JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 59, No. 5, Aug. 2024, pp. 2340–2374

doi:10.1017/S0022109023000637

https://doi.org/10.1017/S0022109023000637

Government Stock Purchase Undermines Price Informativeness: Evidence from China's "National Team"

Tri Vi Dang
Columbia University Department of Economics
td2332@columbia.edu

Wei Li
University of International Business and Economics School of Insurance and Economics
weili.uibe@gmail.com (corresponding author)

Yongqin Wang
Fudan University China Center for Economic Studies School of Economics and Fanhai International School of Finance (FISF)
yongqinwang@fudan.edu.cn

Abstract

We use the 2015 Chinese stock market crash to study the effects of government stock purchases. The Chinese government purchased stocks to stabilize the markets through state-owned financial institutions known as the "National Team." We find that the intervention led to reduced volatility and price informativeness. These impacts are driven by the disclosure of government portfolios. Consistent with investors having a stronger incentive to acquire government intervention information instead of fundamental news, we find reduced information production and information asymmetry following intervention disclosure. The article suggests that government stock purchases involve a trade-off between stability and informational efficiency.

I. Introduction

Unconventional measures to support domestic equity markets, especially by direct asset purchase, are not uncommon in the world. For instance, during the Asian financial crisis, the Hong Kong government spent 120 billion HKD to

We would like to thank Xiongfeng Ai, John Campbell, Greg Cox, Yun Dai, Julan Du, Hanming Fang, Huasheng Gao, Oliver Hart, Hua He, Zhiguo He, Harrison Hong, Stephen Morris, Greg Phelan, Yi Shen, Zhe Shen, Kaisi Sun (discussant), Lucy White, Ruojun Wu, Wei Xiong, Kai Yang, Siyuan Yang (discussant), Xiaoyang Zhuo (discussant), as well as seminar participants at Fudan University, Harvard University, Xiamen University, East China Normal University, 2019 TCFA conference, 2021 Camphor English Conference for Finance, 2022 Financial Engineering and Quantitative Finance Conference (Xiamen University), 2022 Asian Meeting of the Econometric Society in China (AMES 2022), and 2022 AsianFA Annual Conference for helpful comments and suggestions. We are particularly grateful to Jennifer Conrad (the editor), Martijn Cremers (a referee), and Yuhang Xing (a referee) for their insightful suggestions that tremendously improved the paper. All errors are our own. Dang acknowledges financial support from Hong Kong Institute for Monetary and Financial Research (HKIMR). Li acknowledges the financial support from the National Natural Science Foundation of China (Grant No. 72303067). Wang acknowledges financial support from the Natural Science Foundation of China (72073034 and 72121002).

purchase stocks through the HK Reserve Fund. Taiwan and South Korea also established financial stabilization funds.[1] As part of quantitative easing, the Bank of Japan has amassed nearly 19.3 trillion in index-linked exchange-traded funds (ETFs), or roughly 2.9% of the Tokyo Stock Exchange's total market capitalization (Charoenwong, Morck, and Wiwattanakantang (2021)). During the global financial crisis in 2008, the U.S. government invested in select financial institutions and purchased assets through various programs (Veronesi and Zingales (2010)). In response to the COVID-19 shock, Janet Yellen suggested that the lawmakers give the Fed more leeway with equity purchases in the future (`https://www.cnbc.com/2020/04/06/yellen-says-the-fed-doesnt-need-to-buy-equities-now-but-congress-should-reconsider-allowing-it.html`).

Despite their potentially wide-ranging impacts, stock market interventions have received scant attention in the academic literature. Due to the lack of counterfactuals, it is challenging to address the questions of whether and how direct interventions work, and what the potential trade-offs are. In this article, we focus on one such large-scale stock-level intervention in China to examine its impacts on price volatility, informational efficiency, and investor behavior. In contrast to interventions in other economies, which mostly focus on major index constituents, the Chinese stock market intervention includes a diverse range of stocks. Furthermore, the time lag between actual intervention trading and intervention portfolio disclosure facilitates the estimation of intervention disclosure impacts. We show that the disclosure of intervention portfolio could have unintended consequences.

Driven by the massive forced liquidation of highly leveraged investors, the Chinese stock markets experienced a month-long crash that wiped out about 40% of total market capitalization in mid-2015. As a response, the Chinese government tried to support the markets by directly purchasing stocks through four state-owned financial institutions. They were referred to as the "National Team (NT)" (*guojia dui*) in the media. By the end of the third quarter of 2015, the NT had acquired shares in at least 1,401 companies for 1.6 trillion RMB (or 4.3% of the total market value of all listed companies in that quarter). The government openly acknowledged that the NT's primary duty was to stabilize the market and it promised to remain in the following years, intervening as needed.[2]

How would direct equity purchasing programs such as the NT intervention affect market performance? Brunnermeier, Sockin, and Xiong (2022) propose a theoretical framework to analyze the impacts of government asset purchases on price volatility and informational efficiency. They assume three groups of agents: the government, noise traders, and strategic but myopic investors. Government

[1]South Korean government had plans to reactivate its stock market stability fund on Sept. 28, 2022, after its stock market plummeted. https://www.reuters.com/markets/asia/south-koreas-yoon-vows-steps-stabilise-fx-market-2022-10-07/.

[2]Announcement of the China Securities Regulatory Commission about the responsibility and future path of the NT (in Chinese): http://www.csrc.gov.cn/csrc/c101950/c1048128/content.shtml; Recent reports on the Chinese NT include: https://fortune.com/2021/03/09/china-stock-market-today-national-team-state-backed-investment-funds/; https://www.washingtonpost.com/business/when-stocks-crash-china-turns-to-its-national-team/2022/03/15/bd4d319e-a4d8-11ec-8628-3da4fa8f8714_story.html.

interventions can prevent market breakdown and volatility explosion by trading against noise traders. Nevertheless, Brunnermeier et al. (2022) also point out that under some circumstances, as the intervention materially impacts future stock returns, the intervention information better predicts future return than the fundamentals. Investors may be induced to acquire intervention information instead, resulting in lower informational efficiency. As a costly trade-off, government stock purchases reduce volatility but worsen informational efficiency.

Drawing on the theoretical framework in Brunnermeier et al. (2022), we empirically estimate the impacts of the Chinese NT intervention by decomposing the intervention impacts into two parts: a *direct trading effect* and a *disclosure effect.* The direct trading effect is how the intervention affects the market through trading (*What does the government do?*). The disclosure effect, on the other hand, captures the market feedback to the disclosure of the government portfolio details (*What does the market know and how does it respond?*). If the enhanced forecast precision due to intervention portfolio disclosure is material enough, investors may acquire intervention information instead of the fundamental information. This corresponds to the "government-centric equilibrium" Brunnermeier et al. (2022). Otherwise, if the disclosed intervention information does not result in a significant improvement in forecast accuracy, investors collect fundamental information regardless of the disclosure. This is the "fundamental-centric" case. Which equilibrium the Chinese market appears to be in is an empirical question.

Our sample includes all A-share stocks from July 2013 to June 2017, except for the CSI300 index constituents and the Chi-Next stocks.[3] We first examine the patterns of the NT intervention. Unlike previous interventions in other economies, which targeted major index constituents only, the NT's portfolio includes stocks of all size and profitability groups across almost all industries. The most prominent pattern of the NT trading is "buy low, sell high," which is in line with its duty of market stabilization. In accordance with the lower risk compensation demanded due to the perceived "government put," the intervened stocks subsequently underperformed compared to the unintervened stocks, particularly after the initial intervention.

For the overall impact of intervention, we conduct a difference-in-differences (DID)-like analysis. The intervened group consists of stocks in the NT's portfolio, while the remaining stocks are unintervened. The results show that the intervened stocks are associated with lower volatility. Also, a larger NT holding indicates weakly lower informational efficiency.

We then estimate the trading and the disclosure effects. Since the NT's portfolio was disclosed through quarterly statements, intervention disclosure lagged behind actual intervention trading. In the initial intervention period of 2015:Q3, the NT actively traded in the market and there was no public disclosure of its

[3]The CSI300 index is China's most widely acknowledged stock market index, comprising the 300 largest and most liquid A-share stocks. These stocks are the most likely to be intervened and would be anticipated to be intervention targets. To minimize selective intervention concern and also to exclude the impacts of investor anticipation, we exclude the CSI300 index constituents from our sample. Furthermore, trading in CSI300 futures was restricted during this time, which may have influenced spot trading in CSI300 stocks and thus the empirical results. An earlier version shows that results are robust with the CSI300 constituents included. We also exclude Chi-Next stocks due to their considerably lower IPO requirements.

portfolio details. Focusing on 2015:Q3, we find that the intervention is associated with a 5.65% decrease in volatility while its impact on informational efficiency is insignificant. The decrease in volatility is consistent with the market stabilization effect of intervention. Regarding informational efficiency, the intervention may improve efficiency by reducing fire sales, but it may also impair efficiency by introducing new noise. Consequently, the impact of intervention trading on informational efficiency is ambiguous. While it is challenging to provide clean estimates of the trading effect by focusing on 2015:Q3, we provide some additional tests to alleviate concerns about potential confounding factors.

As of Oct. 2015, there were periodic disclosures of the intervention portfolio of the previous quarter. We estimate the disclosure effect by focusing on the stocks disclosed to be intervened in the most recent statement but with no change in the NT holdings in the current period. This "no-trading" requirement excludes the trading effect. We find that the disclosure reduces volatility by 2.92% and price informativeness by 13.1%. The significant market responses to the intervention disclosure are more consistent with the "government-centric" scenario.

Furthermore, we conduct event-based tests on the disclosure effect using variations in disclosure announcement dates. Focusing on short windows around announcements, we find that the volatility of intervened stocks declines significantly after intervention disclosure. For informational efficiency, we examine how the disclosure of intervention information affects the incorporation of fundamental news into asset prices. As the intervention information is disclosed concurrently with the earnings, we expect a more prominent post-earnings-announcement drift (PEAD) in the "government-centric" case. Consistent with this, the intervened stocks have larger PEADs, indicating less efficient prices. The reduced efficiency is associated with considerable mis-pricing. A PEAD-based long–short strategy with a 20-day holding period generates an annualized return of 20.6%.

The findings on the disclosure effect are more consistent with the "government-centric" case, in which investors choose intervention information over fundamental news. We provide direct tests on information production and information asymmetry. Using analyst coverage and onsite visits as proxies, we find that information production declines substantially after intervention disclosure. Similarly, for information asymmetry, we observe lower probability of informed trading, lower analyst forecast dispersion, and smaller information asymmetry component in spreads after intervention disclosure.

We perform multiple robustness checks. First, for the selective intervention concern, our results remain robust after accounting for time-varying trends in control variables, excluding systemically important industries that could be the focus of intervention, controlling for past NT holdings, and so forth. Second, we discuss investor anticipation impacts by focusing on the groups of stocks that experienced less anticipation or excluding stocks with intervention information leakage. Third, we exclude the buy-and-hold impacts by focusing on the holdings of different NT institutions. Other robustness tests include using alternative measures, different fixed effect combinations, and different sample periods.

Our article makes a number of contributions to the literature. We provide novel insights on the impacts of large-scale government interventions in stock markets. To the best of our knowledge, this is one of the first articles to conduct a systematic empirical analysis of the NT and its implications for investor behavior and market efficiency. Using the NT intervention setting, Huang, Miao, and Wang (2019) analyze the associated monetary costs and benefits while Cheng, Jin, Li, and Lin (2022) focus on the price crash risk. In addition to having a different focus, we highlight the importance of the intervention disclosure channel by decomposing the impacts into a trading effect and a disclosure effect. We find that the intervention disclosure, by changing investors' information acquisition, is the main driver for the decrease in informational efficiency.

Barbon and Gianinazzi (2019) and Charoenwong et al. (2021) are also related. They focus on the asset pricing implications of the Bank of Japan's stock ETF purchases. The Bank of Japan intervened at the index level. The NT, however, engaged in stock-level intervention across a wide range of stocks. Furthermore, unlike the price-based analysis, we focus on the intervention impacts on information aggregation and market efficiency. While the intervention is intended to stabilize prices, there could be unintended efficiency costs.

There is also some research on how government policies work from the information channel. Boyarchenko, Kovner, and Shachar (2022) partly attribute the bond market impacts of the Federal Reserve corporate credit facilities to the policy announcement. According to Ehrmann, Gaballo, Hoffmann, and Strasser (2019), forward guidance dampened the response of government bond yields to macroeconomic news, indicating that more precise public information reduces the informativeness of market prices. We find that the provision of intervention portfolio information hinders private information production and reduces informational efficiency in stock markets. As stock prices often serve as signals in guiding real economic decisions, decreased informational efficiency in stock markets may result in unintended real economic costs (Bond, Edmans, and Goldstein (2012), Mace (2022)).

In a normative sense, our article sheds light on the rationales and trade-offs concerning direct government interventions in stock markets. On the one hand, the interventions appear to be justified by the reduced price volatility, to the extent that financial markets are overly speculative (Odean (1999), Deng, Liu, and Wei (2018)). On the other hand, the disclosure of detailed intervention portfolios may jeopardize private information production and price informativeness. The endogenous information choice of investors results in a trade-off: Government intervention can stabilize markets by reducing price volatility, but at the expense of decreased informational efficiency.

The rest of the article is structured as follows: The next section introduces the institutional and conceptual background before developing hypotheses. Section III describes the data and empirical design. Sections IV and V present the empirical findings on volatility and price informativeness, respectively. Section VI provides further evidence on information production and information asymmetry. Section VII concludes.

https://doi.org/10.1017/S0022109023000637

II. Institutional Background and Hypothesis Development

A. Institutional Background

1. The 2015 Chinese Stock Market Crash

The Chinese stock market experienced a rapid rise in the first half of 2015. The boom was fueled by margin financing with both regulated margin trading and unregulated leverage expanding dramatically (Bian, He, Shue, and Zhou (2018)). From Jan. to May 2015, the major Chinese stock indices, SSE50 and CSI300, increased by 17.4% and 32.9%, respectively.

Concerned about the potential risks in the rapid expansion of unregulated margin trading, the China Securities Regulatory Commission (CSRC) issued a strict order on June 12, 2015, prohibiting all securities firms from facilitating unregulated margin trading.[4] To the surprise of the government, the major stock market indices fell 13% in the following week (June 15–June 19, 2015), the largest weekly loss since the global financial crisis. Investors panicked, and the slide accelerated into a month-long stock market crash that erased roughly 40% of total market capitalization. The drastic price drop is widely believed to have been caused by excessive leverage and the subsequent fire sale induced by the deleveraging process. Highly leveraged investors went bust, resulting in massive forced liquidation and further price drops. This is consistent with the leverage spiral described in Brunnermeier and Pedersen (2009) and Geanakoplos (2010). Using account-level data from both regulated and unregulated margin sectors, Bian et al. (2018) provide a detailed description of how deleveraging contributed to the fire sale and price crash in 2015.

To stop the drop in stock prices, the Chinese government adopted various measures, including IPO suspension, trading restrictions on index futures, and banning net sale for securities firms' proprietary trading.[5] Huang et al. (2019) provide a chronology of government interventions during the 2015 Chinese stock market crash.

2. The National Team's Intervention

Among the interventions, the most unusual one is the large-scale direct stock purchase through accounts held by state-owned financial institutions. These

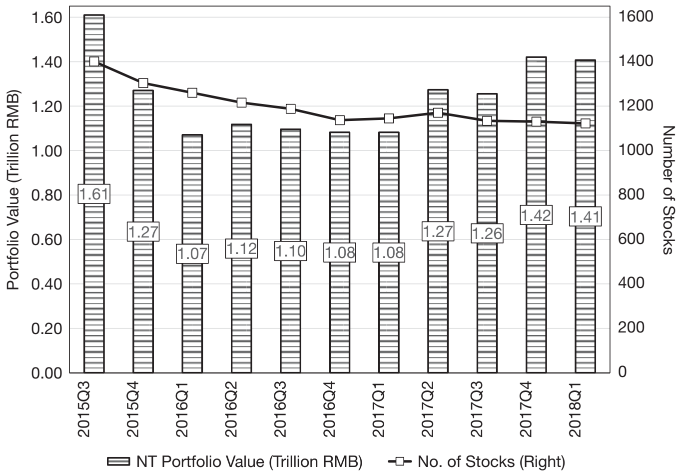
[4]According to a Huatai Securities research report, the total debt held by shadow-financed margin accounts was approximately 1.0–1.4 trillion RMB at its peak, with 600 billion coming from wealth management products (WMPs) offered by commercial banks. As a comparison, the total debt held by regulated margin accounts was about 2.2 trillion RMB. Thus, the total size of shadow-financed margin trading was substantial. The government was particularly concerned due to the large proportion of unregulated funding from the banking sector. The collapse of the stock market could harm banks' balance sheets, threatening the stability of the overall financial system. This also justifies the massive government intervention during the price crash.

[5]We control for the potential impacts of other contemporaneous polices. First, we exclude stocks listed after June 2015 to avoid capturing the impact of IPO suspension. Second, the CSI300 index constituents are also excluded. The CSI300 index future is the most important index future in China. The exclusion of CSI300 index stocks mitigates the impacts of the index future trading restriction. Our results remain robust with the CSI300 stocks included. Finally, the size of equity investment in securities firms' proprietary trading is small (approximately 160 billion), and our results remain robust when listed firms with positive security firm holdings are excluded (see Table B5 in Section B of the Supplementary Material).

FIGURE 1

National Team's Portfolio

Figure 1 presents the National Team's portfolio value (the shaded bars) and the number of stocks (connected dots, right vertical axis) within its portfolio over time. The time spans from 2015:Q3 to 2018:Q1.



accounts are known as the NT. The large-scale direct stock purchases began on July 6, 2015, and the NT was known to be intervening in the market, but the detailed intervention portfolio was not publicly available until Oct. 2015.[6]

The Chinese-listed firms report their top 10 shareholders in their quarterly reports. If an NT institution appeared in the report, we knew that the NT invested in this stock for sure.[7] The detailed NT portfolio became public for the first time in Oct. 2015, when listed firms released their third-quarter reports. According to 2015:Q3 firm statements, the NT was among the top 10 shareholders of 1,401 firms, covering more than half of all listed firms on Chinese stock exchanges (see Figure 1). Its portfolio value exceeded 1.6 trillion RMB (or 4.3% of the total market value in 2015:Q3).

The CSRC promised that the NT would remain in place in the coming years as a market stabilizer, intervening as needed. In the subsequent quarterly reports, the market obtained updated information on the NT portfolio details.[8] Changes in the NT portfolio may foreshadow future government interventions. This also prevented

[6]Prior to the public disclosure in Oct. 2015, there had been a leak of intervention information (see Section C of the Supplementary Material for a detailed description). We obtain a list of 92 stocks that were known to have been intervened during 2015:Q3, prior to the public disclosure. A less rigorous examination yields a list of 481 stocks that were likely intervened. Results are robust upon excluding related stocks.

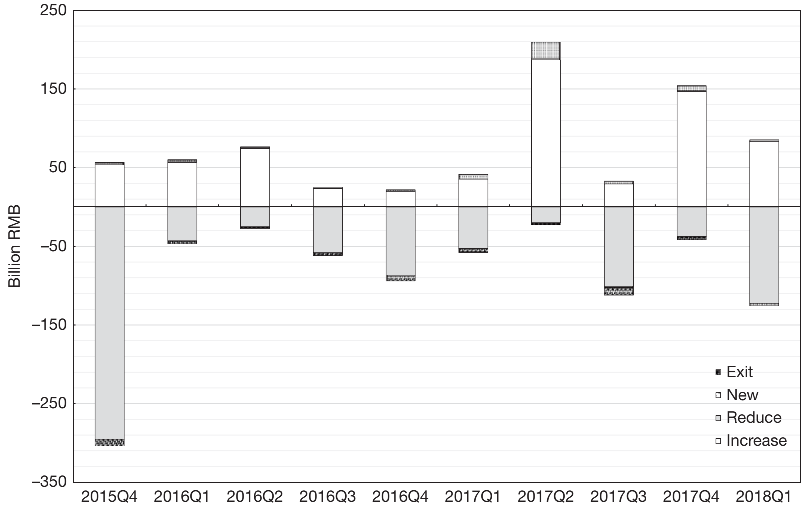
[7]The shareholder lists report the account names, which usually include the name of the holding institution. For instance, the China Securities Finance Corporation is one of the NT institutions, and its 11 accounts appear in the shareholder lists as "China Securities Finance Corporation" or as one of the 10 asset management plans with "China Securities Finance Corporation" in the name. Using the list of NT institutions, the market could track down NT accounts.

[8]In this article, the NT stock holdings are the reported amount in the quarterly reports. Relying on the quarterly reports would underestimate the NT's portfolio. Nonetheless, the quarterly statements are the

FIGURE 2

Inflows and Outflows of the National Team Portfolio

Figure 2 depicts the value of National Team (NT) portfolio inflows and outflows from 2015:Q4 to 2018:Q1. "Increase" aggregates the additional NT purchases on stocks already held in the last quarter. "Decrease" aggregates the partial sales on stocks within its portfolio. "New" sums up the position on all newly invested stocks compared to the previous quarter, and "Exit" aggregates the positions sold on stocks that the NT no longer holds in the current period. Positive numbers indicate inflows while negative numbers indicate outflows. 2015:Q3 data are not included here because all stocks belong to the "New" group in 2015:Q3.



the NT from exiting quickly, which could have triggered a market panic. Figure 1 shows the number of stocks in the NT portfolio over time, and the NT still held more than 1,100 stocks in 2018. Its portfolio value ranged from 1.1 to 1.4 trillion RMB. Further dissecting the NT portfolio inflows and outflows, Figure 2 shows that the NT mostly adjusted its holdings on stocks within its portfolio. New entries and complete exits were rare. Starting from 2017:Q2, large inflows and outflows appear to occur alternately. Rumors circulated that the NT had switched to other unidentified accounts as of 2017:Q2. For this reason, in the main analysis, we use data from 2013:Q3 to 2017:Q2.

A natural follow-up question would be, what kind of stocks did the NT buy? The goal of the NT, as stated openly by the government, was to stabilize the market. In the initial days, the NT invested heavily in blue-chip stocks, aiming to support the major stock market indexes. Yet this did not stop the free fall of the market and the NT expanded its intervention range further. Table A1 in Section A of the Supplementary Material sorts stocks into quintiles based on size and ROE and reports the number of intervened stocks in each quintile. Except for the quintile with the largest size, the NT portfolio is almost evenly distributed across different size or ROE groups. In contrast to government interventions in other economies that focus solely on major index stocks, the NT portfolio includes a diverse range of stocks.

most reliable data sources available. For our study, the underestimation would not be a serious concern, as we elaborate in footnote 16.

https://doi.org/10.1017/S0022109023000637

https://doi.org/10.1017/S0022109023000637

We investigate the stock characteristics related to the NT's initial intervention choice and subsequent portfolio rebalancings using regression analysis (see Table A2 in Section A of the Supplementary Material). The NT appears to adhere to the simple rule of "buy low, sell high."[9] In 2015:Q3, the intervened stocks underperformed the unintervened stocks. In the subsequent periods, within its portfolio, the NT was more inclined to sell (buy) stocks with higher (lower) returns. This is consistent with the NT's duty of market stabilization. Furthermore, Panel D of Table A1 in Section A of the Supplementary Material presents the subsequent monthly returns of the intervened and unintervened stocks. In line with intervention functioning as insurance ("the government put"), the intervened stocks frequently underperformed the unintervened stocks, particularly after the initial intervention.

As shown in Table 1, the NT consists of four groups of financial institutions: i) China Securities Finance Corporation (CSF): CSF undertook the major mission of market rescue and invested in 1,017 firms during 2015:Q3.[10] The value of its holdings accounted for about 70% of the NT's total portfolio. ii) CSF mutual funds: As the sole outside investor, CSF invested 200 billion RMB in five mutual funds in July 2015. These funds invested 94.1 billion RMB in 253 stocks in 2015:Q3. iii) Central Huijin Investment (HJ): HJ invested in 1,122 stocks in 2015:Q3, accounting for about 20% of total NT portfolio value.[11] iv) Investment platforms owned and funded by the State Administration of Foreign Exchange (SAFE): SAFE invested in about 20 stocks with a market value of 50 billion RMB.

The CSF portfolio frequently changes, whereas the HJ portfolio is relatively stable. In fact, most of the changes in the NT portfolio were caused by changes in the CSF portfolio. HJ, the NT's second largest member, traded infrequently.

B. Conceptual Framework and Hypothesis Development

To analyze the impacts of the NT intervention, we use the theoretical framework in Brunnermeier et al. (2022). They propose a model to investigate the impacts of government intervention on China's financial markets. Adding government as a large trader to the standard noisy rational expectations equilibrium model, their model includes three groups of agents: noise traders, investors, and the government. Noise traders create short-term price fluctuations. Strategic but myopic investors speculate on their private information and provide liquidity to noise traders. In addition, the government intends to stabilize the market by also trading against the noise traders. The key assumption is that such intervention generates unintended noise, the magnitude of which increases with the intervention intensity.

[9]The estimated coefficients of other variables indicate that firm size, state ownership, regulator connection, liquidity, volatility, institution, and top 10 shareholder ownership were not significantly related to intervention choice. Firms with a higher ROE, a lower revenue growth rate, and a more concentrated share structure were slightly more likely to be included in NT's portfolio in the initial intervention, but these factors did not matter for subsequent rebalancings.

[10]In addition to the 120 billion RMB raised from securities firms and other sources, the CSF was promised abundant funding from China's central bank, the People's Bank of China (PBC).

[11]We subtract the pre-intervention HJ holdings from the NT portfolio. HJ had invested in 8 stocks prior to the 2015:Q3 intervention: Sinopec, Petro China, New China Life Insurance, Industrial and Commercial Bank, Agricultural Bank, Everbright Bank, China Construction Bank, and Bank of China.

TABLE 1

The National Team Institutions

Table 1 presents detailed information for the four types of National Team institutions, including accounts, Chinese name, inception date, and portfolio value in 2015:Q3. SAFE investment platforms did not appear in the top 10 shareholder lists until 2015:Q4. Its total shareholdings stay around 50 billion RMB in the remaining sample period.

Institution	Accounts	Chinese Name	Inception Date	2015:Q3 Portfolio Value (RMB bn)
China Securities Finance (CSF)	CSF	中国证券金融股份有限公司	2011.10.28	1,137.29
	10 CSF Asset Management Plans	中证金资产管理计划	2015:Q3	
CSF Funds	Harvest New Opportunity Mixed Fund	嘉实新机遇混合	2015.7.13	94.1
	ChinaAMC New Economy Mixed Fund	华夏新经济混合	2015.7.13	
	E-fund Ruihui Mixed Fund	易方达瑞惠混合	2015.7.31	
	China Southern Consumption Vitality Mixed Fund	南方消费活力混合	2015.7.31	
	China Merchants Fengqing Mixed Fund	招商丰庆混合	2015.7.31	
Central Hujin (HJ)	HJ Investment	中央汇金投资有限责任公司	2003.12.16	378.73
	HJ Asset Management	中央汇金资产管理有限公司	2005.11.6	
SAFE Investment Platforms	Buttonwood Investment	梧桐树投资平台	2014.11.5	0
	Beijing Kunteng Investment	北京坤藤投资有限责任公司	2015.8.14	
	Beijing Fengshan Investment	北京凤山投资有限责任公司	2015.8.14	

Under this framework, the asset price is determined by the amount of noise trading, the asset fundamental, and the intervention noise. As government intervention becomes a new pricing factor, strategic investors with limited attention choose between acquiring a private signal about the asset fundamental or the intervention noise before trading. The information that facilitates a better forecast of future prices would be chosen.

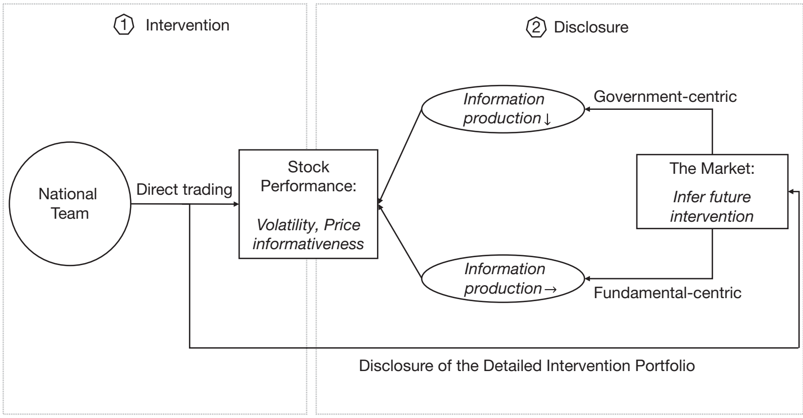
Depending on the parameters, there are two types of equilibria: fundamental-centric and government-centric, differentiated by investors' information choice. In the government-centric equilibrium, investors acquire information on government intervention noise instead of the asset fundamentals. They trade alongside the government against noise traders, reinforcing the government's efforts to reduce price volatility. The cost is a decrease in informational efficiency of asset prices due

https://doi.org/10.1017/S0022109023000637

FIGURE 3

How National Team Intervention Affects Stock Market Performance

Figure 3 depicts how the National Team intervention affects the stock market performance. Following the National Team setting, we divide the intervention into two stages. In the first stage, the National Team executes the actual trading which could affect market performance. In the second stage, the detailed National Team portfolio is publicly disclosed. The public portfolio disclosure provides more precise information and facilitates better inferences about future intervention, thus affecting the stock market performance. If the price forecast precision improvement driven by intervention disclosure is significant enough, investors would choose to learn intervention information over fundamental information, leading to the government-centric equilibrium. Otherwise, the intervention disclosure has no effect on investors' information choices, resulting in the fundamental-centric equilibrium.



to the lack of fundamental information acquisition. In the fundamental-centric equilibrium, investors acquire fundamental information and the efficiency cost of intervention could be saved. The likelihood of government-centric equilibrium increases with intervention intensity.

Brunnermeier et al. (2022) highlight the potential tension between financial stability and informational efficiency. The tension arises because government intervention adds intervention noise to asset prices, which, if sufficiently intensive, may distract investors from fundamental information. This in turn reinforces the impact of government noise on asset prices.

Based on the conceptual framework above, Figure 3 presents a flow chart illustrating how the NT intervention affects the markets. The intervention is decomposed into two stages. In the first stage, the NT executes the actual intervention trading, which may affect the performance of the intervened stocks. We call this the *trading effect.* Corresponding to the NT setting, the market acknowledges the existence of government intervention in the first stage, but there is no public detailed disclosure of the NT portfolio. The market may infer the intervention portfolio composition, and the anticipation may affect the market performance of related stocks. We will go into greater detail about anticipation in the empirical analysis.

In the second stage, the detailed intervention portfolio, which offers more precise information about the intervention, becomes publicly available. This enables more accurate predictions of future interventions and consequently stock prices. Investors choose whichever better forecasts the future price, the fundamental information or the intervention information. The disclosure of detailed intervention information may change investors' information choices, thus affecting stock market performance. We call this the *disclosure effect.*

The disclosure effect exists because investors believe intervention trading affects price; thus, intervention information helps them make better forecasts. In this sense, the disclosure effect depends on the impacts of intervention trading. But the disclosure effect is not a simple repercussion of the trading effect. The disclosure effect affects the market by changing investors' information choices and belief formation, resulting in potentially different impacts. A novel feature of our analysis is the distinction between the trading effect and the disclosure effect, and we quantify their relative importance.

First, the trading effect lowers price volatility by directly offsetting noises through intervention trading. Furthermore, the disclosure of a detailed intervention portfolio allows for better forecasts of future interventions. Thus, it increases the potential benefit of acquiring intervention information, which has two potential outcomes: i) Government-centric equilibrium: The forecast precision improvement is significant enough to induce investors to acquire intervention information instead of the fundamental information. Investors would be more likely to trade alongside the government, strengthening the volatility reduction. ii) Fundamental-centric equilibrium: The benefit induced by detailed intervention disclosure is negligible. Investors would ignore the disclosure and instead learn the fundamental information. The disclosure would have no impact on volatility. We formulate the first hypothesis as follows:

Hypothesis 1. The trading effect reduces price volatility. The disclosure effect reduces price volatility in the government-centric case, but it has no impact on volatility in the fundamental-centric case.

Second, government intervention may have unintended consequences for market efficiency, as measured by price informativeness. The trading effect on price informativeness is ambiguous. Government intervention may improve market efficiency by partially offsetting noise trading, but it also introduces a new noise that impairs efficiency. For the disclosure effect, its impact on price informativeness depends on whether the forecast precision improvement driven by disclosure is significant enough. In the government-centric case, investors acquire intervention information instead of asset fundamental information, and prices are less informative due to lower information production. In the fundamental-centric case, investors acquire fundamental information and price informativeness is unaffected by intervention portfolio disclosure. Our second hypothesis is as follows:

Hypothesis 2. The trading effect on price informativeness is ambiguous. The disclosure effect reduces price informativeness in the government-centric case, but it has no impact on price informativeness in the fundamental-centric case.

The information choices of investors differ in the government-centric and fundamental-centric cases. In the government-centric case, the intervention portfolio disclosure induces investors to learn intervention information, resulting in less private information production. In the fundamental-centric case, however, information production is unaffected. More private information production is associated

https://doi.org/10.1017/S0022109023000637

with greater information asymmetry among investors. We thus formulate the third hypothesis as follows:

Hypothesis 3. The disclosure effect reduces information production (information asymmetry) in the government-centric case, but it has no impact on information production (information asymmetry) in the fundamental-centric case.

III. Data and Empirical Design

A. Data and Summary Statistics

1. Data Sources and Variable Construction

Data for this research come from two main sources. We obtain quarterly reported NT holding records from Wind, a leading Chinese financial data vendor. China Stock Market and Accounting Research (CSMAR) provides the remaining data.

As the NT intervention began in July 2015, our sample period runs from July 2013 to June 2017, spanning approximately 2 years before and after the market crash. This sample period also takes into account the NT's possible stock account shifting as of 2017:Q2, as discussed in Section II.A.2.[12]

We exclude the CSI300 index constituents from our sample due to concerns about the comparability of intervened and unintervened stocks. Given previous interventions in other economies, investors might reasonably expect the CSI300 constituents to be intervened (which turned out to be true). Excluding the CSI300 index constituents also alleviates the anticipation concern. Our results remain robust with the CSI300 index constituents included. Also excluded are ChiNext stocks, stocks listed after June 2015, and observations with important variables missing or with less than 15 trading days in a month.

The main dependent variables are volatility and price informativeness. For volatility, we use intraday volatility (the difference between the highest and lowest price scaled by the average of the two) and interday volatility (the log standard deviation of daily return) (Deng et al. (2018), Cai, He, Jiang, and Xiong (2021)). In the main analysis, we present the results for intraday volatility, and use interday volatility as a robustness check.

For price informativeness, we use the price nonsynchronicity measure (also known as the R^2 measure) (Roll (1988), Bai, Philippon, and Savov (2016)). We estimate the goodness of fit (R^2) based on the market model with a rolling window of a quarter (current month included). The price nonsynchronicity measure is then calculated as $\log\left(\frac{1-R^2}{R^2}\right)$ (Morck, Yeung, and Yu (2000), Chen, Goldstein, and Jiang (2007), Ferreira and Laux (2007), and Fernandes and Ferreira (2008)). For robustness, we try other pricing models for the estimation of R^2. Lower price nonsynchronicity indicates lower price informativeness.

[12]Results are robust for longer (2012.7–2018.6) or shorter sample periods (2014.7–2016.6). Please refer to Table B8 in Section B of the Supplementary Material for more details.

To further investigate the impact of intervention disclosure on price informativeness, we also estimate the return responses to standardized unexpected earnings (SUE), that is, the PEAD (Kacperczyk, Sundaresan, and Wang (2021), Coles, Heath, and Ringgenberg (2022)). With a seasonal random walk model (Chan, Jegadeesh, and Lakonishok (1996), Chordia and Shivakumar (2002)), the SUE is calculated as the difference in current period earnings and the earnings four quarters before, scaled by the standard deviation of the earnings differences over the preceding eight quarters.[13] The Appendix provides a detailed list of variable definitions.

2. Descriptive Statistics

Table 2 presents the summary statistics. We provide summary statistics for each group of stocks to facilitate comparisons between the intervened and the

TABLE 2

Summary Statistics

Table 2 presents the summary statistics. We report the sample means of each variable for the whole sample (also for the propensity score matched sample), the intervened group, and the unintervened group, separately. ln(FREE-FLOAT_MARKET_VALUE) measures the average free-float market value during the current month in log. AVERAGE_DAILY_TURNOVER and ln(AVERAGE_DAILY_TRADING_ VOLUME) are the average daily turnover and log trading volume within the current the month, respectively. INTERDAY_VOLATILITY is the log standard deviation of daily return within the current month. INTRADAY_VOLATILITY is the average difference between daily highest price and lowest price scaled by the average of the two during the current month. AMIHUD is the Amihud ratio measured by $1/D_{i,t}\sum_{d=1}^{D_{i,t}}|R_{i,d,t}|/\mathrm{VOL}_{i,d,t}$, where $D_{i,t}$, $R_{i,d,t}$, and $\mathrm{VOL}_{i,d,t}$ is the number of trading days, stock return, and RMB trading volume for stock i on day d in month t following Amihud (2002). PRICE_NONSYNCHRONICITY equals $\log\left(\frac{1-R_{i,t}{}^2}{R_{i,t}{}^2}\right)$, in which $R_{i,t}{}^2$ is the goodness of fit from the following regression using daily return data from month $t-2$ to month t: $R_{i,d,t} = \alpha_0 + \beta_i \mathrm{RM}_{i,d,t} + \beta_{i,IND}\mathrm{R\_IND}_{i,d,t} + \varepsilon_{i,d,t}$, $R_{i,d,t}$, $\mathrm{RM}_{i,d,t}$, and $\mathrm{R\_IND}_{i,d,t}$ is the return of stock i, market return excluding stock i, and industry return excluding stock i, on day d in month t, respectively. MRET measures the cumulative return within the month and EXMLOSS is the absolute value of the sharpest cumulative loss in 5 consecutive trading days within the quarter. If the lowest cumulative return of a stock in 5 consecutive trading days within the quarter is positive (i.e., no loss), then EXMLOSS = 0. PLIMIT gives the percentage of trading days that close price hits price fluctuation limits within a month. TOP10 and INSTHOLD are the ratios of shares held by top 10 shareholders and institutional investors, respectively. ln(SHNO) is the number of shareholders in log and SHARECONCEN is the sum of squares of shares larger than 5%. REVGROWTH, ROE, and BMR are revenue growth rate, return on equity, and book-to-market ratio, respectively. ln(ASSET) is the log asset value. ANALYST_ COVERAGE is the number of analysts that have issued research reports on the firm in the recent 6 months (current month included). PRICE_FORECAST_DISPERSION is the standard deviation of the analyst forecasts on 1-year ahead stock price with a reference period of 6 months scaled by average price forecast.

	Whole Sample				PSM Sample		
Variables	All	Unintervened (1,076 Stocks)	Intervened (1,031 Stocks)	Diff.	Unintervened (436 Stocks)	Intervened (867 Stocks)	Diff.
ln(FREE-FLOAT_MARKET_VALUE)	22.270	22.110	22.420	−0.316	22.240	22.420	−0.182
AVERAGE_DAILY_TURNOVER (%)	2.828	2.928	2.730	0.198	2.712	2.605	0.107
ln(AVERAGE_DAILY_TRADING_ VOLUME)	18.250	18.150	18.350	−0.203	18.210	18.340	−0.130
INTERDAY_VOLATILITY (%)	0.935	0.948	0.923	0.026	0.924	0.924	0.000
INTRADAY_VOLATILITY (%)	4.196	4.253	4.141	0.113	4.132	4.143	−0.011
AMIHUD	3.079	3.379	2.786	0.592	3.140	2.836	0.304
PRICE_NONSYNCHRONICITY	0.405	0.539	0.274	0.265	0.460	0.282	0.178
MRET	0.016	0.020	0.013	0.006	0.019	0.014	0.005
EXMLOSS	0.099	0.100	0.099	0.001	0.097	0.098	−0.001
PLIMIT	0.020	0.021	0.020	0.001	0.019	0.020	−0.001
TOP10	0.563	0.546	0.579	−0.032	0.554	0.570	−0.016
INSTHOLD	0.399	0.371	0.427	−0.056	0.405	0.430	−0.025
ln(SHNO)	10.430	10.290	10.560	−0.267	10.420	10.570	−0.151
SHARECONCEN	0.160	0.142	0.178	−0.036	0.158	0.173	−0.015
REVGROWTH (%)	10.660	11.210	10.120	1.092	11.420	10.240	1.182
ROE (%)	1.018	0.810	1.222	−0.412	1.041	1.159	−0.118
BMR	0.516	0.478	0.553	−0.075	0.530	0.563	−0.033
ln(ASSET)	22.050	21.770	22.330	−0.567	22.050	22.320	−0.272
ANALYST_COVERAGE	3.126	2.759	3.486	−0.727	3.130	3.424	−0.294
PRICE_FORECAST_DISPERSION	0.051	0.049	0.053	−0.004	0.051	0.052	−0.001

[13]We also try a seasonal random walk model with a drift. The drift term is estimated by averaging the earnings differences over the preceding eight quarters (Sadka (2006)).

https://doi.org/10.1017/S0022109023000637

https://doi.org/10.1017/S0022109023000637

unintervened stocks. Stocks ever held by the NT are classified as the intervened group, whereas the remaining stocks are unintervened. The number of stocks in the two groups are comparable. Of the 2,107 firms, 1,031 firms are in the intervened group and the remaining 1,076 firms are unintervened.

The regression analysis shows that aside from stock return, only ROE, revenue growth rate, and share concentration are significantly related to intervention status (see Table A2 in Section A of the Supplementary Material). On average, compared to the unintervened stocks, the intervened stocks have: i) higher ROE and slower revenue growth rate; ii) more concentrated shareholdings and higher institutional ownership; and iii) broader analyst coverage.

Though these factors may not be directly related to the intervention choice, the between-group differences may contaminate the interpretation of our findings. We thus perform a single nearest-neighbor propensity score matching.[14] There are 867 intervened and 436 unintervened stocks post-matching. As shown in the last three columns of Table 2, the differences between intervened and unintervened stocks shrink substantially post-matching. We perform the empirical analysis based on both the whole sample and the PSM sample.

B. Empirical Design

1. Baseline DID Analysis

We begin with the baseline DID-like analysis to examine the overall impacts of NT holdings. We use the dynamic DID methodology as in Hansman, Jiang, Liu, and Meng (2021). The intervened group consists of stocks in the NT's portfolio and the remaining stocks are the unintervened. We use monthly data, and model 1 presents the baseline DID setup:[15]

$$\text{DEP}_{i,t} = \alpha_1 \text{NT}_{i,t} + \text{CONTROLS} + v_i + \tau_t + \varepsilon_{i,t}, \tag{1}$$

where i stands for stock and t stands for month. v_i and τ_t are stock and year–month fixed effects, respectively. $\text{DEP}_{i,t}$ is the dependent variable for stock i in month t. $\text{NT}_{i,t}$ is the key independent variable in the dynamic DID setting. It is a dummy variable that equals 1 if the NT's position in stock i is positive in month t, and 0 otherwise.[16]

[14] We require common support among matched intervened and unintervened stocks with a caliper that constrains the maximum distance to be 0.01. To avoid capturing the impacts of intervention, we use the data prior to 2015 for matching. Matching variables include INTRADAY_VOLATILITY, INTERDAY_ VOLATILITY, TURNOVER, TRADING_VOLUME, PRICE_NONSYNCHRONICITY, AMIHUD, INSTHOLD, TOP10, SHARECONCEN, ln(SHNO), ln(ASSET), L.BMR, ROE, REVGROWTH, EXMLOSS, PLIMIT, ANALYST_COVERAGE, and ANALYST_FORECAST_DISPERSION.

[15] Though the statements are updated quarterly, the time between the statement disclosures is not of full quarters. For example, the interim reports are mostly disclosed in August, while the third quarter reports are available in October. There are only 2 months (less than a quarter) in between. Estimating the disclosure effect requires using the actual time of disclosure, and monthly data is more appropriate. Section A.1.1 of the Supplementary Material presents the timing of statement disclosures.

[16] Using quarterly report data results in an underestimation of the NT holdings, which has two potential outcomes: i) An underestimation of the NT's percentage holdings. ii) Incorrectly classifying intervened stocks as unintervened. In the former case, the valuation of the NT is unaffected. In the latter, the underestimation reduces the estimated differences in stock performance between these two groups of

The parameter of interest is α_1, which captures both the trading and the disclosure effects. The disclosure effect starts kicking in after the release of 2015:Q3 reports in Oct. 2015. In all subsequent quarterly reports, investors learn about the NT stock holdings in the previous quarter. α_1 captures the NT trading effect in month t. Also, due to the lag in quarterly report releases, investors may receive the information of $\text{NT}_{i,t-1}=1$ in month t and this affects $\text{DEP}_{i,t}$. This disclosure effect will be reflected in α_1 as well.

Model 1 provides an overall estimate of the NT intervention impacts. The following sections introduce empirical specifications for estimating the trading and disclosure effects.

2. The Trading Effect

The trading effect captures how the intervention affects the market through trading. To isolate the trading effect, we focus on the NT's initial trading in 2015:Q3 before the disclosure of its detailed portfolio in Oct. 2015. The post-intervention period is divided into two parts: the initial intervention (July–Sept. 2015) and the subsequent months (Oct. 2015–June 2017). Two dummies INIT_t and REMAIN_t are defined accordingly. Then we interact these two dummies with $\text{NT}_{i,t}$ in model 1 and the new model is as follows:

$$\text{(2)} \quad \text{DEP}_{i,t}=\beta_1\text{INIT}_t\times\text{NT}_{i,t}+\beta_2\text{REMAIN}_t\times\text{NT}_{i,t}+\text{CONTROLS}+\nu_i+\tau_t+\varepsilon_{i,t}.$$

β_1 measures the impact of NT intervention in 2015:Q3. With no previous public disclosure of the detailed NT portfolio, β_1 excludes the disclosure effect. Nevertheless, we cannot rule out the possibility that some sophisticated investors might correctly infer NT's trading plans during 2015:Q3 and the anticipation effect is also captured by β_1. This is one of the reasons for the exclusion of CSI300 index constituents. We acknowledge the challenges in obtaining a clean estimate of the trading effect and try various measures to control for potential anticipation, as elaborated in Section IV.B.

3. The Disclosure Effect

What matters for the disclosure effect is *what investors know when making trading decisions*. Instead of the current period NT portfolio, investors know the (lagged) NT holding data disclosed in the latest quarterly reports. We define $\text{DNT}_{i,t}$ equals 1 if in month t the latest quarterly report disclosed positive NT holdings in stock i, and 0 otherwise. $\text{DNT}_{i,t}$ is updated following quarterly report releases.[17] For example, the third quarter report disclosed in Oct. 2015 that stock i was intervened. In the following months, $\text{DNT}_{i,t}$ would be 1. It was then updated upon the release of the 2015 annual report, possibly in Apr. 2016.

---

stocks, making our results a lower bound of the actual intervention impacts. Thus, underestimating NT holdings would not be a major issue in our study.

[17]For months with statement disclosures, $\text{DNT}_{i,t}$ is updated to the new report if it is released in the first half-month. Otherwise, $\text{DNT}_{i,t}$ is defined according to the previous report for the current month and updated to the new report starting from the next month. If two statements are disclosed in the same half-month (e.g., the previous annual report and the new Q1 report), $\text{DNT}_{i,t}$ is defined according to the statement with more recent accounting period. If two statements are disclosed in the same month but at different halves, we compute the average reported NT holdings and define $\text{DNT}_{i,t}$ accordingly.

https://doi.org/10.1017/S0022109023000637

Including $DNT_{i,t}$ alone does not provide a clear estimate of the disclosure effect. A stock disclosed to be intervened in month t might be concurrently traded by the NT, thus incorporating the trading effect into the $DNT_{i,t}$ coefficient. We further interact $DNT_{i,t}$ with a dummy $UN_{i,t}$. Suppose month t belongs to quarter q, $UN_{i,t}$ equals 1 if $NT\_SP_{i,q} = NT\_SP_{i,q-1}$, that is, if the NT's percentage holding is unchanged. $UN_{i,t} = 1$ suggests that the NT did not trade stock i in month t.[18] The interaction of the no-trade dummy $UN_{i,t}$ and the disclosure dummy $DNT_{i,t}$ controls for the possible influence of NT trading and leaves us with the (pure) disclosure effect. The empirical model is as follows:

$$\text{(3)} \qquad DEP_{i,t} = \gamma_1 DNT_{i,t} + \gamma_2 DNT_{i,t} \times UN_{i,t} + CONTROLS + \nu_i + \tau_t + \varepsilon_{i,t}.$$

γ_2 shows the extent to which the disclosure effect outweighs γ_1, the mixture of the trading and disclosure effects. The disclosure effect is given by $\gamma_1 + \gamma_2$. The possibility of sophisticated investors trying to infer NT trading plans weakens the impact of public disclosure. $\gamma_1 + \gamma_2$ then constitutes a lower bound of the disclosure effect.

Would the disclosure effects differ depending on the content disclosed? We further split the dummy $DNT_{i,t}$ into three dummies: $DINC_{i,t}$, $DUN_{i,t}$, and $DDEC_{i,t}$, with value equals 1 if the latest quarterly statement shows increased, unchanged, and decreased NT holdings in stock i, respectively, and 0 otherwise. Interacting the three disclosure dummies with the UN dummy helps differentiate between the disclosure effects on increased ($\delta_1 + \delta_2$), unchanged ($\delta_1 + \delta_3$), and decreased ($\delta_1 + \delta_4$) NT holdings:

$$\text{(4)} \qquad \begin{aligned} DEP_{i,t} = {} & \delta_1 DNT_{i,t} + \delta_2 DINC_{i,t} \times UN_{i,t} + \delta_3 DUN_{i,t} \times UN_{i,t} \\ & + \delta_4 DDEC_{i,t} \times UN_{i,t} + CONTROLS + \nu_i + \tau_t + \varepsilon_{i,t}. \end{aligned}$$

IV. Intervention and Volatility

A. Intervention and Volatility: Main Results

This section analyzes the impact of NT intervention on volatility, and Table 3 presents the regression results. The dependent variable is intraday volatility measured by the monthly average daily price change (VOLA). We include firm fixed effects and year–month fixed effects in all models and double cluster the standard errors by firm and year–month. For robustness, we also use interday volatility (the log standard deviations of daily returns) and include firm × year fixed effects (see Tables B6 and B7 in Section B of the Supplementary Material).

The control variables include balance-sheet-related variables such as return on equity (ROE), revenue growth rate (REVGROWTH), firm size (ln(ASSET)), and book-to-market ratio (BMR). Share concentration (SHARECONCEN), and ratios of shares held by top 10 shareholders (TOP10) and by institutional investors (INSTHOLD), are also included. Furthermore, we control for the monthly returns (MRET), lagged monthly returns (L.MRET), and Amihud ratio (AMIHUD). These variables potentially facilitate investors' prediction of intervention and may help

[18]Although unlikely, we cannot rule out the possibility that the NT purchased and sold the same amount multiple times, leaving the total holdings unchanged.

TABLE 3
National Team Intervention and Volatility

Table 3 examines the impact of the National Team intervention on volatility. The dependent variable is the monthly average intraday volatility, measured as the difference between the highest and lowest prices scaled by the average of the two. Columns 1–5 use the whole sample while columns 6–8 use the PSM matched sample instead. Column 1 explores the relationship between NT percentage holding (NT_SP) and volatility. Column 2 presents the results for model 1 with the NT status dummy as the main independent variable. Columns 3 and 6 correspond to model 2. In columns 4 and 7, and 5 and 8, we present the results for models 3 and 4, respectively. These columns provide estimates for the disclosure effect. Control variables are the monthly return (MRET), lagged monthly return (L.MRET), share concentration (SHARECONCEN), number of shareholders in log (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10) and institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). The estimated results for control variables are unreported for ease of presentation. Firm and year–month fixed effects are included in all models. At the bottom of the table, we present the *p*-values of the Wald tests on whether the sum of the coefficients equals 0. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and year–month.

	Intraday Volatility (%)							
	Whole Sample				PSM Sample			
	1	2	3	4	5	6	7	8
NT_SP	−0.025***							
	(−3.50)							
NT		−0.143***						
		(−4.27)						
INIT × NT			−0.234***			−0.163***		
			(−3.89)			(−2.70)		
REMAIN × NT			−0.129***			−0.121***		
			(−3.63)			(−3.51)		
DNT				0.004	−0.002		0.029	0.024
				(0.08)	(−0.04)		(0.72)	(0.58)
DNT × UN				−0.125***			−0.155***	
				(−3.70)			(−4.31)	
DINC × UN					−0.199***			−0.206***
					(−5.32)			(−5.12)
DUN × UN					−0.086*			−0.131***
					(−1.78)			(−2.81)
DDEC × UN					−0.087			−0.106**
					(−1.61)			(−2.07)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	2,107	2,107	2,107	2,107	2,107	1,303	1,303	1,303
No. of obs.	76,875	76,875	76,875	76,875	76,875	52,041	52,041	52,041
Adjusted R^2	0.833	0.833	0.833	0.832	0.832	0.832	0.832	0.832
Pr[Coef(DNT) + Coef (DNT × UN) = 0]				0.001			0.001	
Pr[Coef(DINC × UN) + Coef(DNT × UN) = 0]					0.000			0.000
Pr[Coef(DINC × UN) + Coef(DNT × UN) = 0]					0.022			0.007
Pr[Coef(DUN × UN) + Coef(DNT × UN) = 0]					0.108			0.135

capture the anticipation effect. For ease of presentation, the estimated coefficients for control variables are unreported.

We first examine the relationship between NT percentage holding and volatility in column 1 of Table 3. A larger percentage of NT holding is associated with lower price volatility.[19] This demonstrates the volatility reduction effect of the intervention.

[19]We also examine the impact of the NT trading ratio instead of the holding percentage. We compute the ratio of shares purchased by NT over the total shares traded, and use the trading ratio as the independent variable. The findings are similar. The related results are in Table A4 in Section A of the Supplementary Material.

Columns 2–5 present the results for models 1–4, respectively. The coefficient for NT in column 2 shows the overall impact of NT holding on the volatility of intervened stocks.[20] Consistent with Hypothesis 1, being intervened by the NT is associated with a 3.45% decrease in volatility, compared to the intervened group sample mean.

The trading and disclosure effects are estimated in columns 3–5 of Table 3, which correspond to models 2–4. The estimated coefficient for INIT × NT in column 3 is negative and significant at 1% level. The NT intervention in 2015: Q3 is associated with a 5.65% drop in volatility. This is consistent with the NT intervention trading reducing volatility by trading against the noise traders.

Column 4 of Table 3 estimates the impact of intervention portfolio disclosure on volatility, given by the sum of the coefficients of DNT and DNT × UN. The coefficient of DNT × UN is negative and significant at 1% level, suggesting that the intervention portfolio disclosure results in additional volatility decline. The Wald test on whether the sum of the coefficients of DNT and DNT × UN equals 0 gives a p-value of 0.001. The disclosure of intervention information leads to a further 2.92% decline in volatility, as compared to the sample mean. The negative disclosure effect on volatility is more consistent with the government-centric case, where investors acquire intervention information and trade along with the government to strengthen the volatility reduction.

In column 5 of Table 3, we further differentiate between the contents disclosed. Whether the intervention disclosure entails increased, unchanged, or decreased NT holdings, the disclosure is always associated with lower volatility. Investors respond more strongly to increased NT holdings than to unchanged or decreased NT holdings.

To ensure that the results are not driven by differences between intervened and unintervened stocks, we re-run the regression analyses using the PSM-matched sample (see columns 6–8 of Table 3). Despite reducing the number of firms by nearly half, the regression results in columns 6–8 are similar in magnitude and significance to the whole-sample results.

Aside from using alternative measures, other combinations of fixed effects, and other sample periods, we conduct multiple additional tests for robustness. To address concerns about selective intervention, we allow for controls with time-varying trends, exclude systematically important industries, and include the past NT percentage holding as a control. We also exclude the mechanic buy-and-hold effect by focusing on the holdings of different NT institutions. Section B of the Supplementary Material provides a comprehensive introduction to the robustness checks and alternative hypotheses.

Overall, the empirical results support the government-centric scenario described in Hypothesis 1. Government intervention reduces price volatility and both trading and disclosure effects contribute to the volatility drop.

B. Volatility: On the Impact of Anticipation

Some sophisticated investors may correctly anticipate the intervention portfolio, and this affects the estimated trading and disclosure effects. The anticipation

[20]Figure A2 in Section A of the Supplementary Material presents the parallel trend test for model 1 using the whole sample and the PSM sample. The patterns roughly fit the parallel trend assumption.

https://doi.org/10.1017/S0022109023000637

effect exists because investors perceive different intervention probabilities for different stocks. In this subsection, we conduct three additional tests to address this issue.

First, we focus on the stocks whose intervention status is hard to predict. We run a logit regression to predict whether a stock is intervened and use the absolute value of residual as the prediction error.[21] The anticipation impact should be milder for stocks with large prediction errors and we restrict the sample to stocks with prediction errors above the median in columns 1 and 2 of Table 4. The coefficients of INIT × NT remain significantly negative. Furthermore, we observe a stronger disclosure effect among these stocks, which is consistent with the noisy anticipation.

Second, institutional investors are better informed and may possess more intervention-related private information (Nofsinger and Sias (1999), Parrino, Sias, and Starks (2003), and Anderson, Reeb, and Zhao (2012)). In columns 3 and 4 of Table 4, we restrict our sample to firms with institutional ownership below median. The coefficients of INIT × NT and DNT × UN remain negative and significant at 1% level.

Finally, we exclude stocks with intervention information leakage (see Section C of the Supplementary Material for more details) and present the results in columns 5 and 6 of Table 4. The results remain robust.

C. Volatility: A Further Test on the Disclosure Effect

In the previous analysis, we coarsen the disclosure information to monthly data. We further adopt an event-study approach, using the quarterly report announcement date information to capture the disclosure effect. Each quarterly report announcement is treated as an event, and we compare the differences in volatility changes during a short window around the announcement. The empirical model is as follows:

$$\begin{aligned}(5)\qquad \mathrm{VOLA}_{i,t,p} = {} & \alpha_1 \mathrm{DNT}_{i,p} \times \mathrm{POSTAN}_t + \alpha_2 \mathrm{DNT}_{i,p} + \alpha_3 \mathrm{POSTAN}_t \\ & + \alpha_4 \mathrm{INTV}_i \times \mathrm{POSTAN}_t + \alpha_5 \mathrm{AFTER}_p \times \mathrm{POSTAN}_t \\ & + \mathrm{CONTROLS} + \mathrm{FEs} + \varepsilon_{i,t,p}.\end{aligned}$$

$\mathrm{VOLA}_{i,t,p}$ is the intraday volatility (average daily price change, %) of stock i during the announcement of quarterly report p. We compute volatility for a short window before and after the announcement, with $t = 0$ indicating pre-announcement observations and $t = 1$ for post-announcement observations. Again $\mathrm{DNT}_{i,p}$ equals 1 for stock i disclosed to be NT intervened in report p, and 0 otherwise. POSTAN_t is a post-announcement dummy that equals 1 for post-announcement observations,

[21] We run a logit regression using quarterly data from 2015:Q3 to 2017:Q2. The dependent variable is a dummy that equals 1 for intervened stocks, and 0 otherwise. The independent variables include QRET, L.QRET, EXMLOSS, DLIMIT, QUARTERLY_INTRADAY_VOLATILITY, TURNOVER, AMIHUD, 1{STATE_CONTROLLED}, L.INSTHOLD, L. TOP10, L.SHARECONCEN, L.ln(SHNO), 1{REG_CONNECTED}, L.ln(ASSET), L.BMR, L.ROE, L.REVGROWTH. Upon obtaining the absolute value of residual, we compute the average residual for each stock and stocks with an average residual above the median are considered hard to predict.

TABLE 4

Volatility: Subsample Analysis

Table 4 further explores the National Team intervention impacts on volatility by focusing on subsamples. The dependent variable is the monthly average intraday volatility, measured as the difference between the daily highest and lowest prices scaled by the average of the two. In columns 1 and 2, we focus on stocks with prediction errors above the median. The prediction error is the absolute value of the residual, obtained from a logit regression using the status of being intervened as the dependent variable, and observed characteristics such as return, ROE, volatility, turnover as predictors (see footnote 21 for a complete list of predicting variables). In columns 3 and 4, we focus on the subsample with institutional ownership below the median. Columns 5 and 6 exclude the stocks with potential intervention information leakage in 2015:Q3 (see Section C of the Supplementary Material for a detailed discussion on the intervention information leakage). We present the results on model 2 in odd columns and those for model 3 in even columns. Control variables are the monthly return (MRET), lagged monthly return (L.MRET), share concentration (SHARECONCEN), number of shareholders in log (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10) and institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). The estimated results for control variables are unreported for ease of presentation. Firm and year–month fixed effects are included in all models. At the bottom of the tables, we present the *p*-values of the Wald tests on whether the coefficients are equal to the same coefficients estimated in Table 3. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and year–month. Coef(*) – Table 3 refers to the coefficient of variable ∗ estimated based on the whole sample, as shown in Table 3.

	Monthly Average Daily Price Change (%)					
	Prediction Error > Median		INSTHOLD < Median		Excluding Information Leakage	
	1	2	3	4	5	6
INIT × NT	−0.246***		−0.235***		−0.265***	
	(−2.81)		(−4.51)		(−5.91)	
REMAIN × NT	−0.213***		−0.115***		−0.118***	
	(−4.20)		(−2.78)		(−2.81)	
DNT		−0.039		0.042		0.024
		(−0.75)		(0.83)		(0.50)
DNT × UN		−0.168***		−0.130**		−0.123***
		(−4.84)		(−2.43)		(−3.43)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year–month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	1,051	1,051	1,472	1,472	1,759	1,759
No. of obs.	38,341	38,341	38,435	38,435	64,005	64,005
Adjusted R^2	0.837	0.837	0.836	0.836	0.831	0.831
Pr[Coef(INIT × NT) = Coef(INIT × NT) – Table 3]	0.910		0.993		0.348	
Pr[Coef(DNT) = Coef(DNT) – Table 3]		0.281		0.289		0.154
Pr[Coef(DNT × UN) = Coef(DNT × UN) – Table 3]		0.000		0.900		0.903

and 0 otherwise. As $\mathrm{DNT}_{i,p}$ resembles the interaction term in traditional DID, equation (5) is essentially a triple DID setting with the three differences as follows: before and after announcement, intervened and unintervened stocks, and before and after intervention. The coefficient α_1 measures the difference in volatility changes post-announcements for intervened stocks, compared to the unintervened stocks. A negative α_1 suggests a negative disclosure effect of intervention information on volatility.

For control variables, we define an intervention indicator INTV_i for stocks ever intervened by the NT and interact it with POSTAN_t. If the NT-intervened stocks always have lower volatility after announcements, this would be captured by the interaction term. Similarly, we define a dummy AFTER_p that equals 1 for 2015:Q3 and all periods thereafter, and 0 otherwise. If stocks tend to have lower volatility changes after 2015:Q3, this would be captured by the coefficient of $\mathrm{AFTER}_p \times \mathrm{POSTAN}_t$. In addition to the control variables used in the previous

TABLE 5

Volatility: More on Disclosure Effect

Table 5 presents the event-study outcomes on the disclosure effect of NT intervention. For each quarterly report announcement, we compute the average intraday volatility (daily price change, %) for a short window both before and after the announcement date as the dependent variable. [−*j*,*j*] indicates a window covering *j* trading days around the announcement date. DNT equals 1 for stocks disclosed to be included in the NT portfolio, and 0 otherwise. POSTAN equals 1 for post-announcement observations, and 0 otherwise. We also define a dummy INTV for stocks intervened by the NT and it equals 0 for stocks never intervened by the NT, and 1 otherwise. The AFTER dummy equals 1 for 2015:Q3 and all periods thereafter, and 0 otherwise. Control variables include SUE, share concentration (SHARECONCEN), log number of shareholders (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10) and institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). The estimated results for control variables are unreported for ease of presentation. Firm, announcement date, and accounting period fixed effects are included in all models. We use the whole sample in columns 1–4 and the PSM sample in columns 5–8. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and announcement date.

	Average Daily Price Change (%)							
	Whole Sample				PSM Sample			
	[−3,3]	[−5,5]	[−7,7]	[−10,10]	[−3,3]	[−5,5]	[−7,7]	[−10,10]
	1	2	3	4	5	6	7	8
DNT × POSTAN	−0.073	−0.093**	−0.087*	−0.061	−0.100*	−0.089	−0.091*	−0.063
	(−1.52)	(−2.02)	(−1.94)	(−1.58)	(−1.89)	(−1.64)	(−1.65)	(−1.33)
DNT	−0.048	−0.054	−0.052	−0.054	−0.007	−0.032	−0.024	−0.026
	(−1.04)	(−1.27)	(−1.24)	(−1.48)	(−0.14)	(−0.66)	(−0.48)	(−0.60)
POSTAN	−0.044	−0.032	−0.040	0.013	−0.050	−0.037	−0.045	−0.001
	(−0.45)	(−0.49)	(−0.68)	(0.24)	(−0.50)	(−0.55)	(−0.76)	(−0.03)
INTV × POSTAN	0.018	0.030	0.021	0.009	0.015	0.031	0.023	0.019
	(0.51)	(1.03)	(0.74)	(0.36)	(0.40)	(0.95)	(0.75)	(0.69)
AFTER × POSTAN	−0.044	−0.090	−0.099	−0.115	−0.027	−0.101	−0.106	−0.118*
	(−0.38)	(−0.99)	(−1.24)	(−1.64)	(−0.23)	(−1.14)	(−1.39)	(−1.72)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Announcement date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Accounting period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	2,030	2,030	2,031	2,031	1,271	1,271	1,271	1,271
No. of obs.	42,021	42,314	42,584	42,974	29,192	29,399	29,587	29,853
Adjusted R^2	0.564	0.616	0.640	0.661	0.573	0.622	0.644	0.661

analysis, we control for firm, announcement date, and accounting period fixed effects and double cluster the standard errors by firm and announcement date.

Table 5 presents the empirical results. We analyze 3, 5, 7, and 10 trading-day windows around announcements and the findings are consistent. The coefficients of DNT × POSTAN are negative, indicating that the volatility of stocks disclosed to be intervened decreases after the intervention information disclosure. Columns 5–8 use the PSM sample instead. We present the results using the interday volatility measure in Panel C of Table B6 in Section B of the Supplementary Material. The results remain robust. By focusing on short windows around announcements, we thus provide further evidence for the negative disclosure effect on volatility.

V. Intervention and Price Informativeness

A. Intervention and Price Informativeness: Main Results

The previous section shows that NT intervention stabilizes the market and this section further focuses on informational efficiency. Table 6 presents the results

TABLE 6

National Team Intervention and Price Informativeness

Table 6 examines the impact of the National Team intervention on price informativeness. The dependent variable is price nonsynchronicity measure: $\log\left(\left(1-R^2\right)/R^2\right)$. We obtain R^2 from monthly rolling market model regression with a window of 3 months. Columns 1–5 use the whole sample while columns 6–8 use PSM matched sample instead. Column 1 explores the relationship between NT percentage holding (NT_SP) and price nonsynchronicity. Column 2 presents the results for model 1 with the NT status dummy as the main independent variable. Columns 3 and 6 correspond to model 2. In columns 4 and 7, and 5 and 8, we present the results for models 3 and 4, respectively. These columns provide estimates for the disclosure effect. Control variables are monthly return (MRET), lagged monthly return (L.MRET), largest loss in 5 consecutive trading days within the month (EXMLOSS), percentage of trading days triggering the price limits within the month (PLIMIT), share concentration (SHARECONCEN), number of shareholders in log (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10), institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). Also included are the β coefficients estimated from the market model regressions. Control variables are unreported for ease of presentation. Firm and year–month fixed effects are included in all models. At the bottom of the table, we present the p-values of the Wald tests on whether the sum of the coefficients equals 0. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. t-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and year–month.

	Price Nonsynchronicity: $\log\left(\left(1-R^2\right)/R^2\right)$							
	Whole Sample				PSM Sample			
	1	2	3	4	5	6	7	8
NT_SP	−0.006 (−1.33)							
NT		0.003 (0.12)						
INIT × NT			0.007 (0.11)			0.007 (0.10)		
REMAIN × NT			0.002 (0.09)			−0.009 (−0.38)		
DNT				0.062*** (2.90)	0.059*** (2.76)		0.064*** (2.69)	0.058** (2.42)
DNT × UN				−0.098*** (−5.07)			−0.109*** (−5.18)	
DINC × UN					−0.137*** (−6.30)			−0.154*** (−6.21)
DUN × UN					−0.075*** (−2.98)			−0.082*** (−3.01)
DDEC × UN					−0.101*** (−3.09)			−0.098** (−2.61)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	2,107	2,107	2,107	2,107	2,107	1,303	1,303	1,303
No. of obs.	76,875	76,875	76,875	76,875	76,875	52,041	52,041	52,041
Adjusted R^2	0.687	0.687	0.687	0.687	0.687	0.681	0.681	0.681
Pr[Coef(DNT) + Coef(DNT × UN) = 0]				0.111			0.065	
Pr[Coef(DINC × UN) + Coef(DNT × UN) = 0]					0.001			0.000
Pr[Coef(DINC × UN) + Coef(DNT × UN) = 0]					0.545			0.374
Pr[Coef(DUN × UN) + Coef(DNT × UN) = 0]					0.233			0.313

with price nonsynchronicity as the dependent variable. Price nonsynchronicity is calculated as $\log\left(\frac{1-R^2}{R^2}\right)$, where R^2 is the goodness of fit of the market model with a rolling window of a quarter (current month included). More synchronized prices (lower price nonsynchronicity) are less informative.

In addition to the control variables in Table 3, we also control for EXMLOSS and PLIMIT. EXMLOSS is the monthly largest loss in 5 consecutive trading days and PLIMIT is the percentage of trading days hitting the price limits. These two variables also help predict NT intervention but are previously omitted as they capture similar things as volatility. Also included are the β coefficients estimated from the market model regressions. We control for firm fixed effects and year–month fixed effects in all models and double cluster standard errors by firm and year–month. For robustness, we also include firm × year fixed effects (see Table B7 in Section B of the Supplementary Material).

Column 1 of Table 6 shows that the NT percentage holding is negatively associated with price informativeness. An average intervened stock with a 3.08% NT holding has a 4.6% lower price informativeness than an unintervened stock, compared to the whole sample mean. Column 2 corresponds to model 1, and the overall impact of the intervention on price informativeness is insignificant. Columns 3–5 further disentangle the trading effect and the disclosure effect. For the trading effect, the coefficient of INIT × NT in column 3 is positive but insignificant, indicating that the NT purchases in the initial intervention period have weak impacts on price informativeness. The intervention could raise price informativeness by offsetting noise trading but it could also impair efficiency by adding an uncorrectable noise (Brunnermeier et al. (2022)). The final impact of intervention trading on price informativeness depends on the combined effect of these two countervailing forces. The empirical results show that the positive and negative effects offset, resulting in an insignificant trading effect.

Column 4 of Table 6 presents the results for model 3. The disclosure of detailed NT holdings significantly reduces price informativeness, as shown by the negative and significant coefficient of DNT × UN. The sum of the coefficients for DNT and DNT × UN indicates a 13.1% decline in price informativeness, compared to the intervened group sample mean. Furthermore, the coefficients for DINC × UN, DUN × UN, and DDEC × UN in column 5 show that the price informativeness reduction driven by intervention portfolio disclosure ranges from 5.84% to 28.5%. Columns 6–8 present the regression results for the PSM sample. The results are robust.

Similar to the volatility analysis, we perform some subsample analyses to control for the anticipation impact. Restricting to the subsample with high intervention prediction errors, the estimated trading impact is similar to those in Table 6 and we observe a stronger disclosure effect. Results are similar if we focus on stocks with low institutional ownership, or exclude the stocks with potential intervention information leakage. We report the results in Table A3 in Section A of the Supplementary Material.

Aside from the robustness checks mentioned above, other tests include allowing for controls with time-varying trends, excluding systematically important industries, including the past NT percentage holding as a control, and so forth. Section B of the Supplementary Material provides a comprehensive introduction to the additional tests.

The findings support the government-centric case described in Hypothesis 2. Overall, NT intervention weakly reduces price informativeness. The trading effect has an insignificant impact, whereas the disclosure effect decreases price informativeness.

B. Intervention Disclosure and Post-Earnings-Announcement Drift

To directly examine the disclosure effect on informational efficiency, we estimate the impact of the intervention disclosure on the PEAD (Kacperczyk et al. (2021), Coles et al. (2022)). In an efficient market where prices respond instantly to fundamental news, post-earnings-announcement returns should be unrelated to earnings surprise. The presence of PEAD indicates price inefficiency, and we expect a larger PEAD following intervention disclosure. The empirical model is as follows:

$$\begin{aligned}(6)\quad \mathrm{CAR}[j,k,]_{i,p} = {} & \alpha_1 \mathrm{DNT}_{i,p} \times \mathrm{SUE\_DECILE}_{i,p} + \alpha_2 \mathrm{SUE\_DECILE}_{i,p} + \alpha_3 \mathrm{DNT}_{i,p} \\ & + \alpha_4 \mathrm{INTV}_i \times \mathrm{SUE\_DECILE}_{i,p} + \alpha_5 \mathrm{AFTER}_p \times \mathrm{SUE\_DECILE}_{i,p} \\ & + \mathrm{CONTROLS} + \mathrm{FEs} + \varepsilon_{i,p}.\end{aligned}$$

$\mathrm{CAR}[j,k,]_{i,p}$ is the cumulative abnormal return of stock i for quarterly report announcement p from the jth to the kth trading day after the announcement day. We calculate the cumulative abnormal return based on the market model. Following the practice in the literature, we obtain SUE, sort stocks into deciles based on SUE for each quarter (defined as SUE_DECILE), and estimate the return responses to the earnings announcements for different time windows (DellaVigna and Pollet (2009), Liu, Peng, and Tang (2023)). The coefficient of $\mathrm{DNT}_{i,p} \times \mathrm{SUE\_DECILE}_{i,p}$ captures how the PEAD differs once a stock is disclosed to be NT intervened, and we expect it to be positive.

Similarly, we include $\mathrm{INTV}_i \times \mathrm{SUE\_DECILE}_{i,p}$ to control for the average differences in PEAD between intervened stocks and unintervened stocks, and $\mathrm{AFTER}_p \times \mathrm{SUE\_DECILE}_{i,p}$ to control for the average change in PEAD after 2015:Q3. Firm, announcement date, and accounting period fixed effects are all included, and standard errors are double clustered by firm and announcement date.

Table 7 shows how intervention disclosure affects PEAD. On the announcement day, the coefficient of $\mathrm{DNT}_{i,p} \times \mathrm{SUE\_DECILE}_{i,p}$ is negative, though insignificant. This suggests that investors' attention has been diverted away from earnings information, resulting in smaller price responses to earnings on event days. In the following days, the coefficients of $\mathrm{DNT}_{i,p} \times \mathrm{SUE\_DECILE}_{i,p}$ are positive, indicating more prominent PEAD for stocks disclosed to be NT intervened. This is consistent with the reduced informational efficiency among the intervened stocks. In columns 7–12, we use the PSM sample instead and the results remain robust.

Given that the intervention disclosure reduces informational efficiency, we construct a long–short strategy to measure the economic costs of mispricing. For each earnings announcement period, we buy intervened stocks with SUE in the top quintile and sell those with SUE in the bottom quintile. We build a similar portfolio for the unintervened stocks. We then long the portfolio of the intervened stocks and short the portfolio of the unintervened stocks. Intervened stocks are less sensitive to earnings announcements. Therefore, high-SUE intervened stocks should experience higher positive returns in the days following the announcement, while low-SUE intervened stocks should experience more negative market reactions.

Indeed, as shown in Panel A of Table 8, this long–short strategy yields positive returns. The equal-weighted portfolios generate an average 20-day holding

TABLE 7

Intervention Disclosure and Post-Earnings-Announcement Drift

Table 7 presents panel regression analyses on the intervention information disclosure and return responses for the quarterly earnings announcement. CAR[0] and CAR[*j*,*k*] are the abnormal return on the announcement day (day 0) and the cumulative abnormal return from the *j*th to the *k*th trading day after the announcements. Abnormal return is calculated based on the market model. SUE is the standardized unexpected earnings, measured by the difference between current period earnings and the earnings four quarters before, scaled by the standard deviation of the earnings differences over the preceding eight quarters. For each quarter, we sort stocks into deciles based on SUE and define SUE_DECILE accordingly. DNT equals 1 for stocks disclosed to be included in the NT portfolio, and 0 otherwise. We also define a dummy INTV for stocks intervened by the NT and it equals 0 for stocks never intervened by the NT, and 1 otherwise. The AFTER dummy equals 1 for 2015:Q3 and all periods thereafter, and 0 otherwise. Control variables include share concentration (SHARECONCEN), log number of shareholders (ln(SHNO)), revenue growth rate (REVGROWTH), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10) and institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). The estimated results for control variables are unreported for ease of presentation. Firm, announcement date, and accounting period fixed effects are included in all models. We use the whole sample in columns 1–6 and the PSM sample in columns 7–12. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and announcement date.

	CAR											
	Whole Sample						PSM Sample					
	[0]	[1,3]	[1,5]	[1,7]	[1,10]	[1,20]	[0]	[1,3]	[1,5]	[1,7]	[1,10]	[1,20]
	1	2	3	4	5	6	7	8	9	10	11	12
DNT × SUE_DECILE	−0.020	0.102**	0.088*	0.096*	0.073	0.145	−0.051	0.081*	0.097*	0.126**	0.133*	0.221**
	(−0.60)	(2.57)	(1.78)	(1.75)	(1.09)	(1.60)	(−1.38)	(1.84)	(1.72)	(2.03)	(1.78)	(2.09)
SUE_DECILE	0.025	0.038	0.032	0.035	0.001	−0.024	−0.010	0.051	0.081*	0.095*	0.064	0.003
	(1.24)	(1.41)	(0.98)	(0.96)	(0.02)	(−0.40)	(−0.42)	(1.39)	(1.69)	(1.86)	(1.05)	(0.03)
DNT	0.054	−0.788***	−0.499	−0.437	−0.092	−0.193	0.405*	−0.517*	−0.430	−0.491	−0.351	−0.664
	(0.27)	(−3.04)	(−1.63)	(−1.25)	(−0.20)	(−0.28)	(1.75)	(−1.84)	(−1.29)	(−1.36)	(−0.76)	(−0.98)
INTV × SUE_DECILE	0.060**	0.007	0.012	0.003	0.039	0.095	0.088***	0.004	−0.025	−0.037	−0.009	0.072
	(2.36)	(0.22)	(0.33)	(0.07)	(0.80)	(1.45)	(3.02)	(0.10)	(−0.52)	(−0.68)	(−0.14)	(0.83)
AFTER × SUE_DECILE	−0.023	−0.054*	−0.032	−0.040	−0.020	−0.103	0.006	−0.052	−0.070	−0.099*	−0.095	−0.192*
	(−0.88)	(−1.83)	(−0.91)	(−0.92)	(−0.36)	(−1.26)	(0.20)	(−1.35)	(−1.50)	(−1.84)	(−1.40)	(−1.93)
Announcement date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Accounting period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	1,824	1,995	1,995	1,995	1,995	1,995	1,218	1,272	1,272	1,272	1,272	1,272
No. of obs.	14,838	22,101	22,101	22,101	22,101	22,101	10,139	14,881	14,881	14,881	14,881	14,881
Adjusted R^2	0.069	0.054	0.043	0.043	0.034	0.047	0.077	0.071	0.054	0.054	0.044	0.045

TABLE 8

Returns of the SUE-Based Long–Short Strategy

Table 8 presents the returns on the long–short strategy constructed based on SUE and intervention status. For each earnings announcement period, we buy the intervened stocks with SUE in the top quintile and sell intervened stocks with SUE in the bottom quintile. We build a similar portfolio for the unintervened stocks. We then long the portfolio of intervened stocks and short the portfolio of unintervened stocks. To facilitate implementation, we also re-construct the portfolios as buying positive-SUE stocks and selling negative-SUE stocks. SUE is the standardized unexpected earnings, measured by the difference between current period earnings and the earnings four quarters before, scaled by the standard deviation of the earnings differences over the preceding eight quarters. Panel A presents the average returns in the post-intervention period (2015:Q3–2017:Q2), and Panel B presents the differences in post-intervention returns compared to the pre-intervention period returns (2013:Q3–2015:Q2). "EW" and "VW" stand for "equal-weighted" and "value-weighted," respectively. We present the results based on both the whole sample and the PSM sample. "Raw return" means the return calculated based on the raw stock return, while "MM abnormal return" means that the return is computed based on the market model abnormal return. A holding period of [*i*,*j*] means holding the stock from the *i*th to the *j*th trading day after the announcement.

				Holding Period					
				1	[1,3]	[1,5]	[1,7]	[1,10]	[1,20]
Panel A. Average Returns from 2015:Q3 to 2017:Q2									
Whole sample	Raw return	Quintile-based	EW	0.25%	0.75%	0.75%	0.34%	0.29%	1.51%
			VW	0.34%	0.95%	0.98%	0.50%	0.39%	1.66%
		0-based	EW	0.12%	0.39%	0.55%	0.41%	0.45%	1.03%
			VW	0.09%	0.41%	0.55%	0.39%	0.25%	0.74%
	MM abnormal return	Quintile-based	EW	0.12%	0.70%	0.53%	0.24%	−0.02%	1.04%
			VW	0.31%	1.03%	0.85%	0.58%	0.39%	1.51%
		0-based	EW	0.09%	0.42%	0.46%	0.41%	0.45%	0.92%
			VW	0.12%	0.51%	0.54%	0.54%	0.48%	0.90%
PSM sample	Raw return	Quintile-based	EW	0.37%	0.78%	0.61%	0.23%	−0.55%	1.22%
			VW	0.37%	0.90%	0.77%	0.36%	−0.10%	1.79%
		0-based	EW	0.08%	0.25%	0.27%	0.11%	0.04%	1.03%
			VW	0.04%	0.25%	0.19%	−0.04%	−0.05%	0.92%
	MM abnormal return	Quintile-based	EW	0.31%	0.74%	0.62%	0.50%	0.15%	1.67%
			VW	0.41%	0.86%	0.64%	0.52%	0.38%	2.05%
		0-based	EW	0.15%	0.34%	0.40%	0.43%	0.53%	1.35%
			VW	0.15%	0.24%	0.25%	0.28%	0.43%	1.16%
	Average			0.21%	0.60%	0.56%	0.36%	0.22%	1.28%
Panel B. Differences in Returns Compared to Pre-Intervention Periods									
Whole sample	Raw return	Quintile-based	EW	0.48%	1.28%	1.26%	0.28%	0.24%	0.02%
			VW	0.64%	1.74%	2.10%	1.49%	1.02%	0.53%
		0-based	EW	0.37%	0.77%	1.00%	0.66%	0.93%	1.70%
			VW	0.21%	0.71%	1.04%	0.83%	0.64%	0.42%
	MM abnormal return	Quintile-based	EW	0.33%	0.97%	0.61%	0.38%	−0.01%	−0.32%
			VW	0.48%	1.19%	0.98%	0.83%	0.17%	0.06%
		0-based	EW	0.19%	0.53%	0.54%	0.52%	0.48%	0.63%
			VW	0.16%	0.50%	0.47%	0.54%	0.20%	0.02%
PSM sample	Raw return	Quintile-based	EW	0.61%	1.65%	1.80%	1.25%	0.19%	−0.17%
			VW	0.92%	2.17%	2.75%	2.89%	2.34%	2.08%
		0-based	EW	0.43%	0.69%	1.15%	1.06%	1.24%	1.81%
			VW	0.45%	0.68%	1.26%	1.42%	1.76%	2.13%
	MM abnormal return	Quintile-based	EW	0.57%	1.37%	1.43%	1.49%	1.20%	1.31%
			VW	0.72%	1.28%	1.12%	1.45%	1.08%	1.35%
		0-based	EW	0.32%	0.61%	0.94%	1.14%	1.25%	2.10%
			VW	0.33%	0.37%	0.55%	0.89%	1.01%	1.73%
	Average			0.45%	1.03%	1.19%	1.07%	0.86%	0.96%

period return of 1.51% (20.6% annualized). Using the market model abnormal returns instead, the strategy produces an average 20-day holding period abnormal return of 1.04% (13.8% annualized). Results are similar among the PSM sample. To simplify implementation, we also construct the portfolios by buying positive-SUE stocks and selling negative-SUE stocks. The results are similar.

We might be concerned that if the intervened stocks are always more informationally inefficient, the above long–short strategy generates positive returns regardless of intervention disclosure. To address this concern, we present in Panel B of Table 8 the average differences between post-intervention and pre-intervention returns. Upon subtracting the pre-intervention returns, the SUE-based long–short strategy still produces positive returns. This is consistent with the intervention disclosure resulting in significant mispricing.

To summarize, following intervention disclosure, the intervened stocks are less responsive to earnings news and have larger PEADs. Using a SUE-based long–short strategy, we show that the reduction in informational efficiency results in a significant mispricing in the intervened stocks. Overall, these findings are consistent with the government-centric case, where intervention disclosure motivates investors to acquire intervention information instead of the fundamental information, resulting in less efficient stock prices.

VI. Results on Information Production and Information Asymmetry

The information choices of investors differ in the government-centric and fundamental-centric equilibria in Brunnermeier et al. (2022). Though both are theoretically plausible, our empirical findings are more consistent with the government-centric scenario, as evidenced by lower volatility and informational efficiency following intervention disclosure. In this section, we provide further evidence on information production and information asymmetry.

A. Information Production

In the government-centric scenario, investors reduce fundamental information production, whereas in the fundamental-centric scenario, information production is unresponsive to intervention disclosure. Thus, we directly test for changes in information production following intervention disclosure.

Using analyst coverage as a proxy for information production, columns 1 and 2 of Table 9 show that intervention disclosure is associated with lower analyst coverage of the intervened stocks, which is consistent with reduced information production.

We also use the number of company visitors as a proxy for information production. The Shenzhen Stock Exchange, one of China's two major stock exchanges, requires listed firms to disclose detailed company visit (or site visit) records (Chen, Qu, Shen, Wang, and Xu (2022)). The records also include information about the visitor type. Records concerning buy-side visitors with skin in the game might more accurately reflect the information production activity.

Columns 3–6 of Table 9 present the impacts of intervention disclosure on the number of company visitors. Consistent with the government-centric case in Hypothesis 3, the disclosure of NT intervention information results in a significant decrease in both the overall number of company visitors and the number of buy-side visitors.

TABLE 9

National Team Intervention and Information Production

Table 9 examines the impact of the National Team intervention on information production using model 3. The dependent variables are the analyst coverage (columns 1 and 2), the number of onsite company visitors (columns 3 and 4), and the number of buy-side visitors (columns 5 and 6). As the dependent variables are count data, we use a conditional fixed effect Poisson model (Cohn, Liu, and Wardlaw (2022)). Odd columns use the whole sample while even columns use the PSM-matched sample instead. Control variables are monthly return (MRET), lagged monthly return (L.MRET), the largest loss in 5 consecutive trading days within the month (EXMLOSS), percentage of trading days triggering the price limits within the month (PLIMIT), share concentration (SHARECONCEN), number of shareholders in log (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10), institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). Control variables are unreported for ease of presentation. Firm and year–month fixed effects are included in all models. At the bottom of the table, we present the *p*-values of the Wald tests on whether the sum of the coefficients equals 0. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are clustered by firm.

	Analyst Coverage		No. of Company Visitors		No. of Buy-Side Visitors	
	All	PSM	All	PSM	All	PSM
	1	2	3	4	5	6
DNT	−0.156***	−0.097***	0.163*	0.206**	0.189*	0.268**
	(−4.85)	(−2.64)	(1.95)	(2.16)	(1.90)	(2.36)
DNT × UN	−0.087***	−0.082***	−0.233***	−0.265***	−0.234**	−0.285***
	(−3.42)	(−2.90)	(−2.64)	(−2.82)	(−2.26)	(−2.58)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year–month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	1,767	1,138	1,004	654	943	615
No. of obs.	65,063	45,664	37,888	26,277	35,723	24,834
Chi statistic	3,903.828	3,196.090	1,348.916	1,190.761	1,263.791	1,161.760
p-Value	0.000	0.000	0.000	0.000	0.000	0.000
Pr[Coef(DNT) + Coef(DNT × UN) = 0]	0.000	0.000	0.000	0.000	0.024	0.469

Overall, the empirical findings show that after intervention disclosure, private information production decreases, which is consistent with the government-centric case.

B. Information Asymmetry

Furthermore, in the government-centric case with reduced private information production, intervention disclosure reduces information asymmetry. As a comparison, in the fundamental-centric case, the market information asymmetry is unaffected by the intervention disclosure.

We use three proxies for information asymmetry. The first measure is the probability of informed trading or PIN. Proposed by Easley, Kiefer, O'hara, and Paperman (1996), PIN measure infers the probability of informed trading from the buy/sell order imbalances. The second proxy is analysts' price forecast dispersion. Larger forecast dispersion indicates more heterogeneous information obtained by analysts, thus a larger extent of information asymmetry (Diether, Malloy, and Scherbina (2002), Yu (2011)). The third measure for information asymmetry also relies on high-frequency data. The λ from price impact regressions measures the proportion of information asymmetry component in the effective spread using quotes and transaction records (Lin, Sanger, and Booth (1995), Huang and Stoll (1997)). Sections A.1.2 and A.1.3 of the Supplementary Material introduce the construction details of PIN and λ.

TABLE 10

National Team Intervention and Information Asymmetry

Table 10 examines the impact of the National Team intervention on information asymmetry using model 3. The dependent variables are the probability of informed trading (columns 1 and 2), analyst forecast dispersion (columns 3 and 4), and λ from price impact regressions (columns 5 and 6). Odd columns use the whole sample while even columns use the PSM-matched sample instead. Control variables are monthly return (MRET), lagged monthly return (L.MRET), the largest loss in 5 consecutive trading days within the month (EXMLOSS), percentage of trading days triggering the price limits within the month (PLIMIT), share concentration (SHARECONCEN), number of shareholders in log (ln(SHNO)), revenue growth rate (REVGROWTH), return on equity (ROE), book-to-market ratio (BMR), log total asset (ln(ASSET)), ratios of shares held by top 10 shareholders (TOP10), institutional investors (INSTHOLD), and Amihud ratio (AMIHUD). Control variables are unreported for ease of presentation. Firm and year–month fixed effects are included in all models. At the bottom of the table, we present the *p*-values of the Wald tests on whether the sum of the coefficients equals 0. *, **, *** stands for significance at 10%, 5%, 1% level, respectively. *t*-statistics reported in the parentheses below estimated parameters. Standard errors are double clustered by firm and year–month.

	PIN		Price Forecast Dispersion		λ	
	All	PSM	All	PSM	All	PSM
	1	2	3	4	5	6
DNT	−0.066	−0.050	−0.013***	−0.015***	−0.002	−0.004
	(−0.31)	(−0.20)	(−3.08)	(−3.14)	(−0.65)	(−1.08)
DNT × UN	−0.448**	−0.428**	−0.006*	−0.007*	−0.014***	−0.013***
	(−2.54)	(−2.30)	(−1.92)	(−2.01)	(−5.08)	(−4.70)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year–month FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of firms	2,024	1,277	1,501	971	2,041	1,286
No. of obs.	33,299	22,849	55,306	39,041	37,336	25,564
Adjusted R^2	0.175	0.182	0.268	0.267	0.686	0.710
Pr[Coef(DNT) + Coef(DNT × UN) = 0]	0.003	0.012	0.000	0.000	0.002	0.001

Table 10 presents the regression results for all three measures of information asymmetry. Following intervention disclosure, the extent of information asymmetry shrinks substantially, as indicated by the significantly negative coefficients of DNT × UN.[22] For all three measures, the sums of the coefficients DNT and DNT × UN are negative. These results provide further evidence for the government-centric scenario.

VII. Conclusion

This article investigates the large-scale intervention during the 2015 Chinese stock market crash, where the government directly participated in stock trading. In light of Brunnermeier et al. (2022), this article empirically tests corresponding theoretical predictions, and the findings are consistent with their government-centric equilibrium. The results show that the government intervention reduces both the volatility and price informativeness of the intervened stocks. The impacts of government intervention come from two parts: the direct trading effect and the disclosure effect. Direct trading stabilizes the market. But we highlight that the intervention disclosure, by diverting investors' attention away from the fundamentals, results in a further decline in volatility and, more importantly, a drop in price informativeness. The intervened stocks are significantly mispriced due to the

[22]We only have access to high-frequency data for 2014.7–2016.6. So the sample size for regressions on PIN and λ is smaller than other regressions, as the estimation of these two variables relies on high-frequency data.

https://doi.org/10.1017/S0022109023000637

reduced informational efficiency. Furthermore, we find reduced information production and less information asymmetry following the disclosure of intervention portfolio.

The findings shed some light on the rationales and trade-offs of government interventions in financial markets. In a normative sense, reduced volatility might be socially desirable, to the extent that there is too much speculative trading in both developed and emerging economies (Odean (1999), Deng et al. (2018)). Reduced price informativeness is mainly driven by the intervention portfolio disclosure. The findings thus suggest that there is possibly a trade-off between market stability and informational efficiency.

The article also sheds some light on unconventional monetary policy. During the recent global financial crisis, major central banks around the world directly bought assets in quantitative easing, the majority of which were bonds and asset-backed securities. Some central banks, such as the Bank of Japan, have long been purchasing stocks directly. In contrast to the information insensitive debt markets, a major function of equity markets is information production (Holmstrom (2015), Dang, Gorton, and Holmstrom (2020)). The findings thus pinpoint the potentially different impacts of unconventional monetary policies on different markets. Furthermore, the findings may have implications for the real effects of unconventional monetary policy. These could be fruitful future research avenues.

Appendix: Variable Definitions

$\text{NT\_SP}_{i,t}$: NT's percentage holdings of stock i in month t.

$\text{NT}_{i,t}$: A dummy variable that equals 1 if the NT's position in stock i is positive in month t, and 0 otherwise.

INIT_t: A dummy variable that equals 1 if month t belongs to 2015:Q3, and 0 otherwise.

REMAIN_t: A dummy variable that equals 1 if month t belongs to 2015.10–2017.6, and 0 otherwise.

$\text{DNT}_{i,t}$: A dummy variable that equals 1 if in month t the latest quarterly report disclosed that NT was among the top 10 shareholders of stock i, and 0 otherwise. For months with statement disclosures, $\text{DNT}_{i,t}$ is updated to the new report if it is released in the first half-month. Otherwise, $\text{DNT}_{i,t}$ is defined according to the previous report for the current month and updated to the new report starting from the next month. If two statements are disclosed in the same half-month (e.g., the previous annual report and the new Q1 report), $\text{DNT}_{i,t}$ is defined according to the statement with more recent accounting period. If two statements are disclosed in the same month but at different halves, we compute the average reported NT holdings and define $\text{DNT}_{i,t}$ accordingly.

$\text{DINC}_{i,t}$ ($\text{DUN}_{i,t}$, $\text{DDEC}_{i,t}$): A dummy variables that equals 1 if the latest quarterly report disclosed increased (unchanged, decreased) NT holdings in stock i, and 0 otherwise.

$\text{UN}_{i,t}$: A dummy variable that indicates whether the NT holding is unchanged for stock i in month t.

https://doi.org/10.1017/S0022109023000637

INTRADAY_VOLATILITY (%): The monthly average of the daily volatility defined as the difference between intraday highest and lowest prices scaled by the average of the two.

INTERDAY_VOLATILITY (%): The log standard deviation of daily return within a month.

VOLA$_{i,t,p}$: The intraday volatility (average daily price change, %) of stock i during the announcement of quarterly report p. $t=0$ indicates pre-announcement observations while $t=1$ stands for post-announcement observations. Used in the event-study around announcements.

DNT$_{i,p}$: A dummy variable that equals 1 for stocks i disclosed to be NT intervened in report p, and 0 otherwise. Used in the event-study around announcements.

POSTAN$_t$: A dummy variable that equals 1 for post-announcement observations, and 0 otherwise. Used in the event-study around announcements.

INTV$_i$: A dummy variable that equals 1 for stocks ever intervened by the NT, and 0 otherwise. Used in the event-study around announcements.

AFTER$_p$: A dummy variable that equals 1 for 2015:Q3 and all periods thereafter, and 0 otherwise. Used in the event-study around announcements.

PRICE_NONSYNCHRONICITY: $\log\left(\frac{1-R^2}{R^2}\right)$, where R^2 is the goodness of fit from market model regressions: $R_{i,d,t}=\alpha_0+\beta_i \mathrm{RM}_{i,d,t}+\beta_{i,IND} R\_\mathrm{IND}_{i,d,t}+\varepsilon_{i,d,t}$. $R_{i,d,t}$ is the return for stock i on day d in month t in excess of risk-free rate. RM$_{i,d,t}$ is the market return, in excess of risk-free rate and with stock i excluded, and R_IND$_{i,d,t}$ stands for industry return, also with stock i excluded. We use 3-month term deposit rate as the risk-free rate (Liu, Stambaugh, and Yuan (2019)). R^2 for stock i in month t is the goodness of fit for the regression run on daily data from month $t-2$ to month t.

CAR[0], CAR[j,k]: The abnormal return for stock i during quarterly announcement p on the announcement day (day 0); the cumulative abnormal return from the jth trading day after the announcement to the kth trading day.

SUE$_{i,p}$: Standardized unexpected earnings of stock i in earnings announcement p: SUE$_{i,p}=\frac{E_{i,p}-E_{i,p-4}}{\sigma_{i,p}}$, where $E_{i,p}$ and $E_{i,p-4}$ are the earnings in the current announcement p and in the announcement four quarters before ($p-4$). $\sigma_{i,p}$ is the standard deviation of $E_{i,p}-E_{i,p-4}$ over the preceding eight quarters.

SUE_DECILE$_{i,p}$: Sort stocks into deciles based on SUE for each quarter and SUE_-DECILE is defined accordingly.

MRET (QRET): Monthly (quarterly) return.

L.MRET (L.QRET): Lagged monthly (quarterly) return.

EXMLOSS: The absolute value of the largest loss in 5 consecutive trading days within the month (quarter). If the largest loss in 5 consecutive trading days is positive (i.e., no loss), then EXMLOSS = 0.

PLIMIT: The percentage of trading days hitting price limits within the month.

DLIMIT: The percentage of trading days touching the lower price limits within the quarter.

TOP10: Ratio of shares held by top 10 shareholders.

INSTHOLD: Ratio of shares held by institutional investors.

ln(SHNO): Number of shareholders (in log).

SHARECONCEN: The sum of squares of shares larger than 5%.

REVGROWTH (%): Revenue growth rate (%).

ROE (%): Return on equity (%).

BMR: Book-to-market ratio.

ln(ASSET): Total asset (in log).

NO._OF_COMPANY_VISITORS: Total number of onsite company visitors within the month.

NO._OF_BUY-SIDE_VISITORS: Total number of onsite buy-side company visitors within the month.

ANALYST_COVERAGE: Total number of analysts following the stock.

PIN: Probability of informed trading (Easley et al. (1996)).

PRICE_FORECAST_DISPERSION: The standard deviation of analyst price forecast scaled by the average forecast. We require at least three price forecasts for the current year issued within 6 months.

λ: The information asymmetry component (λ) in effective spread estimated from price impact regressions (Lin et al. (1995)): $\Delta M_{t+1} = \lambda z_t + e_{t+1}$, where ΔM_{t+1} is the midquote change and z_t is the effective half spread.

AMIHUD: Amihud illiquidity measure, measured by $1/D_{i,t}\sum_{d=1}^{D_{i,t}} |R_{i,d,t}|/\text{VOL}_{i,d,t}$, where $D_{i,t}$, $R_{i,d,t}$, and $\text{VOL}_{i,d,t}$ is the number of trading days, stock return, and RMB trading volume for stock i on day d in month t, respectively.

$1\{\text{STATE\_CONTROLLED}\}$: Indicator for state-owned firms.

$1\{\text{REG\_CONNECTED}\}$: Indicator for firms with at least one regulator-connected executive or board member.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000637.

References

Amihud, Y. "Illiquidity and Stock Returns: Cross-Section and Time-Series Effects." *Journal of Financial Markets*, 5 (2002), 31–56.

Anderson, R. C.; D. M. Reeb; and W. Zhao. "Family-Controlled Firms and Informed Trading: Evidence from Short Sales." *Journal of Finance*, 67 (2012), 351–385.

Bai, J.; T. Philippon; and A. Savov. "Have Financial Markets Become More Informative?" *Journal of Financial Economics*, 122 (2016), 625–654.

Barbon, A., and V. Gianinazzi. "Quantitative Easing and Equity Prices: Evidence from the ETF Program of the Bank of Japan." *Review of Asset Pricing Studies*, 9 (2019), 210–255.

Bian, J.; Z. He; K. Shue; and H. Zhou. "Leverage-Induced Fire Sales and Stock Market Crashes." NBER Working Paper No. 25040 (2018).

Bond, P.; A. Edmans; and I. Goldstein. "The Real Effects of Financial Markets." *Annual Review of Financial Econnomics*, 4 (2012), 339–360.

Boyarchenko, N.; A. Kovner; and O. Shachar. "It's What You Say and What You Buy: A Holistic Evaluation of the Corporate Credit Facilities." *Journal of Financial Economics*, 144 (2022), 695–731.
Brunnermeier, M. K., and L. H. Pedersen. "Market Liquidity and Funding Liquidity." *Review of Financial Studies*, 22 (2009), 2201–2238.
Brunnermeier, M. K.; M. Sockin; and W. Xiong. "China's Model of Managing the Financial System." *Review of Economic Studies*, 89 (2022), 3115–3153.
Cai, J.; J. He; W. Jiang; and W. Xiong. "The Whack-a-Mole Game: Tobin Taxes and Trading Frenzy." *Review of Financial Studies*, 34 (2021), 5723–5755.
Chan, L. K.; N. Jegadeesh; and J. Lakonishok. "Momentum Strategies." *Journal of Finance*, 51 (1996), 1681–1713.
Charoenwong, B.; R. Morck; and Y. Wiwattanakantang. "Bank of Japan Equity Purchases: The (Non-) Effects of Extreme Quantitative Easing." *Review of Finance*, 25 (2021), 713–743.
Chen, H.; Y. Qu; T. Shen; Q. Wang; and D. X. Xu. "The Geography of Information Acquisition." *Journal of Financial and Quantitative Analysis*, 57 (2022), 2251–2285.
Chen, Q.; I. Goldstein; and W. Jiang. "Price Informativeness and Investment Sensitivity to Stock Price." *Review of Financial Studies*, 20 (2007), 619–650.
Cheng, M.; L. Jin; Z. Li; and B. Lin. "The Effectiveness of Government Stock Purchase During Market Crash: Evidence from China." *Pacific-Basin Finance Journal*, 71 (2022), 101706.
Chordia, T., and L. Shivakumar. "Momentum, Business Cycle, and Time-Varying Expected Returns." *Journal of Finance*, 57 (2002), 985–1019.
Cohn, J.; Z. Liu; and M. Wardlaw. "Count (and Count-Like) Data in Finance." *Journal of Financial Economics*, 146 (2022), 529–551.
Coles, J. L.; D. Heath; and M. C. Ringgenberg. "On Index Investing." *Journal of Financial Economics*, 145 (2022), 665–683.
Dang, T. V.; G. Gorton; and B. Holmström. "The Information View of Financial Crises." *Annual Review of Financial Economics*, 12 (2020), 39–65.
DellaVigna, S., and J. M. Pollet. "Investor Inattention and Friday Earnings Announcements." *Journal of Finance*, 64 (2009), 709–749.
Deng, Y.; X. Liu; and S.-j. Wei. "One Fundamental and Two Taxes: When Does a Tobin Tax Reduce Financial Price Volatility?" *Journal of Financial Economics*, 130 (2018), 663–692.
Diether, K. B.; C. J. Malloy; and A. Scherbina. "Differences of Opinion and the Cross Section of Stock Returns." *Journal of Finance*, 57 (2002), 2113–2141.
Easley, D.; N. M. Kiefer; M. O'hara; and J. B. Paperman. "Liquidity, Information, and Infrequently Traded Stocks." *Journal of Finance*, 51 (1996), 1405–1436.
Ehrmann, M.; G. Gaballo; P. Hoffmann; and G. Strasser. "Can More Public Information Raise Uncertainty? The International Evidence on Forward Guidance." *Journal of Monetary Economics*, 108 (2019), 93–112.
Fernandes, N., and M. A. Ferreira. "Does International Cross-Listing Improve the Information Environment." *Journal of Financial Economics*, 88 (2008), 216–244.
Ferreira, M. A., and P. A. Laux. "Corporate Governance, Idiosyncratic Risk, and Information Flow." *Journal of Finance*, 62 (2007), 951–989.
Geanakoplos, J. "The Leverage Cycle." *NBER Macroeconomics Annual*, 24 (2010), 1–66.
Hansman, C.; H. G. Hong; W. Jiang; Y.-J. Liu; and J. Meng. "Effects of Credit Expansions on Stock Market Booms and Busts." NBER Working Paper No. 24586 (2021).
Holmstrom, B. "Understanding the Role of Debt in the Financial System." Working Paper, Bank for International Settlements (2015).
Huang, R. D., and H. R. Stoll. "The Components of the Bid–Ask Spread: A General Approach." *Review of Financial Studies*, 10 (1997), 995–1034.
Huang, Y.; J. Miao; and P. Wang. "Saving China's Stock Market?" *IMF Economic Review*, 67 (2019), 349–394.
Kacperczyk, M.; S. Sundaresan; and T. Wang. "Do Foreign Institutional Investors Improve Price Efficiency?" *Review of Financial Studies*, 34 (2021), 1317–1367.
Lin, J.-C.; G. C. Sanger; and G. G. Booth. "Trade Size and Components of the Bid–Ask Spread." *Review of Financial Studies*, 8 (1995), 1153–1183.
Liu, H.; L. Peng; and Y. Tang. "Retail Attention, Institutional Attention." *Journal of Financial and Quantitative Analysis*, 58 (2023), 1005–1038.
Liu, J.; R. F. Stambaugh; and Y. Yuan. "Size and Value in China." *Journal of Financial Economics*, 134 (2019), 48–69.
Mace, C. "The Real Effects of Equity Markets on Innovation." *Journal of Financial and Quantitative Analysis*, 58 (2023), 2522–2552.

Morck, R.; B. Yeung; and W. Yu. "The Information Content of Stock Markets: Why Do Emerging Markets Have Synchronous Stock Price Movements?" *Journal of Financial Economics*, 58 (2000), 215–260.

Nofsinger, J. R., and R. W. Sias. "Herding and Feedback Trading by Institutional and Individual Investors." *Journal of Finance*, 54 (1999), 2263–2295.

Odean, T. "Do Investors Trade Too Much?" *American Economic Review*, 89 (1999), 1279–1298.

Parrino, R.; R. W. Sias; and L. T. Starks. "Voting with Their Feet: Institutional Ownership Changes Around Forced CEO Turnover." *Journal of Financial Economics*, 68 (2003), 3–46.

Roll, R. "R2." *Journal of Finance*, 43 (1988), 541–566.

Sadka, R. "Momentum and Post-Earnings-Announcement Drift Anomalies: The Role of Liquidity Risk." *Journal of Financial Economics*, 80 (2006), 309–349.

Veronesi, P., and L. Zingales. "Paulson's Gift." *Journal of Financial Economics*, 97 (2010), 339–368.

Yu, J. "Disagreement and Return Predictability of Stock Portfolios." *Journal of Financial Economics*, 99 (2011), 162–183.