterring effect. The anti-corruption campaign shakes up local officials and firm culture. Firms located in these jurisdictions face improved external and internal operating environments. All other things being equal, firm fundamentals improve, which suggests a better future for the firms. Therefore, analysts upgrade their assessment of firms during CIT site visits. Thus, we observe an increase in analyst earnings forecast optimism.

Collectively, despite the differences in the transmission mechanisms exhibited by these three explanations, they all point to an increase in analyst optimism with respect to earnings forecasts during CIT site visits. Hence, our testable hypothesis is:

H1. *: During Central Inspection Team site visits, analyst earnings forecast optimism increases.*

[1] http://www.ccdi.gov.cn/special/zyxszt/bjzl_zyxs/201706/t20170622_101538.html (accessed April 3, 2020).

3. Research design

3.1. Data

The initiation of CIT site visits is often interpreted as the symbolic start of China's recent anti-corruption campaign (Ding et al., 2020). Hence, we begin our sample period in 2013 and focus on analyst earnings forecasts during 2013–2017. We obtain financial data for firms and analyst characteristics from the China Securities Markets and Accounting Research database (CSMAR). After deleting financial firms and observations with missing firm and analyst characteristics, we are left with 599,870 analyst forecast–firm-year observations. The earnings forecasts include one-, two-, and three-year horizons. The data are winsorized at the 1% level to avoid the impact of extreme values.

In robustness checks, we reexamine the analysis by using the analyst earnings forecasts that are closest in time before, during, and after CIT site visits. For the exact dates of CIT site visits, we hand-collected the information from the website of the Central Commission for Discipline Inspection (http://www.ccdi.gov.cn/special/zyxszt/). CIT site visits initially covered 31 provinces from 2013 to 2014; in 2016–2017, 16 of them were re-inspected. Appendixes A to C present the specific timing of CIT site visits.[2]

3.2. Major variable definitions

3.2.1. Analyst earnings forecast optimism

We follow Hong and Kubik (2003) and Jackson (2005) to construct analyst forecast optimism as:

$$Optimism = (EPS\,forecast–EPS)/P^{*}100 \quad (1)$$

where *EPS forecast* represents the forecasted earnings per share and *EPS* is the actual earnings per share. *P* is the stock price on the trading day prior to the earnings forecast.

3.2.2. CIT site visit

If an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, then *Inspection* is one, and zero otherwise. We depict the definition in Fig. 1.

3.2.3. Control variables

We use the control variables in Cuculiza et al. (2019) and Cao et al. (2019b). Firm-level variables include firm size (*Size*), listing years (*Age*), board size (*Board*), board independence (*Independ*), financial leverage (*Lev*), return on assets (*ROA*), market-to-book ratio (*MB*), intangible assets (*Intangible*), stock return volatility (*Volatility*), stock turnover (*Turnover*), stock returns (*Return*), analyst following (*Analyst*), and analyst research attention (*Report*). The analyst-level variables are: analyst experience in terms of quarters (*Experience*), the number of firms an analyst follows (*Follow_num*), the number of reports issued by an analyst (*Report_num*), brokerage size (*Brokerage*), and forecast horizon (*Horizon*). Appendix D presents the detail definitions of all variables.

3.3. Model

We use the following multiple regression model to gauge the impact of CIT site visits on analyst earnings forecast optimism:

$$Optimism_{i,j,t} = \beta_0 + \beta_1{}^{*}Inspection_{i,j,t} + Controls_{i,j,t} + \alpha_{i,j,t} + \varepsilon_{i,j,t} \quad (2)$$

where $Optimism_{i,j,t}$ is the earnings forecast optimism of analyst *j* on firm *i* at *t*, $Inspection_{i,j,t}$ is a dummy variable with a value of one if analyst *j* makes an earnings forecast on firm *i* at *t* when the CIT makes a site visit in a province in which the followed firm is located, and zero otherwise; $\alpha_{i,j,t}$ represents various fixed effects. We account for province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects in Eq. (2). $\varepsilon_{i,j,t}$ is a random error term. The standard errors of estimates are two-way clustered at the firm and year level. If *H1* is valid, we expect β_1 to be positive and significant.

4. Results and discussion

4.1. Summary statistics and univariate analysis

We present the summary statistics of the sample in Panel A of Table 1. The mean (median) of *Optimism* is 1.709 (0.950) with a standard deviation of 2.937. Given the positive values of the mean and the median, and the fact that the standard deviation is almost twice (three times) the magnitude of the mean (median), it is clear that analysts' earnings forecasts typically have large variation in their optimism bias. This is consistent with the notion that Chinese analysts are optimistically biased. The mean of *Inspection* is 0.061, suggesting 6.1% of the observations are during the period of CIT site visits.

[2] The CIT conducted a total of 12 rounds of inspections. Eight of those inspections were on provinces and the remaining four were on central SOEs, government institutions, universities, etc. We examine these central SOEs in our robustness checks in Section 4.4.4.

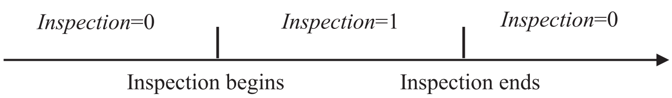

Fig. 1. Illustration of the Setup of the Inspection Variable.

Table 1
Summary Statistics and Univariate Analysis.

Panel A: Summary Statistics

Variable	N	Mean	Std	Q1	Median	Q3
Optimism	599,870	1.709	2.937	0.080	0.950	2.650
Inspection	599,870	0.061	0.239	0.000	0.000	0.000
Size	599,870	22.60	1.409	21.58	22.32	23.34
Age	599,870	1.941	0.796	1.326	1.954	2.703
Board	599,870	2.162	0.193	2.079	2.197	2.197
Independ	599,870	0.374	0.056	0.333	0.333	0.429
Lev	599,870	0.423	0.203	0.255	0.417	0.584
ROA	599,870	0.065	0.049	0.032	0.058	0.092
MB	599,870	1.843	1.764	0.676	1.276	2.389
Intangible	599,870	0.047	0.048	0.018	0.035	0.059
Volatility	599,870	0.030	0.010	0.023	0.027	0.034
Turnover	599,870	476.5	357.2	219.8	378.6	621.4
Return	599,870	0.350	0.563	−0.041	0.235	0.598
Analyst	599,870	18.28	10.87	10.00	17.00	25.00
Report	599,870	45.60	33.92	20.00	38.00	61.00
Experience	599,870	6.625	1.255	6.050	6.893	7.528
Follow_num	599,870	3.159	0.700	2.773	3.219	3.611
Report_num	599,870	4.125	0.855	3.664	4.248	4.718
Brokerage	599,870	3.920	0.547	3.638	3.970	4.263
Horizon	599,870	5.169	0.853	4.977	5.403	5.784

Panel B: Univariate Analysis of Analyst Optimism in Inspection vs. Non-Inspection Periods

(1) *Inspection = 0*		(2) *Inspection = 1*		Difference Tests	
Mean	Median	Mean	Median	t-Test	Wilcoxon Z
				(2)–(1)	(2)–(1)
1.677	0.930	2.201	1.400	33.0235 ***	35.626 ***

Panel C: Univariate Analysis of Analyst Optimism before, during, and after CIT Inspections

(1) Before Inspection		(2) During Inspection		(3) After Inspection		Difference Tests			
Mean	Median	Mean	Median	Mean	Median	t-Test		Wilcoxon Z	
						(2)–(1)	(2)–(3)	(2)–(1)	(2)–(3)
1.675	0.840	2.233	1.430	1.678	1.000	31.801 ***	34.805 ***	40.828 ***	32.036 ***

This table reports the summary statistics for the variables used in this paper. Panel A reports all the key variables during the sample period. Panel B reports the nonparametric test (t-test and *Z*-test) results of *Optimism* for subsamples of *Inspection* = 1 vs. *Inspection* = 0. Panel C reports the nonparametric test (*t*-test and Z-test) results of *Optimism* for subsamples of before, during, and after CIT site visits. Detailed definitions of all variables are provided in Appendix D.

In Panel B of Table 1, we compare the means and the medians of *Optimism* for subsamples of *Inspection = 1* versus *Inspection = 0*. Both the mean and the median of the *Inspection = 1* subsample are larger than those of *Inspection = 0* subsample. The results of the *t*-test for the means and the *Z*-test for the medians are significant at the 1% level, suggesting analyst optimism is greater when the CIT conducts site visits in a province where a firm is located.

In Panel C of Table 1, we further present the means and medians of *Optimism* before, during, and after CIT site visits. Both the *t*-test and *Z*-test show that the mean and median of *Optimism* during CIT site visits are larger than before and after CIT site visits. The findings suggest that analysts raise their earnings forecasts during CIT site visits compared to their pre-visit forecasts. Analyst earnings forecasts revert to their prior levels after the CIT departed. The results in Panels B and C preliminarily support *H1*.

4.2. Baseline results

We present the baseline findings for Eq. (2) in Table 2. Besides the full model, we include two simplified models for robustness. Consistently across columns (1)–(3), the coefficients of *Inspection* are positive and significant at the 1% level, suggesting that analyst optimism increases during CIT site visits in a province. The results are economically significant. Using the full model in column (3) of

Table 2
The Impact of CIT Inspections on Analyst Forecast Optimism.

Dep. Var = Optimism	(1)	(2)	(3)
Inspection	0.199***	0.200***	0.125***
	(4.41)	(4.52)	(2.90)
Size		0.106***	0.098**
		(2.65)	(2.49)
Age		−0.279***	−0.285***
		(−6.57)	(−6.77)
Board		−0.404**	−0.421**
		(−2.11)	(−2.22)
Independ		−0.187	−0.233
		(−0.33)	(−0.41)
Lev		1.397***	1.406***
		(7.49)	(7.61)
ROA		5.435***	5.266***
		(7.06)	(6.93)
MB		0.005	0.003
		(0.27)	(0.19)
Intangible		−1.110**	−1.131**
		(−2.31)	(−2.38)
Volatility		22.289***	21.175***
		(3.57)	(3.44)
Turnover		−0.000**	−0.000**
		(−2.40)	(−2.31)
Return		−0.227***	−0.229***
		(−3.78)	(−3.86)
Analyst		−0.014*	−0.016**
		(−1.77)	(−2.09)
Report		−0.001	−0.000
		(−0.31)	(−0.12)
Experience			0.011
			(1.06)
Follow_num			0.008
			(0.16)
Report_num			−0.049
			(−1.26)
Brokerage			−0.068*
			(−1.96)
Horizon			0.355***
			(21.95)
Constant	1.697***	−0.293	−1.465
	(64.29)	(−0.30)	(−1.48)
Province F.E.	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*
N	599,870	599,870	599,870
Adj_R^2	0.162	0.172	0.181

This table reports the effect of CIT inspections on analyst forecast optimism. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level . *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 2, the coefficient of *Inspection* is 0.125. That is, during a CIT site visit, analysts, on average, increase their earnings forecasts by 0.125. Given the median of *Optimism* is 0.950, a 0.125 increase represents an approximately 13% increase in optimism for a typical firm.

For the analyst-level control variables, we find the coefficient of *Brokerage* is negative and significant at the 10% level in column (3), suggesting that when the brokerage is larger (in terms of number of analysts employed), the analyst forecast is more accurate. In contrast, the coefficient of *Horizon* is positive and statistically significant, indicating that when facing a longer forecasting horizon, forecasts are less accurate. The findings are consistent with those in Cao et al. (2019b) and Huyghebaert and Xu (2015).

4.3. Transmission mechanisms

In this section, we closely examine three mechanisms, including local government pressure, firm bad news hiding, and improved firm fundamentals.

4.3.1. Local government pressure

We contend that during that time, local government officials pressure brokerages to urge analysts to engage in overly optimistic earnings forecasts. The political pressure is effective only if local government officials have sufficient influence. Therefore, we examine whether local government officials are able to exert pressure on: (1) local brokerages, (2) state-owned firms, and (3) state-owned brokerages in their jurisdiction.

If the mechanism is local government pressure, we expect analysts employed by local or state-owned brokerages exhibit stronger optimism bias than those of non-local or non-state-owned brokerages because local government officials have a political channel to pressure these brokerages. Specifically, we define *Local* equals to one if the analyst is from a local brokerage and zero otherwise; *SOE* equals to one if the ultimate controller of a listed firm is a government-owned entity and zero otherwise; and *State* equals to one if the local brokerage is state-owned and zero otherwise.

Following Pevzner et al. (2015), we use a structural equation model (SEM) to conduct the mediation tests for the impact of CIT (*Inspection*) on analyst earnings forecast optimism (*Optimism*). The SEM analysis includes a regression of analyst earnings forecast optimism on CIT and mediating variables and regressions of the mediating variables on CIT, with a number of control variables included in all regression equations. The mediation variables include *Local, Local & SOE*, *Non-Local & SOE*, *State*, *State & SOE*, and *Non-State & SOE*. These are dummy variables to capture the role of ownership nature of the firm and brokerage for the impact of CIT on analyst earnings forecast optimism. We note that these mediation variables are independent of CIT. Therefore, we consider the different local government pressure channels are transmitted via firm, analyst location, and brokerage ownership status on analyst optimism. We outline the several analyses of specific paths for the impact of CIT (*Inspection*) on analyst earnings forecast optimism (*Optimism*) below for clarity:

A. We use *Local* as a mediator variable for the full sample (i.e., we do not consider the effect of brokerage and firm state-ownership). We denote the path as *Local*. We expect the path to be significant;
B. Using only local analyst subsample (i.e., *Local* = 1), we study the mediation effect of state ownership. We denote the path as *Local & SOE*. We expect the path is significant.
C. Using only non-local analyst subsample (i.e., *Local* = 0), we study the mediation effect of state ownership. We denote the path as *Non-Local & SOE*. We expect the path of *Non-Local & SOE* be insignificant.
D. Using only local analyst subsample (i.e., *Local* = 1), we study the mediation effect of state-owned brokerage (*State* = 1*)*. We denote the path as *State*. We expect the path be significant.
E. Using only the subsample of local analysts from state-owned brokerage (*Local* = 1 & *State* = 1), we study mediating effect of *SOE* and denote the path as *State & SOE*. If the testable hypothesis is valid, we expect that the path is significant.
F. Using only the subsample of local analysts from non-state-owned brokerage (*Local* = 1 & *State* = 0), we study the mediating effect of *SOE*. We denote the path as *Non-State & SOE*. We expect the path to be insignificant.

We present the results in Panels A and B of Table 3. In columns (1) and (2) in both Panels and column (3) in Panel B, the direct paths for the impact of *Inspection* on *Optimism* are significant. For the indirect path, the paths of *Local, Local & SOE, State* and *State & SOE* in columns (1) and (2) in both Panels have significant *Z*-values (Z = 46.73, 4.19, 7.55, and 2.78) for the row of *P(Inspection, Path)*P(Path, Optimism)*, suggesting that the impact of *Inspection* on *Optimism* through the channels of local analysts, state-owned firms, and state-owned brokerages. Most importantly, for less local government pressure path (i.e., *Non-Local & SOE* and *Non-State & SOE*), the Z-values are not significant. The results are consistent with the logic in the testable hypothesis.

Collectively, we find that *Inspection* has significant indirect effects on *Optimism* through all "local government pressure" mediating variables.[3] We interpret the findings as that the increase in analyst optimism bias originates from local brokerage, especially from state-owned local brokerage. The mediating effect is weak if analysts are not local or the brokerage firm is non-state owned.

4.3.2. Firm bad news hiding

It is also possible that firms hide bad news during CIT site visits. With less firm-specific bad news in the market, analysts' earnings forecasts are much higher than forecasts during non-CIT site visit periods.

For robustness, we use four approaches to gauge the extent of firm bad news hiding. First, we follow Cao et al. (2018) and Chen et al. (2001) to use stock price crash risk. Specifically, we take the negative of the third moment of a firm's daily market adjusted excess returns scaled by its cubed standard deviation in each month to calculate the negative coefficient of skewness (*NCSKEW*) (Kim et al., 2011a, 2011b). High (low) crash risk means a firm hide less (more) negative news.

Second, we use a firm's financial statement transparency in terms of its discretionary accruals (Hutton et al., 2009). Specifically, we use the modified Jones model (Dechow et al., 1995) to calculate the discretionary accruals in the year. High (low) discretionary accruals mean more (less) firm bad news hiding.

Third, we use the dispersion of all analysts' earnings forecasts for a firm in the year to gauge the extent of firm bad news hiding. High (low) dispersion means hiding more (less) bad news. Specifically, the dispersion of all analysts' earnings forecasts is measured as

[3] The findings using Sobel-Goodman Mediation Tests yield similar conclusions.

Table 3
Transmission Mechanism: Local government pressure.

Panel A: Path analysis of the effect of *Inspection* on *Optimism* through *Local, Local & SOE* and *Non-Local & SOE*

	(1)		(2)		(3)	
	Path = *Local*		Path = *Local & SOE*		Path = *Non-Local & SOE*	
	Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat
Direct Path						
P(Inspection, Optimism)	0.272***	13.80	0.300***	10.70	0.165***	5.61
Mediated Path						
P(Inspection, Path)	0.284***	91.43	0.073***	5.22	0.006	1.27
P(Path, Optimism)	0.568***	54.36	0.034***	7.05	0.396***	16.11
*P(Inspection, Path)*P(Path, Optimism)*	0.161***	46.73	0.002***	4.19	0.002	1.26
N	371,677		113,763		257,914	
Standardized root mean squared residual	0.011		0.059		0.054	

Panel B: Path analysis of the effect of *Inspection* on *Optimism* through *State, State & SOE* and *Non-State & SOE*

	(1)		(2)		(3)	
	Path = *State*		Path = *State & SOE*		Path = *Non-State & SOE*	
	Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat
Direct Path						
P(Inspection, Optimism)	0.172***	5.57	0.202***	5.80	0.027	0.42
Mediated Path						
P(Inspection, Path)	0.044***	10.98	0.012***	2.84	0.0148	1.38
P(Path, Optimism)	0.249***	10.41	0.453***	15.27	−0.311***	−6.31
*P(Inspection, Path)*P(Path, Optimism)*	0.011***	7.55	0.005***	2.78	−0.005	−1.35
N	106,385		83,043		23,342	
Standardized root mean squared residual	0.014		0.060		0.056	

This table reports the "local government pressure" path analysis results of the effect of *Inspection* on *Optimism* by using structural equation model (SEM). The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, Multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. *Local* equals to one if the analyst is from a local brokerage and zero otherwise. *SOE* equals to one if the ultimate controller of a listed firm is a government-owned entity and zero otherwise. *State* equals to one if the local brokerage is state-owned and zero otherwise. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects, analyst fixed effects and year fixed effects. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

the cross-sectional standard deviation of individual analysts' last annual forecast, scaled by the stock price at the end of the prior fiscal year (De Franco et al., 2011; Peterson et al., 2015).

Last, we use the readability of a firm's annual report. High (low) readability means less (more) firm bad news hiding (You and Zhang, 2009; Loughran and McDonald, 2014). We follow the methods in Li (2008) and Lawrence (2013) by focusing on the length of an annual report to define its readability.

For the firm bad news hiding channel, we conduct similar analyses using a SEM in the local government pressure channel. We use four variables: *Crash risk, Transparency, Dispersion,* and *Readability*. We present the findings in Table 4. For direct path, all Z statistics are significant, suggesting that *Inspection* has a direct effect on *Optimism.* For indirect path, the *Z*-values of *P(Inspection, Path)*P(Path, Optimism)* are 1.80, 10.90, 3.77, and 18.91 in columns (1) to (4), respectively. Hence, *Inspection* has significant indirect effects on *Optimism* through all "firm bad news hiding" mediating variables.[4] The results are consistent with the notion that, during CIT visits, hiding bad news is a channel for the increase of analyst optimism bias.

4.3.3. Improved firm fundamentals

We follow Antoniou et al. (2017) to use credit rating (*CR*), stock price volatility (*Volatility*), return on assets (*ROA*), return on sales (*ROS*), sales/total asset (*Sales*), and price-earnings ratio (*PE*) to gauge a firm's fundamentals during various time periods after CIT site visits to examine the improved firm fundamentals explanation:

$$Fundamental_{i,t} = \beta_0 + \beta_1 Year0_{i,t} + \beta_2 Year1_{i,t} + \beta_3 Year2_{i,t} + Controls_{i,t} + \alpha_{i,t} + \varepsilon_{i,t} \quad (3)$$

where $Fundamental_{i,t}$ is *CR, Volatility, ROA, ROS, Sales,* or *PE*. $Year0_{i,t}$ to $Year2_{i,t}$ are dummy variables with values of one if the firm-year observations are in CIT visit year, one year after the visit, or two years after the visit, respectively, and zero otherwise. We include a set of firm-level control variables that are the same as those in Eq. (2) and control for province, industry, and year fixed effects. The

[4] The findings using Sobel-Goodman mediation tests yield similar conclusion.

Table 4
Transmission Mechanism: Firm bad news hiding.

	(1)		(2)		(3)		(4)	
	Path = *Crash Risk*		Path = *Transparency*		Path = *Dispersion*		Path = *Readability*	
	Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat	Coefficient	Z-stat
Direct Path								
P(Inspection, Optimism)	0.465 ***	29.03	0.455***	26.69	0.439***	30.19	0.435***	28.21
Mediated Path								
P(Inspection, Path)	−0.001***	−2.90	0.016***	15.61	0.025***	3.77	0.027***	20.72
P(Path, Optimism)	−0.104***	−2.28	0.375***	15.22	0.804***	275.06	0.850***	46.19
*P(Inspection, Path)*P(Path, Optimism)*	0.0001*	1.80	0.006***	10.90	0.020***	3.77	0.023***	18.91
N	557,403		507,017		599,855		582,145	
Standardized root mean squared residual	0.002		0.017		0.028		0.045	

This table reports the "firm bad news hiding" path analysis results of the effect of *Inspection* on *Optimism by using* structural equation model (SEM). The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. *Crash risk* is the negative coefficient of skewness, measured as the negative of the third moment of firm's daily market adjusted excess returns scaled by its cubed standard deviation in each month. *Transparency* measured as the absolute value of discretionary accruals by using the modified Jones model at the beginning of the year. *Dispersion* is the cross-sectional standard deviation of individual analysts' last annual forecasts, scaled by the stock price at the end of the prior fiscal year. *Readability* of a firm's annual report is measured as the length (vocabulary numbers) of an annual report for the prior year. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects, analyst fixed effects and year fixed effects.*, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

standard errors of estimates in Eq. (3) are clustered at the firm and year level. If CIT site visits are effective in improving firm fundamentals, we expect β_1, β_2, and β_3 to be positive and significant. We present the findings in Table 5. Across all six metrics of firm fundamentals, none of the coefficients of *Year0, Year1,* and *Year2* are significant. Hence, the results do not support the notion that CIT site visits change firm fundamentals. Therefore, the findings in Table 5 do not support the improved firm fundamentals explanation.

4.4. Robustness checks

4.4.1. The endogeneity of CIT site visits

While the literature suggests that CIT site visits are exogenous (Cao et al., 2018; Ding et al., 2020), the decision to go to specific provinces may still be endogenous. We address the potential endogeneity in two approaches. In the first approach, we reason that the earlier rounds of the CIT cite visit should be relative more an exogenous shock than the following rounds. Thus, we conduct two subsample tests to alleviate the concern. First, we partition the full sample into subsamples between the regular inspections (Rounds 1–4) and the special inspections (Rounds 5–8). Appendix A present the specific timing of CIT site visits. Second, we separate the sample to Round 1 and Rounds 2–4 inspection subsamples to reexamine the baseline results. We present the findings in Table 6. For the subsample of regular inspections (Rounds 1–4) in columns (1), the coefficient of *Inspection* is 0.121 (t = 3.54), which is positive and significant at the 1% level. In contrast, for the special inspection subsample (Rounds 5–8) in columns (2), the coefficient of *Inspection* is −0.001 (t = −0.01), which is insignificant. Most important, the difference in the coefficients between columns (1) and (2) is significant at the 1% level. For Round 1 subsample in column (3) and Rounds 2–4 subsample in column (4), the coefficients of *Inspection* are both positive and significant at the 1% and 5% level, respectively. However, the coefficient of *Inspection* in column (3) is 0.598, which is more economically and statistically significant than the 0.079 in column (4). Collectively, the results in Table 6 suggest that, after addressing the endogeneity concerns on CIT, the baseline findings continue to be robust.

In the second approach, we follow Avis et al. (2018), Chu et al. (2020), and Colonnelli and Prem (2020) to directly examine whether the decision to conduct the first round CIT visit is random. In the first step, we compare the major provincial economic, sociological, political, and government official attributes between provinces with and without CIT visit in the first round. We present the results in Panel A of Table 7. The means and medians in both subsamples are not statistically significant in a wide range of attributes (such as per capita GDP, population, turnover of communist party secretary (CPS), gender of governor and CPS, among others). Using the same set of variables in Panel A, we follow Colonnelli and Prem (2020) to conduct a multivariate regression analysis to examine the determinants of conducting Round 1 CIT (*Inspection_First* = 1 if a firm is subject to Round 1 inspection) and any inspection (*Inspection*). The results in Panel B show that, with the exception of *Gender_Secretary* (the gender of the province communist party secretary, CPS) and *Home_Secretary* (whether the CPS was born in the same province) in column (2), all other variables are not significant. Therefore, the results in Table 7 indicate that the decision to go to specific provinces, while not completely random, does not depend on many provincial attributes.

4.4.2. Matching samples

While it is likely that the selection of CIT site visits is random, we follow Bartram et al. (2012) and Cao et al. (2018) to conduct a matching sample analysis. For each treatment firm (those located in CIT site visit provinces), we find a control firm (those not located in CIT site visit provinces) located in a similar province (by GDP), with similar earnings per share (EPS), that has the same analyst

Table 5
Transmission Mechanism: Change of Firm Fundamentals.

Dep. Var=	(1)	(2)	(3)	(4)	(5)	(6)
	CR	*Volatility*	*ROA*	*ROS*	*Sales*	*PE*
Year0	0.022	0.000	0.001	−0.001	0.004	5.409
	(1.95)	(1.09)	(0.94)	(−0.25)	(0.52)	(0.99)
Year1	0.028	0.000	0.001	−0.001	0.002	3.428
	(1.67)	(0.90)	(0.49)	(−0.43)	(0.24)	(0.63)
Year2	0.018	−0.000	0.001	−0.001	−0.010	−0.764
	(0.95)	(−0.98)	(1.45)	(−0.48)	(−1.86)	(−0.18)
Firm Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	1514	8427	8427	8427	8427	8427
Adj_R^2	0.696	0.827	0.637	0.598	0.878	0.280

This table reports the channel test results of improved firm fundamentals. The dependent variables are firm fundamentals, including credit rating (*CR*), stock price volatility (*Volatility*), return on assets (*ROA*), return on sales (*ROS*), sales/total asset (*Sales*), and price-earnings ratio (*PE*). *Year0* to *Year2* are dummy variables with values of 1 if the observation is in the current year, 1-year after, and 2-year after of CIT site visit, respectively. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level . *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6
Subgroup Analyses According to Inspection Rounds.

Dep.Var = Optimism	(1)	(2)	(3)	(4)
	Regular inspections (Rounds 1–4)	Special inspections (Rounds 5–8)	Round 1	Rounds 2–4
Inspection	0.121***	−0.001	0.598***	0.079**
	(3.54)	(−0.01)	(3.48)	(2.31)
Firm Controls	*Yes*	*Yes*	*Yes*	*Yes*
Analyst Controls	*Yes*	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*
N	426,829	72,935	36,873	389,956
Adj_R^2	0.340	0.559	0.286	0.348
Difference in Estimated Coefficients of Inspection	0.122***		0.519***	

This table reports the regression results according to inspection rounds. Columns (1), (2), (3) and (4) use subsamples of regular inspection (Rounds 1–4), special inspection (Rounds 5–8), Round1 and Rounds 2–4, respectively. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

covering both firms. The results for the matched samples in column (1) of Table 8 show that the coefficient of *Inspection* remains positive and significant at the 5% level, which is consistent with the baseline results in Table 2.

4.4.3. Alternative specifications

The baseline results in Table 2 use province fixed effects, industry fixed effects, analyst fixed effects and year fixed effects. For robustness, we use alternative specifications of fixed effects and cluster standard errors at different levels. In columns (2)–(3) of Table 8, we use firm, year and analyst fixed effects and cluster standard errors at the firm and analyst levels, respectively. Column (4) reports the results of using firm×year fixed effects, analyst fixed effects, clustering standard errors at the province and year levels. The results in columns (2)–(4) suggest that the coefficients of *Inspection* remain positive and significant at the 1% or 5% level, which indicates that our results are robust to alternative specifications of fixed effects and standard error clustering methods.

We use all earnings forecasts by analysts in the baseline regressions. For robustness, we keep only the latest EPS forecast (most nearby) to CIT site visit. The results in column (5) of Table 8 show that the coefficient of *Inspection* is positive and significant at the 5% level.

The earnings forecasts include one-, two-, and three-year horizons. The forecasts at two-year or three-year horizons could be biased due to the long horizons. Therefore, we re-run regressions to estimate Eq. (2) by using the subsample of earnings forecast at only one-

Table 7
Are CIT Visits Random?

Panel A: Univariate analysis on the validity of the randomization of CIT

Variable	(1) *Firstround = 0*		(2) *Firstround = 1*		Difference Tests	
	Mean	Median	Mean	Median	*t*-tests	Wilcoxon Z
					(2)–(1)	(2)–(1)
GDPper	10.61	10.49	10.47	10.56	0.697	0.322
Corruption	23.17	22.36	22.57	21.59	0.153	0.215
Population	17.30	17.45	17.42	17.37	−0.285	0.215
Turnover_Secretary	0.385	0.000	0.600	1.000	−0.876	−0.879
Turnover_Governor	0.462	0.000	0.400	0.000	0.245	0.249
Experience_CIT	0.077	0.000	0.000	0.000	0.624	0.631
Gender_Secretary	1.000	1.000	1.000	1.000	0.000	0.000
Age_Secretary	59.42	60.00	61.00	62.00	−0.740	−0.891
Term_Secretary	3.114	3.466	2.895	3.219	0.345	0.323
Home_Secretary	0.077	0.000	0.000	0.000	0.624	0.631
Gender_Governor	0.962	1.000	1.000	1.000	−0.433	−0.439
Age_Governor	57.96	58.50	57.60	57.00	0.208	0.514
Term_Governor	2.792	3.218	3.264	3.332	−0.707	−0.108
Home_Governor	0.231	0.000	0.000	0.000	1.185	1.177
Observations	26		5		–	

Panel B: Multivariate analysis on the validity of the randomization of CIT visits

Dep. Var =	(1)	(2)
	Inspection_First	*Inspection*
GDPper	−0.215	0.147
	(−0.83)	(0.26)
Corruption	−0.004	−0.025
	(−0.46)	(−1.36)
Population	0.026	0.637
	(0.29)	(0.15)
Turnover_Secretary	0.219	0.070
	(1.06)	(0.69)
Turnover_Governor	0.015	0.164
	(0.06)	(1.11)
Experience_CIT	−0.127	0.322
	(−0.56)	(0.81)
Gender_Secretary	0.000	−0.450*
	(0.000)	(−2.59)
Age_Secretary	0.013	−0.017
	(0.59)	(−0.53)
Term_Secretary	0.022	0.008
	(0.22)	(0.11)
Home_Secretary	−0.033	−0.787*
	(−0.15)	(−2.21)
Gender_Governor	0.224	0.149
	(0.92)	(0.53)
Age_Governor	−0.021	−0.002
	(−0.86)	(−0.07)
Term_Governor	0.092	0.066
	(0.96)	(1.22)
Home_Governor	−0.292	−0.084
	(−1.35)	(−0.29)
Constant	1.930	−10.617
	(0.95)	(−0.16)
Province_FE	*No*	*Yes*
Year_FE	*No*	*Yes*
N	31	150
Adj_R^2	0.238	0.029

This table reports the validity of the randomization of CIT inspection. Panel A presents the summary statistics and Panel B presents the multiple OLS regression results. *Inspection_First* is a dummy variable with a value of one if a firm is located in a province with Round 1 CIT visit and zero otherwise. *Inspection* is a dummy variable with a value of one if firm is located in a province with CIT visit (no matter which round the inspection is conducted) and zero otherwise. The set of control variables (lagged one period) includes: per capita gross domestic product (*GDPper*), which is the natural logarithm of a province's per capita gross domestic product; corruption level (*Corruption*), which is the ratio the number of indicted corruption cases to total number of government employees in a province; population (*Population*), which is the natural logarithm of population in a province; *Turnover_Secretary* is a dummy variable with a value of one if a province has a new communist party secretary (CPS) and zero otherwise; *Turnover_Governor* is a dummy variable with a value of one if a province has a new governor and zero otherwise; *Experience_CIT* is a dummy variable with value of one if the leader or deputy leader of the CIT has previous CIT experience and zero otherwise; *Gender_Secretary* has a value of one if the CPS is male and zero otherwise; *Age_Secretary* is the age of CPS; *Term_Secretary* is the natural logarithm of tenure of CPS in months; *Home_Secretary* is a dummy variable with a value of one if the CPS was born in the same province and zero otherwise; *Gender_Governor* has a value of one if the governor is male and zero otherwise; *Age_Governor* is the age of the governor; *Term_Governor* is the natural logarithm of tenure of the governor in months; and *Home_Governor* is a dummy variable with a value of one if the governor was born in the same province and zero otherwise. The regression in column (1) of Panel B does not include any fixed effects while the regression in column (2) Panel B includes province fixed effects and year fixed effects. We report robust standard errors in column (1) while year and province clustered error in column (2). *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

Table 8
Robustness Checks.

Dep. Var= Optimism	(1) Matched Sample	(2) Cluster at Firm	(3) Cluster at Analyst	(4) Cluster at Province & Year	(5) Latest Forecast	(6) One-Year Horizon Forecast	(7) Central SOEs
Inspection	0.109**	0.104**	0.104***	0.096**	0.062**	0.167***	0.191*
	(3.84)	(2.49)	(5.82)	(3.35)	(3.32)	(2.89)	(2.55)
Firm Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Analyst Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*Yes*
Firm F.E.	*No*	*Yes*	*Yes*	*No*	*No*	*No*	*No*
Firm×Year F.E.	*No*	*No*	*No*	*Yes*	*No*	*No*	*No*
N	269,409	599,870	599,870	599,870	57,405	205,841	61,288
Adj_R^2	0.458	0.344	0.344	0.468	0.423	0.241	0.276

This table reports the results of robustness checks. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, Multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. Column (1) reports the results of the matched sample. In columns (2)–(3), we use firm, year and analyst fixed effects and cluster standard errors at firm or analyst level, respectively. Column (4) reports the results of using firm×year and analyst fixed effects and clustering standard errors at province and year level. Column (5) reports the results of the latest analyst earnings forecast (most nearby) to CIT site visit subsamples. Column (6) reports the results of the subsample of earnings forecast at only one-year horizon and column (7) reports the results of CIT site visits to central state-owned firms. The detailed definitions of all variables are provided in Appendix D. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

year horizon. We present the baseline findings for Eq. (2) in column (6) of Table 8. The coefficients of *Inspection* are positive and significant at the 1% level, suggesting that analyst optimism increases during CIT site visits in a province. Thus, our results are robust to alternative earnings forecasts horizon.

Our previous analysis focuses on the impact of CIT site visits on analyst optimism concerning firms located in different provinces. Besides specific provinces, the CIT also inspects central SOEs.[5] Hence, we examine these central SOEs inspected by the CIT as an alternative sample. The results in column (7) of Table 8 indicate that the coefficient of *Inspection* is positive and significant. Hence, CIT site visits affect the information production of analysts, leading them to be more optimistic about the performance of these central SOEs. We note that the local government pressure explanation does not apply here. Instead, these central SOEs have incentives to hide bad news to make them look good during CIT site visits.

4.4.4. Placebo tests

To mitigate the possibility that the results in Table 2 occur due to randomness, we conduct placebo tests. Specifically, we randomize CIT site visit provinces and inspection times to estimate Eq. (2) and repeat it 500 times. Table 9 presents the results. In column (1), by randomizing the provinces, the mean of the coefficients of *Inspection* is −0.001, of which approximately 0.8% are statistically significant at the 1% level. In column (2), after randomizing the inspection time, the mean coefficient of *Inspection* is −0.001, of which approximately 0.4% is statistically significant at the 1% level. Given that both the 0.8% and 0.4% coefficients are small, our baseline findings are not due to randomness and suggest that the impact of CIT site visits on optimism of analyst earnings forecasts reflects economically meaningful results.

[5] Executives in central SOEs are quasi-government employees who may engage in corruption. They report directly to specific ministries of the government. Their ranks are equivalent to provincial officials.

Table 9
Placebo Tests.

(1) *Random Province*	*Optimism*	(2) *Random Time*	*Optimism*
Mean β for Inspection	−0.001	*Mean β for Inspection*	−0.001
Mean t for Inspection	−0.040	*Mean t for Inspection*	−0.112
Mean std for Inspection	0.013	*Mean std for Inspection*	0.014
Mean p-value for Inspection	0.498	*Mean p-value for Inspection*	0.490
[% $\beta > 0$ & p-value $\leq$ 1%]	0.600%	*[% $\beta > 0$ & p-value $\leq$ 1%]*	0.200%
[% $\beta < 0$ & p-value $\leq$ 1%]	0.200%	*[% $\beta < 0$ & p-value $\leq$ 1%]*	0.200%
(% $\lvert\beta\rvert > \lvert\beta^*\rvert$ & $\beta \times \beta^* > 0$ & p-value $\leq$ 1%)	0.000%	*(% $\lvert\beta\rvert > \lvert\beta^*\rvert$ & $\beta \times \beta^* > 0$ & p-value $\leq$ 1%)*	0.000%

This table reports summary statistics of the placebo regression estimates for the baseline models in Table 2, where we randomize CIT site visit provinces and inspection time to estimate Eq. (2) and repeat it 500 times, respectively. We report the mean coefficient estimates, t-values and standard errors for the main independent variable (*Inspection*) across the 500 replications. In brackets, we report the percentage of coefficient estimates that are positive and significant at the 1% level [% $\beta > 0$ & *p-value* $\leq$ 1%] or negative and significant at the 1% level [% $\beta < 0$ & *p-value* $\leq$ 1%]. In parentheses, we report the percentage of coefficient estimates that have larger absolute value than and the same sign as our baseline estimates from Table 2 and is significant at the 1% level (% $|\beta| > |\beta^*|$ & $\beta \times \beta^* > 0$ & *p-value* $\leq$ 1%). The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, Multiplied by 100. All control variables, fixed effects, as well as clustering method of robust standard errors follow Table 2, but not shown for brevity.

4.5. Additional analysis

4.5.1. The impact of analyst heterogeneity

Analysts are not homogenous. Different personal attributes or facing potential conflicts of interests drive analyst optimism bias. The literature suggests that analyst employers' business ties (affiliated) matter (Chan et al., 2020; Firth et al., 2013; Gu et al., 2013; Jiang et al., 2016). In addition, an analyst's personal skillset contributes to their ability. Xu et al. (2013) suggests that star status and experience matter. Hence, we include three analyst attributes. First, we follow Gu et al. (2013) to use *Affiliation*. It is an indicator variable that equals one if an analyst's brokerage receives commission fees in quarter q-1 from any fund company that has stock in its top ten holdings at the end of quarter q-1, and zero otherwise. Because commission fees are available on a half-year basis, this variable is measured using commission fee information in quarter q-1 for the 1st and 3rd quarter and in quarter q-2 for the 2nd and 4th quarter. Second, we follow Xu et al. (2013) to define *Star*, which is an indicator that equals one if an analyst j is ranked by *New Fortune* magazine as a star analyst in the year in which he follows firm i, and zero otherwise. Third, we follow Xu et al. (2013) to define *Experience* as an indicator that equals to one if an analyst has longer working experience using the median experience of analysts (experienced analysts), and zero otherwise. We measure experience as natural logarithm of 1 plus the number of quarters between first quarters making forecasts to current quarter.

Table 10
Analyst Heterogeneity.

Dep. Var = Optimism	*Affiliation*		*Star*		*Experience*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Yes	*No*	*Yes*	*No*	*long*	*short*
Inspection	0.052	0.146**	0.132*	0.066	0.128**	0.043
	(1.13)	(2.92)	(1.72)	(1.19)	(2.61)	(0.95)
Firm Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Analyst Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	351,959	247,911	173,319	426,551	300,212	299,658
Adj_R^2	0.177	0.172	0.164	0.184	0.171	0.184
Difference in Estimated Coefficients of Inspection	−0.094***		0.066***		0.085***	

This table reports the separation analyses results according to analyst heterogeneity. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. *Affiliation* is an indicator variable that equals one if an analyst's brokerage receives commission fees in quarter q-1 from any fund company that has stock in its top ten holdings at the end of quarter q-1, and zero otherwise. *Star* is an indicator that equals one if an analyst j is ranked by New Fortune magazine as a star analyst in the year in which he follows firm i, and zero otherwise. *Experience* is an indicator that equals to one if an analyst has longer working experience using the median experience of analysts (experienced analysts), and zero otherwise. We measure experience as natural logarithm of 1 plus the number of quarters between first quarters making forecasts to current quarter. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors double clustered at the firm and year level . *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

Based on the three attributes, we conduct our subsample analyses by: (1) affiliated analysts vs. non-affiliated analysts, (2) star analysts vs. non-star analysts, and (3) experienced vs. less experienced analysts. We present the findings in Table 10. Across all three metrics of analyst heterogeneity, the coefficients of *Inspection* are positive and statistically significant at the 5% or 10% level for the subsamples of non-affiliated, star, and experienced analysts in columns (2), (3), and (5). In contrast, the same coefficients are insignificant in their counter groups in columns (1), (4), and (6). The tests in the last row of the table show that the differences on the coefficients between the subsamples are significant at the 1% level.

We interpret the findings in columns (1) and (2) as that affiliated analysts have already had high incentives to make optimism bias forecast because their employers face commission fee pressure from their clients (Gu et al., 2013; Jiang et al., 2016). As a result, affiliated analysts have less room for further optimism during the CIT visits. On the contrary, non-affiliated analysts have room to make such upward adjustment in their forecasts. The results in columns (3)–(6) show that star-analysts and experienced analysts made more optimistic predictions during the CIT visit period. Besides the similar interpretation as above, the other possible reason is: because of their greater influence, local governments will focus on the forecasting behavior of such analysts, which can better reflect the existence of local government pressure mechanisms.

Collectively, during CIT visits, analysts are under pressure to make optimistic forecasts even if they are less optimistically biased during non-CIT periods. These non-affiliated, star, or experienced analysts have greater influence. Consequently, local governments put up pressure on them. The findings in Table 10 show support to the local government pressure mechanisms.

4.5.2. *The impact of CIT leaders' prior work experience*

We conjecture that CIT leaders themselves play a key role during inspections. Specially, the connection via CIT leaders' prior work experience in the inspected provinces has two possible opposite effects. When CIT leaders have prior work experience in the inspected provinces, especially lengthy work experience, they have had more opportunities to form complex social networks with local officials. Such a social network between local officials and CIT leaders may predispose CIT leaders to give favorable treatment to local officials. Thus, local government officials find less need to rely on analyst optimism or hide firms' bad news during CIT visits. In contrast, given that CIT leaders have prior work experience in the inspected provinces, they know more about local government and local firms. Given this familiarity, they can effectively conduct corruption inspections on local government officials. To counter the expected effectiveness of inspections, local officials increase their efforts to hide bad news by increasing political pressure on analysts (or on local firms and brokerages), which leads to an increase in analyst optimism. We do not have any a priori reasons to expect either effect to be supported.

To investigate the research question, we partition the full sample into subsamples with and without a CIT leader's prior experience in the inspected province. The results are presented in columns (1) and (2) of Table 11. The coefficient of *Inspection* is positive and statistically significant at the 1% level in column (1) while not significant in column (2), suggesting that analyst optimism increases when a CIT leader has prior work experience in the inspected provinces. In addition, the coefficient is significantly larger in column (1) than in column (2), indicating that if a CIT leader has prior work experience in the inspected provinces, the impact of the CIT on analyst

Table 11
The Impact of CIT Leaders' Previous Work Experience in the Inspected Province.

Dep. Var = Optimism	*Local Work Experience*				*Length of Work Experience*			
	Leader		Leader and Deputy		Leader		Leader and Deputy	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Yes	*No*	*Yes*	*No*	*long*	*short*	*long*	*short*
Inspection	0.307***	0.063	0.299***	0.059	0.419***	−0.079	0.411***	−0.010
	(4.77)	(1.45)	(4.95)	(1.33)	(5.63)	(−1.51)	(5.58)	(−0.17)
Firm controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Analyst Controls	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
N	79,380	520,490	84,128	515,742	56,601	22,779	57,417	26,711
Adj_R^2	0.365	0.347	0.383	0.345	0.464	0.469	0.478	0.467
Difference in Estimated Coefficients of Inspection	0.244***		0.240***		0.498***		0.421***	

This table reports the test results of CIT leaders (or deputy leaders)' previous work experience in the inspected provinces. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, Multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. *Local Work Experience* equals to one if CIT leaders (or deputy leaders) have prior work experience in the inspected province, and zero otherwise. *Length of Work Experience* equals to one if CIT leaders (leaders or deputy leaders) have longer working experience in the inspected province, and zero otherwise. We use the length of work experience (months) to classify the full sample into long vs. short subsamples using the median. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

optimism is more salient. We further classify the subsamples by using either the leader or the deputy leader as the one with prior experience and present the findings in columns (3)–(4). These results show a similar pattern to those in columns (1)–(2). The findings support the argument of familiarity and CIT effectiveness leading to an increase in political pressure by the local officials.

We further investigate the length of time CIT leaders worked in the inspected provinces. We contend that, if CIT leaders had longer prior work experience in the inspected province, the impact of the CIT leader's familiarity on inspection effectiveness would be stronger. Then, having an inspection conducted by a CIT leader with longer prior experience in the inspected provinces would lead to more severe analyst optimism than having leader with less experience conduct the inspection. The findings are presented in columns (5) to (8) of Table 11. In these columns, we confine our analysis to samples in which CIT leaders or deputy leaders had prior work experience in inspected provinces. We partition the full sample into long versus short tenure subsamples based on the median of the number of months of prior work experience of CIT leaders. The results for the long-tenure subsamples in columns (5) and (7) show that the coefficients of *Inspection* are positive and statistically significant at the 1% level. In contrast, the same coefficients are statistically insignificant in columns (6) and (8) for the short-tenure subsamples. In addition, the differences of the coefficients between columns (5) and (6) and between columns (7) and (8) are statistically significant at the 1% level.

Collectively, the results in Table 11 suggest that the social network of CIT leaders further enhances analyst optimism bias and corroborates the local government pressure and bad-news-hiding explanations on the impact of anti-corruption campaigns on analyst optimism bias.

4.5.3. *Optimism reversal in analyst earnings forecasts*

To examine the long-term impact of CIT site visits on analyst optimism bias, we follow Piotroski et al. (2015) and Cao et al. (2018) to augment Eq. (2) with dummy variables that capture analyst optimism bias 30, 60, and 90 days later. Specifically, Eq. (2) becomes:

$$Optimism_{i,j,t} = \beta_0 + \beta_1 \times Inspection_{i,j,t} + \beta_2 \times Post30_{i,j,t} + \beta_3 \times Post60_{i,j,t} + \beta_4 \times Post90_{i,j,t} + Controls_{i,j,t} + \alpha_{i,j,t} + \varepsilon_{i,j,t} \tag{4}$$

where $Post30_{i,j,t}$, $Post60_{i,j,t}$, and $Post90_{i,j,t}$ are dummy variables with values of one if the forecasts are made 0–30 days, 31–60 days, and 61–90 days after CIT site visits end, respectively. The three variables capture the analyst optimism bias, if any, after CIT site visits.

Table 12 reports the results. In column (1), the coefficient of *Inspection* is positive and statistically significant at the 1% level, which is consistent with the results in Table 2. However, the coefficient of *Post30* is negative but insignificant. Interestingly, the coefficient of *Post60* is negative and statistically significant at the 1% level, suggesting that analysts lower their optimism bias during the period of 31–60 days after CIT site visits. Interestingly, the magnitudes for the coefficients of *Inspection* and *Post60* in column (1) are 0.142 and −0.131, which are not significantly different in their absolute values. Hence, the optimism bias of the analysts during CIT site visits appears to be temporary.

According to Cao et al. (2018), state-ownership status of a firm matters. We present the results of analyst forecast optimism

Table 12
Analyst Forecast Optimism Reversion.

Dep. Var = Optimism	(1)	(2)	(3)
	Fullsample	*SOE*	*Non-SOE*
Inspection	0.142***	0.219***	0.141***
	(4.29)	(3.49)	(3.64)
Post30	−0.057	0.114	−0.033
	(−1.51)	(1.10)	(−0.66)
Post60	−0.131***	−0.062	−0.158***
	(−3.46)	(−0.82)	(−3.56)
Post90	0.022	0.056	0.031
	(0.61)	(0.73)	(0.67)
Firm Controls	*Yes*	*Yes*	*Yes*
Analyst Controls	*Yes*	*Yes*	*Yes*
Constant	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*
Analyst F.E.	*Yes*	*Yes*	*Yes*
Year F.E.	*Yes*	*Yes*	*Yes*
N	599,870	207,829	392,041
Adj_R^2	0.459	0.364	0.357

This table reports the result of analyst earnings forecast optimism reversal. Columns (1), (2) and (3) use the full sample, SOE sample and non-SOE sample respectively. The dependent variable is *Optimism*, which is the difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. *Post30*, *Post60*, and *Post90* are dummy variables with values of one if the forecasts are made 0–30 days, 31–60 days, and 61–90 days after CIT site visits end, respectively. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

reversion of SOE subsample in columns (2) in Table 9. The coefficient of *Inspection* is positive and statistically significant at the 1% level, which is consistent with the results in Table 2. None of the three variable coefficients of *Post30, Post60*, and *Post90* are significant. There is no reversal in the post-inspection periods in the SOE sample, which is consistent with Cao et al. (2018).

We present the results of analyst forecast optimism reversion of non-SOE subsample in columns (3) in Table 9. Both the coefficients of *Post30* and *Post90* are not significant. However, the coefficient of *Post60* is negative and statistically significant at the 1% level, suggesting that analysts revert their optimism bias during the period of 31–60 days after CIT site visits. In addition, the magnitudes for the coefficients of *Inspection* and *Post60* are 0.141 and − 0.158, which are not significantly different in their absolute values. Hence, the optimism bias of the analysts during CIT site visits appears to be temporary. After the CIT concludes its visits, analyst optimism bias reverts to pre-site-visit levels. The result of non-SOE sample is also consistent with Cao et al. (2018).

Collectively, after the CIT concludes its visits, analyst optimism bias reverts to pre-site-visit levels in their optimism magnitude. This interpretation is consistent with the statistics in Appendix C that show the time between the end of the inspection and the release of the CIT's inspection findings to the inspected province has a mean of 42.6 days and a median of 40 days. Therefore, after CIT agents depart, analysts roll back their overly optimistic earnings forecasts. The results in Table 12 further corroborate the local government pressure and firms' bad-news-hiding explanations.

4.5.4. The impact of CIT site visit on analyst earnings forecast accuracy

We further examine the impact of *Inspection* on analyst forecast accuracy. Following Walther and Willis (2012) and Jiang et al. (2016), we calculate the analyst forecast accuracy as follows:

$$Accuracy = -1^{*} \mid EPS\,forecast\text{–}EPS \mid /P^{*}100 \tag{5}$$

where *EPS forecast* is the earnings forecast from analyst *j* for firm *i*, and *EPS* is the actual EPS for firm *i*. *P* is the stock price on the trading day prior to the earnings forecast. We present the result of the effect of *Inspection* on analyst forecast accuracy in Table 13. Besides the full model, we include two simplified models for robustness. Consistently across columns (1)–(3), the coefficients of *Inspection* are negative and significant. We find that analysts' optimism increase and accuracy decrease during the CIT period. That is, during the period when the CIT was stationed, the analysts' forecasts became inaccurate due to pressure from the local government and insufficient information. The baseline findings remain qualitatively the same.

5. Summary

Using the quasi-natural experiment of CIT site visits that began in China in 2013, we examine the impact of an anti-corruption campaign on analyst earnings forecast optimism. While several studies investigate the impact of an anti-corruption campaign on a firm's negative information hiding and research and development investment, few studies explore the impact of such a campaign on outside stakeholders of a firm. To the best of our knowledge, we provide the first study on the impact of an anti-corruption campaign on analyst earnings forecasts.

Our findings suggest that analyst earnings optimism bias increases for their followed firms during CIT site visits relative to non-CIT site visit periods. The findings are robust to matched samples, placebo tests, alternative fixed effect specifications, endogeneity of CIT site visits concern, and alternative samples. In terms of mechanisms, the findings are consistent with the local government pressure and bad-news-hiding explanations. We do not find CIT site visits spur firms to improve their fundamentals. We further document that the effect of CIT visits on analyst optimism is more pronounced for star, non-affiliated, and experienced analysts, which is consistent with

Table 13
The Impact of CIT Inspections on Analyst Forecast accuracy.

Dep. Var = Accuracy	(1)	(2)	(3)
Inspection	−0.218*	−0.214*	−0.154***
	(−2.21)	(−2.20)	(−5.56)
Firm Controls	*No*	*Yes*	*Yes*
Analyst Controls	*No*	*No*	*Yes*
Constant	*Yes*	*Yes*	*Yes*
Province F.E.	*Yes*	*Yes*	*Yes*
Industry F.E.	*Yes*	*Yes*	*Yes*
Analyst F.E.	Yes	Yes	*Yes*
Year F.E.	Yes	Yes	*Yes*
N	599,870	599,870	599,870
Adj_R^2	0.184	0.204	0.218

This table reports the effect of CIT inspections on analyst forecast accuracy. The dependent variable is *Accuracy*, which is the negative absolute difference between an analyst's forecast and the actual earnings per share (EPS) of the firm, scaled by the stock price on the day prior to the earnings forecast, multiplied by 100. We multiply the forecast accuracy variable by −1 so that large value in this variable suggest more accurate earnings forecasts. *Inspection* equals to one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise. The detailed definitions of all variables are provided in Appendix D. All regressions include province fixed effects, industry fixed effects and analyst fixed effects and year fixed effects. The t-statistics are reported in parentheses on robust standard errors clustered at the firm and year level. *, ** and *** designate statistical significance at the 10%, 5%, and 1% level, respectively.

the heterogeneous nature of analysts and supporting the notion that, because of their greater influence, local governments focus on pressuring these analysts.

Additional analyses suggest that analyst earnings optimism bias is more salient when CIT leaders had prior work experience in the inspected province, indicating that local government officials intensify their efforts to exert political influence in order to mitigate the potential effectiveness of the CIT inspection that is based on CIT leaders' familiarity with the inspected province. Furthermore, analyst optimism bias reverts to its previous levels 60 days after CIT site visits, especially in the non-SOE sample, which corroborates the baseline findings. It indicates that the impact of CIT site visits on analyst earnings forecast optimism is temporary. After the CIT left, local government officials do not need to apply political pressure, which leads to lower analyst optimism bias, and this effect mainly exists in non-SOE sample.

Collectively, our findings imply that a public policy of anti-corruption campaigns has a potentially wide impact on external stakeholders in addition to a firm-level economic impact. Thus, the implications of such public policy on resource allocation and/or social equity go beyond a firm-level economic impact to behavior of external stakeholders. Hence, policy makers should consider the extended impact of an anti-corruption campaign and provide some parallel considerations to enhance the professionalism of analysts.

Appendix A. Chronological Distribution of CIT Inspections

Year/Month Round	Regular Inspections (31 Provinces)				Special Inspections (16 Provinces "Looking Back")				Total
	2013.05&06	2013.10&11	2014.03	2014.07	2016.02	2016.06	2016.11	2017.02	
1	5								5
2		6							6
3			10						10
4				10					10
5					4				4
6						4			4
7							4		4
8								4	4
Total	5	6	10	10	4	4	4	4	47

This table reports the chronological distribution of CIT inspections. Source of data: Website of the Central Commission for Discipline Inspection (http://www.ccdi.gov.cn/special/zyxszt/).

Appendix B. Time Sequence of Inspected Provinces

Round Province	Regular inspection				Special inspection				
	1	2	3	4	5	6	7	8	Frequency
Jiangxi	1					1			2
Chongqing	1						1		2
Guizhou	1								1
Inner Mongolia	1							1	2
Hubei	1					1			2
Anhui		1			1				2
Yunnan		1						1	2
Jilin		1						1	2
Shanxi		1							1
Guangdong		1							1
Hunan		1			1				2
Shandong			1		1				2
Liaoning			1		1				2
Tianjin			1			1			2
Henan			1			1			2
Beijing			1				1		2
Gansu			1				1		2
Ningxia			1						1
Xinjiang			1						1
Hainan			1						1
Fujian			1						1
Guangxi				1			1		2
Shaanxi				1				1	2
Shanghai				1					1
Sichuan				1					1
Jiangsu				1					1
Hebei				1					1
Zhejiang				1					1

(continued on next page)

(*continued*)

Round Province	Regular inspection				Special inspection				
	1	2	3	4	5	6	7	8	Frequency
Tibet				1					1
Qinghai				1					1
Heilongjiang				1					1
Total Number of Provinces Inspected	5	6	10	10	4	4	4	4	47

This table reports the time sequence of inspected provinces. Source of data:Website of the Central Commission for Discipline Inspection (http://www.ccdi.gov.cn/special/zyxszt/).

Appendix C. Descriptive Statistics of CIT Inspections

Panel A: Descriptive Statistics of CIT Site Visits

Year	Start Date	End Date	Feedback Date	Province
2013	2013/05/27	2013/08/20	2013/09/18	Jiangxi
2013	2013/05/29	2013/07/29	2013/09/25	Chongqing
2013	2013/05/29	2013/07/29	2013/09/25	Guizhou
2013	2013/06/02	2013/07/23	2013/09/23	Hubei
2013	2013/06/03	2013/08/06	2013/09/26	Inner Mongolia
2013	2013/10/29	2013/12/29	2014/02/26	Guangdong
2013	2013/10/30	2013/12/26	2014/02/26	Jilin
2013	2013/10/30	2013/12/28	2014/02/13	Yunnan
2013	2013/10/30	2013/12/29	2014/02/24	Shanxi
2013	2013/10/31	2013/12/27	2014/02/24	Anhui
2013	2013/11/01	2013/12/01	2014/02/24	Hunan
2014	2014/03/24	2014/05/27	2014/07/09	Hainan
2014	2014/03/27	2014/05/27	2014/07/06	Gansu
2014	2014/03/27	2014/05/26	2014/07/13	Fujian
2014	2014/03/28	2014/05/28	2014/07/09	Tianjin
2014	2014/03/28	2014/05/27	2014/07/07	Henan
2014	2014/03/29	2014/05/28	2014/07/09	Shandong
2014	2014/03/31	2014/05/25	2014/07/07	Liaoning
2014	2014/03/31	2014/05/30	2014/07/09	Beijing
2014	2014/03/31	2014/05/31	2014/07/10	Ningxia
2014	2014/03/31	2014/05/24	2014/07/11	Xinjiang
2014	2014/07/27	2014/09/24	2014/11/03	Tibet
2014	2014/07/28	2014/09/29	2014/10/28	Qinghai
2014	2014/07/29	2014/09/27	2014/11/01	Guangxi
2014	2014/07/29	2014/09/28	2014/11/04	Zhejiang
2014	2014/07/29	2014/09/27	2014/10/30	Jiangsu
2014	2014/07/29	2014/09/27	2014/10/31	Sichuan
2014	2014/07/29	2014/09/25	2014/10/30	Hebei
2014	2014/07/30	2014/09/30	2014/10/30	Shanghai
2014	2014/07/30	2014/09/27	2014/10/28	Heilongjiang
2014	2014/07/31	2014/09/28	2014/10/31	Shaanxi
2016	2016/02/27	2016/04/28	2016/05/29	Liaoning
2016	2016/02/27	2016/04/28	2016/05/31	Hunan
2016	2016/02/28	2016/04/28	2016/05/31	Shandong
2016	2016/02/28	2016/04/27	2016/06/01	Anhui
2016	2016/06/29	2016/08/29	2016/10/09	Tianjin
2016	2016/06/29	2016/08/29	2016/10/11	Henan
2016	2016/06/30	2016/08/30	2016/10/10	Hubei
2016	2016/06/30	2016/08/30	2016/10/08	Jiangxi
2016	2016/11/06	2017/01/05	2017/02/12	Beijing
2016	2016/11/06	2017/01/05	2017/02/11	Chongqing
2016	2016/11/08	2017/01/06	2017/02/11	Gansu
2016	2016/11/09	2017/01/06	2017/02/12	Guangxi
2017	2017/02/26	2017/04/27	2017/06/07	Inner Mongolia
2017	2017/02/26	2017/04/26	2017/06/07	Yunnan
2017	2017/02/26	2017/04/26	2017/06/08	Shaanxi
2017	2017/02/27	2017/04/27	2017/06/06	Jilin

Panel B: Summary Statistics of Time Intervals

Variable	N	Mean	Std	Q1	Median	Q3
Interval1	47	59.617	6.173	59	60	61
Interval2	47	42.574	10.98	35	40	47

This table shows the timeline of the inspection to each province. Start Date and End Date is the start and end of the inspection. Feedback Date is the date when CIT feeds back inspection opinions to the inspected province. Panel A reports the descriptive statistics of CIT site visits and Panel B reports the summary statistics of the time intervals among CIT start date, end date and feedback date. *Interval1* denotes the time interval between CIT start date and end date. *Interval2* denotes the time interval between CIT end date and CIT feedback date. Source of data:Website of the Central Commission for Discipline Inspection (http://www.ccdi.gov.cn/special/zyxszt/).

Appendix D. Variable Definitions

Variable	Definition
Dependent Variable	
Optimism	(analyst EPS forecast - actual EPS) / stock price on the day prior to the earnings forecast date $\times$100
Explanatory Variable	
Inspection	A dummy variable with a value of one if an analyst issues an earnings forecast on a firm located in a province during a CIT site visit, and zero otherwise
Firm-Level Control Variables	
Size	Natural logarithm of total assets at the beginning of the year
Age	Natural logarithm of 1 plus years of listing at the beginning of the year
Board	Natural logarithm of board size at the beginning of the year
Independ	The ratio of the number of independent directors to total number of directors at the beginning of the year
Lev	The ratio of total liabilities to total assets at the beginning of the year
ROA	The ratio of net income to total assets at the beginning of the year
MB	Market value to book value of equity at the beginning of the year
Intangible	The ratio of intangible assets to total assets at the beginning of the year
Volatility	Average daily volatility of stock returns during the year prior to the focal year
Turnover	Annual stock turnover volume during the year prior to the focal year
Return	Annual stock return of the year prior to the focal year
Analyst	The total number of analysts following the firm
Report	The total number of analyst earnings forecasts on the firm
Analyst-Level Control Variables	
Experience	Natural logarithm of 1 plus an analyst's working experience in quarters
Follow_num	Natural logarithm of the number of firms covered by an analyst, by the end of this year
Report_num	Natural logarithm of the total number of earnings forecasts issued by an analyst for all the firms, by the end of this year
Brokerage	Natural logarithm of the number of analysts employed by an analyst's brokerage, by the end of this year
Horizon	Natural logarithm of 1 plus the number of days between earnings forecast issued date and earnings announcement date
Grouping Variables	
Local	A dummy variable with a value of one if the analyst is from a local brokerage and zero otherwise
SOE	A dummy variable with a value of one if the ultimate controller of a listed firm is a government-owned entity and zero otherwise
State	A dummy variable with a value of one if the local brokerage is state owned and zero otherwise
NCSKEW	The negative coefficient of skewness, measured as the negative of the third moment of firm's daily market adjusted excess returns scaled by its cubed standard deviation in each month
Transparency	The absolute value of discretionary accruals estimated using the modified Jones model at the begin of the year
Dispersion	Cross-sectional standard deviation of individual analysts' last annual forecasts, scaled by the stock price at the end of the prior fiscal year
Readability	The readability of a firm's annual report, measured as the length (vocabulary numbers) of the annual report at the beginning of the year
Local Work Experience	A dummy variable with a value of one if CIT leaders (or deputy leaders) had work experience in the inspected province, and zero otherwise
Length of Work Experience	CIT leaders' (or deputy leaders)' work experience (in months)
Firm Fundamental Variables	
CR	Long-term credit rating on the firm's bond
Volatility	Average daily volatility of stock returns during the year prior to the focal year
ROA	The ratio of net income to total assets
ROS	The ratio of net income to total revenues
Sales	The ratio of total revenues to total assets (sales)
PE	The price of a stock divided by earnings per share
Analyst heterogeneity variables	
Affiliation	A dummy variable with a value of one if an analyst's brokerage receives commission fees in quarter q-1 from any fund company that has stock in its top ten holdings at the end of quarter q-1, and zero otherwise. Because commission fees are available on a half-year basis, this variable is measured using commission fee information in quarter q-1 for the 1st and 3rd quarter and in quarter q-2 for the 2nd and 4th quarter.
Star	A dummy variable with a value of one if an analyst j is ranked by New Fortune magazine as a star analyst in the year in which he follows firm i, and zero otherwise.
Experience	A dummy variable with a value of one if an analyst has longer working experience using the median experience of analysts (experienced analysts), and zero otherwise. We measure experience as natural logarithm of 1 plus the number of quarters between first quarters making forecasts to current quarter.

This table provides definitions of all variables used.

Appendix E. Securities Analysts' General Release or Reports Involving the Anti-corruption Policy in China

Brokerage	Title of report	Date	Selected exerts from analyst reports	Source
CIB Research	The CCDI Communique first mentions "financial credit" anti-corruption	Jan 18, 2018	"…**focus on investigating and handling corruption cases** of the interest groups formed by intertwining political and economic issues, and focus on **solving corruption issues** in key areas and key links such as selection and employment, approval and supervision, resource development, and financial credit…"	S[a]
Everbright Securities	Strategy Weekly: Prime Minister answers reporters' questions on anti-corruption, structural adjustment, stabilizing growth, and risk control	Mar 17, 2015	"…The government will continue to implement the established strategies of the 'Government Work Report': **anti-corruption**, structural adjustment, stabilizing growth, and risk control. One of the starting points of anti-corruption is streamlining administration and delegating power. **This not only cracks down on corrupt rent-seeking space, but also removes the entry barriers for enterprises, stimulates economic vitality, and creates jobs**…"	S[b]
Shanxi Securities	Jereh Group (ticker ID: 002353). Company review: Anti-corruption arrived and demand rebounds soon	Aug 01, 2014	The oil drilling equipment industry is still in an upward cycle. **With the steady progress of "anti-corruption"** in the domestic oil and petrochemical industries, investment demand in the oil and petrochemical industries will rebound, and **the prosperity of the oil mining equipment industry will rise again.**	S[c]
Haitong Securities	Special Report on State-owned Assets Reform in the Real Estate Industry: Anti-corruption protection reform, reiterated the recommendation of the real estate state-owned reform portfolio	July 30, 2014	The central government's vigorous **anti-corruption** and unified thinking has formed a deterrent on government official and will build a successful reform; we expect that with **the institutionalization and normalization of anti-corruption**, officials will be forced to act and implement reform ideas. Related events have played a positive role in breaking the resistance to reform and promoting the reform of state-owned enterprises.	S[d]

This table provides securities analysts' general release or reports involving the anti-corruption policy in China.

[a] http://pg.jrj.com.cn/acc/Res/CN_RES/INVEST/2018/1/14/9cce00c5-f11f-4c56-9f0b-8d664ac7f50a.pdf

[b] http://www.microbell.com/wap_detail.aspx?id=1537448

[c] http://www.hibor.com.cn/docdetail_1359209.html

[d] http://vip.stock.finance.sina.com.cn/q/go.php/vReport_Show/kind/industry/rptid/2426768/index.phtml

References

Aidt, T., Dutta, J., Sena, V., 2008. Governance regimes, corruption and growth: theory and evidence. J. Comp. Econ. 36, 195–220.

Antoniou, C., Kumar, A., Maligkris, A., 2017. Terrorist Attacks, Managerial Sentiment, and Corporate Policies. Working Paper. available at. https://ssrn.com/abstract=2566740.

Avis, E., Ferraz, C., Finan, F., 2018. Do government audits reduce corruption? Estimating the impacts of exposing corrupt politicians. J. Polit. Econ. 126, 1912–1964.

Bardhan, I., Lin, S., Wu, S.L., 2015. The quality of internal control over financial reporting in family firms. Account. Horiz. 29, 41–60.

Bartram, S.M., Brown, G., Stulz, R.M., 2012. Why are U.S. stocks more volatile? J. Financ. 67, 1329–1370.

Beck, P.J., Maher, M.W., 1986. A comparison of bribery and bidding in thin markets. Econ. Lett. 20, 1–5.

Bradshaw, M.T., 2011. Analysts' Forecasts: What Do We Know After Decades of Work? Working Paper. available at. https://papers.ssrn.com/sol3/results.cfm.

Cai, H.B., Fang, H.M., Xu, L.C., 2011. Eat, drink, firms, government: an investigation of corruption from the entertainment and travel costs of Chinese firms. J. Law Econ. 54, 55–78.

Cao, X., Wang, Y., Zhou, S., 2018. Anti-corruption campaigns and corporate information release in China. J. Corp. Finan. 49, 186–203.

Cao, S.S., Ma, T., Wan, C., 2019. Do local investors always know better? Evidence from China's market segmentation. Account. Horiz. 33, 17–37.

Cao, S., He, X., Wang, C.C.Y., Yin, H.. Market advisor or government mouthpiece: The role of sell-side analysts in emerging markets. Working Paper. available at. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3140069.

Chan, K., Hameed, A., 2006. Stock price synchronicity and analyst coverage in emerging markets. J. Financ. Econ. 80, 115–147.

Chan, K.C., Jiang, X., Wu, D., Xu, N., Zeng, H., 2020. When is the client king? Evidence from affiliated-analyst recommendations in China's Split-Share Reform. Contemp. Account. Res. 37, 1044–1072.

Chen, J., Hong, H., Stein, J.C., 2001. Forecasting crashes: trading volume, past returns, and conditional skewness in stock prices. J. Financ. Econ. 61, 345–381.

Chen, Y., Xie, Y., You, H., Zhang, Y., 2018. Does crackdown on corruption reduce stock price crash risk? Evidence from China. J. Corp. Finan. 51, 125–141.

Chu, J., Fisman, R., Tan, S., Wang, Y., 2020. Hometown favoritism and the quality of government monitoring: evidence from rotation of Chinese auditors. Am. Econ. J. Appl. Econ. (Forthcoming).

Colonnelli, E., Prem, M., 2020. Corruption and firms. Working Paper. Available at SSRN: https://ssrn.com/abstract=2931602(2020.

Correia, M.M., 2014. Political connections and SEC enforcement. J. Account. Econ. 57, 241–262.

Cowen, A., Groysberg, B., Healy, P., 2006. Which types of analyst firms are more optimistic? J. Account. Econ. 41, 119–146.

Cuculiza, C., Antoniou, C., Kumar, A., Maligkris, A., 2019. Terrorist Attacks, Analyst Sentiment, and Earnings Forecasts. Working Paper. available at. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2702051.

De Franco, G., Kothari, S.P., Verdi, R.S., 2011. The benefits of financial statement comparability. J. Account. Res. 49, 895–931.

Dechow, P.M., Sloan, R.G., Sweeney, A.P., 1995. Detecting earnings management. Account. Rev. 70, 193–225.

Ding, H., Fang, H., Lin, S., Shi, K.. Equilibrium consequences of corruption on firms: Evidence from China's anti-corruption campaign Working Paper. available at. https://ideas.repec.org/p/nbr/nberwo/26656.html.

Fan, J.P.H., Rui, O.M., Zhao, M., 2008. Public governance and corporate finance: evidence from corruption cases. J. Comp. Econ. 36, 343–364.
Fan, J.P.H., Guan, F., Li, Z., Yang, Y.G., 2014. Relationship networks and earnings informativeness: evidence from corruption cases. J. Bus. Financ. Acc. 41, 831–866.
Firth, M., Lin, C., Liu, P., Xuan, Y., 2013. The client is king: do mutual fund relationships Bias analyst recommendations? J. Account. Res. 51, 165–200.
Fisman, R., Wang, Y., 2015. The mortality cost of political connections. Rev. Econ. Stud. 82, 1346–1382.
Gan, W., Xu, X., 2019. Does anti-corruption campaign promote corporate R&D investment? Evidence from China. Financ. Res. Lett. 30, 292–296.
Gu, Z., Li, Z., Yang, Y.G., 2013. Monitors or predators: The influence of institutional investors on sell-side analysts. Account. Rev. 88, 137–169.
Hong, H., Kubik, J.D., 2003. Analyzing the analysts: career concerns and biased earnings forecasts. J. Financ. 58, 313–352.
Hope, O.-K., Yue, H., Zhong, Q., 2020. China's anti-corruption campaign and financial reporting quality. Contemp. Account. Res. 37 (2), 1015–1043 (forthcoming).
Hutton, A.P., Marcus, A.J., Tehranian, H., 2009. Opaque financial reports, R2, and crash risk. J. Financ. Econ. 94, 67–86.
Huyghebaert, N., Xu, W., 2015. Bias in the post-IPO earnings forecasts of affiliated analysts: evidence from a Chinese natural experiment. J. Account. Econ. 61, 486–506.
Jackson, A.R., 2005. Trade generation, reputation, and sell-side analysts. J. Financ. 60, 673–718.
Jiang, X.Y., Xu, N., Yuan, Q., Chan, K.C., 2016. Mutual-fund-affiliated analysts and stock price synchronicity: evidence from China. J. Acc. Audit. Financ. 33, 435–460.
Ke, B., Liu, N., Tang, S., 2018. The economic consequences of anti-corruption campaigns: Evidence from China. Working Paper. Available at SSRN 2963478.
Kim, J.-B., Li, Y., Zhang, L., 2011a. Corporate tax avoidance and stock price crash risk: firm-level analysis. J. Financ. Econ. 100, 639–662.
Kim, J.-B., Li, Y., Zhang, L., 2011b. CEO versus CFO: equity incentives and crashes. J. Financ. Econ. 101, 713–730.
Lawrence, A., 2013. Individual investors and financial disclosure. J. Account. Econ. 56, 130–147.
Li, F., 2008. Annual report readability, current earnings, and earnings persistence. J. Account. Econ. 45, 221–247.
Li, H., Zhou, L.-A., 2005. Political turnover and economic performance: the incentive role of personnel control in China. J. Public Econ. 89, 1743–1762.
Libby, R., Hunton, J.E., Tan, H.-T., Seybert, N., 2008. Retracted: relationship incentives and the optimistic/pessimistic pattern in analysts' forecasts. J. Account. Res. 46, 173–198.
Lin, C., Morck, R., Yeung, B., Zhao, X., . Anti-corruption reforms and shareholder valuations: Event study evidence from China. Working Paper. available at. https://ideas.repec.org/p/nbr/nberwo/22001.html.
Loughran, T., McDonald, B., 2014. Measuring readability in financial disclosures. J. Financ. 69, 1643–1671.
Lui, F.T., 1985. An equilibrium queuing model of bribery. J. Polit. Econ. 93, 760–781.
Peterson, K., Schmardebeck, R., Wilks, T.J., 2015. The earnings quality and information processing effects of accounting consistency. Account. Rev. 90, 2483–2514.
Pevzner, M., Xie, F., Xin, X., 2015. When firms talk, do investors listen? The role of trust in stock market reactions to corporate earnings announcements. J. Financ. Econ. 117, 190–223.
Piotroski, J.D., Wong, T.J., Zhang, T., 2015. Political incentives to suppress negative information: evidence from Chinese listed firms. J. Account. Res. 53, 405–459.
Walther, B.R., Willis, R.H., 2012. Do investor expectations affect sell-side analysts' forecast bias and forecast accuracy? Rev. Acc. Stud. 18, 207–227.
Watts, R.L., Zimmerman, J.L., 1990. Positive accounting theory: a ten year perspective. Account. Rev. 65, 131–156.
Xu, N., Chan, K.C., Jiang, X., Yi, Z., 2013. Do star analysts know more firm-specific information? Evidence from China. J. Bank. Financ. 37, 89–102.
You, H., Zhang, X.-J., 2009. Financial reporting complexity and investor underreaction to 10-K information. Rev. Acc. Stud. 14, 559–586.
Zeume, S., 2017. Bribes and firm value. Rev. Financ. Stud. 30, 1457–1489.