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DISCUSSION PAPER SERIES

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# ABSTRACT

## Can Politics Tame the Market? Market Responses to Government Control of Fully and Partially Privatized Firms in China*

This study examines factors influencing full (FP) versus partial (PP) privatization and how markets respond to government control in PP and FP firms. Exploiting China's 2005 NTS reform as a natural experiment, we find that treated PP firms experienced significantly lower post-reform performance, driven by persistent private benefits of control, failure to adopt value-maximizing behavior, and unchanged liquidity and information asymmetry. In contrast, FP firms eliminated all NTS, maximized value; showed higher stock market liquidity and lower information asymmetry, improved market performance; and gained market confidence in the post-reform period. These findings challenge the effectiveness of China's authoritarian approach to private sector development.

**JEL Classification:** G31, G38, L33

**Keywords:** non-tradable shares reform, full and partial privatization, firm value maximization, authoritarian central government, local government incentives, difference-in-differences

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# 1. Introduction

Many governments around the world – both in developed but also in developing and emerging countries – have initiated privatization to sell state assets over the past few decades. The latter has been motivated by the twin rationales of raising revenues and improving operating efficiency of erstwhile state-owned firms. While there is a general agreement that the overall effect of privatization has been positive (e.g., Boardman and Vining (1989), Ehrlich *et al.* (1994), Majumdar (1998), and Dewenter and Malatesta (2001), Megginson and Netter (2001)), the process often fails to completely eliminate state control, both in western and non-western countries (see Jones *et al.* (1999); Bortolotti and Faccio (2009)).[1] This also includes the non-democratic authoritarian regimes, where it may be more convoluted and obscure, giving rise to the emergence of political corporations, promoting corruption and endorsing mismanagement (e.g., see Fan *et al.* (2007), Fisman and Wang (2015); Li and Yamada (2015); González *et al.* (2020)).[2,3] The questions that we raise here are (a) what drives full – as opposed to partial – privatization and (b) whether the market differentially treats partially privatized firms from their fully privatized counterparts. This enables us to separate the political aspects of privatization from the economic considerations of privatization, which are still not well explored in the existing literature (see Section 2 for a more detailed discussion). Analyzing the drivers of full as against partial privatization and also market valuations across differentially privatized companies are vital not only for understanding the nature of investment and market developments *per se*, but also for designing policies to limit market uncertainty.

We focus on China that provides a unique setting to explore these issues. Firstly, despite significant economic growth and the development of its stock markets, domestic investors have not been able to profit significantly in China, resulting in a surprising disconnection between its economic expansion and stock market performance over the past three decades (Allen *et al.* (2021)). Secondly, China has undergone several rounds of privatization since the early 1990s, with the most recent being the 2005 Non-tradeable Shares Reform (NTS), alternatively known

[1] For an extensive survey of the effects of privatization in developing economies, see Megginson and Sutter (2006).

[2] In particular, corruption in Russian voucher privatizations had led to the misuse of state assets on a very large scale, and resulted in increased ownership concentration, inefficiency and bad governance. Fan *et al.* (2007) find that firms with politically connected CEOs underperform those without politically connected CEOs by almost 18% based on three-year post-IPO stock returns and have poorer three-year post-IPO earnings growth, sales growth, and change in returns on sales. Fisman and Wang (2015) document the evidence of huge corruption in Chinese state asset sales in partially privatized firms that resulted in underperformance post-privatization. González *et al.* (2020) demonstrated that some firms were sold under-priced to politically connected buyers during Pinochet's privatizations in Chile and continue to be politically connected even under change of regime.

[3] Even though the authors show that government's voting rights in newly privatized firms have decreased in the late 2000, they still account for 62.4% of the total voting rights.

as split shares reform. The reform aimed at converting non-tradeable shares held by the state and legal entities into tradeable ones held by private individuals/institutions subject to an administrative lock-up period, aiming to inject private capital into state-owned enterprises. In particular, Liao *et al.* (2014) had suggested that "The Split-Share Structure Reform granted legitimate trading rights to the state-owned shares of listed state-owned enterprises (SOEs), opening up the gate to China's secondary privatization." Since the reform introduced by the central government was beyond the influence of individual firms, we treat the 2005 NTS reform as a natural experiment.

There is a long list of studies (e.g., Chen *et al.* (2012); Hou *et al.* (2012); Liu *et al.* (2021); Ji *et al.* (2024)) that supports the exogenous nature of the public policy reforms as natural experiments. The introduction of some government policy reform could be endogenous when self-interested agents including business oligarchs can influence this introduction. Note, however, that the introduction of a reform does not necessarily lead to its adoption, especially in emerging markets (see Banerjee *et al.* (2022)). While the *introduction* of such natural experiments could be endogenous in some cases, their *adoption* becomes exogenous, especially when it is mandatory and subject to the administrative lock-up rule. Our paper is the first to focus on the impact of the *adoption* rather than the *introduction* of the 2005 reform (see further discussion in Section 3.2.2). Since all eligible firms (with NTS) adopted the reform, either partially or fully, by the end of 2006, we treat the years after 2006 as the post-adoption period.

Despite nearly two decades since the 2005 introduction of the reform, previous research (see Section 2) has focused solely on the immediate effects of the introduction of the 2005 reform. However, introduction of a reform does not necessarily mean that it will be adopted fully in an emerging economy because of weak legal and institutional framework. Our study stands out by being the first to examine the phased *adoption* of the 2005 reform, spanning years preceding, during, and after its introduction. By exploring these details, we uncover unexpected repercussions stemming from the *adoption* (as opposed to the *introduction*) of the reform, a previously unexplored aspect, thereby departing from the existing literature. The differential effects of privatization across PP and FP firms in this context may thus help to clarify the ambiguous effects of privatization observed in the previous studies (see Section 2 for further details).

We use the China Stock Market & Accounting Research (CSMAR) database spanning from 2000–2016, allowing us to consider the years before, during and after the adoption of the 2005 reform. We define treated firms as those that held NTS in 2005 and started converting them into tradeable shares by the end of 2006 (the official deadline). The control group consists

of firms that did not hold any NTS in 2005, of which 90% were non-state-owned enterprises.[4] We observe the yearly proportion of non-tradeable shares (NTS) that were converted to tradeable shares among treated sample firms after 2005; the latter enables us to trace the staggered adoption of the NTS reform over time, unlike previous studies.[5] This data is uniquely positioned to test the hypotheses of our interest.

The extent of full privatization, although grown a little over time, still remains limited in China: only 11.5% Chinese firms were fully privatized over the period 1998–2007 (Li and Yamada (2015)) which went up to 18% by 2016. Our first exercise therefore examines why only a minority of firms were fully privatized (FP) while most remained partially privatized (PP). Our first contribution, in this respect, has been to identify that, unlike their centralized counterparts, decentralized local governments in China compete with each other for attracting better performing local businesses to raise local tax revenues in a bid to tackle hard budget constraint (Jiang and Waley (2022)). Indeed we find that treated firms, within a given sector, that have connections with the local governments, are more likely to undergo full privatization instead of partial privatization after the adoption of the 2005 reform, after controlling for all other factors.[6] We further show that the latter arises from the value maximization principle adopted by fully privatized firms, enabling them to contribute more to local tax revenue (see further discussion below).

Second, we argue that the net effect of privatization on market performance would depend on the balance between positive incentives effects and the negative entrenchment effects arising from persistent state control even after 2005 and this will differ across PP and FP firms. Our second contribution is to assess the differential impact of the adoption of the 2005 reform on selected market performance indices of treated (relative to control) PP and FP firms from 2007 onwards. The latter in turn allows us to explore the variations in incentive effects and entrenchment effects of privatization between PP and FP firms. To this end, we estimate a difference-in-differences model to compare market performance indices of treated PP and FP firms with corresponding control firms after the *adoption* of the 2005 reform.

Controlling for various observed and unobserved (firm level, Sector×Year level) factors, we show that after the adoption of the 2005 reform, the PP firms with some NTS had significantly lower Tobin's Q relative to the control firms. This result is economically

[4] The timing of NTS conversion was subject to administrative lock-up rule, which made the treatment to be exogenously given since it was beyond the influence of treated firms (see Section 2 for further details).
[5] Non-tradeable shares are defined as the sum of state shares and state-owned legal persons shares.
[6] Further impact threshold analysis documents that the impact of a firm's connection with the local government is not influenced by any unobserved confounding factor. See further discussion in Section 4.4.6.

significant as PP treated firms (compared to control firms) experience a drop of around 9% of the average firm's market performance in our sample. We then explore the underlying mechanisms. We show that lower market performance of PP firms can be attributed to the prevalence of private benefits of control, an absence of firm value maximization, a decrease in executive incentives as well as no improvement in liquidity and information asymmetry after the adoption of the 2005 reform. All these mechanisms imply that PP firms have entrenchment effects in excess of incentive effects of privatization, thus explaining their lower market performance.

In contrast, treated FP firms never had lower market performance, but had significantly higher operating revenue and productivity than comparable control firms after the adoption of the 2005 reform. We document that this is due to the elimination of most private benefits of control (with the exception of some tunnelling) and adhering to the principle of firm value maximization resulting in greater liquidity and lower information asymmetry after the adoption of the reform. Overall, the positive incentive effects of full privatization are larger or equal to the negative entrenchment effects arising from some persistent government controls, thus explaining the relatively favorable impact of privatization on performance among FP firms.

The paper is developed as follows. Section 2 discusses the literature, the background, and develops the hypotheses. Section 3 explains the data and the empirical strategy while Section 4 analyses the results and various robustness tests. The final section concludes.

# 2. Background, Literature, Contributions and Hypotheses Development

From its establishment in 1991, the Chinese stock market aimed to provide state-owned enterprises with the means to raise capital. However, to maintain control over these enterprises, the domestic A shares were separated into two categories: tradable (TS) and non-tradable shares (NTS), which were owned by the state and legal entities and could not be sold. This restriction has been seen as the biggest obstacle to the Chinese equity markets' full development. Recognizing this issue, the government had initiated reforms after experiencing difficulties since 2001. The predominance of non-tradable shares hindered market expansion, so during 2005-2006, the policy of holding NTS was gradually reversed through the conversion of NTS into TS.

### 2.1. The NTS Reform

In 2005, the government implemented a reform called Non-Tradable Shares (NTS) Reform or split shares reform. The reform was implemented gradually, starting with a pilot program in

which four companies participated on April 29th, 2005. In June of the same year, forty-two more companies joined the reform, followed by 35 large state-owned companies in November. On September 4th, 2006, the China Securities Regulatory Commission (CSRC) issued the "Administrative Measures on the Split Share Structure Reform in Listed Companies", which mandated that all A-share firms convert NTS into TS by the end of 2006. By December 2006, nearly all target firms had adopted the reform, fully or partially.

A sudden changeover could be advantageous for shareholders who cannot trade their shares since they would have more accessible assets. However, this could be unfavorable for shareholders who can trade their shares as they would have to deal with an excess of shares in the secondary market. To prevent this, the NTS reform mandated that the conversion of non-tradable shares would need approval from tradable shareholders. This created a negotiation between the two groups of shareholders to determine the compensation per share.

The negotiation was set up in a way where non-tradable shareholders suggested a compensation rate for each share to tradable shareholders. The tradable shareholders were then allowed to vote on the proposal, and a two-third approval from those who chose to vote was needed for it to be accepted. There was no minimum requirement for the number of voters, and since the tradable shares were widely dispersed among individuals, a few institutional investors (like mutual funds) could hold significant sway in deciding whether or not to accept the proposed compensation. This situation created the potential for agency problems, which are discussed in more detail in Section 4.

To prevent excessive sales of shares, non-tradable shareholders had to wait for a period of 12 months during which their shares could not be traded or transferred. After this initial lockup period, NTS holders owning more than 5% of the listed shares, were further restricted from trading more than 5% or 10% of the company's total shares within a 12 or 24-month period. Therefore, the timing of the conversion of NTS after the 2005 reform was considered exogenous because it was determined by this administrative rule and individual firms cannot influence it. By the end of 2006, 82% of target firms had adopted the reform partially.

NTS is defined as the sum of state shares and legal persons shares;[7] the rest is tradeable shares. Figure 1 presents trends in the Non-Tradable Shares (NTS) and Tradeable Shares (TS)

[7] Legal persons shares are the shares owned by Chinese domestic institutions with a legal personal status. There are four types of legal persons shares: (i) state owned and state controlled legal persons; (ii) collective enterprise legal persons; (iii) private enterprise legal persons; and (iv) institutional legal persons like mutual funds or security firms. Legal persons shares are not tradeable at stock exchanges but can be transferred to other institutions upon approval from the Securities Regulatory Commission and the Ministry of Finance; the latter highlights one possible way through which the state can still influence listed firms even after the NTS reform.

in panel A, state and legal persons shares in panel B, shares held by mutual funds and institutional investors in panel C, NTS conversion approval rate (panel D) and firm's political connection as measured by the presence of a current or past government employee on board (panel E) and Comparison of Tobin's Q among treatment & control firms (panel F) in our sample.

The average state-controlled NTS was above 60% at the start in 2000, which gradually fell to about 25% by 2013–2014 (panel A). At the same time the tradeable share percentage went up from below 40% to nearly 80% by the end of the sample period. In panel B, it is shown that although state shares declined gradually after 2005, they did not disappear completely during the sample period, indicating a trend towards partial privatization. Panel C summarizes the trends in shares held by mutual funds and institutional investors together in the sample. These shares increased steadily from 2002 to 2007, but the trend reversed thereafter until about 2012 and then stabilized, giving a mixed picture. Panel D shows the trend in the approval rate of NTS conversion.[8] It indicates that the approval meetings continued until 2009 in the sample. Panel E measures the likelihood of political connection by using a binary variable indicating if the CEO or any board member of the firm was or is a local government employee and highlights the presence of political connection in both groups though the average was surprisingly higher for FP firms. Finally, panel F presents the average values of Tobin's Q for both treated and control firms, before and after the implementation of the 2005 reform. These comparisons offer initial evidence supporting the existence of parallel trends between the two groups in the pre-reform period.

Figure 2 displays the schedule for the implementation of the NTS reform, which enables us to identify the periods before, during and after the reform. Although the reform was first introduced in 2005, all companies began to adopt it by early 2007, some only partially. As shown in panel D of Figure 1, this adoption continued until 2009.

### 2.2. Literature review

While the privatization programs of the 1990s was hailed as a successful process to boost performance and efficiency (e.g., Megginson *et al.* (1994)), more recent studies challenge this view (Wright (1999); Black *et al.* (2000)). The effect on employment, however, remains ambiguous: Megginson *et al.* (1994) and Boubakri and Cosset (1998) document significant increases in employment, while most of the remaining studies document significant

[8] Once the buyers and sellers of NTS agree on a compensation price, the conversion is approved. The approval rate indicates the rate at which NTS conversion was approved out of all NTS conversion negotiations.

employment reduction. These conflicting results could be due to differences in methodology, sample size, omitted factors, highlighting that privatization does not necessarily imply employment reductions in divested firms (Megginson and Sutter (2006)).

It takes a more intriguing dimension when one considers privatization in authoritarian regimes (e.g., see Fisman and Wang (2015); González *et al.* (2020)). Although successive rounds of privatization have failed to *fully* privatize firms in China, there is limited literature to understand what causes it or what are its impact. Li and Yamada (2015) has been an important recent exception, who builds on Cao *et al.* (1999) to argue that governments select some firms to control allowing others to fully privatize and they do so to balance economic efficiency and employment maximization essential for maintaining social stability. This means that there is a potential simultaneity between the government choice of a firm for intervention (SOE or fully private) and firm-level outcomes. To resolve this simultaneity, they use a Heckman type self-selection model to determine firm value and employment intensity after controlling for the selectivity bias arising from government's choice of SOE. Further estimation biases are likely in their study as they include potentially endogenous variables like operating income, leverage, risks to determine firm value and employment and also do not include firm fixed effects.

There is a relatively large and growing body of research that examines how China's NTS reform affects various outcomes including profits, productivity, employment, Tobin's Q, innovation. This research includes several studies such as Jiang *et al.* (2009); Firth *et al.* (2010); Chen *et al.* (2012); Tan *et al.* (2015); Campello *et al.* (2014); Liao *et al.* (2014); Bin *et al.* (2015).

While most of the existing studies do not explicitly discuss the role of persistent government control even after privatization (especially when privatization is partial), some of them imply this link and present conflicting results. For example, Jiang *et al.* (2009) report that SOEs that underwent share issue privatization (SIP) during 1999–2002 experienced negative profitability changes after the reform, but their decline was less severe than that of their matched non-SIP SOEs. Liao *et al.* (2014) classify SOEs into different levels of state-ownership and find that the NTS reform quickly boosted output, profits, and employment during 2005–2007, with SOEs having the highest level of state-ownership performing the best. However, they focus on SOEs that were forced to adopt the reform instantly after its *introduction*, without correcting for the result and selectivity bias. More importantly, they do not find any changes in firms' operating efficiency or corporate governance measures, which makes it difficult to explain the improved performance of these newly privatized SOEs. Using

a small, non-random pilot survey of firms Campello *et al.* (2014) show positive effects of the *introduction* of the 2005 reform on corporate profitability, investment, value, and productivity.

The current body of literature exploring the effectiveness of privatization is extensive and varied, and the results can differ depending on the methodology employed. The first set of studies compares the performance of newly privatized companies with those still owned by the government. Another approach involves comparing the performance of the same companies before and after privatization, as seen in Megginson *et al.* (1994), Megginson and Netter (2001), in order to minimize potential selection problems. Later studies aimed to address selection bias by using fixed-effects models that account for the fact that better-performing companies are more likely to be selected for privatization, as shown in Frydman *et al.* (1999).

2.2.1. Contributions

The study investigates what determines full versus partial privatization and whether markets perceive their differences and value them differently. This distinction helps separate political motivations from economic ones – an area that remains underexplored. In doing so, one needs to address the potential simultaneity between the government choice of a firm for intervention (SOE or fully private) and firm-level outcomes including performance. In doing so, we deviate from Li and Yamada (2015) who used a Heckman type self-selection model to determine firm value and employment intensity after controlling for the selectivity bias arising from government's choice of SOEs. Later studies aimed to address selection bias by using a fixed-effects model that accounts for the fact that better-performing companies are more likely to be selected for privatization earlier, as shown in Frydman *et al.* (1999). We add to this literature by using standard and staggered difference-in-differences models with firm fixed-effects. In this manner, we exploit the exogenous variation in NTS conversion after the gradual adoption of the 2005 reform to resolve the potential simultaneity between share issue privatization and the firm performance measures among treated partially and fully privatized firms, ceteris paribus. We argue that the difference-in-differences specification with firm fixed effects is a powerful alternative to the self-selection model or standard fixed effects model .

Following the NTS reform literature in China, we study the differential effects of privatization not only on various non-market performance indices like operating revenue, productivity, employment, but also on a number of market performance indicators, two measures of Tobin's Q and BHAR, among treated PP and FP firms after 2005 reform, highlighting the trade-off between the roles of authoritarian state and that of the market.

Many studies including Liao *et al.* (2014), Campello *et al.* (2014) use non-random

samples of SOEs that were forced to adopt the NTS reform instantly after its *introduction* in 2005. We resolve this issue by considering the impact of the *staggered adoption* of the 2005 reform among *all* listed non-financial and non-utility companies using data over a longer time horizon including years before, during and after the 2005 reform. Since the process of privatization was not instantaneous as documented in Figure 1, using this extended period helps us to better differentiate between FP and PP firms using standard as well as staggered difference-in-differences models.

Although China has undergone multiple waves of privatization since the late 1990s, state-affiliated private firms have continued to grow, as highlighted by Bai *et al.* (2021). At the same time, the country's stock market has lagged behind, largely due to institutional weaknesses (Allen *et al.* (2021)). These developments underscore the relevance of our analysis comparing the performance of partially privatized (PP) and fully privatized (FP) firms, where we find that PP firms consistently underperform relative to control groups even after privatization. Our findings contribute to and synthesize existing literature. While they differ from the results of Campello *et al.* (2014) and Liao *et al.* (2014), they align with research by Fan and Wong (2002), Fan *et al.* (2007), Fisman and Wang (2010), and Cao *et al.* (2019), who document enduring state influence in the form of politically connected CEOs, tunneling, related-party dealings, and the use of political promotions as a stand-in for managerial incentives in PP firms. Comparable patterns are observed in several Central and Eastern European countries such as Poland, Hungary, and the Czech Republic, where Aussenegg and Jelic (2007) attribute weak improvements in firm performance post-privatization to ongoing state ownership – similar to our findings.

In contrast, our findings differ from those of Gupta (2005), who analyzed partially privatized firms in democratic India and reported improvements in profitability, productivity, and investment following privatization. She attributed these gains to stronger stock market monitoring and enhanced managerial incentives. However, similar benefits were not observed among Chinese partially privatized (PP) firms, which experienced no significant gains – in fact, a decline was noted in Tobin's Q, revenue, and productivity. This underperformance is likely linked to the persistence of state influence, absence of value-maximizing motives and incentive structures, and limited stock liquidity, despite the privatization reforms introduced around 2005–2006.

### 2.3. Hypotheses development

China has been privatizing and promoting the formation of new companies since the early 1990s, but it has allowed many of its state-owned firms to continue operating. In this section, we develop the hypotheses with specific reference to the 2005 reform that initiated the share issue privatization by allowing conversion of non-tradeable shares into tradeable ones.

**First Hypothesis – Which firms are likely to privatise fully?**

In 1999, the Communist Party adopted the slogan "Grasp the Large, Let Go of the Small," which meant that smaller state-owned firms were to be sold or closed. Large state-owned firms were merged into industrial conglomerates, and the central or local governments consolidated control over these conglomerates. State-owned firms were transformed into limited liability corporations, and their managers were held responsible for performance, leading to the corporatization of state-owned firms to a limited extent. Li and Yamada (2015) had suggested that government tends to balance firm value maximization along with its political objective of ensuring higher employment to secure social stability after privatization. In doing so, only a minority of state-owned firms were, however, permitted to fully privatize, creating tension between the dominance of the government and market forces.

The degree of privatization after the 2005 reform may be influenced by local governments within a decentralized governance structure. Although the central government in China is under the tight grip of the Communist Party, the country had introduced local elections in the 1980s and 1990s to address local governance challenges that resulted in insufficient provision of local public goods (O'Brien and Li (2000); Martinez-Bravo *et al.* (2022)). More importantly, introduction of local elections was successful to establish a competitive decentralized governance mechanism in Chinese towns/villages and districts. With privatization, decentralized local governments took action to sever ownership ties between government and firms in a bid to promote fair competition and local efficiency. This in turn helped raising local tax revenues as financial assistance from the central government was cut after decentralization. Bortolotti and Faccio (2009) argue that if sub-national governments are given substantial authority, it can lead to complete privatization. Gan *et al.* (2018) further emphasize the role of decentralized local governance for implementing privatization in China. In particular, they have discussed the importance of privatization by sale to insiders, which arguably, transfers control rights to private owners (with limited government support), imposes hardened budget constraints, and achieves performance improvement. Jiang and Waley (2022)

note that local governments in China compete to attract residents and industries to create employment and have both financial incentives and monitoring abilities with a view to promote local tax revenue as well as local development. Lei (2021) documents that local governments that grant more favors to firms also receive more assistance from firms to raise the tax revenue, indicating the presence of a reciprocal relationship between firms and local governments. Huang *et al.* (2017) show that SOEs operating in a non-strategic sector are more likely to be decentralized by the local government if the physical distance from the central government is greater.

Taken together, we hypothesize:

*Hypothesis 1. Firms, within a given sector, with links to the decentralized local governments (as against those unconnected ones) are more likely to have a higher likelihood of full privatization after the adoption of the 2005 reform.*

**Impact of privatization on performance**

In 2005, the NTS reform was introduced to enhance the privatization process by transforming non-tradeable shares into tradeable ones. However, this was not fully implemented, and privatization of over 80% of sample firms remained incomplete. The conventional public finance literature considers public enterprises to be productive and able to solve issues of monopoly and externalities that private firms cause. However, an alternative theory suggests that public enterprises are inefficient as politicians use them to achieve their political objectives (Boycko *et al.* (1997)). They may hire more people than needed, operate in economically unsuitable locations, and underprice their products to gain votes or avoid unrest. Privatization is one way to make it more difficult for politicians to influence firms and reduce inefficiencies, but it may not completely eliminate them. Scholars suggest that privatization can only improve social welfare if it provides better managerial incentives than the public enterprise control system. This is because firm performance depends on managers' skills and efforts. The new post-privatization owners can improve efficiency by implementing incentives that align with profit maximization and/or by appointing new and better managers (Roland and Sekkat (2000)). Cragg and Dyck (1999) show that privatized firms in the UK increased the frequency of managerial replacements and introduced stronger incentives, but only after four years of privatization.

The benefits of privatization, which stem from companies aiming to maximize their values, can be reduced if the state still has some control or if private ownership is concentrated

rather than dispersed, according to Claessens *et al.* (2000); Claessens *et al.* (2002). Additionally, Ehrlich et al. (1994) argue that partial privatization does not lead to as much improvement in productivity growth as complete privatization. In China, Fan *et al.* (2007) find that firms with politically connected CEOs perform worse when partially privatized compared to those without. Chen *et al.* (2008) also note that the efficiency gain in performance is positive only when control is transferred to a private entity, rather than a state entity. This is because the state is unlikely to pursue firm value maximization, and radical improvements in management and governance are unlikely to occur if the state remains a controlling owner.

The impact of privatization on the performance of the market thus involves a balance between positive incentives and negative entrenchment effects that arise from the private benefits of control by the government, depending on whether the privatization is partial or full. Partially privatized firms with heavy government controls are likely to experience more negative entrenchment effects than positive incentive effects, resulting in small or even negative effects on their performance, and may be punished by the market for excessive entrenchment. On the other hand, fully privatized firms that have eliminated all or most government controls are likely to experience more positive incentive effects than negative entrenchment effects, resulting in a positive net effect on their performance. The differential performance of fully and partially privatized firms thus illustrates the trade-off between market power and state control after adoption of the 2005 reform.

Accordingly, we hypothesize:

*Hypothesis 2a: Share issue privatization may lower performance, especially market performance, when privatization is partial even after the adoption of the 2005 reform.*

*Hypothesis 2b: Share issue privatization may not lower performance, especially market performance, of fully privatized firms after the adoption of the 2005 reform.*

**Impact of privatization on private benefits of control, executive incentives & liquidity**

Finally, we explore the underlying mechanisms through which the conversion of NTS into tradeable shares (partly or fully), as part of the 2005 reform, may affect performance. Although there is literature available on how the government intervenes in Chinese firms, the connection between the process of privatization and the private benefits of control remains little explored. Jiang and Kim (2015) use data from the Statistical Yearbook of China to examine how investor composition in China has changed from 2005 to 2012, after the introduction of the NTS. During this period, the shares held by individuals, mutual funds, and insurance companies have

gradually declined, while ordinary institutional investors (including states and legal persons) and other institutional investors have increased. The trend towards private benefits of control has been reflected in the Chinese literature (Hsieh and Klenow (2009) and Song *et al.* (2011)), with reports of misallocation of resources favoring SOEs and other large-scale government projects. Additionally, there is bias in favor of domestic state firms wishing to engage in IPOs, who tend to underperform following IPOs, resulting in lower stock returns for domestically listed Chinese firms than externally listed ones during 2000–2018 (Allen *et al.* (2021)). There is also evidence of significant related party transactions (RPTs) among Chinese firms (Fisman and Wang (2010)), as well as the practice of tunnelling (Liu and Tian (2012)) by controlling shareholders. The compensation offered to tradeable shareholders is not just to compensate for the drop in price due to excess supply but also for the gain in risk sharing that benefits non-tradeable shareholders (Li *et al.* (2011)). Finally, Chang and Jin (2016) report that both direct and indirect ownership by the government negatively impacts firm performance, based on data from 2003–2013.

The problems of related party transactions or tunnelling can still occur even after privatization, especially if there is partial privatization, pyramidal ownership structure (see Fisman and Wang (2010)) or when firms and policy makers fail to implement necessary governance practices to meet the privatization's goals (Nestor (2005)). Evidence from privatized firms in the European telecom sector suggests that when firms remain partially privatized (as opposed to fully privatized), policy makers and firm executives should strive to reform corporate governance practices to meet the privatization's objectives (see Nestor (2005)). Failing to do so will result in unsuccessful (partial) privatization compared to full privatization, thus limiting the realization of the positive effects of privatization.

Accordingly, we hypothesize:

*Hypothesis 3a: Privatization may fail to eliminate private benefits of control if government intervention persists even after the adoption of the 2005 reform. This scenario is likely to be more explicit for PP rather than FP firms.*

Looking at the brighter side, privatization, whether done fully or partially, has the potential to create positive effects on the profitability, productivity, and investment of companies that are privatized and listed in the stock market. This is because the stock market can keep an eye on these companies and may offer rewards for good management performance through incentive schemes such as the managerial labor market or corporate control through takeover threats. However, this system of incentivizing executives only works if the company

is focused on maximizing its value. If the company does not prioritize value maximization, then the need for providing incentives disappears, and private benefits of control take over.

In theory, the efficiency of privatized companies can be increased in two ways: by introducing incentives that align with maximizing the value of the firm, or by bringing in new and more competent managers. However, in cases where the government's goals, such as maximizing employment, conflict with the goal of maximizing the value of the firm, these two methods may work against each other in partially privatized companies.

Accordingly, we hypothesize:

*Hypothesis 3b: Privatization may fail to enhance executive incentives if it does not align with maximizing firm value following the adoption of the 2005 reform; this scenario is more probable for PP firms than FP firms.*

The conversion of non-tradable shares into tradable equity represents a critical structural reform aimed at enhancing the efficiency of capital markets, particularly in transitioning or emerging economies like China. By increasing the proportion of shares available for public trading – referred to as the "free float" – this reform is expected to significantly lower information asymmetry, via more frequent trading and quicker adjustment of prices to new information.

Theoretically, higher stock liquidity can enhance firm performance through several mechanisms. Firstly, increased liquidity tends to lower the cost of equity by attracting a wider pool of investors and reducing the risk premium they demand (Amihud and Mendelson (1986); Saad and Samet (2017)), thereby improving a firm's ability to finance growth. Secondly, more liquid markets make it easier for smaller shareholders to acquire significant stakes and take on monitoring roles (Maug (1998)) and facilitate strategic ownership accumulation (Kyle and Vila (1991)). Additionally, liquidity boosts trading by informed investors, enhancing price informativeness and investment efficiency (Khanna and Sonti (2004)). Third, greater liquidity may support more efficient executive compensation structures (Holmström and Tirole (1993)), and limit managerial self-interest (Edmans (2009); Admati and Pfleiderer (2009)). As a result, higher stock liquidity reinforces corporate governance, increases analyst coverage, and helps curb agency problems.

Empirically, higher liquidity improves the ability of prices to reflect underlying fundamentals and therefore lowers information asymmetry. According to Fang *et al.* (2009), firms with more liquid stocks reduce adverse selection and lead to higher firm valuation, as measured by Tobin's Q. The underlying idea is that firms with more liquid stocks benefit from

a more informed investor base, which reduces adverse selection and improves capital allocation and hence better market performance.

These liquidity-related differences are expected to be particularly pronounced among fully privatized (FP) firms, which, unlike partially privatized (PP) firms, no longer maintain ties to the state and rely more heavily on capital markets for discipline and financing. This greater exposure to investor scrutiny and market discipline provides stronger incentives for transparency and information disclosure. As a result, the NTS reform is more likely to reduce information asymmetry in FP firms. Accordingly, we hypothesize:

*H3c: The NTS reform increases liquidity and reduces information asymmetry, particularly for fully privatized firms.*

# 3. Data and Empirical Methodology

### 3.1. Data description

We construct a sample of all non-financial and non-utility publicly listed Chinese firms over 2000–2016, using the China Stock Market & Accounting Research (CSMAR) database (see Appendix Table A1 for a summary of the variables used in our analysis).[9] Since most existing studies (e.g., Liao *et al.* (2014)) focused on the immediate aftermath of the 2005 reform, we constructed our sample to include years before, during and after the adoption of the reform. We extended the analysis to 2016 to capture any long-term trends in the share of tradeable shares. President Xi Jinping came to power in 2012 though by the end of his first five-year term, Xi had consolidated more personal power than Jiang or Hu ever did. He broke tradition by not appointing a successor at the Nineteenth Party Congress in October 2017. In March 2018, the National People's Congress amended the state constitution to abolish the two-term limit for the presidency – an indication that Xi intended to remain in power beyond 2023 (Shirk (2018)). These political changes are likely to affect the process of privatization as has been evidenced from a crackdown on some of the country's most successful private firms. This is why we chose to focus on the period until 2016.

We construct a sample of all non-financial and non-utility publicly listed Chinese firms for this period. State-controlled firms in priority sectors, which were exempt from the 2005 NTS reform, are excluded to avoid compromising the integrity of the sample. Our full sample comprises 27,748 firm-year observations. We exclude 1,680 firm-year observations pertaining to financial and utility firms, resulting in a base sample of 26,068 observations. Of these,

[9] All non-binary variables are winsorized at 1% level to minimize the problems of outliers.

22,110 firm-year observations correspond to public-to-private (PP) firms, and 3,958 to foreign-to-private (FP) firms. The estimation sample without fixed effects includes 15,057 observations. The further reduction in sample size in fixed-effects regressions reflects the exclusion of observations with insufficient within-group variation or perfect collinearity arising from the joint inclusion of firm and sector-year fixed effects. The final estimation sample, which includes all regression variables used in Table 4, consists of 11,572 PP firm-year observations and 2,722 FP firm-year observations. Li (2005) reported that the SOE management received instructions from the government to speed up the sale of non-tradable shares, resulting in the sale of shares to a limited number of prominent private investors rather than many small investors. As a result, private ownership became more concentrated rather than dispersed after the 2005 reform. Despite the adoption of the 2005 reform, the government continued to be actively involved in the market.

Figure 1 was constructed using a sample of Chinese firms that are listed, which illustrates important trends in selected shares within the sample, as discussed in Section 2.2. As the reform was implemented after 2005, state shares among treated firms began to decline, as expected (as shown in Figure 1, panel B). The legal persons shares also saw a sharp decline starting from 2007, but their averages never reached zero. This suggests that state control persisted among newly privatized firms even after 2005. Only about 18% of the sample firms were completely privatized after the reform, indicating that over 80% remained partially privatized. This differentiation between partially and fully privatized firms is important for our analysis; even fully privatized firms were not completely free from government control (as further discussed in Section 3.2.1).

### 3.2. Methodology

#### 3.2.1. Test of Hypothesis 1 – Driver of full privatization

In 2005, the non-tradeable shares reform was initiated to dismantle the dual share structure by converting non-tradable shares into tradable shares. The reform effectively removed the legal and technical obstacles of transferring state-owned shares to public investors, opening up the gate to China's secondary privatization, which, in contrast to the initial share issue privatization (SIP), would further liberalize state-owned shares in full circulation (Liao *et al.* (2014)). In this respect, a distinction between PP and FP firms is central to Hypothesis 1. An FP firm $i$ is defined as one that has eliminated both state and legal persons shares in year $t$, $t \geq 2005$. A PP firm instead is one that has eliminated some, but not all state and legal persons shares in year

*t*, t ≥ 2005; as such PP firms would be unable to tie the grabbing hands of the state fully.

***Insert Table 1 about here***

Table 1 provides an overview of the key features of partially (PP) and fully privatized (FP) firms in the estimation sample, which, in turn, highlights their mean differences, using *t*-tests. On average, FP firms are larger[10] and older than PP firms. FP firms do not have any legal entity shares and are less affected by private benefits of control measures such as tunnelling and related party transactions. Nearly 46% of PP firms have connections to local government officials; either the CEO or a board member was or is currently employed by the local government. In comparison, 55% of FP firms had such local political connections.

Our initial inquiry is focused on determining the factors that drive the degree of privatization in our sample measured by a binary variable called "*FP*" as per Hypothesis 1. If a firm is fully privatized, *FP* takes on a value of one, while it remains at zero for partial privatization. Hypothesis 1 contends that the key driver of privatization is a dummy variable of a local political connection labelled as *Connected*, which takes the value of one if the firm is connected with the local government and zero otherwise. A firm is defined to be connected with the local government if the CEO or any board member of the firm has been a current or previous employee of the local government. Hypothesis 1 suggests that the firm's connection with the local government may increase the probability of full privatization, all else being equal. Further we interact firm's connection with the local government with the *Reform2007* binary variable: the variable takes a value 1 if year >= 2007; it is zero otherwise. The interaction term accounts for any differential effect of local political connection after the adoption of the 2005 reform after 2006 (when all firms adopt the reform, even if partially) on the likelihood of full privatization. Accordingly, we exploit the variation in local government connection among firms in a given sector to estimate the likelihood of full privatization $FP_{ijt}$ for *i*-th firm operating in the *j*-th sector in year *t* as follows:

$$FP_{ijt} = \gamma_0 + \gamma_1 Connected_{ijt} + \gamma_2 Reform07 + \gamma_2 Connected_{ijt} \times Reform07 + \gamma_X X_{it-1} + \phi_j + \phi_j * \tau_t + u_{it} \quad (1)$$

If a firm is not in a priority sector identified by the central government, such as defense, banking and insurance, minerals and metals, mobile technology, or railway construction and engineering, a local government may permit the firm to undergo full privatization. In order to

[10] Note that these FP firms were not bigger at the beginning of privatization but grew bigger afterwards so that they are bigger on average over the sample period.

account for the role of specific industry/sector, we include the sector fixed effects $\phi_j$ *in Equation* (1). As such, Equation (1) considers the within sector variation in the likelihood of full (as opposed to partial) privatization among connected and unconnected firms in our sample. We refrain from using firm fixed effects here as we want to compare the full privatization likelihood between connected and unconnected firms within a sector. Inclusion of firm fixed effects would instead focus on the limited within firm variation in full privatization likelihood over time, weakening the comparison between connected and unconnected firms within a sector. We also include Sector×Year fixed effects as in $\phi_j \times \tau_t$, which accounts for the sector level time-varying unobserved factors. We use cluster-robust standard errors, which are clustered at the firm level, thus correcting for any correlations across observations within a firm.

We find that about 55% of fully privatized (as against 46% of partially privatized firms) were locally connected on an average in our estimation sample (see Table 1) and the mean difference is statistically significant with a *t*-statistic of 8.58. In other words, fully privatized firms are significantly more likely to be connected with the local government in our sample. The latter may give rise to the possible endogeneity concern of the key explanatory variable indicating a firm's connection with the local government.

To allay this endogeneity concern, we have included a number of lagged control variables *X* such as firm size, firm age, the proportion of intangible fixed assets, Tobin's Q, log(employment) of the firm, which could also influence the likelihood of full privatization. The rationale for including these controls is derived from the existing literature, e.g., see Cao *et al.* (1999), Li and Yamada (2015), Huang *et al.* (2017), among others. First, we took the natural logarithm of the total assets as our measure of firm size to explore the role of firm size on the likelihood of full privatization. Second, we include firm's age measured in years (Anderson and Reeb (2003)); it may indicate the nature of its ownership or governance quality in the Chinese context. In particular, older firms are more likely to have been state-controlled in the past, which may still affect their privatization likelihood. Further we included intangibility measured as intangible assets divided by total assets, Tobin's Q and also size of employment to assess their respective roles on the full privatization likelihood. Given that the Chinese government identified some priority sectors where firms were not allowed to fully privatize, we also include sector dummies $\phi_j$ in Equation (1) that accounts for sector-specific time-invariant unobserved factors. Nevertheless, there may remain some unobserved time-varying sector-specific factors, which may still bias our estimates. Accordingly, we further

include Sector×Year dummies to account for sector specific time-varying unobserved factors influencing the likelihood of full privatization in an alternative specification. Inclusion of these controls would minimize any estimation biases due to omitted time-varying firm-level characteristics. Finally, we conduct impact threshold analysis to show that unobserved confounding factors are unlikely to influence the effect of local government connection on the likelihood of full privatization (see Section 4.4.6).

#### 3.2.2. Test of Hypotheses 2a–2b and 3a–3c – Privatization impact on performance and underlying mechanisms

We exploit the variation in exogenous timing of the NTS conversion due to the administrative lock up rule to define the treatment/control groups and use a difference-in-differences (DiD) framework to test Hypotheses 2a–2b (impact on market performance) and 3a–3c (possible mechanisms). This allows us to assess the impact of the adoption (rather than the introduction) of the 2005 reform.

In this paper, we suggest that using the DiD method is an improvement compared to the generalized propensity score matching method (PSM) used in Campello *et al.* (2014) study. The firms with NTS had the option to adopt the reform until the end of 2006, but Campello *et al.* (2014) only used a small pilot survey of firms that adopted the NTS reform early in 2005, which introduced selectivity bias in the sampling. Furthermore, Campello *et al.* (2014) employed a generalized PSM method to account for trends in outcomes, but their impact estimates of the reform introduction could be biased for several other reasons. Firstly, although they controlled for the time trend in outcomes, there may still be unobserved firm-specific factors influencing the outcomes. Secondly, the comparability of the treatment and control groups in PSM is based on selected *observable* firm characteristics that may fail to consider the firm-level unobserved effects that determine why a firm initiated the NTS earlier than others. Lastly, PSM estimates are likely to be very sensitive to changes in selected firm characteristics that determine the PSM and the covariate balancing condition too, especially for small samples.

Instead, we analyze all non-financial and non-utility firms using a DiD model that takes into account of firm and Sector×Year fixed effects too. By virtue of the administrative lock-up rule (see Section 2), we take the timing of the NTS conversion as exogenously given because it was beyond the control of individual firms; this helps us to identify the treatment and control groups of firms within this natural experimental setting around the adoption of the 2005 reform. We shall explain this in more detail below.

Treatment and control firms

We start by defining the treatment and control firms in our sample. The NTS reform was introduced in 2005 and all listed firms with non-tradeable shares (NTS) were required to start adopting the reform by the end of 2006. For our purpose, NTS is defined as the sum of state shares and state-owned legal persons shares.[11] As per the regulation, we define a firm as *treated* if it had held non-tradable shares and had also initiated the process of NTS conversion since the introduction of the 2005 reform; it is zero for control group firms. In particular, the control group includes firms that had zero NTS at the time of introduction of the reform. We have eliminated the state firms with NTS, who were exempted from the reform. Inclusion of these exempted firms would have contaminated our sample otherwise.[12]

Note, however, that treated firms with NTS could not convert these into tradeable shares instantly. Even after finding buyers, administrative lock up rule required that the seller of the NTS had to wait for a year before the transaction could be completed in a bid to avoid over-sale of NTS. This explains the staggered adoption of the NTS conversion under strict government monitoring as documented in panel A and panel D of Figure 1. Since individual firms could not influence the timing of these NTS conversions, we consider the treatment to be exogenously given. Our identification strategy, therefore, relies on the fact that the control group is not exposed to the treatment after 2005 though the outcomes of treatment and control firms were identical prior to 2005.[13] Accordingly, we compare the outcomes of treated and control firms after the adoption of the 2005 reform in a bid to identify the effect of NTS conversion. About 70% of sample firms are classified as `Treated',[14] the remaining 30% are therefore the control firms who did not hold any NTS in our sample. Among the treated firms, 61% were SOEs and the rest non-SOEs, both holding NTS. Control firms were primarily small private firms.

***Insert Table 2 about here***

Table 2 summarizes the means and standard deviations of all regression variables for the estimation sample as well as those separately for the treated and control firms. On average, a higher proportion of treated firms tends to suffer from greater control than cash flow rights,

[11] Later we shall relax this assumption to test if our baseline results hold when NTS is defined as state shares only. See Section 4.4.4.

[12] Note that there was no firm that had zero NTS before 2006 and then had NTS after 2007. Later we also experiment with alternative treatments as Liao *et al.* (2014).

[13] We provide supporting evidence in this respect in Section 4.2.

[14] A firm labelled treated would remain treated throughout the sample. However, new firms are being added as treated in the post-2005 years. In order to address the staggered nature of the reform adoption, we shall also consider the staggered treatment effects in Section 4.4.5.

tunnelling as well as related party transactions; this is then reflected in lower performance indices, both Tobin's Q measures, of treated relative to control firms. It also shows the *t*-tests of mean comparisons of selected characteristics between treated and control firms, which highlight significant mean differences not only in selected outcomes but also in some observed control characteristics including firm size, age, tax shield, and Herfindahl index.

Campello *et al.* (2014) indicate that using the 2005 pilot survey to analyze the impact of share issue privatization is not ideal because listed firms had some degree of discretion about the timing of program compliance before the end of 2006. This induces us to treat 2007 as the first year when all targeted firms had adopted the reform, even if partially, thus allowing us to generate a cleaner effect of the reform adoption (see Figure 1, panel D). Accordingly, we develop a natural experimental framework to compare the outcomes of interest *Y* between treatment and control firms before and after 2007 within a DiD framework. In doing so, we construct a second binary variable *Reform2007* that takes the value of one if year ≥ 2007 and zero otherwise. Later we shall also compare the robustness of our baseline estimates using alternative reform adoption dates between 2005–09.

A difference-in-differences (DiD) model

To test Hypotheses 2a–2b and 3a–3c, we employ a DiD model to determine the outcomes of interest $Y_{it}$ (performance or private benefits of control) of the *i*-th firm in year *t* with firm fixed effects (FE). Firm fixed effects account for the firm-level unobserved factors that may also influence the outcomes of interest.

This is represented by Equation (2) below:

$$Y_{it} = \beta_1 + \beta_N Treated_{it} + \beta_{CG} Reform2007 + \beta_{NCG} Treated_{it} \times Reform2007 + \beta_X X_{it-1} + \phi_i + \beta_3 Sector \times Year_t + \varepsilon_{it} \quad (2)$$

In Equation (2) firm fixed effects are represented by $\phi_i$, which accounts for the firm-level time-invariant factors influencing outcomes; this eliminates the possibility of selection bias, e.g., if a larger firm is selected for privatization. Year fixed effects are subsumed in Reform2007 variable here. We also include the Sector×Year fixed effects to account for unobserved sector level time-varying changes that may also influence the outcomes of interest. These sector-level time trends would account for the effects of other unobserved regulatory changes, for example, in our sample that may also affect selected firm performance. We argue that Equation (2) estimates with firm and Sector×Year fixed effects are rigorous because these minimize any estimation bias arising from inclusion of unobserved firm-level as well as sector-

level time trends, in addition to other lagged covariates. In contrast, a propensity score matching method (PSM) that makes treatment and control firms comparable on the basis of some observable characteristics cannot take account of unobserved firm characteristics and also imposes additional restrictions for the balancing of covariates. Note that our identification is linked to the exogeneity of the timing of the treatment adoption and not on the balancing of covariates between treated and control groups as required by the PSM method. As such we contend that DiD method as in Equation (2) remains our preferred model.

Outcome variables for Hypothesis 2a, 2b: Following previous literature (e.g., Wernerfelt and Montgomery (1988); Lang and Stulz (1994); Beiner *et al.* (2011)); Fang *et al.* (2009), we use Tobin's Q as the key measure of market performance. Tobin's Q is the most common measure of firm's market performance. Tobin's Q is a ratio comparing a public company's market value to its book (or total asset) value. The *market value* refers to the amount a firm is worth on the market (by multiplying shares by the going market share price), while the *book value* refers to the collective value of a company's net assets (less depreciation, debt, etc.). Tobin's Q is viewed as a snapshot of a firm's financial performance: how much would it cost to *replace* a company's total assets vs. how much it would get for selling all of its shares at the current going price. Benefits of using Tobin's Q ratio include enabling comparison of firms regardless of size or industry and predicting investment decisions. Panel F of Figure 1 shows the mean comparisons of Tobin's Q between all treated and control firms before and after the introduction of the 2005 reform. This provides the first confirmation of the existence of parallel trends between treated and control firms in the years prior to the 2005 reform. The trend, however, reversed after 2005.[15]

In our analysis, Tobin's Q is proxied by market value of equity plus total debt divided by book value of total assets as per Jensen (2005) and Moeller *et al.* (2005). We also supplement this measure of Tobin's Q with other alternative market performance measures. Following Bhagat *et al.* (2005), we construct TQ_BDHN which is defined as the sum of market capital, total debt and preferred stock as a share of total assets. Since both these measures are based on market valuation of the firm, we take them to reflect market responses to firm performance. Notably, TQ_BDHN is less susceptible to some of the criticisms recently highlighted in the literature (e.g., Bartlett and Partnoy (2020); Bendle and Butt (2018)). By incorporating total debt and preferred stock in the numerator, it better captures the full market value of the firm, not just equity, and reduces sensitivity to variations in the capital structure.

[15] We perform a more formal placebo test of parallel trends in Section 4.2.

While both measures rely on book value in the denominator, this measure offers a more comprehensive proxy that mitigates distortions from unrecorded intangible assets or leverage mismatches, which are particularly relevant in emerging markets such as China.

Non-market performance indices: In addition, we consider a number of other non-market performance indices used in the existing studies. Since the introduction of the new Chinese GAAP in January 2007 that revised how investment profit and other income are recorded, we consider alternative firm performance and efficiency measures compatible in pre-/post-2007 years. These include: (i) CPI-adjusted operating revenue; (ii) CPI-adjusted operating revenue as a share of the number of employees as the first productivity measure. (iii) A second productivity measure is also considered, which is CPI-adjusted operating profit as a share of number of employees. These non-market performance indices are examined with a view to assessing the comparability of our estimates with the existing literature.

Outcome variables for Hypothesis 3a, 3b, 3c: To test the validity of Hypothesis 3a, we estimate Equation (2) using three key measures of private benefits of control: (i) *control exceeds cash flow share* (CEC): this is a direct measure of private benefits of control captured by a dummy variable CEC that takes the value of one when the control rights of the controlling owner exceeds their cashflow rights. The underlying argument is that greater control rights are likely to be socially sub-optimal as it could lower shareholders' value (e.g., see Claessens *et al.* (2000)). (ii) *Tunnelling*: Tunnelling is illegal appropriation of assets of a company by large controlling shareholders with legal disguises. In the absence of better measures, we follow the literature to measure tunnelling by net other receivables scaled by total assets that reflects how a firm may manage to receive resources from its subsidiaries in various ways. (iii) *Related party transactions* (RPT): It is a transaction between two connected parties and may involve conflict of interests between controlling and minority shareholders. This is measured by related party transactions in loans, mortgages and asset transactions as a share of total related party transactions. Additionally, we consider the impact of privatization on employment, executive incentive and executive turnover (definitions of these variables are given in Appendix Table A1) among PP and FP firms to test the validity of Hypothesis 3b.

Finally, we construct two measures of information asymmetry for testing Hypothesis 3c. Our first measure relates to the average for the year of the daily ratio of absolute return to the dollar volume of stock, as per Amihud (2002). We calculate the ratio of current to one year lagged value of daily ratio as the relevant measure. It can be interpreted as the daily price response associated with one dollar of trading volume, thus serving as a rough measure of price impact. Alternatively, it can be interpreted as a volatility-to-liquidity ratio, which offers

insights into trading efficiency, information asymmetry, and market quality. It indicates how much the price moves per dollar traded. A higher (lower) daily ratio implies illiquid (liquid) stock. A stock with a high ratio may: be thinly traded, attract fewer institutional investors, have higher expected returns (to compensate for illiquidity) and be more volatile due to fewer market participants.

Our second measure pertains to the turnover ratio, the average for the year of the stock daily ratio of its trading volume to the number of shares outstanding, as per Amihud (2002). As before, we calculate the ratio of current to one year lagged value of turnover ratio as the relevant measure. A higher turnover ratio implies that the stock is more liquid, making it easier for investors to buy or sell without significantly affecting the price. It reduces transaction costs and improves price efficiency, making the stock more attractive to institutional investors. High turnover is often associated with lower information asymmetry, as frequent trading can reflect the rapid incorporation of information into stock prices. Conversely, low turnover may indicate insider dominance or limited public information, making prices less responsive to news. Higher turnover may increase monitoring pressure from external investors, promoting better governance and potentially improving firm performance. It can also discourage managerial entrenchment due to more active ownership dynamics. Firms with higher turnover often trade at higher valuations due to perceived liquidity premiums. These firms may find it easier to raise capital through equity markets.

Control variables: To avoid the potential bias caused by omitted variables, we referred to previous studies (such as Coles *et al.* (2008); Frank and Goyal (2009)) that guide our choice of the additional control variables $X$ in Equation (2). First, the size of a firm is an important determinant of its performance, which we measured using the book value of total assets, as previously done by Anderson and Reeb (2003) and Bhattacharya and Graham (2007). In particular, we took the natural logarithm of the total assets as our measure of firm size in this analysis. The size of a firm is an important determinant of its performance, which we measured using the book value of total assets, as previously done by Anderson and Reeb (2003) and Bhattacharya and Graham (2007). In particular, we took the natural logarithm of the total assets as our measure of firm size in this analysis. Firm size is considered an important predictor of firm performance for several reasons. Larger firms often benefit from economies of scale (Ambrose *et al.* (2019)), meaning they can: (i) produce at lower average costs due to bulk buying, specialization, or more efficient operations; (ii) spread fixed costs (like R&D or infrastructure) over a larger output base and (iii) achieve greater operational efficiency,

improving profit margins and return on assets. Larger firms typically have easier access to capital markets (equity or debt) at lower costs, higher credit ratings, which reduce borrowing costs (Rajan and Zingales (1995)) and also better ability to absorb economic shocks and invest in long-term projects. Bigger firms may usually have more market power, brand recognition and customer loyalty.

Another factor that may influence firm performance is a firm's age measured in years (Anderson and Reeb (2003)); it may indicate the nature of its ownership or governance quality, especially in the Chinese context. To account for the presence of state-owned enterprises (SOE), we included a dummy variable that takes a value of one when the state holds more than 50% of total shares in the firms. Further, older firms are more likely to have been state-controlled in the past, which may still affect their performance despite the drop in the state's cash flow rights following the NTS reform. In contrast, newer firms are less likely to be state-controlled and therefore may not suffer from control rights expropriation. Furthermore, larger Chinese firms are more likely to be state-owned and may suffer from inefficiencies as noted by Megginson and Netter (2001).

We also included intangibility (measured as intangible assets divided by total assets) and tax shield (measured as depreciation as a share of total assets) as additional control variables. In particular, businesses that make effective use of their intangible assets can boost growth and obtain a competitive advantage (Lev (2000)). Second, tax shields are used to increase cash flows and to further increase the value of a business. It may also boost performance by lowering the cost of capital and also by making equity investments more attractive (Norbäck *et al.* (2018)). Additionally, since there are two Chinese stock exchanges, we included a dummy variable equal to one for the Shenzhen stock exchange, with the reference being the Shanghai stock exchange. These two exchanges cater to different groups of firms, with many companies listed in Shenzhen being subsidiaries of firms in which the Chinese government maintains controlling interest. In contrast, high-tech and financial/insurance firms tend to dominate the Shanghai stock exchange. All explanatory variables are lagged by one year to minimize potential simultaneity bias. To correct for autocorrelation in errors for a given firm over time, we cluster standard errors within firms; *t*-statistics are based on robust standard errors, where observations are clustered at the firm level.

In addition to the set of $X$ variables discussed above, we include the firm and Sector×Year fixed effects. Note that sector dummies are subsumed in firm fixed effects when estimating Equation (2). Sector×Year dummies account for the unobserved sector-level time-varying factors, e.g., accounting for other sector-level regulatory changes over time, that may

also influence the outcomes of interest. Note, however, the year dummies on their own are subsumed in the *Reform2007* dummy. Table 2 shows that the treated and control firms vary significantly in terms of firm size, age, tax shield and Herfindahl index. There may be other unobserved differences too. Thus controlling for these observed firm-level time-varying characteristics, firm-specific time-invariant and sector-specific time-varying unobserved characteristics, we make the treated and control firms more comparable and focus on the differential effects of the privatization reform on selected outcomes *Y* of treated (relative to control firms) after the adoption of the reform.

In each case, the differential effects of the adoption of the NTS reform on any outcome measure *Y* would be captured by the estimated coefficient $\beta_{NCG}$ of the interaction term: $Treated_i \times Reform2007$ and this is the key coefficient of interest for both Hypotheses 2a, 2b and 3a, 3b, 3c. With respect to market performance indices vis-à-vis Hypothesis 2a–2b, we expect $\beta_{NCG} < 0$ for cases of partial privatization. This means a treated (relative to a control) PP firm is expected to experience a lower performance after the adoption of the reform after the end of 2006. In contrast, we expect $\beta_{NCG} >= 0$ for cases of full privatization. An estimate of $\beta_{NCG} = 0$ would indicate that the NTS reform would fail to have any significant effect on any performance index among treated FP firms if positive incentive effects arising from privatization exactly outweigh the negative entrenchment effects arising from any private benefits of control. With respect to Hypothesis 3a–3b, $\beta_{NCG} > 0$ would indicate positive private benefits of control among the treated privatized firms after 2006 and these effects are likely to be more pronounced for PP (relative to FP) privatization. Taken together, greater private benefits of control (relative to positive incentive effects) of privatization would result in negative performance effects. Finally, we expect $\beta_{NCG} <0$ for daily ratio and $\beta_{NCG} > 0$ for turnover ratio if the reform enhances market liquidity and lowers information asymmetry and this effect is likely to be pronounced for FP rather than PP firms. Using the administrative lock up rule introduced as part of the 2005 reform, we take the timing of the treatment as exogenously given because individual firms could not influence it. As such, we interpret the estimates of the interaction terms as the causal effect of the adoption of the NTS reform among treated (relative to control) firms, *ceteris paribus*.

# 4. Empirical findings

In this section, we present and analyze our key results to test the validity of Hypotheses 1–3.

### 4.1. Test of Hypothesis 1: Likelihood of full privatization

Following Section 3.2.1, Table 3 shows the estimates of whether a firm is fully privatized or not, as per Equation (1). The dependent variable is a binary variable FP that takes a value one for full privatization and zero for partial privatization. Column (1) shows the estimates of the likelihood of full privatization for all firms, column (3) shows those for the treated firms only. Columns (2) and (4) include additional Sector×Year fixed effects, which are our preferred estimates. All regressions include industry fixed effects and all explanatory variables are lagged by one year to reduce any simultaneity bias; all standard errors are clustered at the firm-level.

***Insert Table 3 about here***

Evidently, these estimates are quite comparable across the columns. For brevity, we focus on the estimates of Connected×Reform2007 shown in columns (2) and (4) as the key driver of the likelihood of full privatization respectively among *treated* firms. Note that the connected variable on its own is statistically insignificant, but that interacted with *Reform2007* variable is positive and statistically significant (except in column (4)). Ceteris paribus, locally connected (as against unconnected) firms are significantly more likely to be fully privatized in our sample after the adoption of the reform in 2006, thus supporting the validity of Hypothesis 1. Ceteris paribus, a locally connected firm is about 4.2 percentage points more likely to be fully privatized compared to an unconnected firm in a given sector and this effect is statistically significant (see column (2)). In Section 4.4.6, we use the impact threshold analysis to allay any endogeneity concern of the interaction coefficient. Among other results, larger and older firms within a given sector are more likely to be fully privatized while better performing firms with higher Tobin's Q are less likely to be so.

The question is why locally connected firms in a sector are more likely to be fully privatized in a decentralized set-up. As per Hypothesis 1, local governments would encourage better performing firms to relocate in local rural/urban regions and allow them to privatize fully, which in turn boost local government's tax revenue. To test this, we first compare Tobin's Q and firm's employment (in natural logarithm) and explore if connected firms have significantly higher Tobin's Q and employment than the unconnected ones. On average, Tobin's Q is significantly higher and log(employment) is significantly lower among firms connected to local governments in our sample, indicating their better market performance and lower employment. In other words, connected firms rely more on firm value maximization (and less on employment maximization, which may lower firm value) as they privatize fully. Second, we compare taxes paid by fully (as against partially) privatized firms. In addition to

corporate income taxes, we consider various taxes including value added tax, consumption tax, sales tax, city maintenance and construction tax, resource tax, land appreciation tax, land value added tax and vessel use tax, compensation for mineral resources etc. While some of these taxes, e.g., corporate income taxes are charged by the central government, some others like value added tax (including land value added tax), city maintenance and construction taxes are charged by the local government. So we compare the average taxes payable net of corporate income tax between fully and partially privatized firms. Evidently, the mean taxes net of income taxes are ¥109 million and ¥46.4 million respectively for fully and partially privatized firms after 2006 in our sample. Also the mean difference is statistically significant with a *t*-statistic 25.22 so that the fully privatized firms contribute significantly more to local tax revenues than the partially privatized firms. The latter may explain why local governments and fully privatized firms may align together for their mutual benefits: fully privatized firms maximize firm value and local governments, faced with hard budget constraints, maximize tax revenue.

### 4.2. Test of Hypotheses 2a–2b: Estimates of market performance

We use Table 4 market performance estimates to test the validity of Hypothesis 2a–2b respectively for partially and fully privatized treated firms within a DiD model.

The validity of the DiD estimates crucially depends on the prevalence of parallel trends in key performance outcomes between treatment and control groups before the 2005 introduction of the NTS reform. Satisfaction of the parallel trend requires that the treated and control firms had very similar performance outcomes before 2005.

We start with the traditional tests of parallel trends for both PP and FP firms in models with firm FEs – these estimates are provided in the top panel of Appendix Table A2. Evidently, the interaction terms remained statistically insignificant in the pre-reform years for both measures of Tobin's Q among PP firms, indicating the validity of the parallel trends assumption for Tobin's Q; in other words, Tobin's Q of treated and control PP firms were not significantly different before the 2005 reform. For the FP firms, however, the interaction terms Treated×Year drop for 2001, 2002, 2003 and 2004 because there were only very few observations in the years prior to 2004. The interaction terms are positive and statistically significant for determining Tobin's Q from 2005 onwards. We cannot, therefore, confirm the parallel trends validity for the Tobin's Q measures among FP firms. We therefore consider an alternative placebo test for parallel trends. In particular, we consider a test of parallel trend as

per Higgins *et al.* (2021).[16] To do this, we take our pre-trend data sample (2000–2004) and split it in half, defining the midpoint 2002 as an imaginary treatment event date. We then estimate our difference-in-differences specification using this imaginary treatment date Post2002 using Treated, Post2002, Treated×Post2002 along with firm and year fixed effects for the pre-2007 years. The underlying idea is to see if similar effects emerge prior to the 2007 when the reform was adopted by all treated firms. These estimates are summarized in the bottom panel of Appendix Table A2. Since the estimated coefficient of Treated×Post2002 is statistically insignificant for both measures of Tobin's Q in the pre-2007 sample, we take it as evidence of parallel trends in both measures of Tobin's Q among both PP and FP firms in the pre-2007 years.

Having established the parallel trends in key market performance outcomes between treated and control firms in the pre-2005 years, we now consider the market performance estimates using Equation (2).

***Insert Table 4 about here***

Table 4 shows the DiD estimates of these market performance estimates, namely, Tobin's Q and Tobin's Q BDHN (TQ_BDHN). Columns (1) and (3) show the estimates for PP firms while columns (2) and (4) show those for FP firms. Ceteris paribus, we focus on the estimates of the interaction term Treated×Reform2007 that reflects the treatment effects of the adoption of the reform among treated PP firms after 2007. There is no evidence of significant positive performance effects among the partially privatized PP firms. Evidently, the coefficient estimates of Treated×Reform2007 are negative and statistically significant for both the Tobin's Q measures in columns (1) and (3). In other words, partially privatized treated (relative to control) firms had experienced a significantly lower market performance index after the adoption of NTS reform in our sample, indicating an adverse response from market participants, thus supporting Hypothesis 2a.[17] The estimated coefficient of Treated×Reform2007 is −0.23 so that the Tobin's Q is lower by 0.23 (relative to sample mean of 2.53) among treated (relative to control) firms after 2006. This result is economically significant, suggesting a drop of around 9.17% (−0.2321/2.53) of the average firm's market performance in our sample. The corresponding treatment effect for Tobin's Q BDHN is comparable to that for Tobin's Q. Negative interaction coefficients for determining market

[16] We thank an anonymous referee for pointing out this test.
[17] We obtain very similar results even when we split the PP firms into SOEs and non-SOEs. For brevity, we do not include them here, but these results are available on request.

performance among PP firms indicate that the negative entrenchment effect outweigh the positive incentive effect significantly of privatization after adoption of NTS reform in our sample so that the total treatment effect is negative, thus supporting Hypothesis 2a.

We next consider the FE estimates of market performance indices among the fully privatized firms. In this case, the coefficient estimate of Treated×Reform2007 is positive and statistically significant for both measures of Tobin's Q though only at 10% level. The estimated coefficient of Treated×Reform2007 is 0.79 for determining Tobin's Q so that the treated FP firms have 0.79 higher Tobin's Q after 2006 relative to control firms. This result is also economically significant, as FP firms increase in value by around 31% compared to the average firm's Tobin's Q in our sample. The latter highlights the significance of the positive incentive effects (in excess of the negative entrenchment effects) after privatization among treated FP firms in our sample, thus supporting Hypothesis 2b. Positive interaction coefficient for determining market performance among FP firms indicate that the positive incentive effect significantly outweigh the negative entrenchment effect of privatization after adoption of NTS reform in our sample so that the total treatment effect turns out to be positive.

These results are broadly consistent with prior literature. Among other results, the coefficients of firm size and age are both negative and statistically significant, indicating that larger and older firms have lower Tobin's Q for both PP and FP firms (Anderson and Reeb (2003)). In addition, depreciation has a significant negative effect on Tobin's Q among PP firms only; the corresponding effect is positive, but statistically insignificant for FP firms. Adjusting for depreciation helps compare market performance across firms with different asset structures. Also the Herfindahl index that accounts for industry concentration and hence the level of competition, is negative and significant only in column (1) for PP firms, but statistically insignificant in other columns. This result is consistent with Giroud and Mueller (2011), who show that firm value suffers more from weak governance in less competitive (more concentrated) industries. Their results imply that product market competition and governance act as substitutes in discipling managers, and in our context, the discipling role of competition is particularly important for PP firms, where governance frictions are likely more severe.

Finally, we construct the z-scores for comparing the treatment effects between PP and FP firms. Let $\beta_1$ and $\beta_2$ be the estimated coefficients of Treated×Reform2007 for PP and FP firms respectively. We also suppose that their corresponding standard errors are $SE(\beta_1)$ and $SE(\beta_2)$. We can then construct the z-score as follows: $Z=(\beta_1-\beta_2)/\sqrt{(SE(\beta_1))^2 + (SE(\beta_2))^2}$ as per Clogg *et al.* (1995). The last row of Table 4 shows these z-scores, which are both statistically significant, indicating significant difference in the treatment effects of adoption of NTS reform

between PP and FP firms in our sample and this holds for both measures of Tobin's Q.

**Treatment effects for non-market performance indices**

Table 5 shows the DiD estimates of additional non-market performance indices used in the literature as per Equation (2): ln(Operating Revenue) (column (1)), productivity 1 (column (2)) and productivity 2 (column (3)) for both partially (top panel) and fully privatized (bottom panel) firms. Productivity 1 is measured by operating revenue as a share of number of employees, while productivity 2 is measured by the operating profit as a share of number of employees. In each case, we use the natural logarithm of the productivity measures to allow for non-linearity, if any.

We start by considering the test results for parallel trends. Appendix Table A3 shows the tests for parallel trend test for selected non-market performance indices – top panel shows the traditional tests and the bottom panel shows the placebo tests. Traditional tests work for only PP firms as all the pre-2005 interaction terms remain statistically insignificant for operating revenue, productivity 1 and productivity 2 measures. However, the interaction terms drop for the pre-2005 years and hence we cannot conclude about the presence/absence of parallel trends for the FP firms using the traditional tests. Hence, we consider the alternative placebo test. Evidently, the interaction term Treated×Post2002 is statistically insignificant for operating revenue and productivity 1 measure for PP firms; for FP firms, however, the coefficient of Treated×Post2002 turns out to be significant for the productivity measures. Accordingly, we infer that the parallel trend assumption holds for operating revenue and productivity 1 measure for PP firms while it holds only for operating revenue for FP firms. Hence, we need to interpret the difference-in-differences estimates for one or both productivity measures for PP and FP firms with some caution.

***Insert Table 5 about here***

Considering the PP firms (top panel), the estimated interaction term Treated×Reform2007 is negative but statistically insignificant for all three indices. As per our hypotheses 2a and 2b, treatment effects via the 2005 reform reflect the aggregate of positive incentive effects and negative entrenchment effects of privatization. Statistical insignificance of the treatment effects for PP firms in the post-2007 years, therefore, highlights that the positive incentive effects exactly outweigh the negative entrenchment effects so that the total effect of privatization via NTS is essentially zero among PP firms. In other words, there is no evidence that the treated partially privatized firms had significantly improved any of these

performance indices relative to control firms.

Estimates for the fully privatized firms, however, contrast with the case of partially privatized firms. In particular, the estimated coefficients of the interaction term Treated×Reform2007 are all positive and statistically significant for operating revenue as well as for the two productivity measures. Specifically, operating revenue and productivity measures are respectively 0.39, 0.90, 0.88 higher among treated (relative to control) FP firms after the adoption of the reform by late 2006. These results indicate some performance premium among fully privatized treated (relative to control) firms after 2006, though we should treat the estimates of productivity measures with some caution because the parallel trend assumption does not hold in these cases. We can still conclude that the fully privatized treated (relative to control) firms not only enjoyed improved market performance but also improved operating revenue after adopting the NTS reform from 2007 onwards.[18]

Finally, we compare the treatment effects between treated PP and FP firms using z-scores. Statistical significance of these z-scores indicates significant difference in treatment effects between PP and FP firms for these non-market performance measures as well.

### 4.3. Test of Hypothesis 3a–3c

#### 4.3.1. Presence of private benefits of control

What explains the differential performance of PP and FP firms observed in Section 4.2? Hypotheses 3a–3c offer some explanations. We first test the validity of Hypothesis 3a, i.e., the differential impact of the adoption of the NTS reform on measures of private benefits of control among PP and FP firms. Appendix Table A4 shows the test of parallel trends for selected private benefits of control indices using both traditional and placebo tests. Panel A shows the estimates of traditional tests for private benefits of control indices. Columns (1)–(3) show estimates for PP firms while, columns (4)–(6) show those for FP firms. Columns (1) and (4) show estimates of Control Exceeds Cash (CEC), columns (2) and (5) show estimates of tunnelling, and columns (3) and (6) show those for Related Party Transactions (RPT). For PP firms, the interaction terms Treated×Year are statistically significant for years 2003–2004 for determining CEC, but all the relevant interaction terms are statistically insignificant for tunnelling and RPT in the pre-2005 years. This suggests that the parallel trends assumption holds for tunneling and RPT, but not for CEC among PP firms. For FP firms, however, the

[18] We also ran regressions for all performance indices (Tobin's Q and others) among both PP and FP firms with additional control for lagged liquidity measured by the share of cash and cash equivalents in total assets. These results support the baseline estimates and are available on request.

interaction terms drop for the pre-2005 years, meaning we are unable to draw any conclusion about the validity of parallel trends for FP firms using traditional tests. Panel B shows the corresponding placebo tests following Higgins *et al.* (2021) for the pre-2007 years. The statistical insignificance of Treated×Post2002 supports the validity of parallel trends for tunnelling and RPT among both PP and FP firms in the pre-2007 years. As before, parallel trends do not hold for CEC for both PP and FP firms. As such, we need to interpret the DiD estimates of CEC with some caution.

***Insert Table 6 about here***

Table 6 shows the estimates of selected private benefits of control measures in our sample after including firm fixed effects for partially (top panel) and fully (bottom panel) privatized firms as per Equation (2). Controlling for all other factors, the estimated interaction coefficients Treated×Reform2007 are positive and statistically significant for control exceeds cash (CEC in column (1)) and tunnelling (column (2)) for PP firms (top panel). Thus, treated PP firms (relative to control firms) not only have greater control than cash flow (CEC) rights, but also suffer from tunnelling after the adoption of the reform. Since the parallel trend assumption does not hold for CEC, we do not attach much importance to the statistical significance of the interaction for determining CEC among PP firms. Statistical insignificance of RPT, however, reflects that treated firms refrain from any significant related party transactions after the adoption of the NTS reform though there is evidence of persistent tunnelling and CEC among PP firms.

Although FP firms have eliminated non-tradeable shares, they do not appear to be totally free from all types of private benefits control either. While the interaction estimates of Treated×Reform2007 for CEC (column (1)) and RPT (column (3)) are positive, they remain statistically insignificant indicating absence of CEC and RPT practice among treated FP firms after adoption of NTS. The corresponding estimated interaction coefficient Treated×Reform2007 of tunnelling is still positive and statistically significant; the latter suggests the presence of tunnelling among treated (relative to control) fully privatized firms even after adoption of the NTS reform. This suggests that there have been some changes in private benefits of control in treated firms after the adoption of the NTS reform. While treated PP firms are more likely to experience CEC and tunnelling, treated FP firms are more likely to experience tunnelling only, after 2006. However, neither treated PP and FP firms are likely to engage in RPT after the adoption of the NTS reform. Thus privatization was only partially

successful to curtail private benefits of control after NTS adoption, especially among FP firms that eliminated all state shares.

Tunnelling is an illegal process of transferring out assets and profits from a subsidiary company for the benefit of the parent company, which is an indirect method for expropriation of minority shareholders by the controlling shareholders. Tunnelling is often hidden under legal disguises and hence is difficult to be detected/penalized by the market. As such, tunnelling needs to be punished by the government. But the very presence of tunnelling among treated FP firms indicates that elimination of NTS is no guarantee for the elimination of all private benefits of control in China.

Thus, we interpret these results as evidence that privatization was only partially successful to curtail private benefits of control after NTS adoption, even among FP firms that eliminated all state shares.[19] Nevertheless, presence of tunnelling as a measure of private benefit of control remains more implicit among FP rather than PP firms (that experienced both CEC and tunnelling), supporting Hypothesis 3a. Further, z-scores are statistically insignificant, indicating that there is no significant difference in any private benefits of control measure between PP and FP firms in our sample.

4.3.2. Test of Hypothesis 3b: Effects on employment, executive compensation and turnover

Effects of privatization on employment

In order to test the validity of Hypothesis 3b, we first test whether the firm adopts the firm value maximization principle. To this end, Table 7 shows the firm FE estimates of log employment (natural logarithm of number of firm employees) for PP and FP firms as per Equation (2).[20] Column (1) shows these estimates for partially privatized firms while column (2) shows those for the fully privatized firms. The estimated interaction coefficient Treated×Reform2007 remains statistically insignificant in column (1) for PP firms indicating that the partially privatized treated and control firms did not have significantly different level of employment after the adoption of the 2005 reform.

***Insert Table 7 about here***

Considering the fully privatized firms, the estimated coefficient of

[19] We also show the z-scores for comparing the interaction coefficients for determining the measures of private benefits of control between PP and FP firms. None of the z-scores are statistically significant indicating that we cannot reject the null hypothesis of similarity of the interaction coefficients between PP and FP firms for any measure of private benefits of control.

[20] Placebo test results summarized in Appendix Table A5 shows that parallel trends hold for FP firms, but not for the PP firms. Traditional tests too suggest that the Treated×Year2003 is statistically significant for PP firms – so we need to interpret the estimated coefficient of Treated×Reform2007 with some caution.

Treated×Reform2007 is not only negative but also statistically significant in this case, thus indicating that treated FP firms had significantly downsized employment (relative to control firms) after 2006. The latter, in turn, highlights the emphasis on firm value maximization among FP rather than PP firms, which may be one important factor triggering a favorable market response for FP rather than PP firms. This is further supported by the statistically significant z-score indicating that the employment effect has been statistically different between PP and FP firms, after the adoption of the 2005 reform. We further test the effect of the reform on executive incentives and turnover to infer about the validity of firm value maximization principle.

Effects of privatization on executive incentives and turnover

Executive incentives may work through the appointment of new and better managers, thus resulting in managerial turnover. Using CSMAR, we obtain information on average equity incentives offered to top executives in the listed Chinese firms in our sample. This information is available from 2006 onwards though less than 10% of sample firms seem to offer it. Raw data shows that treated partially (as against fully) privatized firms had paid significantly lower (6% as opposed to 17% for control firms) executive incentives. We construct executive incentive as a dummy variable taking the value of one if the firm in question offers equity incentives to its top executives (CEO, board Chairman and General Manager) and zero otherwise. Executive turnover is a dummy variable that takes the value of one, if anyone of the top three executives changes in a year in a firm and zero otherwise. Raw CSMAR data shows that only about 20% sample firms had experienced managerial turnover that involved turnover of CEO, Chairman or General Manager. Turnover was 5% points higher in treated firms (the likelihood being 21% as opposed 16% in control firms).

Below we consider the estimates of executive incentives and executive turnover as per Equation (2) to see if the above univariate results hold, after controlling for all other factors. Considering both traditional and placebo tests (see Appendix Table A5), parallel trends hold for executive incentives and executive turnover among PP firms. For FP firms, however, traditional tests do not work as before. So we rely on the placebo tests, which indicate parallel trends hold for executive incentives only. So we need to consider the estimates of executive turnover among FP firms cautiously.

***Insert Table 8 about here***

Table 8 shows the firm FE estimates of executive incentives and executive turnover among PP and FP firms as per Equation (2). In this respect, we consider the estimates with Sector×Year fixed effects as we focus on inter-firm variation in executive incentives and turnover within a sector. As before, we focus on the estimated coefficient of the interaction term Treated×Reform2007: our results indicate a significant drop in executive incentives, while executive turnover remains statistically insignificant among treated partially privatized firms after adoption of the 2005 reform. In other words, treated partially privatized firms that did not alter employment after 2005 (a la Table 7) had curtailed executive incentives contrary to an expectation of boosting executive incentives after 2006; the latter is compatible with the absence of firm value maximization in these PP firms. No such reduction is, however, observed for fully privatized firms that lowered employment after 2005, thus adhering to the principle of firm value maximization and cut employment.[21] Z-score for the comparison of the treatment effect between PP and FP firms is −1.43 which is associated with a one-tail significance level of 7.64% (less than 10%). In other words, we consider the z-score to be nearly significant. Considered together with the employment reductions among FP firms only reported in Table 7, this asymmetry is, to an extent, consistent with a shift toward value maximization in FP firms only. In other words, after the NTS, treated PP firms lowered executive incentives without reducing employment (Table 7), indicating limited alignment with value maximization. In contrast, treated FP firms retained executive compensation while cutting employment, reflecting a clearer commitment to value maximization following the reform. This asymmetry aligns with Hypothesis 3b.

4.3.3. Effect of privatization on liquidity and information asymmetry

To test Hypothesis 3c, we constructed two measures to assess information asymmetry changes in the market following the reform. Both measures are based on the approach in Amihud (2002). Our first measure is the *daily ratio*, defined as the ratio of the firm's current to one-year lagged illiquidity ratio, where illiquidity is measured as the firm's daily absolute return divided by its trading volume. A higher (lower) value of the daily ratio implies illiquid (liquid) stock, potentially indicating thin trading, fewer institutional investors, and higher expected returns due to illiquidity premia. Our second measure is the *turnover ratio*, also defined the ratio of current to one-year lagged turnover, where turnover is calculated as the firm's daily trading volume divided by its number of shares outstanding. Unlike the daily ratio, a higher

[21] It is possible that the likelihood of political promotion acts as a substitute for a lack of CEO incentives in China (Cao *et al.* (2019)).

turnover ratio suggests improved liquidity, making it easier for investors to buy or sell without significantly affecting prices.

Tests of parallel trends using traditional tests are not feasible for FP firms due to the small number of treated observations before 2007, which causes many interaction terms to drop. Instead, we rely on placebo tests using a pseudo-reform year (see Appendix Table A5), which support the parallel trends assumption for the daily ratio in both PP and FP firms.

***Insert Table 9 about here***

Table 9 shows the DiD estimates for the two liquidity measures across PP (columns (1) and (3)) and FP (columns (2) and (4)) firms, using the same specification as Table 4. The interaction term *Treated* × *Reform2007* is statistically insignificant for both measures in the PP sample, suggesting no significant change in liquidity post-reform. In contrast, among FP firms, the same interaction is statistically significant in the predicted direction. This implies that treated FP firms experienced both a decline in illiquidity (via a lower daily ratio) and an improvement in trading activity (via a higher turnover ratio) following the reform – consistent with reduced information asymmetry for FP firms.

Overall, the results in columns (2) and (4) confirm that only treated FP firms benefited from greater stock market liquidity after the reform. This additional evidence supports Hypothesis H3c and complements our earlier findings on market valuation and private benefits of control.

### 4.4. Robustness tests

In this section, we perform a battery of robustness tests to assess the validity of our results. Our tests included alternative reform dates to the one used in the main analysis, cross-province heterogeneity that accounts for differences across provinces over time, alternative definitions of partial privatization using different levels for tradable shares, alternative definition of the treatment group using only non-tradable state shares, a staggered event study that extends the static difference-in-differences model we use in the main analysis, an impact threshold test for unobserved confounding factors, alternative definitions of full privatization using cases with very small non-tradeable shares retained even after privatization, and other measures of firm performance.

#### 4.4.1. Alternative reform dates

We test the robustness of our estimates using alternative reform dates. As Figure 2 shows, the NTS reform was introduced in 2005 and all listed Chinese firms with NTS gradually started

adopting it by the end of 2006. So far, we have used the year 2007 as the first year of *adoption* of the reform irrespective of whether it was adopted partially or fully. We experiment with alternative reform dates starting from year 2004 to year 2008. To this end, we generate the following dummy variables:

**Reform2004** is a dummy that takes the value of one for years ≥ 2004 and zero otherwise.
**Reform2005** is a dummy that takes the value of one for years ≥ 2005 and zero otherwise.
**Reform2006** is a dummy that takes the value of one for years ≥ 2006 and zero otherwise.
**Reform2008** is a dummy that takes the value of one for years ≥ 2008 and zero otherwise.

***Insert Table 10 about here***

Table 10 shows the DiD estimates of the interaction terms with firm FEs which replace Reform2007 variable respectively by Reform2004, Reform2005, Reform2006, and Reform2008 (see rows (1)–(4)) using alternative reform dates variables defined above. Columns (1)–(2) respectively show the firm FE estimates of Tobin's Q and Tobin's Q BDHN for partially privatized firms. Columns (3)–(4) show the corresponding estimates with firm fixed effects for FP firms.

The year 2004 was one year before the announcement of the reform and hence it is a false reform year. The estimated coefficients of Treated×Reform2004 in row (1) remain insignificant in both columns among PP firms; so there is no evidence that the false reform year 2004 had generated any statistically significant effect on any measure of Tobin's Q like the year 2007 (as per Table 4) in our sample.

We also examine if the years prior to 2007, namely, 2005 and 2006, had any significant effects on the performance of partially privatized firms. Evidently, the interaction coefficients Treated×Reform2005 (see row (2)) are statistically insignificant for both measures of Tobin's Q. The interaction terms, however, turn statistically significant for 2006 onwards when all firms with NTS adopted the reform – in this case, both coefficients are negative, but significant for Tobin's Q BDHN measure in column (2) only. Further similar effects for Treated×Reform2008 in row (4), thus indicating the persistence of the negative market response among partially privatized firms in our sample, after all firms with NTS adopted the reform. This can be attributed to the fact that the processing cost of information is much higher for the market until everyone adopts the reform (Blankespoor (2019)), even if partially.

Columns (3)–(4) of Table 10 shows the corresponding FE treatment effects estimates of Tobin's Q and Tobin's Q BDHN for FP firms. We were unable to obtain the estimates for 2004, as the interaction term dropped here because of very few observations. The estimated

interaction coefficients remain statistically insignificant for 2005, 2006, and 2008 indicating that the positive incentive effects exactly offset the negative entrenchment effects of privatization for these years. Treatment effects are positive statistically significant only for 2007 among FP firms as shown in Table 4.

Taken together, there is evidence of differential market performance of PP and FP firms and these differential effects among treated firms are most pronounced after the adoption of the 2005 reform – while treatment effects are negative for PP firms, it is zero or positive for FP firms.

4.4.2. Accounting for cross-province heterogeneity

We further test the robustness of Table 4 estimates. Since Chinese provinces are sufficiently heterogenous, we re-estimate Equation (2) by including both Sector×Year fixed effects and Province×Year fixed effects. These estimates shown in Appendix Table A6 generally confirm the validity of Table 4 estimates. As before, the treatment effects are negative and statistically significant for PP firms. Absolute size of the estimated treatment effect coefficients is somewhat bigger (−0.3061 in Table A6 as opposed to −0.2321 in Table 4 for Tobin's Q). The corresponding effects for FP firms remain statistically insignificant, indicating that the positive incentive effects exactly offset the negative entrenchment effects of privatization after the adoption of the NTS reform so that the total effect of privatization turns out to be insignificant for FP firms in this case.

4.4.3. Alternative definitions of partial privatization

Focusing on PP firms, we create alternative treatment measures by identifying those with at least 30% and at least 50% of tradeable shares. This allows us to explore whether there is a threshold of tradeable shareholding at which PP firms begin to exhibit behaviour similar to FP firms. Accordingly, we generate two binary variables: (i) Treatedge30 = 1 if the treated firms have at least 30% tradeable shares and 0 otherwise. (ii) Treatedge50 = 1 if the treated firms have at least 50% tradeable shares and 0 otherwise. These estimates are summarized in Appendix Table A7. Evidently, the estimated coefficients of Treatedge30×Reform2007 are still negative and statistically significant, indicating the persistence of negative entrenchment effects (over and above the positive incentive effects) even after the adoption of 2005 privatization reform when these treated firms have at least 30% tradeable shares. However, the estimated coefficients of Treated50×Reform2007 are still negative though now statistically insignificant, indicating that the negative entrenchment effects exactly offset the positive

incentive effects when the treated firms have at least 50% tradeable shares after the adoption of the 2005 reform. In other words, PP firms with at least 50% tradeable shares behave more like FP firms in our sample.

#### 4.4.4. Alternative treatment definition

So far we have followed the existing literature (e.g., see Xiao (2015)) to define non-tradable shares as the sum of state shares and also state-owned legal persons shares. Average state-owned legal persons shares is around 12% of all shares on average (see Table 2). The corresponding proportion of state-owned shares is around 15% on average in our sample. These shares were not tradable in stock markets until the 2005 reform. For robustness, we relax this definition of non-tradeable shares and generate an alternative treatment variable $Treated_{State}$ as the firms with state shares only from year 2005 onwards. We rerun Tables 4 and 5 using this new treatment variable labelled $Treated_{State}$. These estimates are shown in Tables A8–A9 of the Appendix that confirm the validity of the estimates shown in Tables 4–5. There is confirmation that all our baseline results using *Treated* hold with this alternative treatment variable $Treated_{State}$.

#### 4.4.5. Staggered treatment effects estimates

Figure 1 (panel A) indicates that the sample firms did not fully convert all NTS in one go, but they did so in a staggered way over time with varying treatment times for treated firms. Hence, we follow Goodman-Bacon (2021), among others, to extend the standard difference-in-differences model into a staggered one.

Goodman-Bacon (2021) shows that the estimate of the treatment interaction dummy $\beta_{NCG}$ in Equation (2) will be a weighted average of all possible 2×2 traditional DiD estimators (which is a two-period two-group experiment where one group is treated in the 2nd period), taking account of early and late treatments. Therefore, using Bacon decomposition in Stata, we take account of the weights attached to early and late treatments over time in our sample to obtain the weighted average of the difference-in-differences treatment effect estimate as −0.40 for PP firms. In other words, a treated (relative to control) PP firm is expected to experience a lower Tobin's Q, on average, which is higher (in absolute value) than the standard difference in difference estimate (around −0.23) obtained in Table 4 as per Equation (2). We note similar trend for Tobin's Q BDHN where the weighted average of the treatment effect turns out to be

−0.41; the corresponding standard difference-in-differences estimate in Table 4 is −0.22 for PP firms.[22]

Taken together, we conclude that the size of the treatment effect is overestimated in standard difference-in-differences model as per Equation (2). Nevertheless, the sign of the treatment effect remains the same for PP firms irrespective of whether we use standard or staggered difference-in-differences, again highlighting the robustness of our estimates.

4.4.6. Impact threshold test for unobserved confounding factors

Regression coefficients cannot be interpreted as causal if the relationship can be attributed to an omitted confounding factor. For example, treatment (full or partial privatization) may be driven by whether a firm is connected to the local government or not (see Section 3.2.1 for example). While we observe it, we do not include it as a control in any of our baseline regressions in Section 3.2.1 or Section 3.2.2 because of its potential endogeneity with the outcomes of interest. As such, estimates of Equations (1) and (2) could be biased. Hence, in this section, we assess the robustness of a significant statistical inference to the inclusion of a potentially confounding variable in our analyses.

We follow Frank (2000) and Frank *et al.* (2013), who had proposed a method for testing the robustness of causal inference using the impact threshold of a confounding variable (ITCV). The threshold defines the point at which evidence from a study would make one indifferent to the adoption of the reform. As such, the threshold represents the effect size where the benefits of the adoption of the reform outweigh its costs. The more the estimate exceeds the threshold, the more robust the inference is with respect to that threshold. The ITCV analysis (see Appendix 2 for further details) thus enables one to determine how strong the effect of a hypothetical confounding variable would have to be to overturn current inferences.

For each potentially confounding variable, we calculate the percentage of observations that has to be biased in order for endogeneity to invalidate the inference. First, we ran the ITCV analysis for the key explanatory variable, namely, the firm's link with the local government because of its potential endogeneity to determining the likelihood of fully privatization as per Equation (1) as summarized in Table 3. Focusing on our preferred estimates of the interaction term Connected×Reform2007 shown in columns (2) and (4) of Table 3, we obtained the following threshold estimates: (i) column (1): to invalidate the inference, 56.52% (i.e., 9,063) of the cases would have to be replaced with cases for which there is an effect of zero. (ii)

[22] We were unable to obtain the corresponding estimates for FP firms because of the small sample size.

column (2): to invalidate the inference, 45.31% (i.e., 4,781) of the cases would have to be replaced with cases for which there is an effect of zero. Column (3): to invalidate the inference, 29.18% (i.e., 2,687) of the cases would have to be replaced with cases for which there is an effect of zero. Column (4): to invalidate the inference, 25.28% (i.e., 2,310) of the cases would have to be replaced with cases for which there is an effect of zero. These high thresholds, all in excess of 10% observations, required for invalidating columns (1)–(4) estimates of Table 3 regarding firm's connection with the local government after 2006 would allay concerns for endogeneity arising from the presence of a confounding variable.

Next, we do the same for the key explanatory variable, namely, Treated×Reform2007, in Equation (2), which could potentially be confounding especially if the treatment is driven by some unobserved factors that may cause a spurious association between the outcome and the treatment. For partially privatized firms, we found that the treated firms had significantly lower Tobin's Q after the adoption of the NTS (as per Table 4). The ITCV analysis indicates that to invalidate this inference, 27.35% (i.e., 3,034) of the cases would have to be replaced with cases for which there is an effect of zero. For fully privatized firms, we obtained a significantly positive effect on Tobin's Q among treated firms after the adoption of the NTS reform (see Table 6). The sensitivity analysis using ITCV indicates that to invalidate this inference, 28.07% (i.e., 724) of the cases would have to be replaced with cases for which there is an effect of zero. Since both the threshold estimates are sizeable (in excess of 10%), these estimates from an ITCV analysis further validate the robustness of our baseline inferences as per Equation (2).

4.4.7. Alternative definitions of full privatization

Further, we test the robustness of the impact of full privatization on two Tobin's Q measures if very small state shares are retained even after nearly full privatization (see Appendix Table A10). In particular, we consider 0.5% (columns (1)–(2)), 1% (columns (3)–(4)) and 5% (columns (5)–(6)) state shares in alternative scenarios for full privatization. Evidently, there is confirmation that the baseline FP impact on Tobin's Q holds when there is respectively 0.5% and 1% state ownership even after the 2005 reform. The corresponding effects for Tobin's Q_BDHN though positive, remain statistically insignificant in these cases. The same effect, however, fails to hold when the post-privatization state ownership increases to 5%, indicating presence of significant state intervention in these cases.

4.4.8. Financial performance based on BHARs

To evaluate the market response to the NTS reform, we have relied on market-based measures that capture both investor expectations and realized outcomes. Our primary metric is Tobin's Q, which is widely used in the corporate finance and privatization literature as a forward-looking measure of firm value relative to the replacement cost of assets. As discussed in Section 3.2.2, we employ two variants: a standard formulation based on market equity and debt, and a more comprehensive version (TQ_BDHN) that includes preferred stock (Bhagat *et al.* (2005)). To address potential concerns about measurement error in valuation-based proxies and to assess actual investor outcomes, we complement our analysis with buy-and-hold abnormal returns (BHARs). Following Barber and Lyon (1996), BHARs are well suited for long-run performance evaluation and provide stronger specification properties in settings where reform effects may materialize gradually.

We construct two BHAR measures: a 12-month BHAR following the reform (BHAR12), and a rolling 12-month BHAR (BHAR_tobin12), both benchmarked against the CSMAR market index with dividend reinvestment. These measures reflect realized market reactions to the reform and provide an ex-post complement to Tobin's Q.

In unreported summary statistics, we compare the mean BHARs between treated and control firms across the FP and PP groups. Treated FP firms earn significantly higher BHAR12 than their controls, whereas the difference for PP firms is statistically insignificant. For BHAR_tobin12, treated PP firms exhibit significantly lower returns, consistent with market skepticism toward partial privatization. These results mirror the patterns observed using Tobin's Q and are consistent with our hypotheses regarding the differential investor response to full versus partial privatization.

We further assess robustness using multivariate regressions of BHAR_tobin12 on the same covariates as in Table 4. These results, reported in Appendix Table A11, confirm that treated FP firms earn significantly higher abnormal returns, while treated PP firms do not experience statistically significant effects. The inclusion of Sector×Year fixed effects reduces the magnitude and significance of the coefficients, but the overall directional consistency across specifications reinforces the validity of the results.

Finally, our approach is consistent with established practice in the privatization literature, which advocates for the use of multiple, complementary performance metrics to ensure robust inference (e.g., D'souza and Megginson (1999); Jelic *et al.* (2003); Jelic and Briston (2003)). By reporting consistent findings across two Tobin's Q specifications and two BHAR measures, and by explicitly addressing the limitations of each, we reduce the risk of measurement-driven bias and enhance the credibility of our conclusions.

# 5. Conclusions

With President Xi Jinping consolidating his hold on China's ruling party and economy during the twentieth Party Congress in October 2022, it is timely to re-examine whether state authority can tame the influence of the market. We address this question by analyzing a comprehensive sample of all non-financial and non-utility listed firms in China from 2000 to 2016. We identify the factors determining the likelihood of full privatization, and also how the market responds to the performance of fully (FP) and partially (PP) privatized firms during this period. A distinction between FP and PP firms is important here as it allows us to assess the interplay between the market mechanism and the central government's command in the privatization process in an authoritarian regime.

We take advantage of the exogenous variation in the timing of conversion of the non-tradeable shares (NTS) after the 2005 NTS reform by virtue of the administrative lock-up rule and employ a difference-in-differences approach with firm fixed effects. This allows us to eliminate any simultaneity between the treatment and the outcomes. We also eliminate the influence of unobserved confounding factors in several ways to obtain the causal effects of the *adoption,* instead of its *introduction* on selected market and non-market performance indices.

We find that local government connections increased the likelihood of full privatization, driven by better-performing firms that generated higher local taxes. Crucially, only FP firms benefited from improved market performance, productivity, and liquidity after the reform. In contrast, PP firms experienced significant declines in Tobin's Q, consistent with persistent benefits of control, lack of value-maximizing behavior, and no improvement in stock liquidity. Evidently, the impact of China's 2005 privatization reform on market performance differs from the experiences of other economies as reported by Megginson and Netter (2001) and Gupta (2005), thus challenging the Chinese model of private sector development.

In recent years, there has been a growing concern that the Chinese government is intensifying its crackdown on some of the country's most successful private firms, including tech giants like Alibaba and Tencent. This trend has raised serious doubts about the long-term sustainability of China's authoritarian model of private sector development. The government's increasing intervention in the operations of these firms – through regulatory tightening, antitrust investigations, and even the suppression of prominent business leaders – signals a shift away from the relatively liberalized environment that fueled China's rapid economic growth over the past few decades. This has created an atmosphere of uncertainty, not just for domestic

entrepreneurs but also for foreign investors, who are reevaluating the risks associated with operating in China.

Our research sheds light on the inherent challenges within this authoritarian approach, particularly in the context of partial privatization. While partial privatization allows the state to maintain a degree of control among firms in the strategic sectors, it often leads to conflicting objectives between political oversight and market efficiency. Firms operating under this model struggle to maximize shareholder value due to persistent government interference, favoritism, and the prioritization of political goals (e.g., employment generation) over economic performance (e.g., firm value maximization). This dynamic undermines investor confidence, stifles innovation, and ultimately hampers sustainable growth.

Moreover, the implications of our findings extend beyond China. Many countries, especially those with authoritarian or hybrid political systems, are grappling with similar tensions as they attempt to balance state control with the benefits of market-driven growth. The Chinese case serves as both a cautionary tale and a valuable case study for policymakers worldwide. It highlights the risks of maintaining tight government control over partially privatized firms and underscores the importance of creating an environment where businesses can operate with greater autonomy, transparency, and accountability.

Ultimately, our research emphasizes that while state involvement can offer stability and strategic direction, excessive interference can distort markets, limit competition, and erode the very foundations of economic dynamism. For countries seeking to foster private sector development within non-democratic frameworks, the Chinese experience raises critical questions about how to achieve this balance effectively. A more effective strategy would involve the government concentrating on establishing impartial, transparent, rule-based institutions conducive to market growth, rather than actively engaging as a participant in the market.

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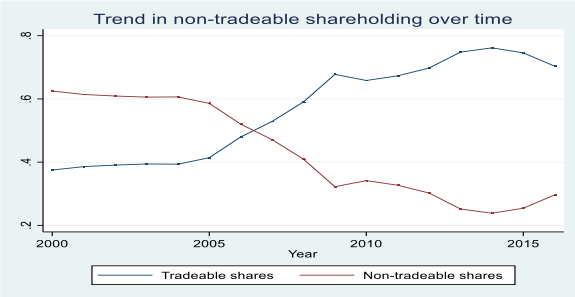
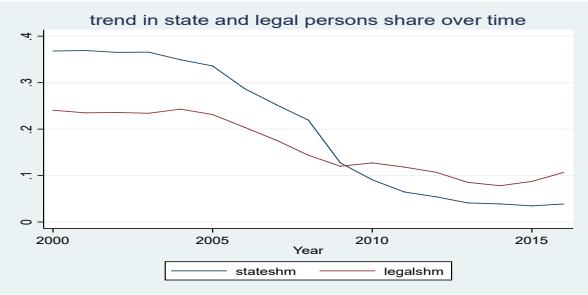
**Figure 1.** Trends in various shareholding and NTS conversion over time

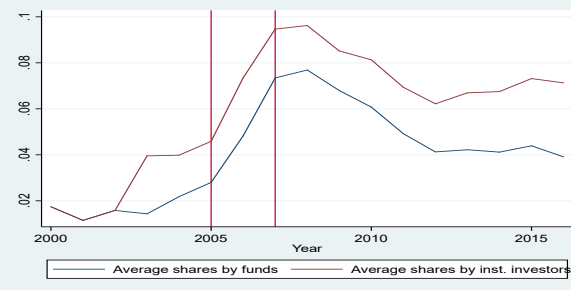
This figure shows the trend in private and state shareholdings for a sample of Chinese listed firms, as extracted from CSMAR. The sample consists of about 2,800 non-financial and non-utility firms.



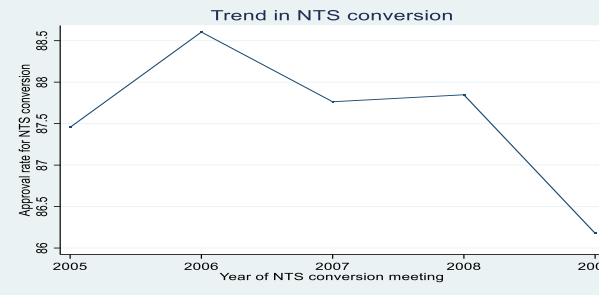

Panel A: Trend in tradeable and non-tradeable shares



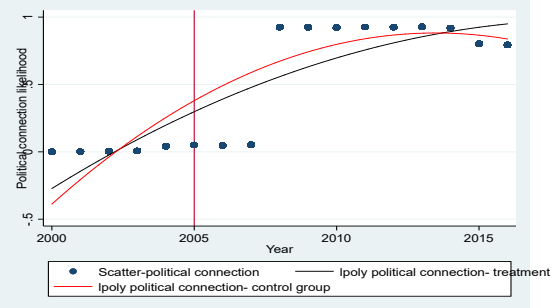

Panel B: Trend in state and legal persons share



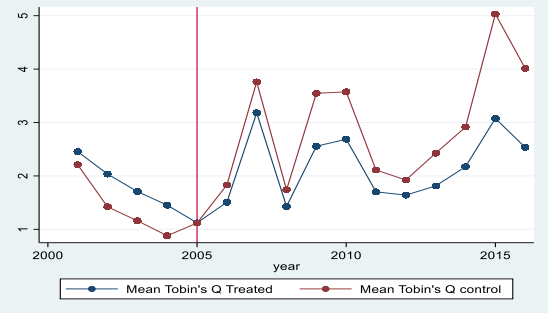

Panel C: Trend in shares held by institutional investors




Panel D: NTS approval rate over time

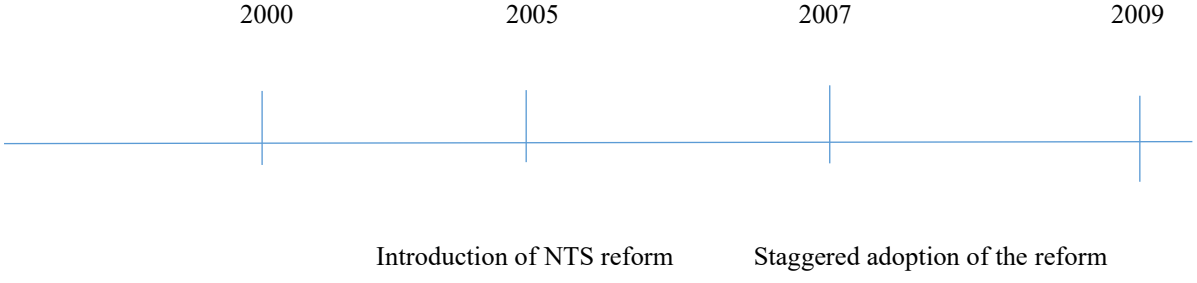



Panel E: Presence of government employee in Board




Panel F: Comparison of Tobin's Q among treatment & control firms

**Figure 2.** Timeline of the introduction and adoption of the NTS reform



## Table 1. A Comparison of partially (PP) and fully (FP) privatized firms

This table shows the average values of selected variables along with standard deviation for PP and FP firms in our sample. The final column shows the comparison of means between PP and FP firms using *t*-tests. The sample includes 12,378 firm-year observations for PP firms and 2,679 firm-year observations for FP firms. See Appendix Table A1 for variable definitions. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | PP firms (1) | | FP firms (2) | | |
|---|---|---|---|---|---|
| Variable | Mean | Std. Dev. | Mean | Std. Dev. | Two-sample *t*-statistic |
| Size | 7.874 | 1.160 | 8.323 | 1.302 | -17.90*** |
| Age | 22.794 | 21.145 | 30.382 | 36.480 | -14.49*** |
| Intangibility | 0.420 | 2.079 | 0.351 | 1.057 | 1.62 |
| Tax shield | 0.120 | 0.635 | 0.161 | 0.182 | -3.36*** |
| Herfindahl index | 0.001 | 0.003 | 0.001 | 0.004 | -5.47*** |
| Local political connection | 0.458 | 0.498 | 0.550 | 0.498 | -8.58*** |
| Legal persons share | 0.144 | 0.211 | 0.000 | 0.000 | 35.26*** |
| Control exceeds cashflow | 0.520 | 0.500 | 0.521 | 0.500 | -0.076 |
| Tunnelling | 0.021 | 0.039 | 0.019 | 0.033 | 2.44** |
| Related party transactions | 0.223 | 0.411 | 0.329 | 0.463 | -11.91*** |

## Table 2. Summary statistics of regression variables for the full sample, treatment, and control firms

This table summarizes the descriptive statistics (mean and standard deviation) of all firms, treated and control firms for a sample of 15,057 firm-year non-financial and non-utility firms between 2000 and 2016. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005-06, and zero otherwise. Column (1) shows the statistics for the entire sample. Column (2) shows the statistics for the treated firms. Column (3) reports summary statistics for the non-treated (control) firms. The last column reports the two-sample *t*-test statistics between treated and non-treated firms. Treated firms are those which hold non- tradable shares before 2007; otherwise it is a control firm. Panel A reports statistics for the control variables and panel B for the outcome variables. All variable definitions are reported in Table A1 of the Appendix. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | | All | Treated | Non-treated | Two-sample *t*-statistic |
|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (2)–(1) |
| **Panel A: Control variables** | | | | | |
| Size | *mean* | 7.954 | 8.110 | 7.672 | 21.79*** |
| | *std* | 1.199 | 1.259 | 1.022 | |
| Age | *mean* | 24.144 | 26.808 | 19.307 | 17.98*** |
| | *std* | 24.753 | 29.355 | 11.145 | |
| Intangibility | *mean* | 0.406 | 0.400 | 0.417 | -0.50 |
| | *std* | 1.937 | 2.141 | 1.497 | |
| Tax shield | *mean* | 0.127 | 0.149 | 0.088 | 6.18*** |
| | *std* | 0.581 | 0.720 | 0.083 | |
| Herfindahl | *mean* | 0.001 | 0.001 | 0.000 | 8.04*** |
| | *std* | 0.004 | 0.004 | 0.003 | |
| Political connection | *mean* | 0.475 | 0.456 | 0.510 | -6.32*** |
| | *std* | 0.499 | 0.498 | 0.500 | |
| Number of observations | *n* | 15,057 | 9,710 | 5,347 | |
| **Panel B: Outcome Variables** | | | | | |
| Tobin's Q | *mean* | 2.529 | 2.222 | 3.087 | -25.90*** |
| | *std* | 2.003 | 1.784 | 2.244 | |
| TQ_BDHN | *mean* | 2.532 | 2.226 | 3.089 | -25.79*** |
| | *std* | 2.008 | 1.792 | 2.247 | |
| Operating revenue (in $10^6$¥) | *mean* | 4,108 | 4,980 | 2,524 | 22.19*** |
| | *std* | 9,133 | 10,314 | 6,150 | |
| Productivity1(in $10^3$¥) | *mean* | 1,416 | 1,570 | 1,135 | 11.14*** |
| | *std* | 2,300 | 2,537 | 1,757 | |
| Productivity2(in $10^3$¥) | *mean* | 120 | 126 | 110 | 3.22*** |
| | *std* | 288 | 321 | 214 | |
| Legal persons share (%) | *mean* | 0.118 | 0.089 | 0.172 | -25.04*** |
| | *std* | 0.199 | 0.165 | 0.241 | |
| Control exceeds cash flow | *mean* | 0.520 | 0.498 | 0.561 | -7.32*** |
| | *std* | 0.500 | 0.500 | 0.496 | |
| Tunnelling | *mean* | 0.021 | 0.023 | 0.016 | 10.67*** |
| | *std* | 0.038 | 0.042 | 0.030 | |
| Related party transactions | *mean* | 0.242 | 0.279 | 0.174 | 14.68*** |
| | *std* | 0.423 | 0.442 | 0.375 | |
| Executive incentives | *mean* | 0.135 | 0.087 | 0.220 | -23.27*** |
| | *std* | 0.341 | 0.282 | 0.414 | |
| Executive turnover | *mean* | 0.236 | 0.262 | 0.187 | 10.40*** |
| | *std* | 0.424 | 0.440 | 0.390 | |
| Number of observations | *n* | 15,057 | 9,710 | 5,347 | |

## Table 3. OLS Estimates of the likelihood of full privatization

The table shows the OLS estimates of the likelihood of full privatization with sector fixed effects among all firms (columns (1)–(2)) and also treated firms (column (3)–(4)). The dependent variable is a binary variable ***FP*** that takes the value of one for fully privatized firms and zero otherwise. The key variables are ***Locally connected*** (which indicates the firm's political connection with the local governments), ***Reform2007*** dummy (takes the value of one for year >= 2007 and zero otherwise) and their interaction (***connected×Reform2007***). We also include a set of firm characteristics including firm size, age, intangible fixed assets shares, firm value measured by Tobin's Q, productivity measured by operating revenue as a share of total employment to explain the likelihood of full privatization. All regressions include sector fixed effects, allowing us to compare full privatization likelihood of connected and unconnected firms within a sector. All firm-level characteristics are lagged by one period. Columns (2) and (4) estimates also include Sector×Year fixed effects to account for time-varying sector-level unobserved factors, e.g., regulatory changes, that may also influence full privatization likelihood. See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | All | | Treated | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Locally connected | -0.0285* | -0.0234** | -0.0165 | -0.0094 |
| | (-1.65) | (-2.24) | (-1.04) | (-1.04) |
| Reform2007 | 0.2041*** | 0.2352*** | 0.2401*** | 0.2637*** |
| | (17.88) | (4.39) | (16.86) | (4.36) |
| Connected×Reform2007 | 0.0645*** | 0.0420** | 0.0651*** | 0.0214 |
| | (3.11) | (2.57) | (2.95) | (1.10) |
| Firm size$_{t-1}$ | 0.0340*** | 0.0301*** | 0.0267*** | 0.0097 |
| | (4.06) | (3.18) | (2.71) | (0.86) |
| Firm age $_{t-1}$ | 0.0016*** | 0.0016*** | 0.0013*** | 0.0013*** |
| | (5.29) | (5.29) | (4.55) | (4.40) |
| Intangibility $_{t-1}$ | -0.0012 | -0.0012 | -0.0025 | -0.0031 |
| | (-0.58) | (-0.50) | (-1.19) | (-1.10) |
| Depreciation $_{t-1}$ | 0.0084 | 0.0089 | 0.0013 | 0.0019 |
| | (0.62) | (0.62) | (0.18) | (0.22) |
| Tobin's Q $_{t-1}$ | -0.0165*** | -0.0136*** | -0.0165*** | -0.0160*** |
| | (-5.84) | (-3.63) | (-3.80) | (-2.94) |
| Log(employment) $_{t-1}$ | -0.0060 | -0.0039 | -0.0060 | 0.0003 |
| | (-0.78) | (-0.49) | (-0.65) | (0.03) |
| Intercept | -0.1751** | -0.1889*** | -0.1448* | -0.0681 |
| | (-2.51) | (-3.54) | (-1.82) | (-1.02) |
| *Sector FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | No | Yes | No | Yes |
| $R^2$ | 0.06 | 0.11 | 0.07 | 0.13 |
| *N* | 13,521 | 13,521 | 9,139 | 9,139 |

## Table 4. Impact of privatization on market performance indices

This table shows the difference-in-differences (DiD) estimates of selected market firm performance measures: Tobin's Q and TQ_BDHN. Columns (1) and (3) show the estimates for PP firms while columns (2) and (4) show those for FP firms. Columns (1)–(2) show the estimates of Tobin's Q and columns (3)–(4) show the estimates of Tobin's BDHN measure (TQ_BDHN). Tobin's Q is the market value of market capitalization plus total debt divided by book value of total assets. TQ_BDHN is the sum of market capital, total debt and preferred stock as a share of total assets as per Bhagat, Dong, Hirshleifer, and Noah (2005). ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and 0 otherwise. Controls include firm size, firm age, intangible fixed assets share, tax shield, Herfindahl index, a dummy for SOE and are lagged by one year. See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parentheses below regression estimates and are calculated using clustered standard errors around firm ids. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms.

| | PP firms | FP firms | PP firms | FP firms |
|---|---|---|---|---|
| | Tobin's Q | | TQ_BDHN | |
| Variables | (1) | (2) | (3) | (4) |
| Treated | -0.1043 | Dropped | -0.0943 | Dropped |
| | (0.57) | | (0.53) | |
| Reform2007 | 9.1672*** | 17.9654** | 8.9864*** | 17.2071** |
| | (2.96) | (2.05) | (2.78) | (2.08) |
| Treated×Reform2007 | -0.2321* | 0.7860* | -0.2306* | 0.7927* |
| | (1.90) | (1.75) | (1.96) | (1.65) |
| Firm size$_{t-1}$ | -0.8890*** | -1.2420*** | -0.8809*** | -1.1543*** |
| | (15.81) | (4.77) | (15.75) | (4.12) |
| Firm age $_{t-1}$ | -0.4907** | -1.4561* | -0.4721** | -1.3924* |
| | (2.40) | (1.83) | (2.22) | (1.86) |
| Intangibility $_{t-1}$ | 0.0029 | 0.0214 | 0.0122 | 0.0113 |
| | (0.45) | (0.79) | (1.62) | (0.47) |
| Depreciation $_{t-1}$ | -0.0105** | 0.7957 | -0.0076* | 0.7910 |
| | (2.44) | (1.11) | (1.68) | (1.11) |
| Herfindahl $_{t-1}$ | -18.6470*** | -33.6118 | 0.0349 | -37.8146 |
| | (2.72) | (1.19) | (0.00) | (1.51) |
| Intercept | 14.4208*** | 42.4554** | 13.9862*** | 40.3587** |
| | (5.83) | (2.43) | (5.38) | (2.44) |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.41 | 0.38 | 0.40 | 0.35 |
| *N* | 11,572 | 2,722 | 11,317 | 2,642 |
| Z-score | −4.59*** | | −1.90* | |

## Table 5. Impact of privatization on non-market performance measures

This table shows the difference-in-differences (DiD) firm FE estimates of non-market firm performance measures. The top panel shows the estimates for partially privatized firms while the bottom panel shows those for fully privatized firms. Operating revenue is CPI-adjusted operating revenue. Productivity 1 is measured by operating revenue as a share of number of employees, while productivity 2 is measured by the operating profit as a share of number of employees. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and 0 otherwise. Controls are the same controls variables used in Table 4 and are lagged by one year. See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parentheses below regression estimates and are calculated using clustered standard errors around firm ids. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | Ln(Operating rev)<br>(1) | Ln(productivity 1)<br>(2) | Ln(productivity 2)<br>(3) |
|---|---|---|---|
| Panel A: Partially Privatized (PP) Firms | | | |
| Treated×Reform2007 | -0.0897 | -0.0375 | -0.0378 |
| | (-1.45) | (-0.39) | (-0.28) |
| Intercept | 11.6130*** | 8.7450*** | 3.7040 |
| | (7.81) | (5.83) | (0.68) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.64 | 0.28 | 0.11 |
| *N* | 14,159 | 14,127 | 12,091 |
| Panel B: Fully Privatized (FP) Firms | | | |
| Treated×Reform2007 | 0.3906*** | 0.9029*** | 0.8828** |
| | (3.11) | (4.87) | (2.53) |
| Intercept | 18.3074*** | 14.7603*** | 12.8496 |
| | (6.92) | (5.52) | (0.74) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.43 | 0.13 | 0.15 |
| *N* | 2,830 | 2,829 | 2,258 |
| Z-score | −3.43*** | −4.50*** | −2.46** |

## Table 6. Mechanism 1. Privatization and private benefits of control

This table shows the difference-in-differences (DiD) estimates (with firm FE) for selected private benefits of control measures. The top panel shows the estimates for partially privatized firms while the bottom panel shows those for fully privatized firms. *Control exceeds cash* (column (1)) is a binary variable if control rights exceeds cash flow rights. It is zero otherwise. *Tunnelling* (column (2)) is measured by net other receivables scaled by total assets that reflects how a firm may manage to receive resources from its subsidiaries in various ways. *Related party transactions* (column (3)) is measured by related party transactions in loans, mortgages and asset transactions as a share of total related party transactions. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and \ero otherwise. Other controls include log of total assets (firm size), firm age, share of intangible assets, tax shield, Herfindahl index, a dummy for SOEs and also for Shenzhen stock listing (naturally SOE dummy and Shenzhen dummy drop in firm FE estimates). See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | Control exceeds cash (1) | Tunnelling (2) | Related Party Transactions (3) |
|---|---|---|---|
| Panel A: Partially Privatized (PP) Firms | | | |
| Treated×Reform2007 | 0.2211*** | 0.0546* | 0.0101 |
| | (11.93) | (1.71) | (1.17) |
| Intercept | 0.2417 | 0.9695*** | 0.0796 |
| | (1.03) | (5.64) | (1.26) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.23 | 0.20 | 0.18 |
| *N* | 14,164 | 14,164 | 14,162 |
| Panel B: Fully Privatized (FP) Firms | | | |
| Treated×Reform2007 | 0.1732 | 0.0353*** | 0.0439 |
| | (1.10) | (4.64) | (0.33) |
| Intercept | 0.2931 | 0.6117 | -4.0103 |
| | (0.41) | (1.20) | (-1.00) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.10 | 0.09 | 0.14 |
| *N* | 2,830 | 2,830 | 2,830 |
| Z-score | 0.30 | 0.59 | −0.25 |

## Table 7. Mechanism 2. Privatization and firm value maximization

This table shows the difference-in-differences (DiD) firm FE regression estimates of firm employment as measured by natural logarithm of employee numbers for partially and fully privatized firms. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes a value 1 for years ≥ 2007 and 0 otherwise. Controls are the same controls variables used in Table 4 and all control variables are lagged by one year. See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores shows the comparison of Treated×Reform2007 coefficients between PP and FP firms for log employment.

| | Partially privatized<br>(1) | Fully privatized<br>(2) |
|---|---|---|
| Treated | -0.0781 | 0.8416*** |
| | (-0.54) | (3.02) |
| Reform2007 | -0.7844 | -0.0072 |
| | (-0.61) | (-0.02) |
| Treated×Reform2007 | -0.0683 | -0.5200*** |
| | (-0.74) | (-5.88) |
| Intercept | 1.7589 | 9.8403* |
| | (0.66) | (1.78) |
| *Other controls* | Yes | Yes |
| *Firm FE* | Yes | Yes |
| *Sector×Year FE* | Yes | Yes |
| $R^2$ | 0.39 | 0.31 |
| *N* | 14,034 | 2,818 |
| Z-score | 3.53*** | |

## Table 8. Mechanism 3. Privatization and executive incentives/ turnover

This table shows the difference-in-differences (DiD) estimates (with firm FE) of executive incentives and executive turnover for both partially and fully privatized firms. Executive incentive is a dummy variable taking the value of one if the firm in question offers equity incentives to its top executives (CEO, board Chairman and General Manager) and zero otherwise. Executive turnover takes the value of one if anyone of the top three executives changes in a year in a firm; it is zero otherwise. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and zero otherwise. Controls are the same controls variables used in Table 4. All right-hand side variables are lagged by one year. See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | PP firms | FP firms | PP firms | FP firms |
|---|---|---|---|---|
| | Executive Incentives (1) | Executive Incentives (2) | Executive turnover (3) | Executive turnover (4) |
| Treated×Reform2007 | -0.0630** | 0.1325 | -0.0171 | -0.1359 |
| | (-2.54) | (0.99) | (-0.47) | (-1.25) |
| Intercept | -0.0713 | 0.1615 | 0.1724 | 0.7097 |
| | (-0.40) | (0.65) | (0.54) | (0.68) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.07 | 0.06 | 0.02 | 0.07 |
| *N* | 13,561 | 2,679 | 13,561 | 2,679 |
| Z-score | −1.44 | | 1.04 | |

## Table 9. Mechanism 4: Privatization and stock liquidity

This table shows the difference-in-differences (DiD) estimates (with firm FE) for two selected illiquidity measures, namely, daily ratio and turnover ratio. Columns (1)–(2) show the estimates of daily ratio and turnover ratio among PP firms while columns (3)–(4) show those for the FP firms. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–2006 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and zero otherwise. Other controls include log of total assets (firm size), firm age, share of intangible assets, tax shield, Herfindahl index, a dummy for SOEs and also for Shenzhen stock listing (naturally SOE dummy and Shenzhen dummy drop in firm FE estimates). See Appendix Table A1 for variable definitions. N is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. Significance level: ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | PP firms | FP firms | PP firms | FP firms |
|---|---|---|---|---|
| | Daily ratio | | Turnover ratio | |
| | (1) | (2) | (3) | (4) |
| Treated | -0.0370 | Dropped | -0.0244 | Dropped |
| | (-0.50) | | (-0.19) | |
| Reform2007 | 0.1449 | 0.5264 | -0.7301*** | -3.1524*** |
| | (0.68) | (1.14) | (-2.74) | (11.00) |
| Treated×Reform2007 | 0.0798 | -0.5621*** | 0.0012 | 0.4617** |
| | (0.87) | (3.44) | (0.02) | (2.25) |
| Intercept | 3.0758*** | -1.3251 | -0.4593 | 2.1149*** |
| | (6.84) | (1.26) | (-0.76) | (3.05) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.43 | 0.58 | 0.41 | 0.52 |
| *N* | 9,945 | 2,309 | 9,945 | 2,309 |
| Z-score | 3.43*** | | −2.15** | |

## Table 10. Market performance estimates using alternative reform dates

This table shows the difference-in-differences (DiD) firm FE regression estimates of selected market performance indices – using alternative reform dates 2004, 2005, 2006, and 2008. We report the results of these four different regression specifications. For brevity, we only report the interaction coefficients Treated×Reform for regressions on two measures of Tobin's Q – columns (1) and (3) show the estimates of Tobin's Q while columns (2) and (4) show that for Tobin's Q BDHN. The corresponding estimates for partially privatized firms are shown in columns (1) and (2) and those for the fully privatized firms in columns (3) and (4). Note, however, that we are unable to obtain the relevant estimates for years 2004–2006 as there were very few treated FP firms in the years preceding 2007. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2004*** is a dummy that takes the value of one for years ≥ 2004 and zero otherwise. ***Reform2005*** is a dummy that takes the value of one for years ≥ 2005 and zero otherwise. ***Reform2006*** is a dummy that takes the value of one for years ≥ 2006 and zero otherwise. ***Reform2008*** is a dummy that takes the value of one for years ≥ 2008 and zero otherwise. Controls are the same controls variables used in Table 4 for all specifications using alternative reform dates 2004, 2005, 2006 and 2008 respectively. All control variables are lagged by one year. All regressions also include firm FEs and also Sector×Year FEs. See Appendix Table A1 for variable definitions. *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm ids. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | PP firms | | FP firms | |
|---|---|---|---|---|
| Regression models | Tobin's Q<br>(1) | Tobin's Q BDHN<br>(2) | Tobin's Q<br>(3) | Tobin's Q BDHN<br>(4) |
| (1) Treated×Reform2004 | 0.3595<br>(0.98) | -0.2007<br>(-0.92) | Dropped | Dropped |
| (2) Treated×Reform2005 | 0.3334<br>(0.92) | -0.2316<br>(-1.22) | 0.6089<br>(0.60) | 0.6083<br>(0.62) |
| (3) Treated×Reform2006 | -0.1693<br>(-1.27) | -0.3248***<br>(-3.62) | 0.7847<br>(0.53) | 0.7845<br>(0.51) |
| (4) Treated×Reform2008 | -0.1747*<br>(-1.69) | -0.2701***<br>(-2.69) | -0.1890<br>(-0.44) | -0.1627<br>(-0.37) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| *N* | 11,572 | 11,095 | 2,722 | 2,581 |

## Online Appendix

## Table A1. Glossary of regression variables

| Panel A: Dependent variables | |
|---|---|
| Tobin's Q | Market value of market capitalization divided by book value of total assets. |
| TQ BDHN | It is the alternative Tobin's Q defined as: (Market cap + total debt + preferred stock) / total assets, see Bhagat *et al.* (2005). |
| Operating revenue | CPI-adjusted operating revenue. |
| Productivity 1 | Operating revenue as a share of number of employees |
| Productivity 2 | Operating profit as a share of number of employees |
| Employment | Natural logarithm of total number of employees |
| Tradeable shares | Calculated as percentage of ownership held by tradeable shareholders. |
| Legal shares | Calculated as percentage of ownership held by legal persons. |
| Control exceeds cash | Excess of control over cash flow rights of controlling owners. |
| Executive incentives | A dummy variable indicating if executives are offered share ownership. |
| Executive turnover | A dummy variable indicating if the CEO, Chairman or the General Manager changes in the firm. |
| Any political connection | A dummy variable that takes the value of one if the CEO or any board member of the firm is a government (local, provincial or central) employee (current or retired); it is zero otherwise. |
| Connected with local government (locally connected) | A dummy variable that takes the value of one if the CEO or any board member of the firm is a local government employee (current or retired); it is zero otherwise. |
| Tunnelling | Net other receivables divided by book value of total assets. |
| Related party transactions | Related party transactions (RPT) in Loans, mortgages and asset transactions as a share of total RPT, which also includes other related party transactions. |
| Daily ratio | The ratio of current-year to one-year lagged Amihud illiquidity measure, where illiquidity is defined as daily absolute return divided by daily trading volume (Amihud (2002)). |
| Turnover ratio | The ratio of current-year to one-year lagged turnover, where turnover is defined as daily trading volume divided by the number of shares outstanding (Amihud (2002)). |
| **Panel B: Independent variables** | |
| Treated | A dummy variable equals to one if a firm hold non-tradable shares before financial year 2007, zero otherwise. |
| Reform2007 | A dummy variable equal to one if year >= 2007 and zero otherwise. |
| Reform2004 | A dummy variable equal to one if year >= 2004 and zero otherwise. |
| Reform2005 | A dummy variable equal to one if year >= 2005 and zero otherwise. |
| Reform2006 | A dummy variable equal to one if year >= 2006 and zero otherwise. |
| Reform2008 | A dummy variable equal to one if year >= 2008 and zero otherwise. |
| $Size_{t-1}$ | Logarithm of book value of total assets, lagged at one financial year. |

| | |
|---|---|
| $Age_{t-1}$ | Calculated by financial year deducted firm's incorporation year, lagged at one financial year. |
| $Intangibility_{t-1}$ | Calculated by the intangible assets divided by book value of total assets, lagged at one financial year. |
| Tax $shield_{t-1}$ | Calculated by depreciation divided by book value of total assets. |
| Herfindahl | Market share of firms: share of firm sales in total industry sales. |
| Link with local government | A dummy variable indicating if CEO or any board member is/was an employee of the local government. |
| Dual CEO | A dummy variable indicating if the CEO and Board Chairman are the same person; it is zero otherwise. |
| Independent directors' share | The number of independent directors as a share of total number of directors. |
| ROA | Earnings before interest and tax as a share of total assets. |
| SOE | A dummy variable taking the value of one if the firm is state-owned and zero otherwise. |
| Shenzhen SE | A dummy variable taking the value of one for firms listed in Shenzhen stock exchange and zero for Shanghai stock exchange. |
| Business group | A dummy variable equal to one if a firm belongs to a business group, zero otherwise. |
| Sector | A dummy variables equal to one if firm belongs to machinery, equipment, furniture, recycling; two if it belongs to chemicals, rubber, plastics, non-metallic products; three if it belongs to metals & metal products; four if it belongs to textiles, wearing apparel, leather; five if it belongs to food, beverages, tobacco; six if it belongs to wood, crock and paper; seven if it belongs to publishing and printing; zero otherwise. |

## Table A2. Tests for parallel trends – Market performance indices

This table shows the tests of parallel trends. Panel A shows the traditional tests of parallel trends. Columns (1)–(2) of panel A show the estimates of PP firms while columns (3)–(4) show the corresponding estimates of FP firms. The dependent variable is Tobin's Q defined as the share of market capital in total assets in columns (1) and (3) of panel A and TQ_BDHN (which is the sum of market capital, total debt and preferred stock as a share of total assets) in columns (2) and (4). Other controls are Treated and Year dummies plus the firm fixed effects. Here we only show the interaction terms Treated×$Year_t$ when $t$ <= 2007. Other interaction terms estimates for $t$>2007 are not shown. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. Panel B shows the placebo tests for parallel trends as per Higgins et al. (2021). To do this, we take our pre-trend data (2000–2004) and split it in half, defining the midpoint 2002 as an imaginary treatment event, but our sample goes up to the introduction of the reform. We, therefore, estimate the performance indices using Post2002 as a placebo for year<2007. Here we show the estimates of Treated×Post2002. Other variables included are Treated and Post2002 plus the firm fixed effects. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. All standard errors are clustered at the firm level. Level of significance: * $p$<0.1; ** $p$<0.05; *** $p$<0.01.

| | PP firms | | FP firms | |
|---|---|---|---|---|
| | Tobin's Q | TQ_BDHN | Tobin's Q | TQ_BDHN |
| | (1) | (2) | (3) | (4) |
| Panel A: Traditional tests | | | | |
| Treated×2001 | 0.7034 | 0.7038 | Dropped | Dropped |
| | (1.17) | (1.17) | | |
| Treated×2002 | 0.8899 | 0.8914 | Dropped | Dropped |
| | (1.21) | (1.21) | | |
| Treated×2003 | 1.0634 | 1.0645 | Dropped | Dropped |
| | (1.33) | (1.33) | | |
| Treated×2004 | 1.1250 | 1.1253 | Dropped | Dropped |
| | (1.35) | (1.35) | | |
| Treated×2005 | 1.3914 | 1.3899 | 3.0552* | 3.1386* |
| | (1.53) | (1.53) | (1.73) | (1.77) |
| Treated×2006 | 1.1933 | 1.1914 | 2.6725 | 2.7568 |
| | (1.32) | (1.31) | (1.56) | (1.61) |
| Treated×2007 | 1.0675 | 1.0648 | 3.6344** | 3.7348** |
| | (1.18) | (1.18) | (2.30) | (2.36) |
| Intercept | 4.6170*** | 4.6180*** | 3.7250** | 3.7411** |
| | (5.05) | (5.05) | (2.17) | (2.18) |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.29 | 0.29 | 0.22 | 0.23 |
| *N* | 16,838 | 16,838 | 3,784 | 3,784 |
| Panel B: Placebo tests | | | | |
| Treated×Post2002 | 0.6735 | 0.6733 | 0.0407 | 0.0407 |
| | (1.35) | (1.35) | (1.07) | (1.07) |
| Intercept | 2.7849*** | 2.7847*** | 3.2774*** | 3.2774*** |
| | (5.37) | (5.37) | (225.67) | (225.67) |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.14 | 0.14 | 0.02 | 0.02 |
| *N* | 2,198 | 2,198 | 42 | 42 |

## Table A3. Test of parallel trends – Non-market performance indices

This table shows the tests of parallel trends for non-market performance indices. Panel A shows the estimates of traditional tests for non-market performance indices: column (1) and (4) show estimates of operating revenue, columns (2) and (5) show estimates of productivity 1 and columns (3) and (6) show those for productivity 2. Columns (1)–(3) show estimates for PP firms while columns (4)–(6) show those for FP firms. Other controls are Treated and Year dummies plus the firm fixed effects. Here we only show the interaction terms Treated×$Year_t$ when $t <= 2007$. Other interaction terms estimates for $t > 2007$ are not shown. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. Panel B shows the corresponding placebo tests as per Higgins et al. (2021). To do this, we take our pre-trend data (2000-2004) and split it in half, defining the midpoint 2002 as an imaginary treatment event, but our sample goes up to the introduction of the reform. We, therefore, estimate the non-market performance indices using Post2002 as a placebo for year < 2007. Here we show the estimates of Treated×Post2002. Other variables included are Treated and Post2002. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. All standard errors are clustered at the firm level. Level of significance: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

| | PP firms | | | FP firms | | |
|---|---|---|---|---|---|---|
| Panel A: Traditional tests | Ln(Operating rev) | Ln(productivity1) | Ln(productivity2) | Ln(Operating rev) | Ln(productivity1) | Ln(productivity2) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated×2001 | -0.0688 | -0.0452 | -0.1968 | Dropped | Dropped | Dropped |
| | (1.09) | (0.55) | (1.53) | | | |
| Treated×2002 | -0.0592 | 0.0673 | -0.1901 | Dropped | Dropped | Dropped |
| | (0.71) | (0.63) | (1.28) | | | |
| Treated×2003 | -0.0755 | 0.1266 | 0.0906 | Dropped | Dropped | Dropped |
| | (0.79) | (1.19) | (0.60) | | | |
| Treated×2004 | -0.0733 | 0.0670 | 0.0877 | Dropped | Dropped | Dropped |
| | (0.71) | (0.60) | (0.57) | | | |
| Treated×2005 | -0.0539 | 0.1164 | 0.1979 | Dropped | Dropped | Dropped |
| | (0.54) | (1.01) | (1.13) | | | |
| Treated×2006 | -0.0345 | 0.1388 | 0.1949 | -0.5826*** | -0.9828*** | -0.8911** |
| | (0.29) | (1.15) | (1.15) | (3.26) | (4.47) | (2.08) |
| Treated×2007 | 0.0070 | 0.1820 | 0.0188 | -0.1600 | -0.2226 | 0.0423 |
| | (0.05) | (1.45) | (0.11) | (0.92) | (0.94) | (0.10) |
| Intercept | 19.7915*** | 12.4701*** | 10.5388*** | 22.4578*** | 16.6741*** | 16.4625*** |
| | (148.23) | (82.78) | (58.21) | (33.59) | (36.81) | (18.73) |
| *Firm FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.34 | 0.26 | 0.07 | 0.05 | 0.05 | 0.05 |
| *N* | 22,086 | 22,004 | 18,968 | 3,958 | 3,957 | 3,100 |

| *Table cont.* | PP firms | | | FP firms | | |
|---|---|---|---|---|---|---|
| Panel B: Placebo Tests | Ln(Operating rev) (1) | Ln(productivity1) (2) | Ln(productivity2) (3) | Ln(Operating rev) (4) | Ln(productivity1) (5) | Ln(productivity2) (6) |
| Treated×Post2002 | -0.0289 | 0.1186 | 0.2110** | -0.1379 | 0.7312*** | 1.4183* |
| | (0.42) | (1.60) | (2.15) | (0.25) | (3.47) | (2.02) |
| Intercept | 20.1443*** | 12.7252*** | 10.9222*** | 20.1730*** | 14.2950*** | 12.2307*** |
| | (177.25) | (108.12) | (51.32) | (54.22) | (76.28) | (24.42) |
| *Firm FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.20 | 0.21 | 0.01 | 0.03 | 0.38 | 0.31 |
| *N* | 6,694 | 6,652 | 5,640 | 74 | 74 | 65 |

## Table A4. Test of Parallel trends – Private benefits of control measures

This table shows the tests of parallel trends for private benefits of control indices. Panel A shows the estimates of traditional tests for non-market performance indices: column (1) and (4) show estimates of Control exceeds Cash (CEC), columns (2) and (5) show estimates of Tunnelling and columns (3) and (6) show those for Related Party Transactions (RPT). Columns (1)–(3) show estimates for PP firms while columns (4)–(6) show those for FP firms. Other controls are Treated and Year dummies plus the firm fixed effects. Here we only show the interaction terms Treated×$Year_t$ when $t <= 2007$. Other interaction terms estimates for $t > 2007$ are not shown. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005-06 and initiates the process of NTS conversion, and zero otherwise. Panel B shows the corresponding placebo tests as per Higgins et al. (2021). To do this, we take our pre-trend data (2000–2004) and split it in half, defining the midpoint 2002 as an imaginary treatment event, but our sample goes up to the introduction of the reform. We, therefore, estimate the private benefits of control indices using Post2002 as a placebo. Here we show the estimates of Treated×Post2002 for year < 2007. Other variables included are Treated and Post2002. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. All standard errors are clustered at the firm level. Level of significance: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

| | PP firms | | | FP firms | | |
|---|---|---|---|---|---|---|
| Panel A: Traditional tests | CEC (1) | Tunnelling (2) | RPT (3) | CEC (4) | Tunnelling (5) | RPT (6) |
| Treated | 0.1031 | -0.0088 | 0.0284 | 0.5432*** | 0.0083** | -0.1562*** |
| | (1.60) | (0.60) | (0.44) | (21.79) | (2.17) | (3.91) |
| Treated×2001 | 0.0066 | 0.0146* | -0.0099 | Dropped | Dropped | Dropped |
| | (0.66) | (1.95) | (0.58) | | | |
| Treated×2002 | -0.0060 | -0.0045 | 0.0262 | Dropped | Dropped | Dropped |
| | (0.44) | (0.50) | (1.62) | | | |
| Treated×2003 | -0.3446*** | -0.0051 | 0.0034 | Dropped | Dropped | Dropped |
| | (8.93) | (0.59) | (0.18) | | | |
| Treated×2004 | -0.3671*** | -0.0055 | -0.0025 | Dropped | Dropped | Dropped |
| | (9.65) | (0.56) | (0.11) | | | |
| Treated×2005 | -0.3172*** | -0.0034 | -0.0465 | Dropped | Dropped | Dropped |
| | (8.15) | (0.31) | (1.18) | | | |
| Treated×2006 | -0.3207*** | -0.0045 | -0.0347 | -0.1769* | -0.0569* | -0.2697** |
| | (8.79) | (0.41) | (0.87) | (1.92) | (1.90) | (2.06) |
| Treated×2007 | -0.2641*** | 0.0144 | 0.0483 | -0.0917* | -0.0311 | -0.1467 |
| | (6.78) | (1.45) | (1.30) | (1.73) | (1.36) | (0.71) |
| Intercept | 0.8463*** | 0.0810*** | -0.1160** | 0.3343** | 0.1174** | 0.0735 |
| | (18.24) | (7.40) | (2.56) | (2.12) | (2.16) | (0.60) |
| *Firm FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Year FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.23 | 0.14 | 0.15 | 0.02 | 0.05 | 0.07 |
| *N* | 22,110 | 22,088 | 22,110 | 3,958 | 3,958 | 3,958 |

| *Table cont.* | PP firms | | | FP firms | | |
|---|---|---|---|---|---|---|
| Panel B: Placebo tests | CEC | Tunnelling | RPT | CEC | Tunnelling | RPT |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated×Post2002 | -0.2806*** | -0.0171 | -0.0095 | -0.1025 | -0.0320 | 0.1213 |
| | (10.83) | (1.37) | (0.48) | (1.26) | (0.84) | (0.85) |
| Intercept | 0.8883*** | 0.0686*** | -0.0548 | 0.7262*** | 0.1138*** | -0.0239 |
| | (15.11) | (8.32) | (0.75) | (7.82) | (3.67) | (0.45) |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.41 | 0.01 | 0.19 | 0.32 | 0.18 | 0.13 |
| *N* | 6,718 | 6,697 | 6,718 | 51 | 51 | 51 |

## Table A5. Tests for parallel trends – employment, executive incentives, turnover and liquidity measures

This table shows the tests of parallel trends for employment, executive incentives and turnover and also liquidity measures. Panels A and B show the estimates using traditional and placebo tests for PP firms while panels C and D show those for the FP firms. We show estimates of ln(Emp), executive incentives, turnover, daily ratio and turnover ratio respectively in columns (1)–(5). Traditional tests include Treated, Year dummies, Treated×Year plus the firm fixed effects. Here we only show the interaction terms Treated×$Year_t$ when $t <= 2007$. Other interaction terms estimates for $t > 2007$ are not shown. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. Placebo tests are done as per Higgins et al. (2021). To do this, we take our pre-trend data (2000–2004) and split it in half, defining the midpoint 2002 as an imaginary treatment event, but our sample goes up to the introduction of the reform. We, therefore, estimate the private benefits of control indices using Post2002 as a placebo. Here we show the estimates of Treated×Post2002 for year < 2007. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. All standard errors are clustered at the firm level. Level of significance: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

| | Employment<br>(1) | Executive Incentives<br>(2) | Executive Turnover<br>(3) | Daily ratio<br>(4) | Turnover ratio<br>(5) |
|---|---|---|---|---|---|
| Panel A. PP firms- traditional tests | | | | | |
| Treated | 0.0290 | -0.0216 | -0.0779 | 0.3268* | -0.0443 |
| | (0.21) | (1.26) | (1.34) | (1.93) | (0.36) |
| Treated×2001 | -0.0453 | 0.0023 | 0.0954 | 1.0216 | 0.1296** |
| | (0.60) | (0.47) | (1.60) | (0.55) | (2.01) |
| Treated×2002 | -0.1306 | 0.0048 | 0.0245 | 0.1081 | 0.1590** |
| | (1.48) | (0.90) | (0.40) | (0.36) | (1.98) |
| Treated×2003 | -0.2077** | 0.0025 | 0.0087 | -0.0010 | -0.0152 |
| | (2.00) | (0.47) | (0.16) | (0.00) | (0.15) |
| Treated×2004 | -0.1610 | 0.0122 | 0.0752 | -0.2672** | -0.0644 |
| | (1.46) | (1.45) | (1.29) | (2.00) | (0.42) |
| Treated×2005 | -0.1862 | 0.0072 | 0.0462 | -0.4084* | 0.0710 |
| | (1.54) | (0.96) | (0.78) | (1.74) | (0.53) |
| Treated×2006 | -0.1875 | -0.0421** | 0.0306 | -0.2767*** | 0.1122 |
| | (1.43) | (1.97) | (0.53) | (2.67) | (0.62) |
| Treated×2007 | -0.1657 | -0.0107 | -0.0000 | -0.2479** | 0.3167*** |
| | (1.11) | (0.63) | (0.00) | (2.49) | (3.24) |
| Intercept | 7.1076*** | 0.0388*** | 0.3349*** | 2.9529*** | 0.3870*** |
| | (59.62) | (2.95) | (6.99) | (6.20) | (5.26) |
| $R^2$ | 0.13 | 0.07 | 0.01 | 0.01 | 0.39 |
| *N* | 23,353 | 23,439 | 23,439 | 15,225 | 15,225 |

| *Table cont.* | Employment<br>(1) | Executive Incentives<br>(2) | Executive Turnover<br>(3) | Daily ratio<br>(4) | Turnover ratio<br>(5) |
|---|---|---|---|---|---|
| Panel B: PP firms – placebo tests | | | | | |
| Treated×Post2002 | -0.1518** | -0.0033 | -0.0124 | -1.3474 | -0.1400** |
| | (1.97) | (1.04) | (0.35) | (0.65) | (2.00) |
| Intercept | 7.1719*** | 0.0190*** | 0.2714*** | 2.9296*** | 0.1968 |
| | (91.87) | (5.46) | (3.41) | (5.25) | (1.03) |
| $R^2$ | 0.00 | 0.02 | 0.00 | 0.01 | 0.35 |
| *N* | 6,675 | 6,718 | 6,718 | 4,910 | 4,910 |
| Panel C: FP firms – traditional tests | | | | | |
| Treated | 0.0821 | -0.0052 | -0.4701*** | 0.6093*** | 0.5395*** |
| | (1.44) | (0.29) | (10.05) | (3.94) | (5.43) |
| Treated×2006 | 0.1650 | -0.1450 | -0.0846 | 0.9177*** | -1.8483*** |
| | (0.72) | (0.89) | (0.54) | (3.64) | (4.64) |
| Treated×2007 | -0.0909 | -0.4472 | -0.3716 | 1.0455*** | -2.1643*** |
| | (0.35) | (1.24) | (1.58) | (4.08) | (10.06) |
| Intercept | 5.4944*** | 0.0152 | 0.4795** | 1.3546*** | 0.0417 |
| | (6.41) | (0.94) | (2.36) | (4.79) | (0.26) |
| $R^2$ | 0.08 | 0.02 | 0.01 | 0.49 | 0.48 |
| *N* | 4,308 | 4,309 | 4,309 | 3,648 | 3,648 |
| Panel D: FP firms – placebo tests | | | | | |
| Treated×Post2002 | -0.3986 | -0.1667 | -0.2500* | 0.0013 | -1.2566** |
| | (0.74) | (1.38) | (1.79) | (0.02) | (2.94) |
| Intercept | 5.0872*** | 0.0000 | 0.2000 | 2.1556*** | 0.3501*** |
| | (10.26) | (0.53) | (1.00) | (34.19) | (3.88) |
| $R^2$ | 0.12 | 0.13 | 0.19 | 0.67 | 0.49 |
| *N* | 51 | 51 | 51 | 40 | 40 |

## Table A6. Difference-in-differences estimates of market performance with firm, Sector×Year and Province×Year fixed effects

This table re-estimates the performance measures with firm, Sector×Year and Province×Year fixed effects. Columns (1)–(2) show estimates of PP firms while columns (3)–(4) show those for FP firms. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and zero otherwise. Other controls include log of total assets (firm size), firm age, share of intangible assets, tax shield, Herfindahl index, a dummy for SOEs and also for Shenzhen stock listing (naturally SOE dummy and Shenzhen dummy drop in firm FE estimates). See Appendix Table A1 for variable definitions. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | PP firms | FP firms | PP firms | FP firms |
|---|---|---|---|---|
| | Tobin's Q | Tobin's Q | TQ_BDHN | TQ_BDHN |
| | (1) | (2) | (3) | (4) |
| Treated | -0.1294 | Dropped | -0.1831 | Dropped |
| | (-0.55) | | (-0.80) | |
| Reform2007 | 11.1369*** | 8.6160*** | 19.4965*** | 8.6342*** |
| | (3.42) | (4.81) | (8.56) | (4.83) |
| Treated×Reform2007 | -0.3061*** | 0.2850 | -0.3443*** | 0.3000 |
| | (-2.73) | (0.75) | (-3.11) | (0.79) |
| Intercept | 23.6184*** | 24.1184*** | 38.6381*** | 23.8729*** |
| | (4.51) | (5.73) | (9.97) | (5.69) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| *Province×Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.48 | 0.44 | 0.43 | 0.41 |
| *N* | 9,506 | 2,642 | 9,506 | 2,642 |
| Z-score | −1.49 | | −1.63 | |

## Table A7. Impact of privatization using alternative definitions of partial privatization

This table re-estimates the performance measures by using some alternative measures of treatment for partially privatized firms. Columns (1)–(2) show estimates of PP firms with at least 30 percent non-tradable shares prior to the reform and columns (3)–(4) show those with at least 50 percent. ***Treatedge30*** is a dummy variable that takes the value of one (across the entire period) when the firm holds at least 30% non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Treatedge50*** is a dummy variable that takes the value of one (across the entire period) when the firm holds at least 50% non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and zero otherwise. Other controls include log of total assets (firm size), firm age, share of intangible assets, tax shield, Herfindahl index, a dummy for SOEs and also for Shenzhen stock listing (naturally SOE dummy and Shenzhen dummy drop in firm FE estimates). See Appendix Table A1 for variable definitions. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | Tobin's Q | TQ_BDHN | Tobin's Q | TQ_BDHN |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatedge30 | 0.0054 | 0.0056 | | |
| | (0.05) | (0.05) | | |
| Treatedge30×Reform2007 | -0.2121* | -0.2135* | | |
| | (-1.91) | (-1.92) | | |
| Treatedge50 | | | -0.1033 | -0.1019 |
| | | | (-1.19) | (-1.19) |
| Treatedge50×Reform2007 | | | -0.0463 | -0.0440 |
| | | | (-0.50) | (-0.48) |
| Reform2007 | 1.3341*** | 1.3354*** | 1.2113*** | 1.2113*** |
| | (5.76) | (5.75) | (5.34) | (5.34) |
| Intercept | 5.5441*** | 5.5838*** | 5.5585*** | 5.6000*** |
| | (4.76) | (4.78) | (4.78) | (4.80) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes |
| $R^2$ | 0.41 | 0.41 | 0.41 | 0.41 |
| *N* | 11,095 | 11,095 | 11,095 | 11,095 |

## Table A8. Impact of privatization on market performance measures using alternative treatment measure $\text{Treated}_{\text{State}}$

This table is similar to Table 4. The key difference is that we present the results using a different definition of treated firms. $\boldsymbol{Treated_{State}}$ is an alternative treatment group as the firms with state shares only in 2005 (instead of both state and legal persons shares). Columns (1) and (2) include only firm fixed effects, while columns (3)–(4) include both firm fixed effects and Sector×Year fixed effects. Other controls are as in Table 4. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | Tobin's Q<br>(1) | TQ_BDHN<br>(2) | Tobin's Q<br>(3) | TQ_BDHN<br>(4) |
|---|---|---|---|---|
| Panel A: Partially Privatized (PP) Firms | | | | |
| $\text{Treated}_{\text{State}}$×Reform2007 | -0.2022**<br>(-1.97) | -0.2048**<br>(-2.00) | -0.1762*<br>(-1.77) | -0.1781*<br>(-1.79) |
| Intercept | 5.3669***<br>(17.96) | 5.3752***<br>(18.01) | 23.5366***<br>(3.03) | 23.5925***<br>(3.04) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | No | No | Yes | Yes |
| $R^2$ | 0.11 | 0.11 | 0.41 | 0.41 |
| *N* | 11,095 | 11,095 | 11,095 | 11,095 |
| Panel B: Fully Privatized (FP) Firms | | | | |
| $\text{Treated}_{\text{State}}$×Reform2007 | 1.2611***<br>(2.75) | 1.2427***<br>(2.72) | 0.7854<br>(1.54) | 0.7872<br>(1.52) |
| Intercept | 5.7213***<br>(3.35) | 5.1019***<br>(2.85) | 37.3630***<br>(4.85) | 36.6501***<br>(4.72) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | No | No | Yes | Yes |
| $R^2$ | 0.14 | 0.12 | 0.39 | 0.36 |
| *N* | 2,581 | 2,581 | 2,581 | 2,581 |
| Z-score | −3.11*** | −3.09*** | −1.85* | −1.83* |

## Table A9. Impact of privatization on non-market performance measures using alternative treatment $Treated_{State}$

This table is similar to Table 5. The key difference is that we present the results using a different definition of treated firms. $\boldsymbol{Treated_{State}}$ is an alternative treatment group as the firms with state shares only in 2005 (instead of both state and legal persons shares). Other controls are as in Table 4. See Appendix Table A1 for variable definitions. *N* is the number of observations and *t*-statistics are reported in parenthesis below regression estimates and are calculated using clustered standard errors around firm id. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms for each outcome variable.

| | Ln(Operating rev)<br>(1) | Ln(productivity 1)<br>(2) | Ln(productivity 2)<br>(3) |
|---|---|---|---|
| Panel A: Partially Privatized (PP) Firms | | | |
| $Treated_{State}$×Reform2007 | -0.0037 | -0.0332 | -0.0679 |
| | (-0.07) | (-0.41) | (-0.60) |
| Intercept | 16.1143*** | 14.6811*** | 10.1801* |
| | (4.97) | (8.81) | (1.71) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.65 | 0.28 | 0.11 |
| *N* | 13,558 | 13,528 | 11,671 |
| Panel B: Fully Privatized (FP) Firms | | | |
| $Treated_{State}$×Reform2007 | 0.3702*** | 0.8737*** | 0.8356** |
| | (2.94) | (4.96) | (2.43) |
| Intercept | 15.1339*** | 9.7362** | 2.8483 |
| | (4.10) | (2.10) | (0.41) |
| *Other controls* | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes |
| $R^2$ | 0.43 | 0.13 | 0.15 |
| *N* | 2,679 | 2,678 | 2,155 |
| Z score | −2.74*** | −4.68*** | −2.50** |

## Table A10. Impact of privatization using alternative measures of full privatization

This table tests the robustness of the effects of full privatization on Tobin's Q measures. It presents difference-in-differences (DiD) estimates of market firm performance measures using alternative definitions of full privatization, namely, 0.5%, 1% and 5% state-ownership respectively. Columns (1) and (2) consider privatized firms with less than 0.5% state-ownership, columns (3) and (4) consider privatized firms with 1% or lower state-ownership while columns (5)–(6) show those with 5% or lower state ownership. See Appendix Table A1 for variable definitions. *N* is the number of observations and *t*-statistics are reported in parentheses below regression estimates and are calculated using clustered standard errors around firm ids. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

| | State shares <= 0.005 | | State shares <= 0.01 | | State shares <= 0.05 | |
|---|---|---|---|---|---|---|
| Variables | Tobin's Q | TQ_BDHN | Tobin's Q | TQ_BDHN | Tobin's Q | TQ_BDHN |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treated×Reform2007 | 0.162*** | 0.655 | 0.1523*** | 0.0639 | 0.0596 | -0.5663 |
| | (4.60) | (1.51) | (5.58) | (0.13) | (0.69) | (1.00) |
| Intercept | 0.3430 | 49.169*** | 0.4119*** | 1.2395* | 0.2120*** | -0.3992 |
| | (0.34) | (4.63 | (13.41) | (1.73) | (3.02) | (0.86) |
| *Other controls* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.10 | 0.33 | 0.06 | 0.27 | 0.05 | 0.27 |
| *N* | 3,580 | 3,580 | 5,513 | 5,513 | 6,448 | 6,448 |

## Table A11. Firm fixed effects estimates of BHAR

This table shows the firm fixed effects estimates of BHAR, which is the 12-month monthly rolling BHAR. Similar to Lyon *et al.* (1999), we calculate BHARs by compounding firm monthly stock returns in the past 12-month period and then subtracting the equivalent compounded 12-month period return of the reference portfolio. As reference portfolio, we use CSMAR's monthly market returns with cash dividend reinvested. ***Treated*** is a dummy variable that takes the value of one (across the entire period) when the firm holds non-tradable shares between 2005–06 and initiates the process of NTS conversion, and zero otherwise. ***Reform2007*** is a dummy that takes the value of one for years ≥ 2007 and zero otherwise. Controls include firm size, firm age, intangible fixed assets share, tax shield, Herfindahl index, a dummy for SOE and are lagged by one year. See Appendix Table A1 for variable definitions. *N* is the number of observations and *t*-statistics are reported in parentheses below regression estimates and are calculated using clustered standard errors around firm ids. Other controls are as in Table 4. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. Z-scores show the comparisons of Treated×Reform2007 coefficients between PP and FP firms.

| | PP<br>(1) | FP<br>(2) | PP<br>(3) | FP<br>(4) |
|---|---|---|---|---|
| Treated | 0.0006 | Dropped | 0.0008 | Dropped |
| | (0.15) | | (0.18) | |
| Reform2007 | -0.0124*** | -0.0216 | -0.0235** | -0.2086*** |
| | (-2.68) | (-1.14) | (-2.31) | (-66.37) |
| Treated×Reform2007 | -0.0055 | 0.0506*** | -0.0038 | 0.0482 |
| | (-1.35) | (2.63) | (0.93) | (0.90) |
| Intercept | 0.0425*** | 0.0892*** | 0.0277 | 0.2032*** |
| | (6.12) | (3.36) | (1.25) | (3.52) |
| *Other controls* | Yes | Yes | Yes | Yes |
| *Firm FE* | Yes | Yes | Yes | Yes |
| *Sector×Year FE* | No | No | Yes | Yes |
| $R^2$ | 0.02 | 0.02 | 0.13 | 0.20 |
| *N* | 11,733 | 2,383 | 11,733 | 2,383 |
| Z-score | −2.95*** | | −0.96 | |

## APPENDIX 2 – Impact Threshold analysis for omitted confounding factors

Regression coefficients cannot be interpreted as causal if the relationship can be attributed to an alternative mechanism. One may control for the alternative cause through an experiment (e.g., with random assignment to treatment and control), or by measuring a corresponding confounding variable and including it in the model. Unfortunately, there are some circumstances when it is not possible to control for all potentially confounding variables. Frank (2000) and Frank *et al.* (2013) had proposed a method known as impact threshold for confounding variables (ITCV, in short) for testing the robustness of causal inference by identifying the impact threshold of a confounding variable.

We ran the ITCV analysis for the key explanatory variable, namely *Treated×Reform2007*, which could be potentially confounding especially if the treatment is driven by some unobserved characteristics of the firm that may cause a spurious association between the outcome and the treatment. For example, the IV may be influenced by the unobserved time-varying characteristic of the firm management to adhere to the reform or to get exempted from it, which could bias the estimates of our outcomes.

To eliminate this possibility, we obtain the impact threshold of a confounding variable for our baseline estimates shown in Tables 3 and 4. The threshold represents the effect size where the benefits of the adoption of the regulation outweigh its costs. The more the estimate exceeds the threshold, the more robust the inference is with respect to that threshold. The ITCV analysis, thus, enables us to determine how strong the effect of a hypothetical confounding variable would have to be to overturn current inferences.

For each potentially confounding variable, we calculate the percentage of observations that has to be biased in order for endogeneity to invalidate the inference. First, we ran the ITCV analysis for the key explanatory variable, namely, the firm's link with the local government because of its potential endogeneity to determining the likelihood of fully privatization in equation (1) as summarized in Table 3. Focusing on our preferred estimates of the interaction term Connected × Reform2007 shown in columns (2) and (4) of Table 3, we obtained the following threshold estimates: (i) column (1): to invalidate the inference, 56.52% (9,063) of the cases would have to be replaced with cases for which there is an effect of zero. (ii) column (2): to invalidate the inference, 45.31% (4,781) of the cases would have to be replaced with cases for which there is an effect of zero. Column (3): to invalidate the inference, 29.18% (2,687) of the cases would have to be replaced with cases for which there is an effect of zero. Column (4): to invalidate the inference, 25.28% (2,310) of the cases would have to be replaced

with cases for which there is an effect of zero. These high thresholds, all in excess of 10% observations, required for invalidating columns (1)–(4) estimates of Table 3 regarding firm's connection with the local government after 2006 would allay concerns for endogeneity.

Next, we do the same for the key explanatory variable, namely, Treated×Reform2007, in equation (2), which could be potentially be confounding especially if the treatment is driven by some unobserved factors that may cause a spurious association between the outcome and the treatment. For partially privatized firms, we found that the treated firms had significantly lower Tobin's Q after the adoption of the NTS (as per Table 4). The ITCV analysis indicates that to invalidate this inference, 27.35% (3,034) of the cases would have to be replaced with cases for which there is an effect of zero. For fully privatized firms we obtained a significantly positive effect on Tobin's Q among treated firms after the adoption of the NTS reform (see Table 6). The sensitivity analysis using ITCV indicates that to invalidate this inference, 28.07% (724) of the cases would have to be replaced with cases for which there is an effect of zero. Since both the threshold estimates are sizeable, these estimates from an ITCV analysis further validate the robustness of our baseline inferences.

## Online Appendix References

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