Government Incentives and Financial Intermediaries: The Case of Chinese Sell-Side Analysts

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Abstract

We study how sell-side analysts produce information in a context where the central government can influence financial intermediaries. Leveraging seven economic periods between 2005 and 2015 when the Chinese government had strong incentives to prop up the stock market, we show that sell-side analysts employed at state-owned brokerages issued relatively optimistic earnings forecasts and stock recommendations during these periods. This relative optimism is particularly pronounced in earnings forecasts for larger firms and for state-owned enterprises, and is found regardless of analysts' experience or star status, or of their brokerages' degree of dependence on commissions. Although these optimistic forecasts are also relatively less accurate, the evidence suggests that they influence investors' beliefs. These findings highlight the role of government incentives in the behavior and output of analysts in emerging-market contexts.

Keywords: Sell-Side Analysts; Forecast Optimism; Forecast Accuracy; Government Incentives; Emerging Markets

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1 Introduction

The functioning of capital markets is buttressed by various market institutions. Each helps to ameliorate the natural frictions that arise from information asymmetries between suppliers of capital (investors) and users of capital (firms and their corporate managers). These institutions are especially important in emerging markets, since their presence and quality determine the risks faced by capital providers and contribute to capital formation and economic growth (Chow, 1993; Boskin and Lau, 1990). Identifying voids in capital-market institutions and understanding the nature of institutional weaknesses is thus critical to the development of emerging-market economies (Khanna and Palepu, 2010). As Ang (2016) suggests, a recipe for kick-starting development in emerging markets is "[allowing] dynamic markets to flourish first... and building strong institution later."

The functioning of capital-market institutions in one such emerging market, China, has attracted substantial scholarly attention in recent years. This eruption of interest is rooted in China's stunning economic growth over the last 20 years—from $600 billion in gross domestic product in 1995 to over $8 trillion in 2015, making it the second largest economy in the world—and in the commensurate growth of its capital markets.[1] In 2005 the total market capitalization of firms listed on the Chinese exchanges totaled $401 billion; by 2015 that figure had skyrocketed to over $8 trillion. China is now home to the world's second largest equity market, but its market institutions are generally considered less mature and robust than those in the West. Substantial research on the functioning and voids shortcomings of Chinese financial-market institutions remains outstanding.

In particular, it is an open question how fully the information produced by financial

[1]These data are drawn from the International Monetary Fund's World Economic Outlook database, accessed in April 2017.

intermediaries in China can be relied on by market participants. We tackle this question by examining the forces that influence the information produced by sell-side analysts in China; specifically, we study the extent to which the central government's incentives influence such information.

We focus on sell-side analysts because they are "preeminent market information intermediaries" (Bradshaw, 2011): analysts demand and extract information from managers; process and distill complex economic, financial, and strategic information; and produce analyses, forecasts, and recommendations about firms. In principle, this process helps to identify and monitor investment risks, and thus aids investors in appropriately allocating scarce capital. Because analysts' outputs can affect market participants' beliefs, understanding the economic forces that shape analysts' information production contributes to a broader understanding of the quality of information intermediaries. Though substantial research has been devoted to understanding U.S. analysts' incentives and information production (see, e.g., Ramnath, Rock, and Shane, 2008; Bradshaw, 2011; Kothari, So, and Verdi, 2016), and though political forces play an important role in many economies, particularly emerging markets, little is known about how analysts' incentives are influenced by political forces in those contexts.

Ex ante it is unclear how analysts may respond to external pressures to influence the information and analyses they produce. On the one hand, analysts' reputational concerns may mitigate the influence of external parties' incentives and pressures; on the other hand, concern for their career trajectories within the brokerage firm, which may (implicitly) depend on the central government's influence, could elevate analysts' responsiveness to the government's incentives. The extent to which sell-side analysts' information production is "captured" thus depends on how analysts trade off these two conflicting forces (Jackson and Moerke, 2005; Cowen, Groysberg, and Healy, 2006).

We hypothesize that analysts who work at government-owned brokerage firms (the majority of analysts in the in China) will be more likely to respond to government incentives than will analysts employed at non-state-owned brokerage firms. State-owned brokerages are likely to be more sensitive to political influence because: (i) the government is the controlling shareholder and thus a strong voice; (ii) their senior managers' appointments and promotions are likely to depend on government guidance; and (iii) they tend to be larger and thus to have greater market influence and greater potential as policy tools.

To identify the effect of government incentives on the information produced by analysts, we examine analysts' earnings-forecast optimism during seven economic events ("government intervention periods") between 2005 and 2015: four attempts to rescue the financial market between 2005 and 2015, the 17^{th} and 18^{th} National Congress Meetings of the Communist Party of China in 2007 and 2012, and the 2008 Beijing Olympics (see Table A1 for details). Our maintained assumption is that the central government of China had strong incentives to prop up the equity market at these times, either to limit the extent of market panic (in the case of the four financial-market rescue events) or to create the appearance of thriving financial markets (in the case of the Olympics and the National Congress Meetings). We hypothesize that analysts at government-owned brokerage firms are more responsive to government incentives, and thus that their earnings forecasts will be relatively more optimistic during those periods.

Leveraging these economic shocks and the expected differences in incentives between analysts at state-owned brokerage firms (treatment) and at non-state-owned brokerage firms (control), we document the following main findings. First, consistent with our hypothesis, analysts at state-owned brokerage firms issue forecasts that are relatively more optimistic during government intervention periods. We also find that the relative optimism of government-brokerage analysts is concentrated in the largest stocks in the market, which have the greatest weight in and influence on equity-market indexes, and in state-owned enterprises (SOEs). These findings support our maintained assumption that the government aimed to prop up the equity market during these intervention periods, and the hypothesis that analysts at state-owned brokerage firms produce information in such a way as to comply with government incentives.

Second, we show that the relative optimism exhibited by government-brokerage analysts during government intervention periods is consistent across various types of analysts, regardless of experience, star status, or how frequently they provide new forecasts for a given firm. Moreover, this pattern of relative optimism also characterizes both analysts at more commission-driven brokerages and those at less commission-driven brokerages; this finding suggests that government-brokerage analysts' behavior during government intervention periods is not due to different brokerage-firm economics.

Third, we show two additional ways in which government-brokerage analysts exhibit relative optimism during government intervention periods: they maintain relatively more optimistic stock recommendations, and they slow the pace of forecast issuances and revisions. We do not find differences in revisions, however, between government- and non-government brokerage analysts.

Fourth, we show that the relatively more optimistic forecasts of government-brokerage analysts during government intervention periods are less accurate, suggesting that their optimism is not due to better information. Market participants do not appear to discount the information produced by government-brokerage analysts during these periods, suggesting that the relative optimism of state-owned brokerage analysts can be expected to influence investors' beliefs about firms' prospects.

Jointly, our analyses suggest that analysts attempt to balance their external (reputational) and internal (within-brokerage) career concerns. Our analysis shows that government-brokerage analysts on average issue lower earnings forecasts during intervention periods that coincide with negative economic shocks, suggesting that they care about preserving credibility in the marketplace. But these analysts' forecasts remain relatively more optimistic than those of non-government-brokerage analysts during these intervention periods, suggesting a degree of compliance with the government's incentives.

Our paper contributes to the growing literature on the role of the central government in shaping China's information environment. Prior literature has investigated how China's institutional environment shapes listed firms' reporting incentives, either via formal rules set by regulators, which can affect firms' earnings management behavior (e.g., Chen and Yuan, 2004; Haw, Qi, Wu, and Wu, 2005), or via the government's political influence on affiliated firms, which can affect the timing of negative news releases (Piotroski, Wong, and Zhang, 2015, e.g.,). Most closely related to our work is the burgeoning stream of literature that examines how government intervenes in financial intermediaries' information production. Several papers have examined how the government's influence affects the timing and quality of information produced by various news media (e.g., Piotroski, Wong, and Zhang, 2017; You, Zhang, and Zhang, 2017; Hope, Li, Liu, and Wu, 2017). Although others (i.e., Piotroski, Wong, and Wu, 2012) have examined the government's role in the structure and competitive landscape of the brokerage industry, we are the first to document the impact of government incentives on the forecasts and recommendations of government-brokerage sell-side analysts.

We also contribute to the literature on state-owned and private enterprises (e.g., Megginson, 2016; Fan and Wong, 2002; Faccio, Masulis, and McConnell, 2006; Hung, Wong, and Zhang, 2012). Prior studies argue that the inefficiency of state-owned enterprises is driven by their incentive to pursue social objectives instead of profit maximization (Aharoni and Ronen, 1989; Chen, Ljungqvist, Jiang, Lu, and Zhou, 2017; Toninelli, 2000). We contribute to this stream of work by illustrating a novel channel—information production by brokerage analysts—through which the government's objectives, such as stabilizing capital markets, can be fulfilled. Our findings also highlight the dual roles of capital-market institutions in China: serving as information intermediaries and (implicitly) executing governmental policies (Hope et al., 2017; Wong, 2014).

Finally, we contribute to research on analysts' incentives in the production of information for capital markets (Bradshaw, 2011). The effect of U.S. analysts' incentives on their earnings forecasts has been exhaustively studied: for example, this literature has shown that analysts issue more optimistic reports to obtain or maintain access to management (Francis and Philbrick, 1993; Bradshaw, Lee, and Peterson, 2016; Chen and Matsumoto, 2006), and to generate investment banking business (Dugar and Nathan, 1995; Michaely and Womack, 1999) or trading business (Hayes, 1998; Irvine, 2001; Jackson and Moerke, 2005; Firth, Lin, Liu, and Xuan, 2013; Gu, Li, and Yang, 2013). But relatively little attention has been paid to analysts' incentives in contexts where the central government can have a strong influence on capital-market institutions, namely most emerging-market settings. Our paper suggests that government incentives can be a determinant of the properties of analysts' forecasts; it also characterizes a specific conflict of interest that analysts must juggle in such settings.

The remainder of the paper proceeds as follows. Section 2 provides institutional background on the brokerage industry in China. Section 3 presents our main empirical analysis. Section 4 concludes.

2 Institutional Background and Hypothesis Development

This section summarizes the development of the Chinese brokerage industry. We also explain how the industry is regulated and the channels through which the central government may exert influence. Finally, we develop hypotheses on how the government's incentives are likely to affect those of sell-side analysts.

2.1 The Brokerage Industry in China

In response to the July 1991 reopening of China's two stock exchanges, the Shanghai Stock Exchange and the Shenzhen Stock Exchange, financial institutions obtained licenses to engage in securities trading and underwriting; thus the brokerage industry emerged in China. These brokerage firms were controlled either by large state-owned banks or by state-owned enterprises. For instance, Huaxia Securities, one of the largest securities brokerages in the 1990s, was founded in 1992 and owned by the Industrial and Commercial Bank of China.

The Chinese stock market rapidly grew in size, and in November 2001 the China Securities Regulatory Commission (CSRC) issued a notice permitting non-state-owned enterprises to invest in or control brokerage firms.[2] In 2002, Minsheng Securities was the first non-state-owned enterprise to obtain a brokerage licence; the company's large shareholders included such well-established non-state-owned enterprises as China Oceanwide, New Hope Group,

[2] The CSRC was established in October 1992 as the main regulator of securities markets, comparable to the U.S. Securities and Exchange Commission. The CSRC's main responsibilities include enacting and enforcing policies, laws, and regulations concerning securities markets; supervising securities issuers and financial institutions; and imposing penalties for misconduct or violations of rules or laws related to securities and futures.

and Fosun International. By the end of 2002, the number of non-state-owned brokerage firms had climbed to about 20.

During a period of market decline (2002–2003) the prosperity of non-state-owned brokerage firms attracted the attention of regulatory agencies. In 2003 the CSRC re-emphasized that brokerage firms were strictly prohibited from using trading-settlement funds, entrusted assets, and customers' entrusted bonds for other purposes. Over the next five years the CSRC implemented severe sanctions, including revocation of business licenses, on non-compliant brokerage firms: most of the approximately 30 sanctioned firms were non-state-owned. Since that time, the Chinese brokerage industry has grown steadily; approximately 15% of all brokerage firms were non-state-owned in 2015.

2.2 Government Influence on China's Brokerage Industry

In many respects, the operations and performance of Chinese brokerage firms depend on ththe CSRC. First of all, brokerage firms must obtain business licenses from the CSRC before they can engage in securities trading or underwriting. Furthermore, every prospective IPO firm requires the approval of the CSRC before it can be listed on an exchange. Thus the underwriting fees earned by brokerage firms, which account for a significant portion of their total revenues, to some extent depend on the CSRC. Permission is also necessary when brokerage firms wish to engage in new businesses, such as margin trading and issuance of asset-backed securities. Jointly, therefore, the CSRC's formal powers constitute a mechanism through which the central government exerts influence on brokerage firms' behavior.

Aside from these formal regulatory channels, the CSRC can influence brokerage firms via an alternative, informal, and frequently employed mechanism known as *window guidance*. A phenomenon that originated in Japan in the 1950s, window guidance is the communication of agendas by regulatory agencies in closed-door discussions with the directors of financial institutions. By contrast to formal mechanisms, window guidance is non-mandatory and less rigid, but it can entail implicit threat. That is, non-compliance with window guidance may trigger regulatory actions; thus window guidance can be an effective instrument for enforcing compliance with government incentives. For example, in an effort to stabilize the stock market, the CSRC met with 21 brokerage firms on July 4, 2015. The 21 participating firms jointly announced that they would invest no less than 120 billion RMB in blue-chip ETFs and that they would not sell the stocks they had held as of July 3 as long as the Shanghai Composite index remained below 4,500 points.

Thus the brokerage industry, and the analysts it employs, could be subject to the central government's influence and pressure, at least in part via the formal and informal regulatory mechanisms available to the CSRC. We argue, moreover, that state-owned brokerage firms are likely to be more sensitive to political influence than non-state-owned brokerage firms, for three reasons. First, state-owned brokerage firms are ultimately controlled by the central government or by local government, whose incentives could directly shape these firms' behavior. Second, the senior managers of state-owned brokerage firms are appointed and dismissed by the government, and their promotions and demotions are likely to be depend in part on government guidance. Thus the motivations of these brokerage firms' management teams are much more likely than non-state-owned firms' to be aligned with those of the government. Finally, state-owned brokerage firms are usually larger in size and more influential in capital markets than their non-state-owned counterparts. To the extent that the Chinese government pursues policy objectives by influencing the brokerage industry, therefore, it is more likely to target state-owned brokerages.

For these reasons we hypothesize that, to the extent that government incentives are embodied in information production at brokerage firms, they are more likely to be reflected in information produced by government-owned firms and their analysts. However, *ex ante* it is unclear how strongly analysts respond to government incentives. To the extent that analysts' career trajectories at government-owned brokerage firms depend on their level of compliance, they face a tradeoff between their external reputations and career opportunities on the one hand their internal career prospects on the other (Jackson and Moerke, 2005; Cowen et al., 2006). The next section empirically examines the influence of government incentives on analysts at government-owned brokerages. Specifically, we analyze whether the earnings forecasts produced by government-brokerage analysts are relatively more optimistic during periods when the central government aims to prop up the stock market.

3 Empirical Results

This section presents the results of empirical analyses of differences in the earnings forecasts of government-brokerage analysts and non-government-brokerage analysts during periods when the Chinese government could plausibly have stronger incentives to prop up the stock market. We will begin by describing our sample construction and research design, and will then report the results of our analyses.

3.1 Sample Selection and Research Design

Our sample consists of annual earnings forecasts from 2005 through 2015. Because no single database in China provides comprehensive coverage of analysts' forecast data, we construct a comprehensive dataset by combining data from five data vendors. We begin with earnings-forecast data provided by the China Stock Market & Accounting Research (CS-

MAR) database, to which we add any new forecasts found in the following data sources: CBAS, the Wind Financial database, the RESSET financial research database, and HIBOR. We assign a unique code to each analyst, whom we identify by name across the various datasets. For a new forecast to be included in our sample, it must be (i) issued by a different analyst, (ii) issued on a different date, or (iii) issued for a different firm. Following prior literature (e.g., Clement and Tse, 2005), we include only one-year-ahead earnings forecasts issued between the prior and current fiscal-year earnings announcements. We merge in information about the brokerage, the analyst, and the covered firm, and eliminate observations that lack information on brokerage ownership (i.e., state-owned or private) or analyst characteristics.

Our overall sample consists of 234,328 earnings forecasts covering 2,112 unique listed firms between 2005 and 2015. These forecasts are issued by 5,056 analysts at 94 distinct brokerage firms; over 80% are state-owned and fewer than 20% are non-state-owned. A state-owned brokerage firm typically employs an average of 30 analysts each year; a non-state-owned brokerage firm typically employs about 20 analysts per year. Overall, about 14% of the annual earnings forecasts in our sample are issued by analysts at non-state-owned brokerages.

Our main outcome of interest is the observed optimism in an analyst's forecast for a firm's annual earnings. To measure this outcome, we follow prior literature (i.e., Clement and Tse, 2005; Clement and Law, 2014) and normalize an analyst's *Raw Optimism*—the one-year-ahead earnings-per-share (EPS) forecast for a given firm minus the firm's actual EPS—to range from 0 to 1. That is, the main dependent variable in our study is defined as

$$\mathit{Optimism}_{ij\tau T} = \frac{\mathit{Raw\ Optimism}_{ij\tau T} - \min_{jT}\left(\mathit{Raw\ Optimism}_{ij\tau T}\right)}{\max_{jT}\left(\mathit{Raw\ Optimism}_{ij\tau T}\right) - \min_{jT}\left(\mathit{Raw\ Optimism}_{ij\tau T}\right)}, \tag{1}$$

where $Optimism_{ij\tau T}$ is the normalized optimism of an analyst i's forecast for firm j's annual earnings issued at date τ in year T; $\min_{jT}\left(Raw\ Optimism_{ij\tau T}\right)$ and $\max_{jT}\left(Raw\ Optimism_{ij\tau T}\right)$ are the sample minimum and maximum of $Raw\ Optimism_{ij\tau T}$ for all the forecasts issued for firm j in year T (i.e., varying at the firm-year level).

As explained in Clement and Tse (2005) and Clement and Law (2014), this normalization facilitates the interpretation and comparison of regression coefficients while conserving the relative distance between forecasts issued for the same firm for the same year. Since variation in this optimism measure, by construction, captures the relative optimism of forecasts issued for the same firm, normalization also has the advantage of neutralizing the effect of firm-level factors at a particular time.[3] In other words, our effects are mainly identified by within-firm and across-analyst variation in $Optimism_{ij\tau T}$.[4] As such, our empirical tests will control for the effects on forecast optimism arising from differences in analyst characteristics.

To examine how analysts employed at state-owned brokerage firms respond to government incentives, we study how their forecasts differ from those issued by analysts at non-state-owned brokerage firms during periods when the central government had an explicit or implicit desire to prop up the stock market ("government intervention periods). We identify seven such periods between 2005 and 2015 (see Table A1 for details): the four market-rescue attempts; the six-month period surrounding the 17th and 18th National Congress meetings of the Communist Party of China; and the six months surrounding the 2008 Beijing Olympic Games.[5] To account for the possibility of baseline differences between the forecasts of ana-

[3]Clement and Law (2014) explains that "this [scaled optimism] metric is conditional on the same firm-year... [thus] this adjustment is identical to controlling for firm-year fixed effects."

[4]Though this normalization is a popular standard in the literature on analysts' forecast properties, we verified in untabulated results that our main findings are robust to alternative normalizations, such as scaling *Raw Optimism* by total assets per share.

[5]Our results are robust if we change the intervention periods for the National Congress meetings and the 2008 Beijing Olympic Games to the interval beginning three month before an event and ending one month after an event.

lysts employed at state-owned and non-state-owned brokerages, we benchmark and compare their intervention-period differences in optimism against non-intervention-period differences.

Thus our main tests employ difference-in-difference (DID) empirical specifications as follows:

$$Optimism_{ij\tau T} = \beta_0 + \beta_1 Event_{\tau T} \times Govbro_i + \beta_2 Event_{\tau T} + \beta_3 GovBro_i + \gamma' X_{i\tau T} + f_T + \xi_{ij\tau T}, \quad (2)$$

where $Event_{\tau T}$ is an indicator variable that takes a value of 1 if the earnings forecast is issued on a date and year that falls within a government intervention period and 0 otherwise; and $GovBro_i$ is an indicator variable that takes a value of 1 if the earnings forecast is issued by an analyst employed (at the time of the forecast) by a state-owned brokerage firm and 0 otherwise, and $X_{i\tau T}$ is a set of analyst characteristics observed as of the date of the earnings forecast.

A brokerage firm is classified as state-owned ($GovBro_i = 1$) when we determine its ultimate controller to be a government entity. Following prior literature (La Porta, Lopez-De-Silanes, and Shleifer, 1999; Fan and Wong, 2002; Claessens, Djankov, Fan, and Lang, 2002), we define the ultimate controller as the shareholder that possesses the determining controlling rights in the company and is not controlled by another entity. To identify the ultimate controller, we track each firm's ownership pyramid and find the ultimate owners of all shareholders whose ownership stake in a brokerage firm is greater than 10%. Whether the brokerage firm is state-owned is then determined by the identity of its largest ultimate owner. In our sample, all of the largest ultimate owners possess more than 20% controlling rights of brokerage firms.

To account for analyst characteristics ($X_{i\tau T}$) that could explain the variation in *Optimism*, we control for the effect of the number of years that the analyst has issued forecasts at the firm (*Firmexp*); the number of years that the analyst has issued forecasts included in the database (*Genexp*); the analyst's forecasting frequency at the firm in the current year; the number of companies the analyst follows (*Companies*); the number of industries the analyst follows (*Industries*); the elapsed time from the forecast date to the fiscal year end (*Horizon*); and the number of unique analysts employed by the brokerage firm (*Brokersize*). Following Clement and Tse (2005), all analyst-level controls are normalized to range from 0 to 1, like the normalization of *Raw Optimism* to form Optimism. Definitions of these regression controls appear in Table A2, and their distributional summary statistics and correlations are reported in Table 1.

The main coefficient of interest in equation (eq:regeqn) is β_1 (i.e., the "DID coefficient"), which compares the average differences in forecast optimism between state-owned and non-state-owned brokerage analysts during intervention event periods to the average differences in forecast optimism of earnings forecasts between the two types of brokerage-firm analysts during non-event periods. Under our hypothesis that analysts at state-owned brokerages may work to fulfill the government's incentives to manage or prop up the stock market, we expect a positive and significant β_1. That is, we expect analysts at state-owned brokerages to issue more optimistic earnings forecasts during periods when the government had greater incentives to boost the stock market.

3.2 Main Results

We begin by examining how government-brokerage analysts' forecast optimism differs from that of analysts at non-state-owned brokerages during government intervention periods.

3.2.1 Earnings-Forecast Optimism during Government Intervention Periods

Table 2, Panel A, column 2, shows that, during intervention periods, *Optimism* declines overall by about 13% (from 0.47 to 0.42). This pattern reflects the fact that the majority of the intervention periods we consider—the four market rescues and the Olympics, which overlaps with the second market rescue—are characterized by significant market declines, during which the fundamentals of the economy were anticipated to decline. Columns 3-5 show that, although a decline in *Optimism* characterizes the forecasts of both government-brokerage and non-government-brokerage analysts (row 3, columns 3 and 4), the state-owned-brokerage analysts are relatively more optimistic (column 5). Another approach is to compare the first two rows of Panel A: though non-government-brokerage analysts are on average slightly more optimistic during non-intervention periods, government-brokerage analysts are on average significantly more optimistic (at the 1% level) during intervention periods. Indeed, Panel B shows that, in all seven intervention periods, mean *Optimism* is higher among government-brokerage analysts than among non-government-brokerage analysts. During five of the seven events, the difference in mean *Optimism* is statistically significant at the 10% level.

We then examine whether these univariate results are robust to the inclusion of controls. Table 3 reports DID regression estimates (following Eq.2) of how government incentives during intervention periods affect government-brokerage analysts' *Optimism*. Columns 1-3 examine each type of event separately—the four market-rescue events (*Rescue*), the two National Congress meetings (*Meeting*), and the Olympics (*Olympic*); column 4 pools all the events (*Event*). These multivariate tests are consistent with the univariate analyses. In each specification, we find a positive and statistically significant (at the 1% level) DID coefficient. The magnitude of the effect is also economically significant. Interpreting the coefficients in column 4, we find that *Optimism* among non-government-brokerage analysts declines on average by 0.039 during the intervention periods (the coefficient on *Event*), consistent with analysts' expectations of deteriorating fundamentals during these times; the government-brokerage analysts' forecasts undo about 44% of this decline in *Optimism*.

3.2.2 Heterogeneity in *Optimism* by Type of Forecast-Target Firm

To provide further evidence that the observed differential in *Optimism* reflects government-brokerage analysts' responses to government incentives, we test whether the baseline effects documented in Table 3 are most pronounced with reference to the firms that the government has the strongest incentives to support. Specifically, we argue that the central government of China had strong incentives to prop up the equity market during each of the seven events, either to minimize market panic (in the case of the four financial-market rescue events) or to create the impression of robust financial markets (in the cases of the Olympics and the National Congress meetings). Because firms with larger market capitalization have greater weight in (and thus greater influence on) the major stock indexes, we expect government analysts' *Optimism* during intervention periods to be concentrated on these firms.

Table 4, column 1, estimates the baseline DID specification of Table 3, column 4, but explores heterogeneity in the optimism effect for the 500 largest firms by market capitalization (*Weighted*) and all other firms (*Non-Weighted*). We split our observations of the treatment group (forecasts issued by government brokerage analysts) into forecasts for large firms and for all other firms. Indeed, we find the *Optimism* effect among government-brokerage analysts to be (i) concentrated in forecasts for *Weighted* firms and (ii) statistically larger than the effect on forecasts for *non-Weighted* firms.

We also expect the central government to have stronger incentives during these intervention event periods to support state-owned enterprises (SOEs), as shown in prior literature (e.g., Faccio et al., 2006; Faccio, 2006; Fisman and Wang, 2015). To test this hypothesis, Table 4, column 2, estimates the baseline DID specification of Table 3, column 4, but examines the optimism effect on government-brokerage analysts' earnings forecasts for SOEs and for non-SOEs. Consistent with our expectations, we find that the relative optimism among government-brokerage analysts during government intervention periods is concentrated in forecasts for *SOE* firms, and is statistically larger than the effect of forecasts for *non-SOE* firms. Jointly, the analyses reported in Tables 3 and 4 support the hypothesis that, during periods when the central government had strong incentives to prop up the stock market, government-brokerage analysts respond to government incentives by issuing relatively optimistic earnings forecasts.

3.2.3 Heterogeneity in *Optimism* by Analyst Types

As we argued above, analysts at government-owned brokerage firms face a tradeoff between their external reputations and their internal promotional prospects (Jackson and Moerke, 2005; Cowen et al., 2006). The main analyses of Tables 2 and 3 suggest that analysts at government-brokerage firms attempt to balance these conflicting considerations. During intervention periods, particularly those that coincide with negative economic shocks, government-brokerage analysts who issued forecasts on average revised downward, in keeping with the declining fundamentals, which suggests that they care about preserving credibility in the marketplace. But our DID estimates also show that these analysts' forecasts are relatively more optimistic—they revise less severely than do non-government-brokerage analysts during economic downturns—which suggests a degree of compliance with the government's incentives.

We further examine compliance with the government's incentives during intervention periods by studying the relative *Optimism* of different types of government-brokerage analysts. Table 5 estimates the baseline DID specification of Table 3, column 4, but splits the "treatment" observations into mutually exclusive types: column 1 splits the earnings forecasts of government-brokerage analysts into those issued by inexperienced (i.e., where *Firmexp* is less than 0.5) and experienced analysts; column 2 splits the same forecasts into those issued by star and non-star analysts;[6] column 3 splits the same forecasts into those issued by analysts who frequently issue forecasts for the firm in question (i.e., *Frequency* is greater than 0.5) and those by analysts who do not do so frequently; and column 4 splits the same forecasts into those issued by analysts at brokerages where commission fees account for more than 30% of total revenues (*High Commission*) and those at other government-owned brokerages.

The results of Table 5 suggest that the *Optimism* effect is pervasive across different types analysts. We find the effect to be equally large in magnitude (and in statistical significance) among inexperienced and experienced analysts, star and non-star analysts, and frequent and infrequent forecasters. We also find government-brokerage analysts' relative optimism during intervention periods to be similar among analysts at commission-driven and less commission-driven brokerages. This finding suggests that these analysts' relative optimism is not due to differences in brokerage-firm economics, providing further support for the hypothesis that government-brokerage analysts respond to government incentives during intervention periods.

[6]Star status is bestowed by the magazine *New Fortune*. Like *Institutional Investor*'s selection of star analysts in the United States, star analysts in China are selected annually by surveying institutional investors.

3.2.4 Alternative Channels of Government-Brokerage Analysts' Relative Optimism

We further examine the different channels or manners through which government-brokerage analysts' relative optimism may manifest during government intervention periods. We first determine whether the pattern of relative *Optimism* also characterizes analysts' stock recommendations. If government-brokerage analysts were complying with government incentives to prop up the stock market, we would expect to see complementary evidence in stock recommendations.

To test this hypothesis, we estimate the four specifications of Table 3 using *REC*—which assigns the recommendations "strong buy," "buy," "hold," "sell," and "strong sell" the respective numerical values 1, 0.75, 0.5, 0.25, and 0—as the dependent variable of interest. The results, reported in Table 6, show that, government-brokerage analysts on average make relatively more optimistic stock recommendations during each of the intervention periods, consistent with our empirical results using earnings-forecast optimism.

We also examine analysts' revisions (i.e., new earnings forecasts) during intervention periods. To perform such an assessment, we select from our sample only those observations in which an analyst issues a revision fewer than 200 days after a prior forecast for the same firm and year, resulting in 118,814 one-year-ahead forecasts between 2005 and 2015. Following prior literature (e.g., Imhoff and Lobo, 1984; Stickel, 1991), forecast revision (*Revision*) is calculated as the difference between an EPS forecast (current forecast) and the most recent prior EPS forecast issued by the same analyst for the same firm for the same year (prior forecast), deflated by prior forecast. Using *Revision* as the primary dependent variable of interest, we again estimate the four specifications of Table 3.[7] Interestingly, we do

[7] Our results are similar if we scale the revision measure in a fashion similar to Eq. 1.

not find statistically or economically significant differences between the revisions issued by government-brokerage analysts and non-government-brokerage analysts during any of the government intervention periods.

We further examine the time lag between the current and most recent prior forecast (*Forecast Gap*). Table 8 estimates the four specifications of Table 3, but uses *Forecast Gap* as the dependent variable of interest. Table 8, column 1, suggests that when government-brokerage analysts issue forecast revisions during market-rescue periods, they do so more slowly, or revise older prior forecasts. Interestingly, during non-event periods government-brokerage analysts tend to update more frequently (i.e., the interval between forecasts tends to be shorter) than non-government-brokerage analysts (i.e., the negative and statistically significant coefficient on *GovBro*). This comparative promptness disappears, however, during market downturns (i.e., the sum of the coefficients on *GovBro* × *Rescue* and *GovBro* is statistically no different from 0).

Column 2 suggests a relative delay in government-brokerage analysts' forecasts during the National Congress Meetings. However, this relative delay appears to be driven by increased promptness on the part of non-government-brokerage analysts at these times (i.e., the negative and significant coefficient on *Meeting*). Specifically, in examining the behavior of government-brokerage analysts, we find no differential delay (i.e., the sum of the coefficients on *GovBro* × *Meeting* and *Meeting* is statistically no different from 0).

Finally, in column 3 we do not find evidence of differential delay during the Olympics. Although the coefficient on *GovBro*×*Olympic* is positive, we do not obtain statistical significance at the 10% level.

Jointly, our findings suggest that government-brokerage analysts comply with the government's incentives at least in part by delaying (downward) forecast revisions during economic downturns. However, when these analysts revise, they do so in a manner similar to that of non-government-brokerage analysts, suggesting that government-brokerage analysts are weighing internal promotional incentives against external reputational concerns.

3.2.5 Distinguishing the Compliance Hypothesis from the Information Hypothesis

Our overall evidence is consistent with the hypothesis that government-brokerage analysts communicate relatively optimistic information to market participants to comply with the government's incentives during intervention periods ("the compliance hypothesis"). But these results could also signify that government-brokerage analysts possess superior information about firms at these times ("the information hypothesis"). For example, state-owned brokerage firms may be more capable of acquiring information about firms' future prospects during uncertain or bad times, in particular they may be able to predict which firms will receive preferential assistance (i.e., a bailout) from the Chinese government. If so, our main results would reflect a differential information-quality effect rather than a differential optimism effect.

To ascertain whether the information hypothesis explains our main findings, we test whether earnings forecasts issued by government-brokerage analysts during intervention periods exhibit differential accuracy. Under the information hypothesis, we expect forecasts issued by government-brokerage analysts during the intervention periods to be relatively more accurate; under the compliance hypothesis, we expect the forecasts of government-brokerage analysts during intervention periods to be relatively less accurate.

Table 9 reports the results of this test for each event type and for all the events together. We estimate the four specifications of Table 3, but use *Accuracy* as the dependent variable of interest. The results in Table 9 suggest that the forecasts issued by government-brokerage-analysts during intervention periods are on average relatively less accurate: the coefficients on each of the four DID coefficients are negative and statistically significant at the 1% level. The analysis in Table 9 thus contradicts the information hypothesis and supports the compliance hypothesis.

3.2.6 Assessing Market Impact

Having established that our findings of relative optimism among government-brokerage analysts are consistent with compliance with government incentives, we conclude the empirical analyses by assessing whether such optimism is likely to have an impact on market participants' beliefs. It is difficult to directly test how analysts' forecasts and recommendations influence investors' beliefs, but we can make inferences based on how market prices react to forecast revisions issued by government-brokerage analysts and those issued by non-government-brokerage analysts.

To perform such a test, we compute two measures of cumulative abnormal return (using the market model) around each forecast revision: the first is measured from one day before until one day after the date of the revision ($CAR(-1,1)$); the second is measured from two days before until two days after the date of the revision ($CAR(-2,2)$).

Table 10, columns 1 and 3, report coefficient estimates from regressions of $CAR(-1,1)$ and $CAR(-2,2)$ respectively, on *GovBro*, *Revision*, and *GovBro*$\times$*Revision*, and the set of controls included in the main tests in Table 3. In each specification, we obtain a positive and significant coefficient on *Revision*: that is, when non-government-brokerage analysts revise their forecasts, more positive revisions yield more positive market responses and vice versa. In both specifications, we obtain a coefficient on *GovBro*$\times$*Revision* that is not statistically different from 0 at the 10% level, suggesting that markets do not react differently, on average, to the forecast revisions of the two types of analysts, and that government-brokerage analysts' relative optimism is likely to impact the market's overall assessment of firms' future prospects.

In Table 10, columns 2 and 4, we estimate the same specifications as above but split the revisions into two types: those issued during intervention periods (*Event*) and during non-intervention periods (*Non-Event*). In column 2 we find that, though market participants react more strongly, in terms of $CAR(-1, 1)$, to forecast revisions issued by government-brokerage analysts during intervention periods (i.e., a positive coefficient on *Revision & Event* $\times$ *GovBro*), this effect is not statistically significant at the 10% level. Furthermore, though market participants react less strongly to forecast revisions issued by government-brokerage analysts during non-intervention periods (i.e., a negative coefficient on *Revision & Non-Event* $\times$ *GovBro*), this effect too is not statistically significant at the 10% level. However, when we compare market responses to government- and non-government-brokerage analysts' revisions, the event-period differences are significantly more positive than the non-event-period differences. In particular, the last two rows in the table report the F-statistics and the associated p-values from a Wald test of the null hypothesis that *Revision & Event* $\times$ *GovBro* $-$ *Revision & Non-Event* $\times$ *GovBro* $= 0$, which is rejected at the 5% level.

Column 4 shows very similar results using $CAR(-2, 2)$. Market participants react more strongly, in terms of $CAR(-2, 2)$, to forecast revisions issued by government-brokerage analysts during intervention periods (i.e., a positive coefficient on *Revision & Event* $\times$ *GovBro*), an effect that is statistically significant at the 1% level. Furthermore, though market participants react less strongly to forecast revisions issued by government-brokerage analysts during non-intervention periods (i.e., a negative coefficient on *Revision & Non-Event* $\times$ *GovBro*), this effect is not statistically significant at the 10% level. Finally, we find in column 4 that, when we compare market responses to government- and non-government-brokerage analysts' revisions, the event-period differences are significantly more positive than the non-event period differences. The Wald test of the null hypothesis that *Revision* & *Event* $\times$ *GovBro* $-$ *Revision* & *Non-Event* $\times$ *GovBro* $= 0$ is rejected at the 1% level.

Overall, our analysis provides some evidence that markets respond more strongly to the revisions of government-brokerage analysts. In combination with our findings in Table 7 that the revisions of the two types of analysts on average do not differ during intervention periods, this evidence suggests that the information production of government-brokerage analysts, and their relative optimism at such times, is likely to affect investors' beliefs about the covered firms' future prospects. Thus our findings highlight an institutional void in the Chinese capital market.

4 Conclusion

In many countries the central government plays a critical role in coordinating capital-market institutions, and the government's incentives can influence the functioning of these institutions. This paper examines the functioning of an important information intermediary in capital markets—sell-side analysts—by studying how analysts at government-owned brokerages respond to government incentives.

We find that, at times when China's central government had strong incentives to prop up the stock market, government-brokerage analysts tended to issue relatively more optimistic earnings forecasts and stock recommendations. Though these optimistic forecasts are also relatively less accurate, market participants appear to respond to them more strongly than they do to revisions issued by non-government-brokerage analysts. Our evidence thus suggest that such relative optimism is likely to influence investors' beliefs.

Much has been written about analysts' incentives and about how these incentives affect analysts' forecasts and recommendations. Our work is the first to examine analysts' incentives vis-a-vis the government in a context where government can have a significant influence on capital-market institutions. Our work highlights the role of government incentives in analysts' behavior and output in such contexts. We hope that empirical evidence of a critical institutional void will contribute to strengthening Chinese capital-market institutions in the future.

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Table A1. Government Intervention Events and Periods

Government Intervention Events	Intervention Periods	Notes
1st rescue	1/23/2005–6/6/2005	
2nd rescue	4/24/2008–10/30/2008	
3rd rescue	4/1/2012–12/4/2012	
4th rescue	7/1/2015–12/31/2015	
1st National Congress of CPC	8/1/2007–1/31/2008	The meeting was held 10/15/2007–10/21/2007
2nd National Congress of CPC	9/1/2012–2/28/2013	The meeting was held 11/2012–11/14/2012
Beijing 2008 Olympic Games	6/1/2008–11/31/2008	The games took place 8/8/2008–8/24/2008

Table A2. Definitions of Variables

Table A2 reports definitions of variables used in our regressions.

Variable	Definition
Optimism	Forecast optimism (analyst EPS forecast minus actual EPS) for analyst i following firm j in year t minus the minimum forecast optimism for analysts who follow firm j in year t, with this difference scaled by the range in forecast optimism for analysts following firm j in year t.
Rescue	Equals 1 if forecasts are issued during a market-rescue period (1/23/2005–6/5/2005; 4/24/2008–10/30/2008; 4/1/2012–12/4/2012; 7/1/2015–12/31/2015) and 0 otherwise.
Meeting	Equals 1 if forecasts are issued within the six-month period surrounding the meetings of the National Congress of Chinese Communist Party (8/1/2007–1/31/2008; 9/1/2012–2/28/2013) and 0 otherwise.
Olympic	Equals 1 if forecasts are issued within the six-month period surrounding the Beijing 2008 Olympic Games (6/1/2008–11/30/2008) and 0 otherwise.
Event	Equals 1 if *Rescue* equals 1 or *Meeting* equals 1 or *Olympic* equals 1, and 0 otherwise.
GovBro	Equals 1 if a brokerage is ultimately controlled by state-owned enterprises or by the State-owned Assets Supervision and Administration Commission of the State Council and 0 otherwise.
Firmexp	Number of days of firm-specific experience for analyst i who follows firm j in year t, minus the minimum number of days of firm-specific experience for analysts who follow firm j in year t. This difference is scaled by the range in number of days of firm-specific experience of analysts following firm j in year t.
Genexp	Number of days of general experience for analyst i, who follows firm j in year t, minus the minimum number of days of general experience for analysts who follow firm j in year t. This difference is scaled by the range in number of days of general experience of analysts following firm j in year t.
Industries	The difference between the number of industries (with the same two-digit CSRC industry code) followed by analyst i, who follows firm j in year t and the minimum number of industries followed by analysts who follow firm j in year t. This difference is scaled by the range in the number of industries followed by analysts who follow firm j in year t.
Frequency	Number of firm-j forecasts made by analyst i, who follows firm j in year t, minus the minimum number of firm-j forecasts for analysts who follow firm j in year t. This difference is scaled by the range in the number of firm-j forecasts issued by analysts who follow firm j in year t.
Horizon	The difference between the number of days from the forecast date to fiscal year-end for analyst i who follows firm j in year t and the minimum number of days from the forecast date to fiscal year-end for analysts who follow firm j in year t. This difference is scaled by the range in the number of days from the forecast date to fiscal year-end for analysts who follow firm j in year t.
Brokersize	The difference between the number of analysts employed by the brokerage employing analyst i, who follows firm j in year t, and the minimal number of analysts employed by brokerages whose analysts follow firm j in year t, deflated by the range in the number of analysts employed by the brokerage whose analysts follow firm j in year t.
Faccuracy	Forecast error (absolute value of the difference between EPS forecast and actual EPS) for analyst i who follows firm j in year t, minus the minimum forecast error for analysts who follow firm j in year t. This difference is scaled by the range in forecast error for analysts who follow firm j in year t.

FGAP	The difference between the number of days elapsed since the last forecast about the same firm by analyst i, who follows firm j in year t, and the minimum number of days elapsed since the last forecast about the same firm by analysts who follow firm j in year t. This difference is scaled by the range in the number of days elapsed since the last forecast about the same firm by analysts who follow firm j in year t.
REC	Equals 1 for strong buy, 0.75 for buy, 0.5 for neutral, 0.25 for sell, and 0 for strong sell.
CAR	Cumulative abnormal return calculated using a market model.
Revision	The difference between the current EPS forecasts (current forecast) and the most recent EPS forecast issued by the same analyst about the same firm for the same year (prior forecast), deflated by the prior forecast.

Table 1.

Descriptive Statistics: Main Variables

This table provides descriptive statistics on the variables in our main sample, which consists of 234,328 one-year-ahead analysts' forecasts issued from 2005 to 2015. Panel A reports the following pooled distributional summary statistics for each variable: sample minimum (Min), 25^{th} percentile (P25), average (Mean), 50^{th} percentile (Median), 75^{th} percentile (P75), maximum (Max), and standard deviation (SD). Panel B reports the correlation table, where significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

Panel A: Distributional Summary Statistics

	Min	P25	Mean	Median	P75	Max	SD
Optimism	0	0.173	0.455	0.434	0.714	1	0.312
GovBro	0	1	0.862	1	1	1	0.345
Rescue	0	0	0.208	0	0	1	0.406
Meeting	0	0	0.084	0	0	1	0.277
Olympic	0	0	0.034	0	0	1	0.180
Event	0	0	0.256	0	1	1	0.436
Firmexp	0	0.037	0.372	0.290	0.667	1	0.340
Genexp	0	0.251	0.516	0.522	0.799	1	0.314
Frequency	0	0.167	0.495	0.500	1	1	0.377
Companies	0	0.109	0.354	0.264	0.531	1	0.305
Industries	0	0.086	0.321	0.222	0.500	1	0.305
Horizon	0	0.330	0.568	0.571	0.890	1	0.322
Brokersize	0	0.258	0.502	0.477	0.761	1	0.313
Revision	-0.038	-0.002	-0.002	0	0	0.024	0.008
CAR(-1,1)	-0.122	-0.021	0.006	0.003	0.031	0.169	0.049
CAR(-2,2)	-0.146	-0.026	0.007	0.003	0.037	0.197	0.058
Faccuracy	0	0.389	0.641	0.739	0.929	1	0.325
Fgap	0	0	0.446	0.295	1	1	0.435
REC	0	0.750	0.819	0.750	1	1	0.167

Table 1

Continued

Panel B: Correlation Table

	Optimism	*Rescue*	*Meeting*	*Olympic*	*Event*	*GovBro*	*Firmexp*	*Genexp*	*Frequency*	*Companies*	*Industries*	*Horizon*	*Brokersize*	*Accuracy*	*FGap*
Optimism	1.00														
Rescue	-0.05***	1.00													
Meeting	-0.08***	0.20***	1.00												
Olympic	-0.03***	0.32***	-0.06***	1.00											
Event	-0.07***	0.88***	0.52***	0.32***	1.00										
GovBro	0.00	-0.03***	-0.03***	0.03***	-0.02***	1.00									
Firmexp	-0.03***	0.00	0.02***	-0.01***	0.01***	0.04***	1.00								
Genexp	-0.02***	-0.01***	0.02***	0.00*	0.00	0.06***	0.45***	1.00							
Frequency	0.01***	0.00	0.02***	-0.03***	0.00	0.02***	0.25***	0.11***	1.00						
Companies	0.01***	-0.01***	-0.05***	-0.04***	-0.03***	-0.02***	0.08***	0.21***	0.05***	1.00					
Industries	0.01***	-0.01***	-0.04***	-0.03***	-0.02***	-0.02***	-0.01***	0.14***	-0.00	0.74***	1.00				
Horizon	0.32***	-0.08***	-0.24***	-0.04***	-0.17***	0.00*	-0.09***	-0.06***	-0.04***	-0.02***	-0.03***	1.00			
Brokersize	0.01***	-0.02***	-0.01***	0.04***	-0.01***	0.09***	0.23***	0.25***	0.15***	0.10***	-0.03***	0.00	1.00		
Accuracy	-0.57***	0.03***	0.13***	0.02***	0.07***	-0.00	0.05***	0.04***	0.05***	-0.03***	-0.04***	-0.45***	0.00	1.00	
FGap	-0.03***	-0.02***	-0.02***	0.01***	-0.01***	-0.00*	-0.01***	-0.01***	-0.09***	-0.00	-0.00	-0.02***	0.00	0.02***	1.00

Table 2.

Descriptive Statistics: Earnings-Forecast Optimism by Intervention Events

This table reports the means of our main dependent variable of interest, *Optimism*, between the earnings forecasts issued by state-owned (*GovBro*=1) and non-state-owned (*GovBro*=0) brokerage firms, as well as their mean differences (*Diff(0-1)*). Panel A pools all intervention events, described in Table A1, and compares the event (*Event*=1) and non-event (*Event*=0) means. Panel B reports, for each event, the mean *Optimism* for the relevant sub-sample. Variables are defined in Table A2. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

		Optimism			
	(1) *N*	(2) Total Sample	(3) *GovBro*=0	(4) *GovBro*=1	(5) *Diff(0-1)*
Panel A: All Events					
Event=0	174,440	0.4669	0.4702	0.4664	0.0038*
Event=1	59,888	0.4190	0.4055	0.4214	-0.0159***
Diff(0-1)		0.0479***	0.0647***	0.0450***	0.0197***
Panel B: Individual Events					
1st Rescue	593	0.5947	0.5767	0.5955	-0.0188
2nd Rescue	8,487	0.4451	0.4400	0.4457	-0.0056
3rd Rescue	28,211	0.4373	0.4292	0.4391	-0.0099**
4th Rescue	11,520	0.3630	0.3468	0.3654	-0.0186**
National Congress	19,577	0.3709	0.3390	0.3774	-0.0384***
Olympic	7,845	0.4076	0.3901	0.4093	-0.0192*

Table 3.

Government Intervention Periods and Earnings-Forecast Optimism

This table reports the results of OLS regressions of *Optimism* on an event indicator, an indicator for a state-owned brokerage firm (*GovBro*), an interaction of the two indicators, and analyst-level control variables. Column 1 examines differential forecast optimism during the market-rescue events (*Rescue*); column 2 examines differential forecast optimism during the National Congress meetings (*Meeting*); column 3 examines differential forecast optimism during the Olympics (*Olympic*); and column 4 examines differential forecast optimism during all three types of events (*Event*). All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

	(1) *Optimism*	(2) *Optimism*	(3) *Optimism*	(4) *Optimism*
GovBro × *Rescue*	0.0119***			
	(0.002)			
Rescue	-0.0460***			
	(0.009)			
GovBro × *Meeting*		0.0263***		
		(0.010)		
Meeting		-0.0386		
		(0.027)		
GovBro × *Olympic*			0.0136***	
			(0.001)	
Olympic			-0.0446***	
			(0.007)	
GovBro × *Event*				0.0170***
				(0.003)
Event				-0.0390*
				(0.023)
GovBro	-0.0056***	-0.0057**	-0.0033	-0.0077**
	(0.002)	(0.003)	(0.002)	(0.003)
Firmexp	-0.0018	-0.0014	-0.0015	-0.0016
	(0.004)	(0.003)	(0.004)	(0.004)
Genexp	-0.0046	-0.0042	-0.0040	-0.0044
	(0.004)	(0.005)	(0.005)	(0.005)
Frequency	0.0032***	0.0032***	0.0032***	0.0032***
	(0.001)	(0.001)	(0.001)	(0.001)
Companies	-0.0133*	-0.0144**	-0.0143**	-0.0139**
	(0.007)	(0.007)	(0.007)	(0.007)
Industries	0.0270***	0.0272***	0.0273***	0.0270***
	(0.005)	(0.005)	(0.005)	(0.005)
Horizon	0.3068***	0.3069***	0.3094***	0.3048***
	(0.037)	(0.035)	(0.037)	(0.035)
Brokersize	0.0090**	0.0088**	0.0086**	0.0088**
	(0.004)	(0.004)	(0.004)	(0.004)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	234,328	234,328	234,328	234,328
Adj R^2	0.1105	0.1097	0.1097	0.1100

Table 4.

Heterogeneous Effects on Earnings-Forecast Optimism by Type of Covered Firm

Our sample consists of 234,328 one-year-ahead analysts' forecasts issued from 2005 to 2015. This table reports OLS results of regressing forecast optimism on each event and an interaction between the event indicator and an indicator for government brokerage. "Type" denotes indicator variables for (1) firms whose market capitalization ranked among the top 500 of publicly-listed firms in China; and (2) state-owned enterprises (SOE). Variables are defined in Appendix 1. All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Type: Weighted*	(2) *Type: SOE*
GovBro & Type × Event	0.0338***	0.0382***
	(0.005)	(0.010)
GovBro & Non-Type × Event	-0.0039	-0.0008
	(0.004)	(0.003)
GovBro & Type	-0.0184***	-0.0030
	(0.004)	(0.006)
GovBro & Non-Type	0.0051	-0.0113***
	(0.005)	(0.003)
Event	-0.0398*	-0.0402*
	(0.022)	(0.022)
Firmexp	-0.0016	-0.0017
	(0.004)	(0.003)
Genexp	-0.0041	-0.0049
	(0.005)	(0.005)
Frequency	0.0035***	0.0032***
	(0.001)	(0.001)
Companies	-0.0147**	-0.0137**
	(0.007)	(0.007)
Industries	0.0262***	0.0269***
	(0.005)	(0.005)
Horizon	0.3051***	0.3038***
	(0.035)	(0.035)
Brokersize	0.0093***	0.0085**
	(0.004)	(0.004)
Time FE	Yes	Yes
Industry FE	Yes	Yes
Observations	234,328	234,328
Adj R^2	0.1109	0.1112
F-Stat	23.76	10.16
p-Value	0.00	0.00

Table 5.

Heterogeneous Effects on Earnings-Forecast Optimism by Analyst Type

Our sample consists of 234,328 one-year-ahead analysts' forecasts issued from 2005 to 2015. This table reports OLS results of regressing forecast optimism on each event and an interaction between the event indicator and an indicator for government-brokerage analysts of different types. In each specification, "Type" denotes indicator variables for (1) analysts with less experience (i.e., where *Firmexp* is less than 0.5); (2) star analysts; (3) analysts who issue forecasts frequently (i.e., *Frequency* is greater than 0.5); and (4) analysts at brokerage firms where commission fees account for more than 30% of total revenue respectively. Variables are defined in Appendix 1. All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) *Type: Inexperienced Analyst*	(2) *Type: Star Analyst*	(3) *Type: Frequent Analyst*	(4) *Type: High Commission*
GovBro & Type × Event	0.0169***	0.0157***	0.0173***	0.0171***
	(0.004)	(0.006)	(0.003)	(0.005)
GovBro & Non-Type × Event	0.0174***	0.0173***	0.0159***	0.0226***
	(0.005)	(0.004)	(0.004)	(0.008)
GovBro & Type	-0.0053	-0.0081*	0.0028	-0.0062
	(0.004)	(0.005)	(0.005)	(0.004)
GovBro & Non-Type	-0.0105**	-0.0076**	-0.0144***	-0.0186***
	(0.004)	(0.003)	(0.003)	(0.006)
Event	-0.0391*	-0.0390*	-0.0386*	-0.0400*
	(0.023)	(0.023)	(0.023)	(0.022)
Firmexp	-0.0016	-0.0016	-0.0038	-0.0016
	(0.003)	(0.004)	(0.003)	(0.004)
Genexp	0.0016	-0.0042	-0.0038	-0.0046
	(0.008)	(0.004)	(0.005)	(0.005)
Frequency	0.0032***	0.0032***	0.0016*	0.0032***
	(0.001)	(0.001)	(0.001)	(0.001)
Companies	-0.0139**	-0.0137**	-0.0153**	-0.0142**
	(0.007)	(0.007)	(0.007)	(0.007)
Industries	0.0272***	0.0270***	0.0271***	0.0272***
	(0.005)	(0.005)	(0.005)	(0.005)
Horizon	0.3050***	0.3048***	0.3052***	0.3050***
	(0.035)	(0.035)	(0.035)	(0.035)
Brokersize	0.0089**	0.0090***	0.0082**	0.0102**
	(0.004)	(0.003)	(0.003)	(0.004)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	234,328	234,328	234,328	234,328
Adj R^2	0.1100	0.1100	0.1104	0.1101
F-Stat	0.01	0.04	0.14	0.25
p-Value	0.94	0.85	0.71	0.61

Table 6.
Stock-Recommendation Optimism

This table reports the results from OLS regressions of analysts' recommendations (*REC*) on an event indicator, an indicator for a state-owned brokerage firm (*GovBro*), an interaction of the two indicators, and analyst-level control variables. The dependent variable *REC* assigns the recommendations "strong buy," "buy," "hold," "sell," and "strong sell" the numerical values of 1,0.75,0.5,0.25, and 0 respectively. Column 1 examines differential recommendations during market-rescue events (*Rescue*); column 2 examines differential recommendations during the National Congress meetings (*Meeting*); column 3 examines differential recommendations during the Olympics (*Olympic*); and column 4 examines differential recommendations during all three types of events (*Event*). All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levelss. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

	(1)	(2)	(3)	(4)
	REC	*REC*	*REC*	*REC*
GovBro × *Rescue*	0.0090***			
	(0.003)			
Rescue	-0.0106			
	(0.011)			
GovBro × *Meeting*		0.0100**		
		(0.005)		
Meeting		-0.0158**		
		(0.007)		
GovBro × *Olympic*			0.0173***	
			(0.003)	
Olympic			-0.0491***	
			(0.003)	
GovBro × *Event*				0.0087**
				(0.004)
Event				-0.0098
				(0.009)
GovBro	-0.0250***	-0.0241***	-0.0234***	-0.0253***
	(0.004)	(0.004)	(0.004)	(0.004)
Firmexp	0.0018	0.0019	0.0021	0.0018
	(0.003)	(0.003)	(0.003)	(0.003)
Genexp	-0.0027	-0.0028	-0.0028	-0.0027
	(0.005)	(0.005)	(0.005)	(0.005)
Frequency	0.0556***	0.0555***	0.0554***	0.0555***
	(0.005)	(0.005)	(0.005)	(0.005)
Companies	-0.0499***	-0.0500***	-0.0498***	-0.0499***
	(0.006)	(0.006)	(0.006)	(0.006)
Industries	-0.0013	-0.0014	-0.0015	-0.0014
	(0.005)	(0.005)	(0.005)	(0.005)
Horizon	-0.0154***	-0.0168***	-0.0162***	-0.0157***
	(0.005)	(0.006)	(0.005)	(0.005)
Brokersize	0.0108	0.0107	0.0105	0.0108
	(0.009)	(0.009)	(0.009)	(0.009)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	291,090	291,090	291,090	291,090
Adj R^2	0.0796	0.0797	0.0802	0.0796

Table 7.

Earnings-Forecast Revision

This table reports the results from OLS regressions of the number of earnings-forecast revisions (*Accuracy*) on an event indicator, an indicator for a state-owned brokerage firm (*GovBro*), an interaction of the two indicators, and analyst-level control variables. Column 1 examines differential accuracy during the market-rescue events (*Rescue*); column 2 examines differential accuracy during the National Congress meetings (*Meeting*); column 3 examines differential accuracy during the Olympics (*Olympic*); and column 4 examines differential accuracy during all three types of events (*Event*). All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

	(1) *Revision*	(2) *Revision*	(3) *Revision*	(4) *Revision*
GovBro × *Rescue*	0.0000			
	(0.000)			
Rescue	-0.0015***			
	(0.000)			
GovBro × *Meeting*		-0.0001		
		(0.000)		
Meeting		0.0021***		
		(0.000)		
GovBro × *Olympic*			-0.0001	
			(0.000)	
Olympic			-0.0013***	
			(0.000)	
GovBro × *Event*				0.0001
				(0.000)
Event				-0.0010
				(0.001)
GovBro	-0.0000	-0.0000	-0.0000	-0.0001
	(0.000)	(0.000)	(0.000)	(0.000)
Firmexp	-0.0012***	-0.0012***	-0.0012***	-0.0012***
	(0.000)	(0.000)	(0.000)	(0.000)
Genexp	0.0003***	0.0003***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)
Frequency	0.0002***	0.0002***	0.0002***	0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)
Companies	-0.0003***	-0.0003***	-0.0004***	-0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Industries	0.0002	0.0002*	0.0002*	0.0002*
	(0.000)	(0.000)	(0.000)	(0.000)
Horizon	-0.0004	0.0002	-0.0003	-0.0005
	(0.000)	(0.000)	(0.001)	(0.000)
Brokersize	0.0001	0.0001	0.0001	0.0001
	(0.000)	(0.000)	(0.000)	(0.000)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	118,814	118,814	118,814	118,814
Adj R^2	0.0268	0.0274	0.0247	0.0251

Table 8.

Days Elapsed since Last Forecast

This table reports the results from OLS regressions of the number of days elapsed since the prior forecast (*FGAP*) on an event indicator, an indicator for a state-owned brokerage firm (*GovBro*), an interaction of the two indicators, and analyst-level control variables. Column 1 examines differential days elapsed during the market-rescue events (*Rescue*); column 2 examines differential days elapsed during the National Congress meetings (*Meeting*); column 3 examines differential days elapsed during the Olympics (*Olympic*); and column 4 examines differential days elapsed during all three types of events (*Event*). All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

	(1) *Forecast Gap*	(2) *Forecast Gap*	(3) *Forecast Gap*	(4) *Forecast Gap*
GovBro × *Rescue*	0.0134***			
	(0.004)			
Rescue	-0.0547			
	(0.043)			
GovBro × *Meeting*		0.0128**		
		(0.006)		
Meeting		-0.0804***		
		(0.017)		
GovBro × *Olympic*			0.0011	
			(0.004)	
Olympic			-0.0179***	
			(0.004)	
GovBro × *Event*				0.0114**
				(0.005)
Event				-0.0536
				(0.050)
GovBro	-0.0109***	-0.0096***	-0.0078***	-0.0110***
	(0.002)	(0.001)	(0.002)	(0.003)
Firmexp	-0.0190***	-0.0182***	-0.0185***	-0.0188***
	(0.003)	(0.003)	(0.003)	(0.003)
Genexp	-0.0130***	-0.0133***	-0.0129***	-0.0131***
	(0.004)	(0.005)	(0.004)	(0.004)
Companies	0.0049**	0.0029	0.0038**	0.0042**
	(0.002)	(0.002)	(0.002)	(0.002)
Industries	0.0057**	0.0059**	0.0060**	0.0057**
	(0.003)	(0.003)	(0.003)	(0.003)
Horizon	-0.0330***	-0.0433**	-0.0294***	-0.0388***
	(0.009)	(0.018)	(0.008)	(0.012)
Brokersize	0.0065**	0.0067**	0.0063**	0.0066**
	(0.003)	(0.003)	(0.003)	(0.003)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	136,032	136,032	136,032	136,032
Adj R^2	0.0031	0.0035	0.0024	0.0030

Table 9.

Earnings-Forecast Accuracy

This table reports the results from OLS regressions of forecast accuracy (*Accuracy*) on an event indicator, an indicator for a state-owned brokerage firm (*GovBro*), an interaction of the two indicators, and analyst-level control variables. Column 1 examines differential accuracy during the market-rescue events (*Rescue*); column 2 examines differential accuracy during the National Congress meetings (*Meeting*); column 3 examines differential accuracy during the Olympics (*Olympic*); and column 4 examines differential accuracy during all three types of events (*Event*). All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively. Variables are defined in Table A2.

	(1) *Accuracy*	(2) *Accuracy*	(3) *Accuracy*	(4) *Accuracy*
GovBro × *Rescue*	-0.0081***			
	(0.001)			
Rescue	0.0163			
	(0.014)			
GovBro × *Meeting*		-0.0089***		
		(0.003)		
Meeting		0.0351*		
		(0.019)		
GovBro × *Olympic*			-0.0188***	
			(0.004)	
Olympic			0.0471***	
			(0.006)	
GovBro × *Event*				-0.0088***
				(0.002)
Event				0.0162
				(0.022)
GovBro	0.0028	0.0021	0.0014	0.0034
	(0.003)	(0.002)	(0.002)	(0.003)
Firmexp	-0.0033	-0.0035	-0.0034	-0.0033
	(0.003)	(0.002)	(0.003)	(0.003)
Genexp	0.0115***	0.0115***	0.0114***	0.0115***
	(0.004)	(0.004)	(0.004)	(0.004)
Frequency	0.0036***	0.0035***	0.0036***	0.0036***
	(0.001)	(0.001)	(0.001)	(0.001)
Companies	-0.0073	-0.0067	-0.0069	-0.0072
	(0.007)	(0.007)	(0.007)	(0.007)
Industries	-0.0325***	-0.0325***	-0.0326***	-0.0325***
	(0.007)	(0.007)	(0.007)	(0.007)
Horizon	-0.4529***	-0.4483***	-0.4530***	-0.4518***
	(0.023)	(0.020)	(0.023)	(0.021)
Brokersize	-0.0035	-0.0035	-0.0033	-0.0035
	(0.005)	(0.005)	(0.005)	(0.005)
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	234,328	234,328	234,328	234,328
Adj R^2	0.2161	0.2164	0.2162	0.2161

Table 10.
Market Reaction to Forecast Revision

This table reports OLS results of regressing cumulative abnormal three- and five-day returns surrounding a forecast revision on forecast revision, an indicator for a government brokerage, the interaction of the two and other control variables in columns 1 and 3. We further split the revisions into those issued during an event period and a non-event period in columns 2 and 4. The last two rows of the table report the F-statistics and associated p-values from a Wald test of the null hypothesis that *Revision* & *Event* $\times$ *GovBro* $-$ *Revision* & *Non-Event* $\times$ *GovBro* $= 0$. Our sample consists of 118,814 one-year-ahead analysts' forecasts that were issued from 2005 to 2015 and that were issued fewer than 200 days after the previous forecast issued by the same analyst about the same firm for the same year. Variables are defined in Appendix 1. All specifications include industry- and year-fixed effects. Standard errors, reported in parentheses, are two-way-cluster robust, clustering at the analyst and year levels. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	$CAR(-1,1)$ (%)		$CAR(-2,2)$ (%)	
	(1)	(2)	(3)	(4)
Revision $\times$ *GovBro*	-0.0230		0.0447	
	(0.056)		(0.073)	
Revision & *Event* $\times$ *GovBro*		0.0809		0.2053***
		(0.056)		(0.073)
Revision & *Non-Event* $\times$ *GovBro*		-0.0710		-0.0298
		(0.060)		(0.061)
Revision	0.4458***		0.4586***	
	(0.058)		(0.080)	
Revision & *Event*		0.3918***		0.3728***
		(0.106)		(0.108)
Revision & *Non-Event*		0.4736***		0.5026***
		(0.036)		(0.052)
GovBro	0.0009	0.0010	0.0012*	0.0013*
	(0.001)	(0.001)	(0.001)	(0.001)
Firmexp	0.0006	0.0006	0.0008	0.0008
	(0.001)	(0.001)	(0.001)	(0.001)
Genexp	-0.0016**	-0.0016**	-0.0020*	-0.0020*
	(0.001)	(0.001)	(0.001)	(0.001)
Frequency	0.0003***	0.0003***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)
Companies	-0.0002	-0.0002	-0.0000	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)
Industries	0.0019**	0.0019**	0.0023**	0.0023**
	(0.001)	(0.001)	(0.001)	(0.001)
Horizon	-0.0044	-0.0044	-0.0040	-0.0041
	(0.005)	(0.005)	(0.007)	(0.007)
Brokersize	0.0021***	0.0021***	0.0021**	0.0021**
	(0.001)	(0.001)	(0.001)	(0.001)
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Observations	118,814	118,814	118,814	118,814
Adj R^2	0.0093	0.0093	0.0087	0.0087
F-Stat		4.47		14.20
p-Value		0.03		0.00