The Real Effect of Privatization: Evidence from China's Split Share Structure Reform

ABSTRACT

We examine the real effect of privatization in terms of technological innovation. To address endogeneity concerns, we explore plausibly exogenous variation in privatization generated by China's split share structure reform, which mandatorily converts non-tradable shares to be freely tradable and opens up the gate to the privatization of state-owned enterprises. Using a difference-in-differences approach, we find that privatization prospects have a positive effect on innovation. Better interest alignments between controlling and minority shareholders and enhanced stock price informativeness appear two plausible underlying mechanisms. Our paper sheds new light on the real effect of privatization and has important implications for policymakers.

Key Words: Privatization, innovation, interest alignments, stock price informativeness

JEL number: G30; G38; O32; L33

1. Introduction

Technological innovation is undoubtedly a critical driver of a country's economic growth (Solow, 1957) and competitive advantages (Porter, 1992).[1] An emerging literature takes up the task of trying to understand what drives innovation. Although this literature documents positive and negative empirical links between innovation and various country-, market-, industry- and firm-level characteristics, it has largely ignored the link between an important aspect of a country's public policy, privatization, and technological innovation. In this paper, we contribute to this nascent literature by examining how privatization affects firm innovation.

There has been an intensive debate among academics, practitioners, and policy makers in the past a few decades about the economic impact of privatization, which is generally defined as the deliberate sale by a government of state-owned enterprises (SOEs) or assets to private economic agents (Megginson, 2010). Advocates claim that privatization removes market frictions, improves risk sharing, lowers agency costs, and facilitates efficient resource allocations. As a result, privatization improves productivity and economic efficiency.

However, critics of privatization argue that privatization leads to social and economic instability, declines in national economic growth, acuter expropriation of minority shareholders by large shareholders, and asset sales by governments at excessively low prices (e.g., Hoff and Stiglitz, 2004). For example, Newbery and Pollitt (1997) show that consumers and government lose during the privatization of the Central Electricity Generating Board in UK. Florio (2004) focuses on the British privatization program and argues that the program's negative impact on the distribution of income and wealth might entirely negate the benefits the program may bring.[2] Moreover, some anecdotes such as the failed market reform in

[1] According to Rosenberg (2004), 85% of economic growth could be attributable to technological innovation. Chang et al. (2013) show that an increase in patent stock per capita by one standard deviation portends a 0.85% increase in GDP growth.

[2] Megginson (2006) provides counter arguments and points out a few weaknesses of Florio (2004)'s analyses.

Russia in the 1990s and China's failed attempts of privatization in the 1990s and early 2000s provide support for the critics' arguments. Megginson and Netter (2001) and Megginson (2010) provide comprehensive surveys of the privatization literature.

While there are likely merits to both sides of these arguments, in practice it is difficult to identify the causal effect of privatization on the real economy, such as technological innovation, due to its endogenous nature. First, a sample of traditional share issue privatizations (SIPs) by definition biases towards the very largest firms sold during the privatization program, causing a selection bias concern. Second, comparing innovation output of privatized firms and SOEs could result in misleading conclusions because of the fundamental but unobservable differences between these two groups of firms. Finally, expected changes in a firm's innovation output may cause its inclusion in the privatization program, leading to a reverse causality concern. Therefore, a correlation between privatization and innovation output may tell us little about the causal effect of privatization on innovation.

In this paper, we explore a quasi-natural experiment, the split share structure reform commenced in 2005 in China (hereafter, the share reform), which provides plausibly exogenous variation in privatization to tackle the above endogeneity problem. The share reform allows previously non-tradable shares, including those of SOEs held by the Chinese government, to be freely traded on stock exchanges. Thus, it effectively removes both legal and technical obstacles of transferring state-owned shares to public investors, opening up the gate to further privatization. Taking advantage of this unique setting, we attempt to provide the first rigorous empirical study that examines the causal effect of privatization prospects on firm innovation.

China's privatization process proceeds in several phases with two major milestones involving financial markets. The first major milestone is the establishment of two exchanges,

the Shanghai Stock Exchange and the Shenzhen Stock Exchange, in early 1990s. From the very beginning, the Chinese government chooses to impose the split share structure on stocks listed on these two exchanges. Under the split share structure, about two-thirds of domestically listed A-shares are not publicly tradable although their holders have identical voting and cash flow rights as holders of tradable shares. Typically, state and legal persons are holders of non-tradable shares while domestic institutional and individual investors as well as foreign individual investors are holders of tradable shares.

Over the years, the Chinese government realizes that the split-share ownership creates obstacles to the functioning and development of China's financial markets.[3] After a few failed privatization attempts (see, e.g., Liao, Liu and Wang (2014) for details), in April 2005, the Chinese government initiates the split share structure reform, the second major milestone for China's privatization. The share reform involves mandatory conversion of all non-tradable shares into tradable shares, subject to shareholder approvals and appropriate compensation to holders of tradable shares. Specifically, the share reform specifies a time period during which large (and typically controlling) shareholders of Chinese listed firms are required to convert their previously non-tradable shares into shares that are freely tradable on stock exchanges. We provide a detailed discussion of the institutional details of China's secondary privatization in Section 3.

The share reform is likely to generate exogenous variation in privatization and provides a unique opportunity to examine the effect of privatization on innovation because of its three important features. First, the share reform is initiated for reasons other than promoting technological innovation. According to the blueprint of the share reform, *Some Opinions of the State Council on Promoting the Share Reform, Opening, and Steady Growth*

[3] The typical concerns include reduced information efficiency of stock prices, conflict of interest between tradable and non-tradable shareholders, ineffective corporate governance because of lack of the market for corporate control, and increased speculation due to easy stock price manipulation.

of Capital Markets, issued by the State Council on January 31, 2004, the goals of the share reform are to optimize ownership structure, improve corporate governance, increase capital returns, and promote financial market development. Based on our earlier discussion about the institutional background of the share reform, it is clear that the goal of the share reform is mainly to resolve the split-share structure that is stemmed from the transition of China's economy from a planned economy to a market-oriented economy rather than promoting or demoting innovation. Hence, China's share reform provides a quasi-natural experiment that is exogenous to firm innovation.

Second, the share reform is mandatory. The China Securities Regulatory Commission (CSRC) sets August 2005 as the start date and all Chinese firms are expected to finish the reform by the end of 2006 (Firth, Lin, and Zou, 2010). The mandatory nature of the share reform means that no firms can endogenously choose whether to and when to convert non-tradable shares. Instead, the actual timing of the reform depends on the time required to implement and complete the reform procedures, i.e., the time it takes to communicate with shareholders and to obtain the necessary votes.

Finally, the share reform is carried out simultaneously on both SOEs and non-SOEs by removing the split-share structure. It allows us to use non-SOEs as a benchmark when evaluating the innovation performance of SOEs. For both types of firms, the conversion of non-tradable shares to tradable shares is the same, except that non-tradable shares of SOEs are held mainly by governments and those of non-SOEs are held mainly by private investors. Thus, by comparing the post-reform innovation output of SOEs (i.e., the treatment firms) with those of non-SOEs (i.e., the control firms), we can separate out the net effect of privatization on innovation, uncontaminated by other unobservable firm characteristics or economic conditions.

One concern about our strategy that uses China's share reform to identify the effect of privatization on innovation is that the selection of firms or industries that are state-owned, and therefore the firms or industries being privatized, is not random. While this is a reasonable concern and a core challenge in all privatization studies, we include firm fixed effects in all of our tests to absorb time-invariant unobservable firm characteristics that may be correlated with the selection of state ownership, which helps to mitigate this concern.

It is important to note that while it is unclear when and how the Chinese government will completely privatize SOEs after the share reform when the reform is started, ownership by the largest shareholders in SOEs (typically the Chinese government) on average drops from 49.3 percentage points before the share reform to 39.5 percentage points, a 19.9% decrease (= (49.3% - 39.5%)/49.3%), four years after the share reform in our sample. This significant reduction in government ownership occurs because the share reform imposes a market-based compensation negotiation scheme in which non-tradable shareholders (mainly the government) offer an average of 0.305 shares for each tradable share held by their shareholders. As a result, every tradable share before the reform on average converts to 1.305 shares after the reform (Li et al., 2011). Hence, the 2005 share reform removes legal and practical barriers of in-depth privatization, which opens up the gate for China's secondary privatization.

To make good use of plausibly exogenous variation in privatization generated by the share reform, we use a difference-in-differences (DiD) approach to analyze how innovation output of SOEs changes surrounding the share reform compared with that of non-SOEs. After performing a variety of diagnostic tests to ensure that the parallel trends assumption, the key identifying assumption of the DiD approach, is satisfied, we find a positive effect of privatization prospects on firm innovation in both a univariate comparison and a multivariate analysis framework. Our regression results suggest that the expectation of privatization leads

to a 13.4% increase in patent quantity and an 11.5% increase in patent quality for SOEs compared with non-SOEs.

We next perform additional robustness and placebo tests for our baseline DiD analysis. First, although the share reform provides an exogenous shock to privatization, it is still possible that our results are driven by reverse causality, i.e., expected changes in innovation productivity could trigger the share reform. While our discussion on the institutional background of the share reform in Section 2 suggests this alternative argument is unlikely, to address this concern, we use the methodology developed in Bertrand and Mullainathan (2003) and examine the dynamics of innovation output surrounding the share reform. We do not find a prior trend in innovation output, but observe a larger increase in innovation output of SOEs compared with that of non-SOEs only after the share reform. Second, to address the concern that endogenous timing in implementing and completing the reform may bias our results, we re-run our DiD analysis, using 2005 when the law is passed as the reform year.[4] We continue to observe the positive effect of privatization on firm innovation. Third, to address the concern that our DiD results could have been driven by chance, we run a placebo test that randomly and artificially assigns our sample firms into SOE and non-SOE groups and repeat the DiD regressions to explore the effect of the share reform. We find that the DiD estimates obtained from this randomization test are on average zero.

We further identify two possible underlying mechanisms through which privatization encourages firm innovation: better interest alignments between controlling and minority shareholders and enhanced stock price informativeness. To this end, we examine how cross-sectional variation in related-party transactions and stock price informativeness before the reform alter our main results. We find that the positive effect of privatization on firm innovation is more pronounced when a firm has a larger volume of related-party transactions

[4] The CSRC and stock exchanges impose restrictions on the time frame over which firms should complete their reforms. Yet, managers may still have some discretion in influencing the actual timing of a firms' reform.

and lower stock price informativeness before the reform. Coupled with the findings that related-party transactions of SOEs decrease more than those of non-SOEs and stock price informativeness of SOEs increases more than that of non-SOEs after the reform, our evidence suggests that better interest alignments between controlling and minority shareholders and enhanced stock price informativeness are two plausible underlying mechanisms.

The rest of the paper organizes as follows. Section 2 discusses the related literature. Section 3 presents background information about China's split share structure reform. Section 4 describes sample selection and reports summary statistics. Section 5 presents the main results. Section 6 explores possible underlying mechanisms through which privatization affects firm innovation. Section 7 concludes.

2. Relation to the existing literature

Our paper contributes to two strands of literature. First, our paper contributes to the literature on privatization. There has been a large literature documenting various aspects of privatization in both transition and non-transition economies. One of the first studies in this literature is Megginson, Nash and van Randenborgh (1994) that examines the post-privatization performance of firms divested through SIPs. Megginson and Netter (2001) and Megginson (2010) provide excellent surveys of earlier studies in this literature. Recent studies focus on evaluating the consequences of privatization programs in emerging countries. Using information from Indian privatization programs, Dinc and Gupta (2011) show that profitable firms, firms with a lower wage bill, and firms in region where the governing party faces less competition from opposition parties are more likely to be privatized early. Gupta (2005) finds that privatization in India has a positive effect on firm profitability, productivity, and investment, consistent with the argument that while the government still remains the controlling owner in partial privatization programs, the stock market can play a role in monitoring and rewarding managerial performance. In a cross-country setting, Goyal,

Jategaonkar, Megginson, and Muckley (2014) show that the major determinants of dividend payout premium of firms after privatization that is documented by the previous literature are improved firm operating performance and mitigated agency costs.

A few recent studies use China's share reform to tackle various aspects of privatization. Li et al. (2011) focus on the compensation paid by holders of non-tradable shares to those of tradable shares. They find that the compensation size is positively associated with the gain in risk sharing and highlight the role of risk sharing in China's privatization. Firth, Lin and Zou (2010) find that state and mutual fund ownership have contrasting effects on the compensation ratio. Chen, et al. (2012) find that the share reform leads to better incentive alignments between controlling and minority shareholders and relaxes financial constraints. Liao, Liu, and Wang (2014) show that privatization allows SOEs to achieve an improvement in output, profitability, employment, productive efficiency, and governance. For example, they find that, after the share reform, SOEs exhibit a 6% higher increase in operating revenue than non-SOEs in the same size and industry group. This literature, however, has ignored how a critical driver of economic growth, innovation, is affected by privatization programs. As a result, this literature is silent on the channel through which privatization spurs economic growth.

Our paper is also related to the emerging literature on the financing and motivation of innovation. Holmstrom (1989) shows that innovation activities are inherently different from routine tasks, and hence may not mix well with routine tasks in an organization. Manso (2011) theoretically shows that managerial contracts that tolerate failure in the short run and reward success in the long run are best suited to motivate innovation activities. Empirical evidence shows that various firm characteristics and economic forces affect managerial incentives of investing in innovation. For example, larger institutional ownership (Aghion et al., 2013), corporate instead of independent venture capitalists (Chemmanur et al., 2014), debtor-

friendly bankruptcy laws (Acharya and Subramanian, 2009), lower labor union power (Bradley et al., 2015), lower stock liquidity (Fang et al., 2014), and private instead of public equity ownership (Lerner et al., 2011) all enhance managerial and employees' incentives to innovate.[5] However, the existing literature has been focused on the effect of market- and firm-characteristics on innovation but been silent on how a government's public policy, such as privatization programs, affects innovation activities. Our paper contributes to this line of research by providing the first rigorous empirical analysis to tackle this issue.

3. Background of China's secondary privatization in 2005

China establishes the Shanghai and Shenzhen stock exchanges in 1990 and 1991, respectively, marking the start of China's SIPs. SOEs go public and issue tradable shares to institutional and individual investors and these tradable shares are divided into A-shares (primarily for domestic investors) and B-shares (for foreign shares). However, because the government wants to maintain its control or influence over SOEs, it retains substantial ownership in the majority of listed SOEs by holding non-tradable shares that are prohibited from trading in the secondary market. Hence, while the SIPs is considered as the first wave of privatization in China, it is at best partial privatization because it transfers only a small portion of SOE ownership to the public and does little to reduce the state's dominating roles in making corporate decisions.

Over the years since then, the Chinese government recognizes that the split-share ownership creates obstacles to the functioning and development of China's financial markets. After a few failed privatization attempts, in April 2005, the Chinese government initiates the split-share structure reform to liberalize state-owned shares in full circulation. The goal of the share reform, stated by the CSRC, is to "establish modern corporate governance structure,

[5] Other studies have examined the effects of product market competition, financial analysts, general market conditions, firm boundaries, banking competition, and failure tolerance on corporate innovation (e.g., Aghion, Bloom, Blundell, Griffith and Howitt, 2005; He and Tian, 2013; Nanda and Rhodes-Kropf, 2013; Cornaggia et al., 2015; Seru, 2014; Tian and Wang, 2014).

improve capital allocation and utilization efficiency, optimize capital structure, and improve return to investment." By the end of 2011, 99% of firms in our sample complete their reforms. Although the share reform is mandatory, there are almost no regulatory interventions in the negotiation process between tradable and non-tradable shareholders. Thus, the share reform represents an exogenous policy shock but with no direct government interventions.

The share reform involves negotiations between tradable and non-tradable shareholders and generally takes several steps. First, after more than two-thirds of non-tradable shareholders agree to reform, the board of directors authorizes the management to set up a tentative reform schedule with the stock exchange. Next, non-tradable shareholders propose a reform plan that specifies the considerations they intend to pay to tradable shareholders in exchange for the permission to convert. The considerations may take the form of cash, stocks, stock options, and/or warrants. Controlling shareholders may also include plans for organizational changes and asset restructuring or business combinations with other affiliates controlled by controlling shareholders. In addition, controlling shareholders may make promises on dividend payments and future capital injections to make the proposal more attractive to tradable shareholders.

Tradable shareholders then review the proposal and provide feedback. After a few rounds of negotiations between tradable and non-tradable shareholders on proposal terms, tradable shareholders vote on the share reform plan. The shares of the listed firm stop trading on the voting date. If the proposal is approved by at least two-thirds of the tradable shareholders, then the proposal is submitted to the CSRC for approval. After the CSRC's approval, the reform plan becomes effective and trading resumes. The non-tradable shares will remain non-tradable for 12 months to stabilize the stock market. Finally, after the 12-month lockup period, non-tradable shares can be traded as normal tradable shares. However, non-tradable shareholders who hold more than 5% of a firm's outstanding shares can sell no

more than 5% of shares outstanding during the 12 months after the lockup period or no more than 10% during the 24 months after the lockup period.

Although the share reform does not immediately convert SOEs to pure private entities, it allows their shares that are initially not tradable to be freely tradable on exchanges and generates expectations for further privatization by removing the legal and technical barriers to transferring state-owned shares to public investors (Liao et al., 2014). The privatization expectations will, in turn, alter SOEs' investment strategies such as their investment in innovation.

4. Sample construction and descriptive statistics

We obtain information to construct our sample firms from several sources. Financial information about Chinese listed firms is retrieved from the China Stock Market & Accounting Research (CSMAR) Database. Firm ownership data that are used for defining SOEs and non-SOEs are obtained from the CSMAR database and the China Center for Economic Research (CCER) Database. Patent grant information is obtained from the State Intellectual Property Office of China (SIPO). Following the procedure in Bessen (2009), we match patent data and firm financial data by firm names. We also manually check for matching accuracy.

Our final sample consists of 13,977 firm-year observations for 1,289 non-financial firms, including 801 SOEs and 488 non-SOEs, over a 12-year period from 2000 to 2011. We start our sample period from 2000 because China adopts a consistent and unified set of accounting standards for publicly traded firms from the year. Because our purpose is to examine the differential effects of the share reform on existing SOEs and non-SOEs, we require that the sample firms to be listed on the Shanghai or Shenzhen Stock Exchanges at the end of 2004, the year prior to the share reform. By the end of 2011, all sample firms except for 6 SOEs and 2 non-SOEs have completed their share reforms.

4.1 Measuring innovation

There are three types of patents granted under the Chinese patent law: invention patents, utility model patents, and design patents. Chinese invention patents are granted for a new technical solution relating to a product, a process, or an improvement, which is similar to the U.S. utility patents. The Chinese utility model patents are granted for new and practical technical solutions related to the shape and/or structure of a product, which is similar to European and Japanese utility model patents. The utility model patents protect new, functional aspects of a product that do not meet the higher inventiveness level required for an invention patent. The Chinese design patents are granted for new designs related to the shape, pattern or their combinations, or the combination of color, shape, and/or pattern that is aesthetically pleasing and industrially applicable. In other words, a design patent protects the "look" of a product that makes it recognizable. The SIPO database covers all three types of patents. For each patent, SIPO provides information on patent application date, application ID, publication ID, granting date, and patent ID along with the names of inventors and applicants.

Because design patents involve limited technological advancements, we construct our innovation outcome measures using only invention and utility model patents. We extract invention and utility model patent applications filed (and eventually granted) by our sample firms, including those filed by their subsidiaries, from the SIPO database and use them to construct two measures for a firm's innovative outcomes.[6] Our first measure of innovation output is, *Pat,* defined as the total number of invention and utility model patents that are applied and eventually granted to a firm in a year. We define the variable by application year rather than by granting year because previous research shows that application year is a better proxy that captures the actual time of innovation (Griliches, Pakesn and Hall, 1988). To

[6] Our data cover all Chinese patents granted by the end of September, 2014.

address concerns related to variable skewness, we use the natural logarithm of one plus *Pat* as the main innovation outcome measure in our analysis.

A potential concern about this variable is that it measures only the quantity but not the quality of innovation. It is possible that, after the share reform, firms switch their innovation strategy to producing a larger number of patents at the expense of quality. Then the conclusion that an increase in *Pat* indicates improved innovation performance could be misleading. We therefore need a measure that captures patent quality. Existing innovation literature uses the number of future citations a patent receives as a measure for patent quality, assuming that more influential and higher impact patents receive a larger number of subsequent citations. A practical difficulty we face in this study is that the SIPO database does not provide sufficient and reliable information on citations for Chinese patents. Thus, we choose to measure a patent's quality based on its originality. Based on the Chinese patent law, invention patents are the most original ones among all three types of patents. As a result, we use *InvPat*, defined as the number of invention patent applications filed by (and eventually granted to) a firm in a year, as a proxy for a firm's innovation quality. To address issues related to skewness, we use the natural logarithm of one plus *InvPat* in our analysis.

4.2 Defining SOEs and control variables

We define a firm's state-owned enterprises (SOEs) status based on its state ownership information in the year prior to the firm's share reform. We obtain ownership information from the CSMAR database. We first identify civilian-run firms by matching our sample firms with the CCER civilian-run firm database and label them as non-SOEs. We then check whether the largest controlling shareholders of the remaining firms are affiliated with the Chinese government by manually searching their background information through annual

reports and public press.[7] We define a firm in our remaining sample to be a SOE if its largest shareholder is affiliated with the Chinese government and holds at least 25% of the firm's outstanding shares.[8] This procedure identifies 801 SOEs and 488 non-SOEs in our sample.

As discussed in Section 3, China's secondary privatization is featured with mandatory conversion of non-tradable shares of listed firms to tradable shares. We define the share reform completion year as the year in which a firm's non-tradable share conversion proposal is finalized.

Following the innovation literature, we control for a vector of firm and industry characteristics that may affect a firm's innovation output in our analysis. Our control variables include firm size, age, leverage, asset tangibility, profitability (measured by ROA), and sales growth rate. Table 1 Panel A provides detailed definitions of the variables used in our analysis.

4.3 Descriptive statistics

Table 1 Panel B reports the summary statistics for our sample. To mitigate the effect of outliers, we winsorize all variables at the 1st and 99th percentiles.

[Insert Table 1 Here]

Our sample firms on average generate 7.3 patents per year. 2.3 of them are invention patents that are equivalent to utility patents in the US patent system. He and Tian (2013) report that an average US firm in their sample generates 9.8 utility patents per year. Thus, Chinese listed firms appear to be less innovative than US listed firms. A typical sample firm has been listed on the exchanges for 8 years and has annual sales of RMB 3 billion. It has a ROA of 2.3% and a sales growth rate of 24.8% per year. Following Bates, Kahle, and Stulz

[7] We follow this procedure to identify SOEs because the state ownership information provided by the CSMAR database is not very reliable. There are misclassifications or missing values of state and non-state ownership in the CSMAR database.

[8] We use the 25% threshold to ensure that the government has a significant influence on the listed firms. Our main findings do not change if we set the threshold for defining SOEs to be 20%, 30%, or 50%.

(2009), we define net leverage variable as net debt (i.e., long-term debt minus cash) scaled by the sum of net debt and market value of equity. The mean net leverage of our sample firms is -8.2%, suggesting that Chinese listed firms tend to hold slightly more cash than long-term debt during our sample period.[9]

In Figure 1, we present innovation output of SOEs and non-SOEs surrounding the share reform. The solid line in Panel A represents the average total number of invention and utility model patents produced by SOEs, and the dash line displays the number of invention and utility model patents produced by non-SOEs. The number of patents is trending closely in parallel for the two groups in the four years leading up to the share reform, suggesting that the parallel trends assumption of the DiD approach is likely satisfied. However, the gap between the two lines widens after the share reform because SOEs increase their patent generation at a faster pace than non-SOEs. Panel B displays the number of invention patents produced by the two groups of firms. Non-SOEs increase their invention patents at a relatively stable speed over time. The number of invention patents produced by SOEs initially grows at a slower speed than non-SOEs. However, SOEs increase their invention patent production more rapidly after the share reform, which widens the difference in invention patent counts between these two groups of firms. The figures in both panels show that, after the share reform, SOEs enhance their innovation productivity more relative to non-SOEs.

[Insert Figure 1 Here]

5. Main results

A standard approach to evaluate the effect of China's privatization on innovation is to run an OLS estimation that regresses a firm's innovation output on a variable that captures the privatization program in China. However, as we discussed before, this approach suffers

[9] Bates, Kahle and Stulz (2009) show that U.S. firms hold more cash than long-term debt after 2004. It appears that Chinese firms exhibit a similar pattern in our sample.

from sample selection and endogeneity concerns. First, a sample of traditional SIPs is likely to bias towards very large firms sold during the privatization program, causing a selection bias concern. Second, fundamental but unobservable differences between SOEs and non-SOEs that are related to innovation output could lead to spurious or biased inferences. Third, expected changes in a firm's innovation output could cause its inclusion in the privatization program, leading to concerns on reverse causality. Therefore, a correlation between privatization and innovation output obtained from a naïve OLS regression tells us little about the causal effect of privatization on innovation.

Our identification strategy is to explore a quasi-natural experiment, China's split share structure reform, that represents an exogenous shock and provides plausibly exogenous variation in privatization, We adopt a difference-in-differences (DiD) approach to examine the effect of privatization prospects on innovation. The DiD approach has some key advantages. First, the DiD methodology rules out omitted time trends that are correlated with privatization and innovation in both SOEs (the treatment group) and non-SOEs (the control group). Second, the DiD approach controls for constant unobserved differences between the treatment and the control group that may bias our estimation. The quasi-natural experiment setting also has a key advantage: the share reform takes place in different times for different firms. This feature allows us to avoid a common identification difficulty faced by studies with a single exogenous shock, namely, potential omitted variables that coincide with the shock could directly affect firm innovation. In this case, the causal effect of privatization is still not identified.

We start with a univariate DiD analysis in a sample of SOEs and matched non-SOEs that are selected from the propensity score matching algorithm in Section 5.1. We then perform the DiD tests in a multivariate regression framework in Section 5.2. In Section 5.3, we perform additional robustness and placebo tests for our main DiD analysis.

5.1 Univariate DiD analysis

For each SOE, we select a matched non-SOE using a propensity score matching algorithm. When applying the propensity score matching procedure, we first estimate a probit model based on all sample firms with non-missing matching variables in the year prior to the share reform.

In the probit model, the dependent variable is a SOE dummy that equals one for SOEs and zero otherwise. We include a vector of firm characteristics in the probit regression, including firm size, net leverage, firm profitability (ROA), sales growth, firm age, asset tangibility, and patent growth. We define patent growth as the average annual change in *Pat* over three years prior to the share reform. We include this variable to ensure the satisfaction of the parallel trends assumption of the DiD approach.[10] All other variables are measured at the fiscal year end before the share reform. In addition, we include industry and year dummies in the probit model.

We report the probit model results in Column (1) of Table 2 Panel A (labeled as "Pre-Match"). The estimation results suggest that the specification captures a significant amount of variation in the choice variable, as indicated by a pseudo-R^2 of 10.1% and a p-value from the χ^2 test of the overall model fitness well below 0.001. We then perform a nearest-neighbor propensity score matching procedure, using the predicted probabilities (propensity scores) obtained from the estimation in Column (1). Specifically, we match each SOE firm (labeled as a treatment firm) to a non-SOE firm (labeled as a control firm) with the closest propensity score. We end up with 418 one-to-one pairs of matched firms (836 observations).

[Insert Table 2 Here]

[10] As Lemmon and Roberts (2010) point out, the parallel trends assumption does not require the level of outcome variables (innovation variables in our setting) to be identical across the treatment and control firms or across the two regimes, because these distinctions are differenced out in the estimation. Instead, this assumption requires similar trends in the innovation variables during the pre-reform regime for both the treatment and control groups.

Because the validity of the DiD estimate critically depends on the satisfaction of the parallel trends assumption, we undertake three diagnostic tests to check whether this assumption holds. First, as we discussed before, Figure 1 shows that the number of patents is trending closely in parallel for both SOEs and non-SOEs in the four years leading up to the share reform. This observation suggests that the parallel trends assumption of the DiD is likely satisfied.

Second, we re-estimate the probit model using the matched sample and report the estimation results in Column (2) of Table 2 Panel A (labeled as "Post-Match"). None of the independent variables is statistically significant. In particular, the insignificant coefficient for pre-reform patent growth suggests that the treatment and control firms have a similar growth rate in innovation outcomes before the share reform. In addition, the pesudo-R^2 drops dramatically from 10.1% prior to the matching to 1.3% post the matching, and the χ^2 test for the overall model fitness suggests that we cannot reject the null hypothesis that all of the coefficient estimates of independent variables in column (2) are zero (i.e., the *p*-value is 0.656).

Finally, we report the univariate comparisons in firm characteristics between the treatment and control firms and their corresponding *t*-statistics in the year before the share reform in Table 2 Panel B. None of the observed differences between the treatment and control firms' pre-reform characteristics is statistically significant. In particular, the univariate comparison for the pre-reform patent growth is statistically insignificant and economically small, suggesting the satisfaction of the parallel trends assumption.

Overall, the diagnostic test results show that the propensity score matching process has removed meaningful observable differences in pre-reform characteristics between the treatment and control groups and the parallel trends assumption is not violated. As a result,

the matching procedure increases the likelihood that the observed difference in changes of innovation output between SOEs and non-SOEs is caused by the share reform.

Table 2 Panel C reports the univariate DiD test results. We calculate the DiD estimator for *Ln(Pat)* by first subtracting the logarithm of one plus the total number of invention and utility model patents a firm generates each year during the four-year period preceding the share reform from that during the four-year period after the share reform for each treatment and control firm. Columns (1) and (2) present the average differences for, respectively, the treatment and control group. Column (3) reports the DiD estimation of *Ln(Pat)*, which is the difference between columns (1) and (2). The DiD estimate for *Ln(InvPat)* is calculated in a similar way and is reported in the second row of Panel C.

The results reported in Panel C columns (1) and (2) show that both the treatment and control firms experience improvements in innovation output after the share structure reform. More importantly, the DiD estimates of the innovation output variables reported in column (3) are all positive and statistically significant at the 5% or 1% level. This finding suggests that the increase in innovation output is larger for the treatment group than for the control group after the share reform. The economic effect is sizable: for example, the DiD estimate for *Ln(InvPat)* is 0.203, suggesting that, compared to the average change in *Ln(InvPat)* in our matched sample (0.595), the treatment firms experience an approximate 33.8% larger increase in invention patent counts than matched control firms over a nine-year period surrounding the share reform.[11] The magnitude of the DiD estimate for *Ln(Pat)* represents a large economic significance.

The evidence from the univariate DiD tests suggests that SOEs experience a larger increase in their innovation output compared to non-SOEs after the share reform. Thus, the

[11] In Table 3 Panel C, the changes in *Ln(InvPat)* are 0.701 and 0.498, respectively, for the control and treatment groups. Thus, the average change in *Ln(InvPat)* for the combined propensity-score-matched sample is 0.595 = (0.701 + 0.498)/2.

privatization expectation generated by the share reform appears to have a positive effect on Chinese firm's innovation output.

5.2 Multivariate DiD analysis

In this section, we perform the DiD test in a multivariate regression framework. Specifically, we estimate the model in equation (1) with the full panel of sample observation for all sample firms.

$$y_{i,t+4} = \alpha_i + \beta SOE_i \times Post_{i,t} + \gamma' Z_{i,t} + \delta_t + \varphi_i + \varepsilon_{i,t} \quad (1)$$

where *i* indexes firm and *t* indexes year. The dependent variable $y_{i,t}$ represents either *Ln(Pat)* or *Ln(InvPat)* measured at year *t+4*.[12] SOE_i is a dummy variable that takes the value of one for SOEs and zero for non-SOEs. $Post_{i,t}$ is a dummy variable that equals one for firm-year observations after a firm completes the share reform and zero otherwise. *Z* is a vector of control variables that may affect a firm's innovation output and is defined in Table 1 Panel A. We include year fixed effects,δ_t, to account for time-specific shocks to a firm's innovation output and firm fixed effects, φ_i, to absorb any time-invariant unobservable firm characteristics that may bias the results. We cluster standard errors by firm in all regressions.

The coefficient estimate of $SOE_i \times Post_{i,t}$ is the DiD estimate that captures the causal effect of privatization prospects on innovation. If the share reform leads SOEs to achieve a larger increase in innovation output than non-SOEs, this coefficient should be positive and statistically significant. Note that we include only the interaction term $SOE_i \times Post_{i,t}$ in the regressions, but not the two dummy variables themselves because these two variables are absorbed by firm and year fixed effects, respectively.

[Insert Table 3 Here]

[12] We choose to use a four-year-ahead innovation output variable as the dependent variable because it generally takes time for innovation processes to generate observable outputs due to the fact that innovation represents a long-term investment in intangible assets. Our main results, however, do not change if we use the patent output variables two or three years ahead as the dependent variables.

Table 3 presents the results from estimating equation (1). In column (1), the dependent variable is the innovation quantity variable, *Ln(Pat)*. The coefficient estimate of the interaction term is positive and significant at the 1% level, consistent with the univariate analysis result, suggesting that the share reform increases innovation output more for SOEs compared with non-SOEs. The economic effect of the share reform on firm innovation output is sizable. The magnitude of the DiD coefficient estimate suggests that, compared to the innovation output prior to the share reform, SOEs exhibit a 13.4% larger increase in innovation output than non-SOEs four years after the share reform. The regression in column (2) takes the innovation quality measure, *Ln(InvPat)*, as the dependent variable. The coefficient estimate of the interaction term is positive and significant at the 1% level, suggesting that, compared to patent quality prior to the share reform, SOEs exhibit a 11.5% larger increase in innovation quality than non-SOEs four years after the share reform. This finding suggests that firms do not increase their innovation quantity at the expense of innovation quality. Instead, both innovation quantity and quality of SOEs exhibit a substantially larger increase than non-SOEs surrounding the share reform.

Overall, the evidence from both the univariate and multivariate analyses suggests that privatization expectation generated by the share reform appears to have a positive effect on firm innovation.

5.3 Robustness checks and placebo tests

In this subsection, we perform robustness checks and placebo tests to ensure that our DiD results are not driven by reverse causality, by endogenous timing in completing the share reform, and by chance.

First, as discussed earlier, although the share reform represents a plausibly exogenous shock to China's privatization prospects, it is still possible that our results are driven by reverse causality due to the concern that expected changes in innovation productivity trigger

the share reform. For example, the government may choose to launch the share reform in response to an improved prospect on innovative productivity. Another concern is that there may be some pre-existing trends in innovation output that is not captured by our visual check in Figure 1 between SOEs and non-SOEs that could drive our results even in the absence of the share reform.

To address this concern, following Bertrand and Mullainathan (2003), we examine the dynamics of innovation output surrounding the share reform by estimating the following model:

$$y_{i,t} = \alpha_i + \beta_1 SOE_i \times Before_{i,t}^{-1} + \beta_2 SOE_i \times Current_{i,t}^{0} + \beta_3 SOE_i \times After_{i,t}^{1} + \beta_4 SOE_i \times After_{i,t}^{2} + \beta_5 SOE_i \times After_{i,t}^{3} + \beta_6 SOE_i \times After_{i,t}^{4+} + \tau_1 Before_{i,t}^{-1} + \tau_2 Current_{i,t}^{0} + \tau_3 After_{i,t}^{1} + \tau_4 After_{i,t}^{2} + \tau_5 After_{i,t}^{3} + \tau_6 After_{i,t}^{4+} + \gamma' Z_{i,t} + \delta_t + \varphi_i + \varepsilon_{i,t} \quad (2)$$

where $Before_{i,t}^{-1}$ is a dummy variable that equals one if the observation is one year before a firm completes the share reform and zero otherwise, $Current_{i,t}^{0}$ is a dummy variable that equals one if the observation is in the share reform completion year and zero otherwise. Similarly, $After_{i,t}^{1}$, $After_{i,t}^{2}$, and $After_{i,t}^{3}$ are dummy variables that equal one if the observation is the first, second, and third year after a firm completes the share reform and zero otherwise, respectively. $After_{i,t}^{4+}$ is a dummy variable that takes the value of one for all years starting from the fourth year after the share reform and zero otherwise. All other variables have the same definitions as in equation (1). If there is a pre-existing trend in the innovation output of SOEs and non-SOEs, we should observe statistically significant coefficient estimates of β_1 and β_2.

[Insert Table 4 Here]

We report the results estimating equation (2) in Table 4.[13] In both regressions, the coefficient estimates of β_1 and β_2 are not significant, suggesting that SOEs and non-SOEs do not exhibit a significantly different trend in innovation output prior to the share reform.[14] In contrast, we find that the coefficient estimates of β_4, β_5, and β_6 are positive and significant, suggesting that our main results are robust. In addition, the dynamics of the effect (i.e., the positive effect of the share reform on innovation output starts to emerge only from the second year after the reform) are consistent with the notion that it takes time to observe innovation output improvement after the share reform, because innovation represents a long-term investment in intangible assets.

[Insert Table 5 Here]

Our second robustness test addresses a potential concern on endogenous timing in implementing and completing the reform. The share reform is mandatory in nature. On top of that, the CSRC and stock exchanges impose restrictions on the time periods over which firms should complete their reforms (Chen et al., 2012; Firth et al., 2010). As a result, firms have very limited discretion over timing. As argued by Chen et al. (2012), the actual timing of completing the reform depends largely on the time it takes to implement the procedures, such as communicating with shareholders and obtaining necessary votes. Thus, it is reasonable to believe that the timing of the reform is exogenous to firms' innovation output.[15] Nevertheless, to ensure that our results are robust and not driven by endogenous timing on completing the reform, we perform additional tests in Table 5 to address this potential concern.

[13] Note that there are more observations in Table 4 than those in Table 3. This is because the regressions in Table 3 require information about innovative outcomes 4 years ahead. As a result, the last 3 years of observations are not used in regressions in Table 3.

[14] In unreported analysis, we confirm that our results do not change if we include $SOE_i \times Before_{i,t}^{-2}$ as an additional control variable, in which $Before_{i,t}^{-2}$ is a dummy variable that takes the value of one if the observation is two years before a firm completes the share reform and zero otherwise.

[15] Another advantage of setting is that the share reform takes place in different times for different firms. This feature allows us to avoid a common identification difficulty faced by studies with a single plausible exogenous shock, namely, the existence of potential omitted variables coinciding with the shock that directly affect firm innovation.

The regressions in Table 5 Panel A are the same as those reported in Table 3, except that they take 2005, the year when the law is passed, as the reform year. By doing this, we impose a uniform reform year and examine whether our results are robust. In both columns, we obtain positive and significant coefficients for the DiD estimator. Thus, our evidence continues to support the positive effect of privatization on firm innovation even not allowing endogenous timing in completing the reform.[16]

The third test we do addresses the concern that our DiD results could have been driven by chance instead of reflecting a causal effect of privatization prospects. Hence, we conduct a placebo test by running simulations that artificially assign SOEs and non-SOEs status to our sample firms. Specifically, in each simulation, we randomly draw 801 "SOEs" from the pool of all firms that includes both SOEs and non-SOEs in the pre-reform year. We then treat the remaining 488 firms as "non-SOEs". We perform the DiD tests based on this simulated sample following the specifications in equation (1) and then repeat the simulation process 5,000 times.

[Insert Table 6 Here]

In Table 6, we summarize the distributions of the simulated DiD estimates (i.e., the coefficient estimates of *SOE*×*Post*) by reporting statistics including the mean, 5th percentile, 25th percentile, median, 75th percentile, 95th percentile, and standard deviation. We also report the corresponding *t*-statistics. Although the mean and median of simulated DiD estimates are positive, they are much smaller in magnitude than those reported in Table 3. In addition, their corresponding *t*-statistics is small and statistically insignificant. Therefore, we cannot reject the null hypothesis that the DiD estimates obtained from this placebo test are zero, which suggests that our main results are unlikely to be driven by chance.

[16] In untabulated tests, we address the concern on endogenous timing choice following the approach used by Chen et al. (2012). In particular, we control for the interaction terms between Post and the six timing factors used by Chen et al. (2012). We continue to observe positive and significant coefficient estimates of *SOE* ×*Post*. Thus, including the timing factors does not alter our main results.

Overall, the above robustness and placebo test results suggest that the documented positive effect of privatization prospects on firm innovation, using plausibly exogenous variation generated by China's split share structure reform, is unlikely to be driven by reverse causality, by endogenous timing in completing the reform, or by chance. Therefore, the positive effect of privatization prospects on firm innovation appears to be causal.

6. Possible underlying mechanisms

Having established a causal link between privatization prospects and firm innovation, in this section, we aim to further understand the underlying economic mechanisms through which privatization enhances firm innovation. We hypothesize that privatization encourages corporate innovation through two possible underlying mechanisms: better aligning interests between controlling and minority shareholders and improving stock price informativeness.

6.1 Interest alignments

The first plausible mechanism that allows privatization to promote innovation is better interest alignments between controlling and minority shareholders. As Grossman and Hart (1988) and Shleifer and Vishny (1997) point out, controlling shareholders have incentives to pursue their private benefits at the expense of minority shareholders. The split share structure exacerbates the conflict of interest between controlling and minority shareholders in SOEs. Because their shares are non-tradable, controlling shareholders of SOEs do not directly benefit from stock price appreciation. Thus, they have limited incentives to invest in long-term and value-enhancing projects, such as innovation, that can boost the firms' stock prices and market value. Instead, they may have incentives to spend corporate resources on their own agenda based on political concerns. For example, government-affiliated controlling shareholders could request SOEs to boost local employment or to engage in projects that are strategically important for the government although these activities are not at the best interest

of minority shareholders. Government-affiliated controlling shareholders could even directly exploit minority shareholders through various tunneling activities (Jiang, Lee and Yue, 2010; Jian and Wong, 2010).

One of main purposes of the share reform is to align the interests between tradable shareholders (who are typically minority shareholders) and non-tradable shareholders (who are typically controlling shareholders). Although privatization expectation triggered by China's share reform may not fully eliminate the conflict of interest between controlling and minority shareholders, it substantially alleviates the problem by providing controlling shareholders with more incentives to invest in value enhancing projects. This is because these controlling shareholders can benefit from stock price appreciation after the share reform.

To the extent that controlling shareholders of non-SOEs hold non-tradable shares, the share reform also helps to align the interests of controlling and minority shareholders of the non-SOEs. However, the incentive alignment effect should be stronger for SOEs than for non-SOEs because of two reasons. First, the controlling shareholders of non-SOEs typically hold tradable shares (besides non-tradable shares) even before the share reform, providing them incentives to invest in value enhancing projects. Second, controlling shareholders of non-SOEs have large economic stakes in firms, which provides them incentives to invest in long-term value enhancing projects. In contrast, although the government (principal) also has large economic stakes in SOEs, these SOEs are managed by government officials (agent) whose goals are mainly to pursue political promotions instead of firm value enhancements.

If aligning incentives between controlling and minority shareholders is an underlying economic mechanism through which privatization enhances firm innovation, the positive effect of privatization on innovation should be more pronounced for firms with more serious conflicts of interest before the share reform. Following Liao, Liu, and Wang (2014), we use related-party transactions as a proxy for potential conflicts of interest between controlling and

minority shareholders in Chinese SOEs. Through related-party transactions, firm resource can be transferred between listed firms and their controlling shareholders. Existing literature suggests that related-party transactions are one of the most widely used rent-seeking methods by controlling shareholders in China (e.g., Cheung, Rau and Stouraitis, 2006; Liao, Liu and Wang, 2014) and represents serious conflicts of interest between controlling and minority shareholders. Thus, we use the volume of related-party transactions to capture the degree to which firms are exposed to the conflict of interest between controlling and minority shareholders. We define *RelatedTrans* as the amount of transactions with related parties scaled by lagged total assets. We obtain information on related-party transactions from the CSMAR database.

We partition our sample into two subsamples based on whether a firm's average *RelatedTrans*, calculated using the most recently available four years of data before the reform, is above or below the sample median. In the presence of incentive misalignments, a larger fraction of related-party transactions is conducted on terms unfavorable to minority shareholders. Therefore, firms with above median related-party transactions are considered to have a higher pre-reform exposure to conflicts of interest between controlling and minority shareholders. As a result, these firms should benefit more from improved incentive alignments due to the share reform. To test this conjecture, we perform the DiD test in equation (1) separately for each of the two subsamples.

[Insert Table 7 Here]

Table 7 Panel A presents the regression results. Columns (1) and (2) report the results with *Ln(Pat)* as the dependent variable. Columns (3) and (4) report the results with *Ln(InvPat)* as the dependent variable.

The DiD estimates (i.e., the coefficient estimates of *SOE*×*Post*) are positive and significant at the 1% level in columns (2) and (4) in which firms with more severe pre-reform

conflicts of interest between controlling and minority shareholders are examined. However, in columns (1) and (3) in which firms with less pre-reform related-party transactions are examined, the DiD estimates have much smaller magnitudes and are statistically insignificant. For example, the coefficient estimate of *SOE*×*Post* is 0.057 (*t*-statistic = 0.840) in column (1), but 0.244 (*t*-statistic = 3.213) in column (2). The magnitude of the coefficient is about four times larger for firms with above-median *RelatedTrans* than for those with below-median *RelatedTrans*. We test the equivalence of the DiD estimates between the low and high *RelatedTrans* groups using a Wald test and report the test statistics at the bottom of the table. We observe that both test statistics are significant at the 1% or 5% level. Hence, we are able to reject the null hypothesis that the DiD estimates are the same across these two groups of firms. The evidence in Table 7 Panel A suggests that the effect of privatization expectation on innovation is more pronounced for firms with more related-party transactions and hence more severe conflicts of interest between controlling and minority shareholders before the share reform.

The validity of this mechanism relies on the premise that the conflict of interest between controlling and minority shareholders of SOEs is indeed mitigated after the share reform. We next examine this premise in the DiD framework and report the results in Table 7 Panel B. We estimate equation (1) after replacing the dependent variable with *RelatedTrans*. The coefficient estimate of *SOE*×*Post* is negative and significant, suggesting that SOEs experience a larger reduction in related-party transactions than non-SOEs surrounding the share reform. This finding is consistent with the view that privatization helps align the interest between controlling and minority shareholders of SOEs.

Taken together, the evidence reported in Table 7 suggests that better aligned interest between controlling and minority shareholders is a plausible underlying economic

mechanism through which privatization prospects triggered by the share reform promote firm innovation.

6.2 Stock price informativeness

The second possible mechanism through which privatization enhances innovation is enhancement in stock price informativeness. A stock market with low stock price informativeness can stifle firm innovation. Various theoretical studies (e.g., Grossman 1976, Subrahmanyam and Titman, 1999) argue that although market participants individually possess less firm-specific information than corporate managers, they collectively could be more informed about a firm's current status and its external business environment. Financial markets aggregate various pieces of information and incorporate them into security prices. From informative stock prices, firm managers are able to learn new information and insights that are relevant to corporate decision making, including investment decisions on innovation activities.[17] This so-called feedback effect of stock prices is weak for SOEs before the share reform, because a large chunk of their stocks are non-tradable and hence cannot fully aggregate various pieces of information into stock prices. As a result, SOEs suffer a lower innovation output because of less informative stock prices.

Existing evidence shows that improvements in information efficiency due to partial privatization enhances the profitability and productivity of Indian SOEs (Gupta, 2005). To the extent that the share reform improves stock price informativeness of SOEs more compared to non-SOEs so that it enhances the feedback effect of stock prices and facilitates active learning by Chinese SOE managers, it should help managers make value-enhancing investment decisions and stimulate innovation output. Hence, stock price informativeness is likely an underlying economic mechanism through privatization prospects promote firm

[17] Consistent with this view, Chen et al (2011) and Luo (2005) find managers learn and incorporate private information contained in stock prices when making investment decisions.

innovation. Based on this argument, we expect the positive effect of the share reform on innovation is more pronounced for firms with less informative stock prices before the share reform.

To explore this underlying mechanism, we perform the DiD tests separately based on subsamples of firms with different levels of stock price informativeness before the share reform. Following Gul, Kim, and Qiu (2010), we use the stock price non-synchronicity measure as a proxy for stock price informativeness. We define *Info* as the logit transformation of (1-R^2), where R^2 is obtained from estimating the regression model in equation (3) using daily stock return in each year.

$$RET_{i,t} = \alpha_i + \beta_1 MKTRET_t + \beta_2 MKTRET_{t-1} + \beta_3 INDRET_t + \beta_4 INDRET_{t-1} + \varepsilon_{i,t} \quad (3)$$

In equation (3), $RET_{i,t}$ is daily stock returns for firm *i* in day *t*, $MKTRET_t$ is value-weighted Chinese market returns, and $INDRET_t$ is value-weighted industry returns at day *t*. Following Gul, Kim, and Qiu (2010), we require at least 200 trading days of return data for estimating *Info*.

Previous research has argued that *Info* captures the amount of firm-specific information reflected in its stock price (e.g., Ferreira and Laux, 2007; Chen, Goldstein, and Jiang, 2007; Gul, Kim, and Qiu, 2010). The rationale is that if a firm's stock price contains more firm-specific information, then the market model explains a smaller proportion of stock price fluctuation, leading to a lower R^2 (i.e., higher *Info*).

[Insert Table 8 Here]

In Table 8 Panel A, we report the results estimating equation (1) separately for subsamples that are partitioned based on whether a firm's average *Info*, calculated using the most recently available four years of data before the reform, is below or above the sample median. Similar to Table 7, we report the results for firms with below-median *Info* in columns (1) and (3) and those for firms with above-median *Info* in columns (2) and (4).

The coefficient estimates of the DiD variable, *SOE*×*Post*, are statistically significant only in the subsample of firms with low pre-reform stock price informativeness, but not in the subsample with high pre-reform stock price informativeness. The magnitudes of the DiD estimates are also over four times larger for firms with low pre-reform stock price informativeness than firms with high stock price informativeness. To check if the differences in the DiD estimates across the two subsamples are statistically significant, we again conduct the Wald test to test the equivalence of the DiD estimates between the two regressions. The *p*-values of the tests are significant at the 1% or 5% level, suggesting that the positive effect of privatization prospects on firm innovation is more pronounced for firms with low pre-reform stock price informativeness.

The above analysis is based on the rationale that the share reform improves stock price informativeness of SOEs more than that of non-SOEs. To verify this premise, we examine the change in stock price informativeness surrounding the share reform in the DiD framework and report the results in Panel B of Table 8. We estimate equation (1) with *info* as the dependent variable. The DiD estimate is positive and significant at the 1% level, which suggests that SOEs experience a larger improvement in stock price informativeness than their non-SOE peers after the share reform. This finding is consistent with the view that the share reform coverts non-tradable shares to be freely tradable and improves stock price informativeness of SOEs to a larger extent. More informative stock prices enhance the feedback effect of stock prices and facilitate active learning by SOE managers, which, in turn, promotes firm innovation. Overall, the evidence reported in this subsection supports our conjecture that enhanced stock price informativeness is a plausible underlying mechanism through which privatization prospects triggered by the share reform promote firm innovation.

7. Conclusion

In this paper, we examine the effect of privatization on technological innovation. This topic is of interest to academics, practitioners, and policy makers both because innovation is crucial for a nation's economic growth and competitive advantage and because policy makers and regulators could alter laws and regulations to achieve privatization. To tackle this research question and address endogeneity concerns, we explore plausibly exogenous variation in privatization generated by a quasi-natural experiment – China's 2005 split share structure reform, which mandatorily converts non-tradable shares held by controlling shareholders to be freely tradable on exchanges and opens up the gate to further privatization of SOEs.

Using a DiD approach, we show that privatization prospects have a positive effect on technological innovation. Additional robust and placebo tests suggest that our findings are not driven by pre-existing trends in innovation output before the share reform, driven by endogenous timing in completing the reform, and by chance. We further show that better aligned interest between controlling and minority shareholders and improved stock price informativeness are two plausible underlying economic mechanisms through which privatization prospects enhance firm innovation. Our paper sheds new light on the real effects of privatization and has important policy implications for policy makers who aim to promote technological innovation.

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Table 1: Variable definitions and summary statistics

This table presents the variable definitions and descriptive statistics for the sample firms. Panel A defines all variables used in our analyses. Panel B reports the descriptive statistics for the sample firms. The sample consists of 13,977 firm-year observations for 1,289 non-financial firms over the 12-year period from 2000 to 2011. We require that the sample firms to be listed on the Shanghai and Shenzhen Stock Exchanges at the end of 2004, the year prior to the share reform. All variables are winsorized at 1% and 99%.

Panel A: Variable definitions

Variable	Definition
Pat	Total number of invention and utility model patent applications filed (and eventually granted) by a firm in a given year
InvPat	Total number of invention patent applications filed (and eventually granted) by a firm in a given year
Post	An indicator variable that takes the value of one for years after a firm completes the share reform and zero otherwise
SOE	An indicator variable that takes the value of one for firms with more than 25% government ownership as of the fiscal year end prior to the share reform
Leverage	(Long term debt - Cash)/(Market value of equity + Long term debt- Cash)
Tangibility	PP&E/Total assets
Profitability	Return on total assets (ROA)
Sales	Sales in millions of RMB
SalesGrowth	Annual sales growth rate
Age	Number of years since the firm's IPO
Patent Growth	Patent growth, defined as mean value of (Pat_t-Pat_{t-1}) in the 3-year period before the share reform
RelatedTrans	Total amount of related-party transactions scaled by lagged total assets.
Info	The logit transformation of *1-* R^2, where R^2 is obtained from estimating the regression model specified in *Equation (3)* using daily stock returns

Panel B: Summary statistics

Variable	N	25th	Median	Mean	75th	S. D.
Pat	13,977	0.000	0.000	7.312	3.000	31.390
InvPat	13,977	0.000	0.000	2.267	1.000	10.800
SOE	13,977	0.000	1.000	0.629	1.000	0.483
Cash (Mil RMB)	13,977	99.97	245.50	574.49	561.34	1,057.0
LT debt (Mil RMB)	13,977	1.217	50.10	523.26	272.76	1,528.7
Leverage	13,977	-0.133	-0.045	-0.082	0.003	0.235
Tangibility	13,977	0.152	0.265	0.293	0.416	0.185
Profitability	13,977	0.009	0.030	0.023	0.055	0.082
SalesGrowth	13,977	-0.016	0.144	0.248	0.333	0.689
Age	13,977	5.000	8.000	8.266	11.000	4.206
Sales (Mil RMB)	13,977	400.80	977.60	2,995.00	2382.00	6,717.0

Table 2: Univariate DiD test results

This table reports the diagnostics and results of the DiD tests on the effect of privatization on innovation. Sample selection begins with all firms with non-missing matching variables and non-missing innovation outcome variables in the year prior to the share reform. We match firms using a one-to-one nearest neighbor propensity score matching, without replacement, on a set of observable firm characteristics. Panel A reports parameter estimates from the probit model used in estimating the propensity scores for the treatment and control groups. The dependent variable in the probit model is the SOE dummy. The "Pre-Match" column contains the parameter estimates of the probit model estimated using the sample prior to matching. These estimates are then used to generate the propensity scores for matching SOE and non-SOE firms. The "Post-Match" column contains the parameter estimates of the probit model estimated using the subsample of matched treatment-control pairs after matching. Definitions of all other variables are listed in Panel A of Table 1. The models in both columns of Panel A are estimated with industry and year fixed effects. Coefficient estimates are reported and t-statistics are displayed in parentheses below. Panel B reports the balance test results for the pairs of treatment and control firms after matching. Panel C reports the DiD test results and their corresponding t-statistics in parentheses below. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Panel A: Propensity score regression and diagnostic regression

Dep. Var.	Pre-Match	Post-Match
	(1)	(2)
Leverage	0.098	-0.014
	(0.739)	(-0.091)
Tangibility	0.340	0.016
	(1.270)	(0.053)
Profitability	-0.523	0.170
	(-0.953)	(0.291)
SalesGrowth	0.078	0.051
	(1.097)	(0.669)
Log(Age)	0.030	-0.058
	(0.356)	(-0.631)
Log(Sales)	0.282***	0.031
	(8.344)	(0.761)
Patent Growth	-0.017**	-0.003
	(-2.126)	(-0.413)
Constant	-5.766***	-0.454
	(-7.526)	(-0.504)
Year FE	Yes	Yes
Industry FE	Yes	Yes
Observations	1,178	827
Pseudo R-square	0.101	0.013
P-value of χ^2	< 0.001	0.656

Panel B: Balance tests

	Treatment	Control	Diff.	T-test	P-value
Leverage	-0.180	-0.157	-0.023	-1.090	0.277
Tangibility	0.288	0.290	-0.002	-0.160	0.873
Profitability	0.011	0.008	0.003	0.560	0.578
SalesGrowth	0.193	0.158	0.035	0.860	0.388
Log(Age)	1.972	2.007	-0.035	-0.940	0.346
Log(Sales)	20.200	20.090	0.110	1.320	0.188
Patent Growth	0.792	0.789	0.003	0.010	0.995

Panel C: Univariate DiD tests

	Treatment (After-Before)	Control (After-Before)	DiD	Observations
	(1)	(2)	(3)	(4)
Ln(Pat)	0.884***	0.696***	0.188**	418
	(14.498)	(11.316)	(2.171)	
Ln(InvPat)	0.701***	0.498***	0.203***	418
	(13.612)	(9.722)	(2.803)	

Table 3: The effect of privatization on innovation: DiD regressions

This table reports the results of the DiD regressions designed for testing the effect of privatization on innovation. Variable definitions are reported in Panel A of Table 1. All regressions include firm and year fixed effects. The t-statistics reported in parentheses are based on standard errors clustered by firm. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Dep. Var.	*Ln(Pat)* $_{t+4}$	*Ln(InvPat)* $_{t+4}$
	(1)	(2)
SOE×Post	0.134***	0.115***
	(2.880)	(3.272)
Leverage	0.007	0.001
	(0.104)	(0.017)
Tangibility	-0.066	-0.077
	(-0.626)	(-1.061)
Profitability	0.182	0.124
	(1.615)	(1.593)
SalesGrowth	-0.008	-0.010
	(-0.570)	(-1.089)
Log(Age)	0.274***	0.119*
	(3.093)	(1.832)
Log(Sales)	0.082***	0.052***
	(3.860)	(3.727)
Constant	-1.088**	-0.721**
	(-2.352)	(-2.327)
Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	8,965	8,965
R-squared	0.788	0.745

Table 4: Privatization and innovation: dynamics

This table reports the results of the dynamic DiD regressions designed for examining innovation output surrounding the privatization year. $Before^{-1}$ is a dummy variable equal to one if it is one year before a firm completes the share reform. $After^{t}$ ($t \in \{1, 2, 3, 4^{+}\}$) is a dummy variable equal to one if it is t year after a firm completes the reform. $Current^{0}$ is a dummy variable for the share reform completion year. All regressions are estimated with firm and year fixed effects. The t-statistics in parentheses are based on standard errors clustered by firm. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Dep. Var.	$Ln(Pat)_t$	$Ln(InvPat)_t$
	(1)	(2)
$SOE \times Before^{-1}$	0.028	0.026
	(0.645)	(0.797)
$SOE \times Current^{0}$	0.046	0.028
	(0.897)	(0.756)
$SOE \times After^{1}$	0.091	0.057
	(1.597)	(1.320)
$SOE \times After^{2}$	0.138**	0.103**
	(2.247)	(2.207)
$SOE \times After^{3}$	0.110	0.109**
	(1.577)	(2.068)
$SOE \times After^{4+}$	0.167**	0.149***
	(2.276)	(2.752)
$Before^{-1}$	-0.040	-0.021
	(-0.879)	(-0.557)
$Current^{0}$	-0.057	-0.014
	(-0.845)	(-0.256)
$After^{1}$	-0.025	0.016
	(-0.285)	(0.224)
$After^{2}$	0.051	0.049
	(0.481)	(0.546)
$After^{3}$	0.179	0.152
	(1.377)	(1.380)
$After^{4+}$	0.274*	0.224*
	(1.770)	(1.676)
Leverage	-0.064	-0.033
	(-1.144)	(-0.847)
Tangibility	0.130	0.098
	(1.362)	(1.505)
Profitability	-0.038	-0.054
	(-0.396)	(-0.786)
SalesGrowth	-0.049***	-0.025***
	(-4.566)	(-3.470)
Log(Age)	0.248***	0.153***
	(3.437)	(2.776)
Log(Sales)	0.169***	0.101***
	(7.753)	(6.995)
Constant	-3.297***	-2.185***
	(-6.959)	(-6.819)
Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	13,977	13,977
R-squared	0.712	0.660

Table 5: Robustness check for endogenous timing

This table reports the results of the robustness tests addressing concerns on endogenous timing in implementing and completing the share reform. We treat 2005 as the reform year for all sample firms. All regressions include firm and year fixed effects. The t-statistics reported in parentheses are based on standard errors clustered by firm. ***, **, and * indicate significance at the 1%, 5% and 10% levels, respectively.

Dep. Var.	*Ln(Pat)* $_{t+4}$	*Ln(InvPat)* $_{t+4}$
	(1)	(2)
SOE×Post$_{2005}$	0.123**	0.115***
	(2.319)	(2.957)
Leverage	0.005	-0.001
	(0.072)	(-0.017)
Tangibility	-0.069	-0.080
	(-0.653)	(-1.110)
Profitability	0.181	0.122
	(1.591)	(1.567)
SalesGrowth	-0.009	-0.011
	(-0.659)	(-1.204)
Log(Age)	0.272***	0.118*
	(3.071)	(1.806)
Log(Sales)	0.086***	0.055***
	(4.036)	(3.943)
Constant	-1.155**	-0.778**
	(-2.500)	(-2.515)
Year FE	Yes	Yes
Firm FE	Yes	Yes
Observations	8,965	8,965
R-squared	0.788	0.745

Table 6: Placebo tests

This table reports the placebo test results for the multivariate DiD analysis. The results are obtained from randomization tests based on a sample from 5,000 simulations. For each simulation, we draw a random sample of 801 "SOEs" from the pool of actual SOE and non-SOE sample firms, and then treat the remaining firms as "non-SOEs". We then perform the DiD tests as in Table 3 on this simulated sample. We repeat the simulation process 5,000 times and summarize the distributions of the coefficients and corresponding t-statistics for the main variable of interest, *SOE×Post*. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Variable	Mean	P5	P25	Median	P75	P95	S.D.	N
Model (1) of Table 3								
Coefficient of *SOE×Post*	0.024	-0.042	-0.003	0.024	0.051	0.091	0.040	5,000
T-stat for *SOE×Post*	0.520	-0.887	-0.059	0.516	1.087	1.963	0.860	5,000
Model (2) of Table 3								
Coefficient of *SOE×Post*	0.038	-0.011	0.018	0.038	0.058	0.085	0.030	5,000
T-stat for *SOE×Post*	1.056	-0.311	0.493	1.062	1.623	2.397	0.831	5,000

Table 7: Mechanisms – conflicts of interest between controlling and minority shareholders

This table reports the results from our cross-sectional tests based on the degree of interest conflicts. The multivariate difference-in-differences (DiD) models in Panel A are estimated on median partitioned subsamples, using the innovation outcome variables as the dependent variables. The Wald test reported at the bottom of Panel A tests the equivalence of the coefficients for *SOE×Post* between the high and low groups. The partition variable *RelatedTrans* is defined as the total value of the related-party transactions scaled by lagged total assets. The difference-in-differences (DiD) models in Panel B are estimated on the whole sample, using *RelatedTrans* as the dependent variable. Variable definitions can be found in Panel A of Table 1. All regressions are estimated with firm and year fixed effects. The t-statistics reported in parentheses are based on standard errors clustered by firm. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Panel A: Subsamples analysis

	$Ln(Pat)_{t+4}$		$Ln(InvPat)_{t+4}$	
Partition Var.	*Low*	*High*	*Low*	*High*
Related Trans	(1)	(2)	(3)	(4)
SOE×Post	0.057	0.244***	0.055	0.167***
	(0.840)	(3.213)	(1.061)	(2.963)
Leverage	-0.001	-0.023	-0.029	0.040
	(-0.006)	(-0.239)	(-0.409)	(0.562)
Tangibility	0.009	-0.023	-0.006	-0.079
	(0.057)	(-0.154)	(-0.059)	(-0.790)
Profitability	0.222	0.211*	0.134	0.119
	(0.819)	(1.723)	(0.685)	(1.501)
SalesGrowth	0.005	-0.008	0.004	-0.011
	(0.156)	(-0.485)	(0.181)	(-1.003)
Log(Age)	0.088	0.356**	0.046	0.118
	(0.608)	(2.189)	(0.407)	(1.121)
Log(Sales)	0.132***	0.058**	0.099***	0.034**
	(2.704)	(2.500)	(3.225)	(2.311)
Constant	-2.185**	-1.290**	-1.716***	-0.652*
	(-2.226)	(-2.388)	(-2.738)	(-1.919)
Year FE	Yes	Yes	Yes	Yes
Firms FE	Yes	Yes	Yes	Yes
Observations	3,990	4,021	3,990	4,021
R-squared	0.788	0.749	0.754	0.680
H_0: $\beta_{SOE*Post}^{High} = \beta_{SOE*Post}^{Low}$				
χ^2 Test	8.019***		4.683**	
P-Value	0.005		0.030	

Panel B: The effect of privatization on *RelatedTrans*

Dep. Var.	*RelatedTrans*
	(1)
SOE×Post	-0.013*
	(-1.913)
Leverage	0.006
	(0.856)
Tangibility	-0.018
	(-0.834)
Profitability	-0.101***
	(-3.878)
SalesGrowth	0.027***
	(7.669)
Log(Age)	0.039***
	(3.407)
Log(Sales)	-0.019***
	(-4.841)
Constant	0.320***
	(4.124)
Year FE	Yes
Firms FE	Yes
Observations	12,060
R-squared	0.426

Table 8: Mechanisms – stock price informativeness

This table reports the results from our cross-sectional tests based on stock price informativeness. The multivariate difference-in-differences (DiD) models in Panel A are estimated on median partitioned subsamples, using the innovation outcome variables as the dependent variables. The partition variable *Info* is defined as the logit transformation of $1\text{-}R^2$ where R^2 is estimated by equation (3). The Wald test reported at the bottom of Panel A tests the equivalence of the coefficients for *SOE×Post* between the high and low groups. Variable definitions used in the analysis can be found in Panel A of Table 1. The difference-in-differences (DiD) models in Panel B are estimated on the whole sample, using *Info* as the dependent variable. All regressions are estimated with firm and year fixed effects. The t-statistics reported in parentheses are based on standard errors clustered by firm. *******, ******, and ***** indicate significance at the 1%, 5% and 10% levels, respectively.

Panel A: Subsample analysis

Dep. Var.	*Ln(Pat)*$_{t+4}$		*Ln(InvPat)*$_{t+4}$	
Partition Var.	*Low*	*High*	*Low*	*High*
Info	(1)	(2)	(3)	(4)
SOE×Post	0.206***	0.050	0.184***	0.019
	(2.867)	(0.666)	(3.256)	(0.338)
Leverage	-0.024	0.013	0.039	-0.025
	(-0.261)	(0.118)	(0.521)	(-0.347)
Tangibility	0.085	-0.052	-0.087	0.006
	(0.467)	(-0.343)	(-0.646)	(0.063)
Profitability	0.168	0.224*	0.056	0.118
	(0.466)	(1.672)	(0.211)	(1.322)
SalesGrowth	0.024	-0.019	0.020	-0.020*
	(0.657)	(-1.052)	(0.779)	(-1.894)
Log(Age)	0.139	0.223	0.053	0.078
	(0.977)	(1.303)	(0.471)	(0.683)
Log(Sales)	0.178***	0.083***	0.097**	0.070***
	(2.882)	(2.686)	(2.348)	(3.764)
Constant	-3.264***	-1.512**	-1.726**	-1.269***
	(-2.661)	(-2.199)	(-2.060)	(-3.023)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Observations	3,842	3,902	3,842	3,902
R-squared	0.788	0.751	0.753	0.695
H_0: $\beta^{High}_{SOE*Post} = \beta^{Low}_{SOE*Post}$				
χ^2 Test	5.180**		9.395***	
P-Value	0.023		0.002	

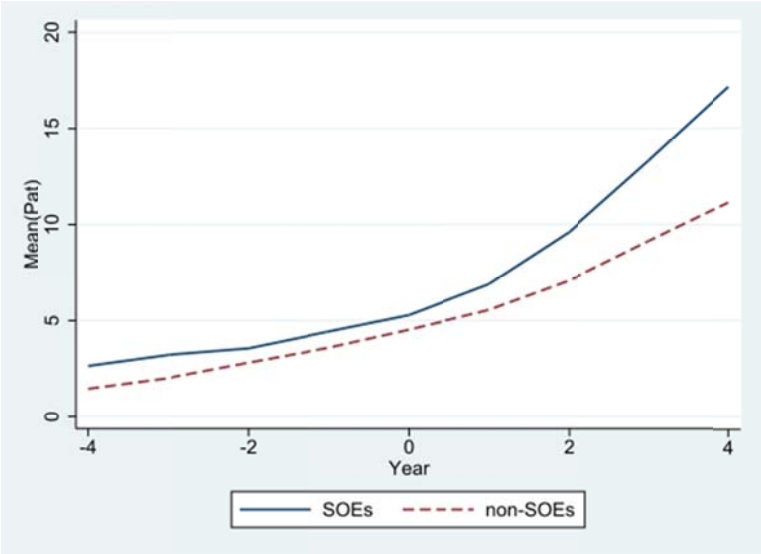
Panel B: The effect of privatization on stock price informativeness

Dep. Var.	*Info*
	(1)
SOE×Post	0.065**
	(2.114)
Leverage	0.146***
	(4.128)
Tangibility	-0.017
	(-0.232)
Profitability	0.005
	(0.047)
SalesGrowth	0.086***
	(8.674)
Log(Age)	0.266***
	(3.784)
Log(Sales)	-0.113***
	(-9.007)
Constant	2.765***
	(10.640)
Firm FE	Yes
Year FE	Yes
Observations	11,112
R-squared	0.580

Figure 1: Patent dynamics around the share reform

This figure shows the mean difference in innovation captured by average number of patents for SOE and non-SOE firms from four years before privatization to four years after privatization. Year 0 is the year in which the firm completes its split share structural reform. Panel A shows the difference in invention and utility model patents (*Pat*) and Panel B shows the difference in invention patents (*InvPat*).

Panel A: Difference in the number of invention and utility model patents around the share reform



Panel B: Difference in the number of invention patents around the share reform