Is the Stock Market Just a Side Show? Evidence from a Structural Reform*

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JEL Classification Numbers: G31, C21, O16, D21.
Keywords: Stock market liquidity, corporate behavior, governmental intervention, time-varying treatment effect, generalized propensity score.

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Abstract

The 2005 split-share reform in China mandated the conversion of previously non-tradable shares into tradable status. The reform was swift and changed investors' ability to trade corporate equities in a US$400 billion market. This paper examines the effects of the 2005 stock market reform on firms' real and financial outcomes. It does so exploiting multiple institutional features of the conversion program. We first examine a small pilot trial conducted at the beginning of the reform, which we are able to replicate using the same data and selection criteria that was used by regulators. We also take advantage of the staggered nature of the larger conversion schedule, whereby over a thousand firms converted their outstanding shares at different times within a pre-specified window. These various wrinkles produce counterfactuals against which to gauge the economic effects of secondary equity trading. Using a time-varying treatment estimation approach, we identify increases in corporate profitability, investment, value, and productivity as pre-existing shares were allowed to trade in organized exchanges. We also identify changes in firms' propensity to issue new shares, pay dividends, and engage in merger deals. Our findings provide new insights on the role of stock markets in shaping corporate behavior and on the impact of regulation on economic growth.

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1 Introduction

Firms can issue equity to access financing and evidence suggests that funds raised via primary equity issues (IPOs and SEOs) are used for corporate investment, inventory accumulation, and R&D spending. It is less clear, however, whether secondary equity transactions — among market investors — influence firm behavior. It has long been argued that secondary stock market transactions are largely a "side show" to corporate activity (see Bosworth, 1975). At the same time, there are reasons to believe those transactions might matter. In the presence of agency problems, for example, secondary stock market transactions are important to the extent that they allow for changes in corporate control (Stein, 1988; Shleifer and Vishny, 1990). Those transactions might also matter if market prices convey information about firms' prospects (Bekaert and Harvey, 1995; Dow and Gorton, 1997). In addition, an active secondary market might be relevant in ensuring ex-post liquidity for investors wishing to finance firms in primary markets (Levine, 1991; Bencivenga et al., 1995).

It is difficult to test empirically whether the trading of corporate shares in public markets matters according to the arguments just listed. For one thing, firms with publicly traded stocks are very different from those with private capital. This makes it difficult to compare public and private firms for drawing conclusions about the economic role of stock market trading. In addition, firms choose when they go public, and that choice is confounded with factors such as underlying firm quality, value, and financing needs. This makes it difficult to compare firms before and after they go public to learn about the effects of markets on firms in a causal sense. To gauge the economic effects of stock transactions that take place in public exchanges, one would like to observe public firms whose stocks are traded in the market coexisting with similar public firms whose stocks are not traded. While these types of counterfactuals are rarely observed, recent developments in the Chinese stock market put us close to a quasi-experiment on the effect of secondary equity trading on corporate policies and outcomes.

Stock ownership in China is divided into three main classes: shares reserved for domestic investors (A-shares), shares available to foreign investors (B-shares), and shares of companies listed or cross-listed overseas (H-shares, for those listed in Hong Kong). A-shares represent over 90% of the market and were, until recently, split into tradable and non-tradable categories. Both share categories gave their owners identical cash flow and voting rights, yet the vast majority of corporate stocks in China could not be traded in the organized exchanges (non-tradable shares represented over 70% of total shares in 2004). This unique structure — an outcome of past reforms — created a number of difficulties as Chinese firms' opportunity set expanded in the early 2000s. Chinese central planners acknowledged the problem and in 2005 implemented an unprecedented reform to address the issue.

The 2005 share reform swiftly converted non-tradable shares into tradable status. The reform started with a pilot trial in May of 2005 with a set of 46 firms selected into conversion. In September of that year, the pilot unfolded into a fully-fledged program under which all listed firms were mandated to conclude their conversions by the end of 2006. In this watershed event, a sizeable secondary market emerged within a window dictated by a government program — a far cry from the kinds of endogenous, slow-moving processes in which equity markets typically evolve. Companies in a US$400 billion market had to respond to a structural shift in stock liquidity within a short period of time.[1]

The split-share reform in China provides a unique setting in which to identify connections between public stock markets and corporate outcomes. From a theoretical standpoint, this intervention has numerous potential implications. First, after the reform, the wealth of shareholders should become more closely tied to stock prices (previously, most shareholders only cared about dividend payments). Creating an observable measure of performance (market prices of liquid securities) could, in turn, provide managers with incentives to improve efficiency. Second, the increase in liquidity and depth of secondary markets should allow firms to raise more capital in primary equity markets, since investors would then acquire securities that remain liquid after they are issued. Third, and relatedly, the reform could reduce the cost of equity financing, possibly affecting corporate funding, investment, and growth. Finally, by making stocks transferable and liquid, the reform could jump-start a takeover market, allowing for a more efficient capital reallocation process and the replacement of inept managers and poor governance structures.

We gauge the impact of the 2005 share reform on companies' real and financial outcomes using quasi-experimental strategies. We do this by exploiting institutional features of the reform program in conjunction with a time-varying treatment estimation approach that allows us to measure the impact of the reform in the short and long runs. Our findings on corporate profits, investment, employment, merger activity, valuation, and productivity provide a direct assessment of the reform from a corporate wealth standpoint. More broadly, they help shed new light on the role of the stock market in the economy.

We use a couple of different strategies to evaluate the effects of the reform. First, we study the effect of equity conversions on the group of firms that participated in the initial pilot trial. Materials published by the China Securities Regulatory Commission (CSRC) (the counterpart of the U.S. SEC) and government-run media report the criteria used for selecting firms into the pilot. We are able to use the same data analyzed by policy-makers — the very data commissioned by the Chinese government to conduct the reform —, which allows us to "recreate" the pilot using a method of selection on observables. In doing so, we match each firm in the pilot with a control firm that central planners could plausibly have chosen for the trial program. The matching is based on an extensive set of covariates, including firms' geographical characteristics (such as provincial GDP), industry characteristics (e.g., size and concentration), investment, savings, profitability, productivity,

[1] Notably, the reform was not meant to work as an instrument of corporate control changes. In fact, regulators put in place a number of provisions to avoid such changes as an immediate result of the conversion process. It is thus not surprising that less than 1% of the firms observed a change in control as a direct result of conversions.

size, age, ownership concentration, state ownership, and the ratio of non-tradable to tradable shares. Under this approach, we estimate a difference-in-differences model that accounts for firm observables and time-fixed unobservable effects to gauge the impact of the reform.

Because the number of pilot firms is small and could have an idiosyncratic distribution of unobserved characteristics, we further gauge the impact of the reform on the hundreds of firms that entered the program right after the pilot trial. In addition to the larger number of firms in the treatment group (greater test power), one advantage of this second approach is the reduced odds that our inferences are compromised by biases arising from selection based on expected outcomes or outcome manipulation by the government. Moreover, in contrast to the "one-time" event that characterizes the pilot, the sequential nature of the larger compliance program makes it less likely that other coinciding macro events underlie the effects we observe. The disadvantage is more room for self-selection into treatment, since, after the pilot phase, the timing of program compliance becomes largely — but not entirely — a firm's decision. This issue is interesting in its own right and leads us to use an alternative estimation technique that is worth discussing.

As firms gradually join the reform starting in September 2005, it becomes increasingly difficult to identify a control match for every treated firm — eventually, all firms comply with the reform (become treated). In this setting, conventional matching and difference-in-differences estimators based on a stale, binary definition of treatment status are problematic (see Brand and Xie, 2007). Accordingly, we use a time-varying treatment approach that takes into account that: (1) firms spend different periods of time in the reform window (with earlier compliers spending more time under the treatment status), (2) the pools of treated and control firms change over time (implying time-varying composition effects), and (3) the effect of the reform may not be constant over time. Under this approach, treatment is not defined as a constant indicator variable (treated *versus* untreated), but rather as the number of months since joining the reform (length of treatment exposure). The approach is unique in allowing one to measure short- and long-run effects of the reform.

The main difficulty with the larger sample experiment is self-selection with respect to the conversion timing. The conversion process required the approval of a super-majority of votes by both tradable and non-tradable shareholders of the firm. Numerous reports point to difficulties in reaching such a high level of collective agreement, and that regulatory hurdle added noise to firms' conversion timing. Another factor disturbing conversion timing was the government's desire to promote an "orderly conversion process." To wit, in order to avoid downward pressure on stock prices, the CSRC limited the number of firms allowed to convert their shares at any particular point of the reform window. Accordingly, firms had to "wait in line" to convert their shares even after their shareholders agreed on converting. Notwithstanding factors that should make it difficult for firms to time the conversion of their shares, one could argue that firms monitored market developments during the reform process and anticipated the potential effects of share conversion. In this case, one might be concerned with the possibility that firms "optimized" their entry into the program. In addition, it is possible that outcome-related variation could induce firms to join the program at particular points in time.[2]

Standard fixed-effect and difference-in-difference models will not control for firm-specific trends that affect firms' decisions to join the reform and the timing of those decisions. In turn, we implement a multi-valued treatment approach that minimizes these types of selection problems. In particular, we use a Generalized Propensity Score (GPS) estimator (cf. Imbens, 2000; Imai and van Dyk, 2004) that controls for heterogeneity associated with idiosyncratic time variation (or trends) in outcomes as well as potential expected effects of the reform, making them "orthogonal" to the entry date decision. In essence, the GPS approach exploits pre-treatment firm characteristics and outcomes to create multiple counterfactual histories for each firm. These counterfactuals allow one to compare firms that have the same probability of complying with the reform at the same date, yet comply at different dates. Differences in these firms' outcomes reveal the impact of the reform across time.

Our estimations suggest that the 2005 share reform in China altered firm policies and affected corporate wealth by bolstering the market for secondary equity transactions. The paper's main results can be summarized as follows. First, we find that stock conversions significantly enhanced stock liquidity and reduced ownership concentration. Importantly, we find that real corporate activity is significantly impacted by the share reform. For example, relative to the baseline case of no conversion, investment in fixed assets increased an additional 10 percentage points two years after a firm's stocks were allowed to trade (this represents a 50% boost in the annual investment rate). At the same time, stock conversions did not prompt firms to employ more workers. Following reform compliance, firms also experienced positive effects on their profitability, with net operating revenue growing, on average, 14% more than in the counterfactual case of non-compliance. Return on equity of complying firms increased up to 1.5 percentage points 18 months after conversion (over 33% above the benchmark), declining later as firms issued new equity. Notably, gains in economic performance were accompanied by improvements in productivity, as measured by the ratio of sales to capital. After 18 months, sales were 30% higher given the same amount of fixed assets. In the long run, this ratio remained 20% higher than in the case of non-conversion. Finally, the estimated effect of conversions on the ratio of market-to-book value of equity was positive and increased up to two years after compliance. Indeed, that measure of corporate value almost doubled 24 months after a firm's stock started to trade freely in the market, staying 70% higher in the long run.

Firms also changed their financial policies as a result of the share reform. We find, for example, that firms cut dividends after converting their stocks into tradable status. This is an interesting result in its own right, suggesting that shareholders put less emphasis on dividends after their shares could

[2] To use a concrete example, one could imagine that fluctuations in firm value, say, a steep run-up in stock price, might facilitate share conversion (induce early treatment). To the extent that equity values may drop after a run-up, we may have pre-treatment conditions influencing both the timing of the treatment and observed outcomes.

be traded in the stock markets. Conversions also prompted firms to issue more stocks, suggesting they gained greater access to equity financing (in primary markets) given the greater liquidity in secondary markets. A more liquid stock market should allow firms to more actively engage in merger and acquisition deals, since stocks are often used to finance these transactions. This is what we find in the data. Looking at changes in corporate governance structures, we find that stock ownership concentration (among the top 5 shareholders) dropped significantly as a result of conversions into tradable status. We find, however, no strong evidence of changes in CEO turnover associated with stock conversions.

We argue that the more liquid, deeper stock market that emerged as a result of the split-share reform in China led to significant changes in firms' real and financial policies. To better characterize the mechanism we propose, we exploit heterogeneity in potential treatment outcomes associated with reform. In particular, we examine if firms that had the most to gain from the reform program indeed observed the largest responses to the conversion process. Looking at pre-conversion distributions of stock liquidity and ownership concentration, we find that firms whose stocks were less liquid and more concentrated before the reform experienced the largest gains in corporate growth, productivity, profitability, and value as a result of having their shares converted into the tradable status. Evidence of these heterogeneous effects is consistent with our hypothesis that the lifting of restrictions on equity trading had positive real implications for Chinese firms.

There is a growing literature on the 2005 split-share reform in China. As we describe below, the conversion process involved non-tradable shareholders compensating tradable shareholders for the right to sell their shares. Bortolotti et al. (2008) document that in 88% of the conversion agreements tradable shareholders received shares as a main form of compensation from non-tradable shareholders. Firth et al. (2010) find that firms owned by the state offered higher compensations to non-tradable shareholders than firms owned by private investors. Li et al. (2011) show that the size of the compensation packages paid to tradable shareholders was negatively correlated with the bargaining power of non-tradable shareholders. While these papers provide important insights on the 2005 reform, they focus on the negotiations that characterize the conversion schemes adopted by reform-complying firms. The existing literature abstracts from the real-side, long-term implications of the reform on corporate outcomes (such as investment, employment, or productivity).

Finally, a number of recent studies look at the economic consequences of equity market liberalization and our results have clear connections with their findings (e.g., Levine and Zervos, 1998; Bekaert et al., 2005; Gupta, 2005; Chari and Henry, 2008; Quinn and Toyoda, 2008). Bekaert et al., for example, use country-level data from 95 countries to study the effects of the openness of equity markets to foreign investors. The authors find that market liberalization initiatives lead, on average, to a 1% increase in GDP growth. Our paper extends findings in this literature by exploring a number of different perspectives. First, our analysis consists of a firm-level, within-country examination that builds on well-defined institutional features of a sweeping reform. This, in turn, allows us to describe in detail *how* economic outcomes are affected by market liberalization (e.g., changes in corporate investment, labor productivity, stock liquidity, equity financing, and merger activity). Second, market liberalization in our paper refers to market access that is granted to regular domestic investors (as opposed to sophisticated foreign investors). In particular, we gauge the impact of a deregulation initiative that brought into the equity market an estimated 25 million new domestic investors at once. Third, we adopt quasi-experimental methods in dealing with problems of endogeneity and self-selection commonly associated with observational data. In this way, the approach we use uniquely identifies the mechanisms through which liberalization affects markets and firms.

To our knowledge, this paper is the first of its kind to characterize in detail the impact of secondary equity markets on firms' real and financial decisions. While our inferences are ultimately related to the conditions of the Chinese economy, we believe our findings provide a new insight on the role equity markets play in shaping observed behaviors and outcomes in the corporate sector.

The remainder of the paper is organized as follows. Section 2 describes the 2005 reform, explaining in detail the conversion process of non-tradable shares. It also discusses the potential effects of the reform on firm outcomes. Section 3 describes the data. Section 4 details our identification strategy and estimation methodology. Section 5 presents our empirical findings. Section 6 concludes the paper.

2 The 2005 Share Reform

2.1 Institutional Background

The recent move towards market liberalization in China is seen by many observers as an ex-post fix to the unsuccessful reform of State-Owned Enterprises (SOEs) initiated in 1979. Since that reform, the profitability of SOEs declined, with many firms becoming immersed in debt. With unclear allocation of property rights and onerous social responsibilities (provision of employment, education, health care, and other benefits), SOEs had very few incentives to improve their operating efficiency (see Bai et al., 2006). More broadly, the failure to modernize the SOE system was seen as a failure of the Chinese-style planned society.[3] By many accounts, the Chinese government understood this problem.

In the early 2000s, central planners implemented a series of Share Issue Privatizations (SIPs) to recapitalize the SOEs. To keep some degree of control over the privatized firms, the government established *share classes* based on their relationship with the state, with all "state-related" shares becoming non-tradable in the organized exchanges. Under that arrangement, owners of non-tradable shares could only sell their shares under strict government control. Sale prices were determined by the government using accounting information — not market values —, and were set deliberately low to avoid transfers. Even then, the government retained the ultimate say on any transactions involving

[3] These views were articulated in an International Monetary Fund report issued in 1993 titled "China at the Threshold of a Market Economy."

those shares.[4]

Non-tradable and tradable shares have the same voting and cash flow rights, but non-tradable shares account for about two-thirds of all shares. Cross-firm variation in the proportion of these share classes was determined according to conflicting interests within an intricate web of bureaucracies, including central-government asset management committees, central finance and industry ministries, local governments (various layers), and local-state asset management committees. All of these parties had a say in determining which shares of any given company would be deemed as state-related. Not surprisingly, firms came out of the privatization process displaying significant cross-sectional variation in the proportion of non-tradable shares in their books. Figure 1 shows a histogram of the proportion of non-tradable shares across A-share firms listed at the end of 2004. Out of 1378 firms, 1350 (or 98%) had anywhere between 20% and 80% of their stocks under the non-tradable category.

Figure 1 About Here

Existing research shows that the split-share structure created various governance problems (see, e.g., Claessens et al., 2002; Fan and Wong, 2002; Song and Tong, 2004). The majority of listed firms remained under the control of holders of non-tradable shares. As the wealth of these shareholders was largely insulated from changes in stock market prices, conflicts between controlling and minority (tradable) shareholders emerged. In its worst form, non-tradable shareholders sought to tunnel resources (often through "related party transactions") out of the listed firms at the expense of tradable shareholders (see Cheung et al., 2006; Deng et al., 2008; Jian and Wong, 2010). As the top management and board of directors of SOEs were often appointed by the state, political career concerns and entrenchment led to an inefficient corporate governance system. The predominance of non-tradable shares further made the market for corporate control virtually inexistent in China. Tradable shareholders, in turn, became largely short-term investors. They were not interested in participating in firm management and reluctant to provide the liquidity depth firms would need in the equity markets (Tenev et al., 2002).

2.2 The Time Line of the Reform

By early 2005 it was clear that the split-share structure had given rise to an illiquid stock market, with the better Chinese companies choosing to list overseas. The issue came to the forefront of economic policy on April 29, when the CSRC issued a document titled "Circular on Issues Related to the Pilot Program of Non-Tradable Share Reform in Listed Companies."[5] Within days, the Shanghai

[4] Proposed transactions were submitted in writing. Central and local governments commonly took months (sometimes years) before issuing a decision.

[5] The directive was issued Friday night before a prolonged May 1^{st} holiday and was interpreted by newspapers at the time as a signal that the Chinese government intended to push the reform without consulting companies, investors, or the organized exchanges. *People's Daily* (equivalent to Russia's Soviet-era *Pravda*) quotes the chairman of the CSRC, Shang Fulin, as saying the following about the reform: "An arrow that has left the bow can never be taken back."

Stock Exchange and the Shenzhen Stock Exchange issued the circular "Operation Instruction on Pilot Program of Non-Tradable Share Reform in Listed Companies," which formally launched a far-reaching reform of the existing share ownership system. A first batch of pilot firms was announced on May 9 including four firms. On June 20, a final batch of 42 companies was added to the pilot program.

On September 4, the CSRC issued "Administrative Measures on the Split Share Structure Reform in Listed Companies," a document determining that the conversion of non-tradable share into tradable shares should be adopted by all A-share firms by December 2006. By the end of 2005, 434 companies had complied with the reform, accounting for 37% of the total market capitalization and 32% of listed firms at that time. About half of the listed firms joined the reform by the first semester of 2006, and less than 3% had not complied by December 2006. Figure 2 below shows the number of firms complying with the reform program over time.

The pilot firms had to start their share conversion process immediately after the government announced their selection. Chinese central planners did not divulge an official document with the criteria used for selecting firms into the pilot. However, materials published by the CSRC and government-run media provide the guidelines. The criteria used for selection considered four general firm attributes: profitability, representativeness, geographic location, and industry of operation.

According to the government's plan, a profitable firm should be able to afford a conversion proposal with a relatively high compensation package (explained shortly), making it easier to receive the approval of tradable shareholders. Given that stock prices in China were at the time hardly informative about the value of firms' assets (Wang et al., 2009), governmental officials measured firm profitability based on accounting figures, such as operating cash flows and return on equity. Representativeness was associated with characteristics such as firm size and history (i.e., the largest SOE in a particular province, or the best-known company in a particular industry). Central planners emphasized a "balance" in the ownership structure of the pilot firms. Accordingly, among the batch of 46 pilot firms, 22 were private firms and 24 were SOEs. The government also wanted to have the reform spread across various provinces from the start, avoiding a concentration in the large provinces ("geographical balance"). Accordingly, 17 of the 31 Chinese provinces had firms selected into the pilot. Finally, the government had a preference for firms in competitive industries, since concentrated industries were often associated with national interests or state monopolies.

All of the criteria listed above can be observed using publicly available data. Indeed, as we explain below, we are able to conduct our analysis using the same data provider that was commissioned by the Chinese government to implement the reform. Accordingly, a matching-based strategy can provide a set of control firms that the government could plausibly have chosen for the pilot trial.

2.3 Steps of the Conversion Process

Share conversions involved non-tradable shareholders proposing a compensation package to tradable shareholders. These packages included cash, warrants, and most frequently, additional shares. Only holders of A-shares participated in these negotiations, which thus excluded foreign investors. A typical conversion agreement worked as follows. The firm would announce the start of negotiations on its conversion plan. Afterwards, representatives of non-tradable shareholders proposed a compensation plan to the tradable shareholders. In case the parties agreed, the board would make an announcement on the plan within a few weeks. It would take about a week to register the plan and another one to two weeks to bring the plan to a vote by all shareholders. If the plan was voted favorably by tradable and non-tradable shareholders the plan was approved. Payments to tradable shareholders were made a few days following the vote. Afterwards, a lock-up period applied under which non-tradable shareholders could not immediately sell all of their shares at once.[6]

Although the conversion protocol was relatively straightforward, reaching agreements on conversions was notoriously difficult (see, e.g., Xiong and Yu, 2011; Firth at al., 2010). A main reason was the CSRC's requirement that conversions had to be agreed upon by a super-majority (two-thirds) of both tradable and non-tradable shareholders. More often than not, there were disagreements between (and within) the two classes of investors regarding different steps of the conversion process. From an identification standpoint, the upshot of this institutional feature is the noise that is added to the firm's conversion timing. Another feature of the reform that added noise to compliance timing was the fact that the CSRC limited the number of firms getting approval to convert their shares at any particular point in time. Central planners feared a scenario in which stock prices would plummet if too many firms converted their shares at once. To avoid this problem, they imposed caps on the number of conversions, precluding firms from converting their shares at will. Specifically, after voting on a final conversion plan, firms had to wait for their petition to be selected into CSRC's "approval lists." These lists were issued on a weekly basis and limited the number of converting firms to about twenty at a time (down to eight towards the end of the reform window). Finally, calling for a vote on conversion but failing to pass it implied a "cooling off" period of at least three months before another vote could be called.

Various institutional elements of the reform made it difficult for firms to "optimally time" the conversion of their shares. Additional evidence we gather further suggests that shareholders were told to bargain over outcomes they were unsure about.[7] These features of the data are helpful in minimizing worries about endogenous biases in our tests (in particular, self-selection). Even so, as

[6] For example, the combined sales of shares by non-tradable shareholders could not exceed 10% of the firm's total shares within a certain number of months.

[7] We conducted an event study to gauge whether Chinese shareholders anticipated the potential gains from the reform. Cumulative abnormal returns (CARs) measured for the pilot firms on the announcement of their selection into the program are economically and statistically insignificant.

we explain in Section 4, we explicitly tackle potential sources of endogeneity in our experiment.

2.4 Share Conversions and Aggregate Stock Market Liquidity

Our empirical strategy takes advantage of the conversion program just described to look for effects of stock liquidity on firm outcomes. The working hypothesis is that share conversions increased liquidity in secondary markets. We provide broad evidence in support of this hypothesis in Figure 2, where we superimpose the time line of corporate compliance with the conversion program (solid line) and stock market turnover (dashed line). We compute market turnover as a 12-month moving average of the log ratio of the number of shares traded on the Shanghai Stock Exchange over the total number of shares outstanding (following Amihud et al., 1997). Inspection of Figure 2 suggests that stock turnover in the Chinese equity markets moved in tandem with firm adherence to the share reform program. Later in the analysis, we perform detailed, firm-level estimations considering different measures of liquidity. These estimations also show that the split-share reform lead to a sharp increase in stock liquidity.

FIGURE 2 ABOUT HERE

2.5 Potential Effects of the Reform

The 2005 reform was designed to boost liquidity for corporate stocks in organized exchanges in China. Government planners alluded — often vaguely — to increases in firm efficiency and easier access to capital as likely consequences of the split-share reform. In this section, we lay out a set of priors concerning the potential consequences of the reform and briefly describe the outcome variables we analyze in our tests (implementation details are provided in the next section).

We hypothesize that corporate shares would become more liquid after conversion into tradable status. Accordingly, we consider increases in stock liquidity as a primary indicator of the effects of the reform. Liquidity may also ease firms' access to external finance by enhancing the price discovery process and reducing information asymmetries between managers and investors (Myers and Majluf, 1984). Moreover, access to primary equity markets — IPO and SEO activities — might be facilitated when investors expect to be able to resell their claims at a later date if they need. With these priors in mind, we expect firms to issue equity more actively after their shares become tradable.

One could expect firms to improve operating and financial performance under better incentives and more flexible financing opportunities potentially generated by the 2005 reform.[8] Enhanced liquidity brought about by the reform could also lower the cost of equity, which in turn could broaden the pool of profitable investment projects (Bekaert and Harvey, 2000; Amihud and Mendelson, 1988). Focusing on real-side effects of the reform, our empirical analysis considers measures of firm investment, profitability, productivity, and value as outcome variables. These are standard measures of

[8] Evidence consistent with these priors following previous market-oriented reforms in China can be found, for example, in Chang and Wong (2004).

performance in corporate finance, and if the share reform was relevant these variables should respond to the conversion process. We also look at employment as one of our real outcome variables. Given the unique characteristics of the labor market in China, one would expect firms to lay off workers after a reform that moves them closer to market-oriented objectives. Notably, however, firms had already implemented large lay off programs in the late 1990s (Sun and Tong, 2004). This makes it difficult to predict the effect of the reform on firm employment. Yet, we measure the reform's impact on outcomes such as employment demand and productivity as a way to gauge potential links between the stock markets and the economy.

While our estimations focus primarily on the effect of the reform on real-side activities, it is important to look at firms' financial policies and related variables. These outcomes are interesting in their own right, but also help us understand the mechanisms through which equity markets affect corporate activity.

Historically in China, owners of non-tradable shares could only benefit from their holdings via dividend payments. The reform, however, could change the preferences of those investors. For example, after shares become tradable, all shareholders would be able to profit from capital gains. As a result, firms could place relatively less emphasis on dividends. In addition to equity issuance, we assess changes in firms' financial policies by looking at their dividend payments.

Corporate finance theory suggests that a better alignment of interests among investors in the firm should lead to better decisions (Jensen and Meckling, 1976). In China, conflicts of interests between tradable (minority) and non-tradable (majority) shareholders are known to be associated with corporate mismanagement and even fraud. These problems became acute in recent years, with minority shareholder expropriation conducted primarily by way of "related party transactions" ("tunneling").[9] We measure the incidence of these fraud-laden transactions in firms converting their shares to see if market liquidity has an impact on these activities. To the extent that market prices might more quickly respond to expropriation and fraudulent activities by corporate controllers after stocks become liquid, we would expect to see less related party transactions following share conversions. A more direct mechanism through which existing governance structures change is via CEO replacement. Accordingly, we also examine the frequency with which firms change their CEOs after shares are traded in secondary markets. In addition, we also study changes in ownership concentration after firm shares are converted into tradable status.

Finally, one could conjecture that the ability to freely trade shares might unfreeze the market for corporate control. Newly-converted shares could even be used as a currency to acquire other firms. Accordingly, a potential outcome of the reform is observing firms engaging in M&A activities after converting their shares. Our empirical investigation also looks at corporate mergers.

[9] Deng et al. (2008) report that 90% of the SOEs that went public between 1997 and 2000 were later involved in "disadvantageous transactions with their parent firms." Those transactions averaged 13% of the listed firms' assets.

3 Data

3.1 Data Sources and Sampling

Our raw dataset comprises all A-share companies listed in the Chinese public exchanges at the end of 2004. We exclude companies with B-/H-shares, ST/PT status,[10] and companies with previous fraud-related court cases as indicated by the CSRC. That is, we exclude companies that were considered ineligible according to the CSRC's share reform principles. We also exclude financial firms as their investment, employment, and financial decisions are very different from non-financial firms. Data on the share reform are from WIND Financial Information Systems, which was commissioned by the CSRC to compile a database for regulatory use. One advantage of using this dataset is that of ensuring that the econometrician and the policy-maker use the same information. All accounting and stock price information is from Shenzhen GTA Inc. We also manually collect data from companies' annual reports if they are missing from commercial databases. Our final sample has 1,054 firms, representing about 80% of the A-share firms. For these firms, detailed data are available from 2002 through 2009.

3.2 Variable Construction

We consider an extensive list of real and financial outcomes in our analysis. We use the growth in the log of a firm's fixed assets (ΔK) to measure capital investment spending. To measure employment growth, we use changes in the log number of employees (ΔL). We use the log ratio of sales over fixed capital ($Sales/K$) as a measure of productivity. The log ratio of operating revenue over operating expenses ($NetIncome$) and return on equity (ROE) are used as measures of firm profitability. We use the market-to-book equity ratio (M/B) to gauge market valuation.

We examine firms' dividend policy through the ratio of total cash dividends over net income ($Dividend$). To measure equity financing ($Issuance$), we collect data on equity issuance activity (including SEOs and rights offerings) from 2002 to 2009. We also collect information on the merger and acquisition deals that firms conduct before and after converting their shares ($M\&A$). Following prior literature (Cheung et al., 2006; Deng et al., 2008), we classify as related party transactions (RPT) those disclosed in the annual report under the categories of "equity transfers," "goods and service related trading," "assets purchase and sale," and "cash payments." Accordingly, we count the number of these transactions for the firms in our sample. We expect enhanced stock liquidity to diminish the concentration of firm ownership and use the Herfindahl index of top 5 shareholders to gauge ownership concentration ($OwnerConcent$). We also compute the likelihood of observing firms replacing their CEOs before and after stock conversion ($CEOTurnover$)

We construct two measures of stock liquidity. The first is the liquidity ratio (*LiqRatio*). This

[10] A firm is designated as a "special treatment" (ST) firm if it reports a net loss for two consecutive years. A firm is designated a "particular transfer" (PT) firm if it suffers a net loss for three consecutive years (PT entails virtual suspension from trading).

standard measure is computed on a monthly basis and is defined as the sum of daily trading volume divided by the sum of the absolute value of daily return. The liquidity ratio measures the trading volume in dollars associated with a one percent change in stock price, and is thus a proxy for market depth (cf. Amihud et al., 1997). The second proxy for liquidity is share turnover ($ShareTurnover$), defined as monthly number of shares traded divided by the number of shares outstanding. For each of these two measures, we obtain yearly proxies from 2002 to 2009 by averaging monthly observations.

Our matching approach is comprehensive and includes a number of covariates. We account for the proportion of non-tradable shares ($NonTradable$), since this is likely to capture an important source of heterogeneity in firms' propensity to join the reform early. We further classify and match firms according to whether they are ultimately controlled by the state ($StateControl$). Other firm-level control variables include log of number of shares ($Shares$), proportion of shares owned by the State ($StateShares$), firm's age (Age), log of total assets ($Assets$), log of total sales ($Sales$), ratio of cash flow over assets ($CF/Assets$), ratio of fixed assets over number of employees (K/L), debt-to-asset ratio ($Debt$), bank loans over assets ($Loans$), cash-to-assets ratio ($Cash$), price-earnings ratio (P/E), and firm's representation in the industry ($IndRep$) and in the province GDP ($ProvRep$). Given the original goals of the reform program, we include in our matching the log per capita GDP of the province in which the firm is established ($ProvGDP$), as well as a proxy for industry size ($IndSales$) and industry concentration ($IndConcent$). Table 1 lists all variables that are either investigated or used as controls in our tests, providing a brief description of their operationalization.

Table 1 About Here

3.3 Descriptive Statistics

Summary statistics for our sample firms in 2004 are presented in Table 2. Column 1 (full sample) indicates that firms had, on average, nine years of operation under their current charter (recall they were privatized in the 1990s). Sixty-two percent of their shares were non-tradable in 2004 and 37% of shares were owned by the state. Firms seemed to be profitable (average ROE of 4.5%) and with positive prospects (average M/B of 2.1). These and other summary statistics are similar to those found in contemporary papers on Chinese firms (e.g., Li et al., 2011; Jiang et al., 2010). We omit their discussion for brevity.

Following the reform process, we divide our sample into three groups: (1) "pilot group" includes 43 non-financial firms in the May/June-2005 pilot program; (2) "converting in 2005" comprises non-pilot firms that converted their shares at or before December 2005; and (3) "control group" comprises firms that converted their shares after June 2006. We detail shortly how our binary-treatment tests use these three groups of firms.

Table 2 About Here

Table 2 suggests that pilot firms, as well as firms that converted their shares in 2005, are significantly different from control firms for most observables in 2004, before the reform was announced. In fact, firms that complied with the reform earlier were, among other things, bigger, more profitable, more productive, and had more concentrated ownership. Moreover, these firms had also grown faster than those that joined the reform later. These differences suggest that the timing of the reform compliance was not random and might be related not only to the expected outcomes but also to their variation after conversion. Accordingly, it is important to control for pre-treatment characteristics that might be related to both treatment assignment and potential outcome variation. The next section presents our quasi-experimental identification strategy. It adjusts our estimates for pre-treatment differences in covariate and outcome dynamics to obtain causal parameters.

4 Estimation Strategy

We set out to estimate the effects of the 2005 reform. Our goal is to compare outcomes that accrue to firms that join the reform (at the time they join it) to the counterfactual situation of not joining the reform or joining it at a different time. This section discusses the assumptions we make to implement our quasi-experimental strategy.

Even though all A-share firms were forced to change their share structure, they did not comply with the reform all at the same time. This is interesting for identification of causal effects in that, for each point in time, we can compare firms that have already joined the reform with firms that have yet to do so. We must take into account, however, that the timing of compliance is endogenous. Firms could potentially choose when to join the conversion process based on expected outcomes, leading us to incorrect conclusions about the effects of the reform. Another source of problems for our estimations is the fact that idiosyncratic dynamics in firm outcomes could confound our inferences, leading us to assign causation to trend effects that coincide with reform compliance. As we detail in this section, we use a difference-in-difference model combined with a time-varying propensity score matching estimator to address these issues. Before doing so, it is worth providing intuition for our estimation problem and methodology.

In our setting, comparisons between treated and untreated firms can only be made for a limited period. In particular, because firms gradually join the reform, the number of untreated firms decreases as we increase the treatment spell. Moreover, the treated group gradually comprises firms with different time exposures to the reform (different "treatment dosages"). If the effect of the reform is not constant over time, it can be difficult to interpret any empirical estimate due to the composition of the treated group. Accordingly, for our estimations the treatment assignment is defined according to the date when the firm joins the reform.[11] The treatment window is the length between the date

[11] The only exception is the analysis of firms in the pilot program. This is a one-time experiment where we use the standard "being in the reform" (treated) *versus* "not being in the reform" (untreated) comparison approach.

when the firm joins the reform and the date when the effect is assessed. To calculate the average effect of the reform, we estimate a dose-response function that maps time spells into potential outcomes. Under this approach, the difference between two points along the dose-response function measures the effect of complying with the reform in a specific period vis-à-vis complying in a later period.

The dose-response function is estimated using a panel model that accounts for firm- and time-specific effects. Despite these controls, time-varying heterogeneity in outcome dynamics and potential effects can still be a source of endogeneity — they may influence the timing of program compliance and observed outcomes. We need to control for this source of bias by making the entry date orthogonal to time-varying effects. We do so using large sets of covariates under a Generalized Propensity Score (GPS) function. The GPS function gives, for each firm, the probability of joining the reform at a particular point in time conditional on the distribution of pre-treatment covariates and outcomes. As we control for the GPS, along with firm- and time-specific effects, we can hypothesize that, for each point in time within the reform window, a firm's decision to convert its shares is a "conditionally random" event. The role of the GPS is that of identifying and comparing similar firms that did not join the reform at the same point in time, despite having similar odds of doing so. We formalize the steps used in the implementation of the GPS approach shortly.

In the remainder of this section we introduce the notation used in our time-varying treatment effect estimation. We then discuss the assumptions required to estimate the effects of the reform on firms' outcomes, as well as the role of the GPS. The third part presents the empirical method used to estimate the effect on pilot firms and firms that joined the reform in 2005. The last part presents the econometric model used to estimate the dose-response function.

4.1 Notation

The following notation is a time-varying version of the potential outcome approach proposed by Rubin (1974). In this setting, treatment is not assigned at one point in time to all units. Instead, units gradually become treated, so that every date other than the date they are treated can be seen as a counterfactual state.

Let $\mathcal{Y}_{it} = \{Y_{it}(d) \,|d \in \mathcal{D}\}$ be the set of potential outcomes of firm $i \in \{1, \ldots, N\}$ at time $t \in \{0, \ldots, T\}$, where $\mathcal{D} = \{1, 2, \ldots, K\}$ is the set of potential treatment values, and $Y_{it}(d)$ is an ordinary variable (or vector) that maps a particular treatment, $d \in \mathcal{D}$, to a potential outcome. In this time-varying treatment setting, d indicates when the firm may join the reform. For instance, $d = 1$ if the firm were treated since the first period after the reform was announced, whereas $d = K$ if the firm joined the reform in the last period. Accordingly, a greater d indicates less exposure to the treatment. This notation is different from the standard treatment-effects framework notation, where $d = 0$ in the absence of treatment and $d > 0$ for some type of treatment. The notation is useful, however, in cases of time-varying treatment assignment, when the appropriate comparison is not whether one is treated but *when* treatment occurs (see Brand and Xie, 2007).

While d is the ordinary variable that indicates a potential treatment level, $D_i \in \mathcal{D}$ is the random variable that indicates the actual treatment received by firm i. It is worth stressing the difference between d and D_i which indicate, respectively, when the firm *may* be treated and when it is *factually* treated. Finally, note that t refers to the period when the outcome is assessed.

We can simplify the notation by dropping the i subscript, letting $\mathcal{Y}_{it} = \mathcal{Y}_t$, $Y_{it}(d) = Y_t^d$, and $D_i = D$. Then each firm has a set of potential outcomes, as presented in Table 3. The effect of the reform is given by comparisons between different cells in the same column. For example, at period T (column T), $Y_T^2 - Y_T^T$ is the effect of being in the reform for $T-2$ months with respect to joining the reform in period T. It is worth noting that the outcomes under the diagonal of the table (shadowed area) represent situations when the firm has not yet joined the reform. In a more general framework, it is also important to distinguish these pre-reform outcomes because it allows us to assess possible anticipation effects (more on this below).

<u>Table 3 About Here</u>

Given the full set of potential outcomes, the average treatment effect (ATE) and average treatment effect on the treated (ATT) at period t are defined as the expected differences between two potential outcomes (Heckman and Vytlacil, 2007):

$$ATE : \tau_{t,k,k'} \equiv E\left[Y_t^k - Y_t^{k'}\right], \tag{1}$$

$$ATT : \gamma_{t,k,k'} \equiv E\left[Y_t^k - Y_t^{k'} \middle| D = k\right]. \tag{2}$$

In particular, the ATE parameter represents the expected effect of randomly taking some firm from the overall population of firms and forcing it to join the reform program at date k instead of date k'. The ATT parameter represents the mean effect of joining the reform at date k instead of k' on those firms that have actually complied with the reform at k.

As the potential treatment starting time, d, can assume many values (non-pilot firms can comply with the reform at different times during the Sept. 2005 to Dec. 2006 window), it is difficult to obtain an average estimate for each potential outcome (or each cell in Table 3). For this reason, Imbens (2000) and Hirano and Imbens (2004) consider what is called the dose-response function, which in our case can be represented by:

$$\mu_t(d) = E\left[Y_t^d\right], \text{ with } d \in \mathcal{D}. \tag{3}$$

Then the ATE parameter can be defined as follows:

$$E\left[Y_t^k - Y_t^{k'}\right] = \mu_t(k) - \mu_t(k'). \tag{4}$$

Similarly, we can define the ATT parameter as follows:

$$E\left[Y_t^k - Y_t^{k'} | D = k\right] = \mu_t(k, D = k) - \mu_t(k', D = k), \tag{5}$$

where $\mu_t(d, D = k) = E\left[Y_t^d \middle| D = k\right]$ is the conditional dose-response function.

The left graph in Figure 3 gives us an example of ATE calculated from a dose-response function. For outcome evaluation at time t, we compare two points on this function. The first point is the expected outcome at t if the firm joined the reform at the early date k. The second point is the expected outcome at t if the firm joined the reform later, at date k'. Since $\mu_t(k) > \mu_t(k')$, the effect of starting earlier (i.e., being more time under treatment) is positive in this example. Note also that this hypothetical function is constant beyond t. This means that at time t the reform has no effect on firms that have not yet joined it.

Figure 3 About Here

To go from the multi-valued-treatment framework to a binary-treatment framework and apply approaches commonly used in the treatment effect literature, we have to assume that the dose-response function is locally constant. Namely, the response at period t is assumed to be constant if treatment was given up to some period k and after another period k', with $k' > k$. In practical terms, it implies that parameters (1) and (2) can be rewritten as:

$$\tau_{t,k,k'} = E\left[Y_t^{d \leq k} - Y_t^{d \geq k'}\right], \tag{6}$$

$$\gamma_{t,k,k'} = E\left[Y_t^{d \leq k} - Y_t^{d \geq k'} \middle| D \leq k\right]. \tag{7}$$

The right graph in Figure 3 depicts the implication of the local constancy assumption. In calculating the ATE, one would (separately) average the points along the dose-response function up to k, and those beyond k'. One would then take the difference between these two averages.

4.2 Identification Assumption and the Role of GPS

The fundamental evaluation problem is a missing data problem. In our setting, we cannot observe two or more potential outcomes given by different compliance dates for the same firm — if we observe Y_t^k, we cannot observe $Y_t^{k'}$ for the same firm. If $D = k$, the set of missing counterfactual outcomes, $\mathcal{Y}_t \backslash Y_t^k$, must be estimated in order to obtain an estimate for parameters (1) and (2).

A standard assumption we first consider is Imbens' (2000) "weak conditional independence assumption," also known as Rubin's (1976) "missing at random assumption." It implies that, conditional on pre-determined covariates X_0, assignment to treatment D is independent from the potential outcome given by D, $Y_t^{d=D}$. The assumption can be stated as:

$$\mathbf{1}(D = d) \perp Y_t^d \Big| X_0,$$

for each pair $d \in \mathcal{D}$ and $t \in \{0, \ldots, T\}$.

Although this assumption guarantees identification when the treatment assignment is based on observables, it requires that the potential outcome Y_t^d is (conditionally) unrelated to the probability of D being equal to d. That is, firms did not take the time d-specific outcome into account when they decided to comply with the reform at d. This condition is strong and might be violated in the data. The estimation approach used for cases in which treatment assignment can be influenced by potential outcomes (due to unobserved heterogeneity) is the difference-in-difference/fixed-effect model. This model assumes that:

$$\mathbf{1}(D = d) \perp Y_t^d - Y_{t'}^d, \text{ with } t \neq t'.$$

However, this condition, too, is likely to be violated if the treatment assignment is associated with the dynamics of the potential outcome. In our setting, the past firm performance and share valuation might influence shareholders' decision to agree on share conversion (assignment into treatment). These performance and value dynamics may also affect the post-treatment outcomes of firms that convert their shares, confounding any causal relations.

Similar to what is suggested by Heckman et al. (1997) and Abadie (2005) for this type of problem, we use a more general independence assumption that allows for selection on unobserved outcomes and variables related to outcome dynamics:

$$\mathbf{1}(D = d) \perp Y_t^d - Y_{t'}^d \Big| X_0, \text{ with } t \neq t'. \tag{8}$$

This assumption implies that, conditional on pre-treatment covariates (including pre-treatment outcome dynamics), idiosyncratic shocks to firms' outcomes are independent from the date when they joined the reform. This allows us to use pre-treatment firm characteristics and decisions to predict the part of outcome dynamics that is related to the compliance date. With this compliance model we can then simulate a randomization of assigned dates (akin to standard treated–control matching for one-time treatment assignments).

Conditioning on a high-dimensional X_0 can be difficult in practice, especially in small samples. For the case of binary treatment, Rosenbaum and Rubin (1983) show that if both the "balancing property" and the conditional independence assumption are satisfied, then it suffices to adjust for a unidimensional propensity score to identify the parameter of binary treatment effect. For the case of continuous and multiple treatments, several studies define what is called Generalized Propensity Score (GPS).[12] The GPS, $R(X_0)$, is the conditional probability of receiving the treatment, D:

$$R(X_0) \equiv r(D, X_0) = \Pr(D = d | X_0), \tag{9}$$

where $r(., X_0)$ is called the GPS function.

[12] See Joffe and Rosenbaum (1999), Imbens (2000), Robins et al. (2000), Lechner (2001), Lu et al. (2001), and Imai and van Dyk (2004).

In this context, Hirano and Imbens (2004) provide a generalization of Rosenbaum and Rubin's result. They show that it suffices to adjust for the GPS to identify the dose-response function, $\mu_t(d)$, under the weak conditional independence assumption and the following weak balancing condition:

$$X_0 \perp \mathbf{1}(D = d) | \, r(d, X_0), \text{ for all } d \in \mathcal{D}.$$

The GPS function is usually unknown and its parameters must be estimated. Assume that for every X_0 there exists a unique finite dimensional parameter θ such that $r(d, X_0) = r(d, \theta(X_0))$ for all $d \in \mathcal{D}$. That is, $r(d, X_0)$ depends on X_0 only through $\theta(X_0)$. Then, as suggested by Imai and van Dyk (2004),[13] all information in X_0 that is contained in the GPS function can be summarized by an unique value, $\theta(X_0)$. The question is then how to model the GPS function in a way that naturally fits the application of interest.

Note that in our setting the treatment assignment, D, represents the date when the firm joins the reform. Reform compliance must happen within a pre-determined time window. Moreover, once the firm is treated, it cannot become untreated. As such, the probability of receiving treatment $d \in \mathcal{D}$, $r(d, X_0)$, can be naturally modeled as a survival problem. This, in turn, allows us to estimate the GPS using the Cox's proportional hazard model (Cox, 1972). There are two reasons for estimating the GPS using the Cox model. First, for each firm the GPS can be assumed to be function of a constant parameter, $\theta = \exp(X_0\beta)$, which does not depend on D. This parameter can then be defined as the GPS index. Second, as the baseline distribution of D is estimated nonparametrically, the GPS function estimated by the Cox's model is more flexible than that estimated using the Gaussian model, as in Hirano and Imbens (2004).

To facilitate the balance of covariates, the GPS index, θ, is nonparametrically estimated using a restricted cubic spline in which knots are selected using backward elimination of weak predictors (cf. Royston and Sauerbrei, 2007). Let $\tilde{X}_0$ be an increased-dimension vector with cubic spline terms and $\hat{\beta}$ be the estimated spline coefficients of the Cox model. The estimate for the GPS index is given by:

$$\widehat{\theta} = \exp\left(\tilde{X}_0'\widehat{\beta}\right), \tag{10}$$

while the estimated GPS is given by:

$$\begin{aligned} \widehat{R} &\equiv \widehat{r}(D, X_0) \\ &= \widehat{\theta} \cdot \widehat{\lambda}_0(D) \cdot \widehat{S}_0(D)^{\widehat{\theta}}, \end{aligned} \tag{11}$$

where $\widehat{\lambda}_0(.)$ is the estimated baseline hazard function and $\widehat{S}_0(.)$ is the estimated survival function. In what follows, we discuss how we implement the GPS function in our estimation approach.

[13] See also Joffe and Rosenbaum (1999) and Lu et al. (2001).

4.3 Binary Treatment Effect Estimator (PSM)

Under the weak conditional assumption, we can estimate parameters (6) and (7) using conditional versions of the difference-in-differences (DID) model. The traditional way to adjust for covariates is to include them linearly in the model. Although this accommodation of covariates controls for heterogeneity in outcome variation, $\Delta_{t,t'}Y = (Y_t - Y_{t'})$, it does not control for heterogeneity in the treatment effect. Namely, it assumes that the treatment effect is constant across different groups of firms. This assumption is likely to be violated, leading to inconsistent ATE and ATT estimates (see Freedman, 2008).

Another way to adjust for covariates is by propensity score matching (PSM) (Heckman et al., 1997, 1998). From the estimated GPS function, we can calculate the propensity score, $\widehat{p}_{k,k'}$, as follows:

$$\widehat{p}_{k,k'}(X_0) = \frac{\widehat{\Pr}(d \leq k \,|X_0)}{\widehat{\Pr}(d \leq k \,|X_0) + \widehat{\Pr}(d \geq k' \,|X_0)} = \frac{1 - \widehat{S}_0(k)^{\widehat{\theta}}}{1 - \widehat{S}_0(k)^{\widehat{\theta}} + \widehat{S}_0(k')^{\widehat{\theta}}}. \tag{12}$$

The PSM estimator is performed by matching the estimated propensity score, $\hat{p}_{k,k'}(X_0)$, between the group of treated firms that joined the reform earlier (up to period k) and a group of control firms that joined later (after period k'). One can then compute differences in outcomes, $\Delta_{t,t'}Y$, in this matched sample. Matching methods do not assume that the treatment effect is constant over different groups of firms, so we can compute both the ATE and ATT parameters.

Implementation of our binary treatment tests allows us to estimate only the ATT parameter due to relatively small number of firms. This parameter is estimated by nearest-neighbor matching (NNM) with the bias correction suggested by Abadie and Imbens (2006).[14] Our matching estimations use overlap regions defined as follows (Dehejia and Wahba, 1999, 2002):

$$C_{k,k'} = \left\{ i : \widehat{p}_{k,k',i} \in \left[\min_{\{j:\mathbf{1}(D_i \leq k) = \mathbf{1}(D_j \geq k')\}} \left(\widehat{p}_{k,k',j}\right), \max_{\{j:\mathbf{1}(D_i \leq k) = \mathbf{1}(D_j \geq k')\}} \left(\widehat{p}_{k,k',j}\right) \right] \right\}. \tag{13}$$

Our binary treatment estimations consider two thresholds, k, for the treatment group. The first is June 2005, which corresponds to the pilot firms. The second is December 2005, encompassing firms that complied with the reform up until the end of 2005. The threshold for the control group, k', is July 2006. This is to say that we extract our controls from a pool of firms that had yet to join the reform as of June 2006. This threshold is set so as to allow for sensible outcome comparisons between treated and control units; that is, exposure to treatment is sufficiently different to produce measurable potential effects.

To see this last point, note that if k and k' were too close, then one could end up comparing treatment effects across units that receive treatment almost at the same time. More concretely, if we would set k' in January 2006, then our treatment–control comparisons would be contrasting the

[14] We also estimate the ATT using kernel matching (KM) as a robustness check, but results are very similar.

behavior of firms complying with the reform in December 2005 (treated) and those complying in January 2006 (control). The outcomes of those firms would likely be indistinguishable at the treatment evaluation time t, say, December 2006. By setting k' in July 2006 we guarantee a minimum of six months of separation between treated and control firms. In other words, the 2005 treated firms have at least six months greater exposure to treatment at the time of treatment evaluation. For the pilot firms, that difference is at least 12 full months.[15]

The baseline period, t', is December 2004, well before the reform was publicly discussed. We use the following treatment evaluation assessment dates, t: December 2005, December 2006, and December 2007. According to this time frame, in December 2005 we assess the short-run effects of the program on firms with at most 7 months in the reform. In December 2006, we compare firms between 12 and 19 months in the program with similar firms between 0 and 5 months in the program. This allows us to gauge the existence of medium-run effects. Finally, in December 2007, we compare firms between 24 and 31 months in the reform with firms between 12 and 17 months in the reform to assess longer-term effects. As a falsification test, we also estimate the treatment effect by setting December 2003 as the baseline period, t', and December 2004 as the assessment period, t. That is, we estimate the treatment effect *before* the share reform takes place.

4.4 Multi-Valued Treatment Effect Estimator (GPS)

Let $Z_{it} = \max(0, t - D_i)$ be the time of exposure to treatment. Then consider the following fixed-effects model:

$$Y_{it} = \mu(Z_{it}) + \varphi_t + \eta_i + \upsilon_{it}, \tag{14}$$

where $\mu(.)$ is the dose-response function of Z_{it} on Y_{it}, η_i is the firm-specific effect, φ_t is the time-specific effect, and υ_{it} is the error term. In the estimations performed for multi-valued treatment tests, $\mu(.)$ is assumed to be a restricted cubic spline function with five knots, $k_n = 6, 12, 18, 24, 30$.

An estimate for the dose-response function, $\mu(q)$, is given by the within-group estimator for equation (14). As we have discussed, the consistency of this estimator requires that the heterogeneity in the outcome variation, $\upsilon_{it} - \upsilon_{it-1}$, is not related to the treatment assignment, D_i. To weaken this assumption, we can control for pre-treatment covariates, X_0, by means of either the estimated GPS, $\hat{R}$, or the GPS index, $\hat{\theta}$.

Since $R(X_0)$ represents the conditional probability of the firm being assigned to its actual treatment, if $R(X_0) = 1$, then the compliance date, D_i, can be perfectly predicted by X_0. If $R(X_0) = 0$, then D_i is completely unpredictable. Giving higher weight for those firms whose $R(X_0) \to 0$ and lower weight for those whose $R(X_0) \to 1$ is a way of simulating an experiment (making D_i conditionally random). A simple way to operationalize this approach is to weight all firm observations by

[15] To check the robustness of our findings, we have experimented with different values for k'. Our results are qualitatively similar even when we set it in January 2006.

the inverse of their estimated GPS (Imbens, 2000; Robins et al., 2000):

$$\omega_i = \frac{1}{\sqrt{\hat{R}_i}}.$$

This method is usually called Inverse Probability Weighting (IPW). A consistent estimator for the dose-response function, $\mu(q)$, is thus given by a weighted version of the within-group estimator for equation (14) (see Wooldridge, 2007).

Hirano and Imbens (2004) suggest the inclusion of the estimated GPS, $\hat{R}_i$, in equation (14), interacting it with Z_{it}, to control for covariates. Note, however, that this regression cannot be interpreted as an estimate for $\mu(.)$ because $\hat{R}_i$ also depends on D_i. The estimate for $\mu(.)$ requires a second step in which the estimated GPS, $\hat{R}_i$, is replaced by the GPS function evaluated at the treatment level of interest, $r(d, X_0)$. As the GPS index, $\hat{\theta}_i$, does not depend on D_i, Imai and van Dyk (2004) suggest the inclusion of $\hat{\theta}_i$ in equation (14) in lieu of $\hat{R}_i$. In this way, the estimation of the dose-response function becomes straightforward. On the other hand, they also suggest to split the sample by group values given by $\hat{\theta}_i$, estimate a regression for each subsample, and then integrate the estimates to obtain the average effect. Although this stratification matching procedure makes the model less parametric, it also allows for discontinuity in the dose-response function across subsamples.

Instead of splitting the sample, our strategy is to estimate the following spline equation, where the control for $\hat{\theta}_i$ is non-parametric:

$$Y_{it} = \mu(Z_{it}) + \mu(Z_{it}) \cdot h\left(\hat{\theta}_i\right) + \varphi_t + \eta_i + \upsilon_{it}, \tag{15}$$

where $h(.)$ is a mean-centered cubic spline function of $\hat{\theta}_i$. For $h(.)$, there are four knots placed at equally spaced quantiles of $\hat{\theta}_i$. As all components of this function are mean-centered, the second term in the right-hand side of (15) is zero for the average firm. Thus, the within-group estimator for $\mu(.)$ directly gives us the estimate for the average dose-response function. It is worth noting that combining IPW and regression of equation (15) has a "double robustness" property. If the regression model is correctly specified, then weighting by ω_i does not affect its consistency. Likewise, adjusting for the GPS index as in equation (15) does not affect the estimate if the covariates have already been balanced by weighting with ω_i (see Robins and Rotnitzky, 1995).

Besides controlling for covariates, the GPS estimates are also used to delimit the overlap sample. The overlap region is defined as follows:

$$C = \left\{ i : \widehat{\theta}_i \in \left[\min_j \left(\widehat{\theta}_j\right), \max_j \left(\widehat{\theta}_j\right) \right], \text{ with } |D_i - D_j| \geq \varepsilon \right\}, \tag{16}$$

where ε is the width that delimits how similar the firms are in terms of treatment. This overlap rule implies that for every firm on the common support, there are comparable firms with sufficiently distinct treatments. We let the width, ε, be equal to 6 months in our estimations.[16]

[16] We also defined a common support with $\varepsilon = 12$, but there was no significant change in terms of balance.

From any estimate for the dose-response function, we can obtain an estimate for the average time-varying treatment effect (ATE) of the following form:[17]

$$\begin{aligned} \widehat{\tau}_{t,k,k'} &= \widehat{\mu}\left(t-k\right) - \widehat{\mu}\left(t-k'\right) \\ &= \widehat{\mu}\left(q\right) - \widehat{\mu}\left(q'\right). \end{aligned} \tag{17}$$

In the estimations that follow, we consider $q' = 0$ and $q \in [1, 30]$. In words, we will be comparing the effects of the reform on firms treated from 1 to 30 months with the counterfactual case of no treatment.

5 Results

5.1 GPS Estimation and Its Balancing Property

Our time-varying matching approach uses a large number of control variables. They capture firm idiosyncratic characteristics (e.g., location, industry, ownership), policy decisions (made before the reform was announced, such as dividend payments or capital investments), and pre-reform outcomes (e.g., firm performance and value).[18] Our estimations compare firms that have similar characteristics, have followed similar corporate policies, observed similar past outcomes, and face the same economic conditions, but are different with respect to the date of compliance with the share reform.

After calculating both the binary propensity score and the GPS index for each firm, we can identify the relevant overlap samples. Figure 4 shows the overlap between treated and control groups' distributions in the binary comparison. On the left panel, we compare the pilot firms (with k equal to June 2005) and control firms (with k' equal to July 2006) in terms of the estimated propensity score. The panel on the right shows the overlap between non-pilot firms that joined the reform in 2005 (k equal to December 2005) and the 2006 control firms. Although treated and control firms are unevenly distributed in the propensity score line, the figure shows that there is a sufficient number of treated–control matches within the common support, delimited by the dotted lines.

FIGURE 4 ABOUT HERE

To verify the balancing property of the propensity score, we estimate the average difference in pre-treatment covariates between treated and their matched controls after matching (via NNM). The differences between pilot and control firms are shown in Panel A of Table 4. Notice that, after matching, we find no significant differences between these groups. This balance is obtained not only for those covariates included in our model, but also for all other pre-treatment outcomes and covariates available from our dataset. Panel B of Table 4 presents the average differences between firms treated in 2005 and the control firms. Once again, no significant cross-group differences are identified after

[17] We do not estimate the ATT because there is no particular group of interest when the treatment is continuous.

[18] The set of control variables are discussed in Section 3 and listed in Table 1.

matching. We infer that the estimated propensity scores satisfactorily balance the pre-treatment conditions of the firms used in our contrasts.

TABLE 4 ABOUT HERE

As discussed in Section 4, the definition of overlap sample is different when the treatment is multi-valued. Figure 4 illustrates the differences between firms outside of the common support region and those inside the support in terms of the binary propensity score, $\widehat{p}_{k,k'}$. Differently from Figure 4, Figure 5 depicts the dispersion of the GPS index, $\widehat{\theta}$, at every point in time (an independent plot for each month starting from May 2005). The small triangles in the figure indicate that for 49 out of $1,054$ firms, the GPS cannot find a similar control among firms initiating their treatment at least 6 months later. For this reason, we exclude those firms from the sample when implementing the GPS-based approach.[19]

FIGURE 5 ABOUT HERE

Imai and van Dyk (2004) propose a procedure to test the balancing property of the GPS function. In it, each pre-treatment covariate is regressed on the treatment assignment, D, controlling for $\hat{\theta}$. If the coefficient of D is significantly different from zero, then the estimated GPS does not satisfy the balancing property for that covariate.[20]

Table 5 reports the Imai-van Dyk regression coefficients and p-values, before and after controlling for the estimated GPS. Without the GPS control (under column 1), only a couple of covariates are balanced; i.e., most of pre-treatment characteristics and outcomes are significantly related to the treatment assignment. Controlling for the GPS index (column 2), in contrast, eliminates all significant relations between covariates and the compliance date.

TABLE 5 ABOUT HERE

5.2 Binary Matching Results

This section uses a standard treated–control assignment approach to measure the impact of the share conversion program. We do this separately for the pilot firms in the sample (43 firms) and for the firms converting in 2005 (300 firms). To shorten the analysis, we focus on real business outcomes such as corporate investment, employment, productivity, and profitability. These tests help motivate

[19] When estimating the time-varying effects using the GPS-based approach, we also exclude from the sample the 25 firms that did not comply with the reform by February 2007. While including these firms makes our results stronger, one concern is whether these firms qualified (or ever intended) to join the reform.

[20] More specifically, one estimates the following equation for each $x_0 \in X_0$:

$$x_0 = b_0 + b_1 D + g\left(\hat{\theta}\right) + \xi,$$

where $g\left(\hat{\theta}\right)$ is a cubic spline function. Then one tests whether $b_1 = 0$.

our analysis on the real implications of stock market trading. Our main results are presented in the next section, where we use a time-varying, multi-valued treatment approach to evaluate a wide range of outcomes related to the reform.

5.2.1 Effects of the Reform on Pilot Firms

Estimates for real effects of the reform on pilot firms are shown in Table 6. To study the changes brought by the reform, we consider changes in outcome variables from the end of 2004 (prior to the reform) to: (1) the end of 2005 (top panel), (2) the end of 2006 (middle panel), and (3) the end of 2007 (bottom panel). These windows give us a glimpse at the effects of the reform over time. As discussed above, however, these binary, stale comparisons should be taken with a grain of salt. In addition to the estimated effect on the outcomes of interest, we report the conditional difference between treatment and control groups in terms of months spent in the reform (Z). This allows us to interpret our estimated effects with respect to the average time of exposure to the program.

The OLS estimates of column 1 (which lack any controls) suggest that pilot and control firms had distinct outcome variations in the reform window. For example, the growth in fixed assets (ΔK) and number of employees (ΔL) were disproportionately higher for pilot firms from 2004 to 2007. With about six months into the reform (end of 2005), we also find positive significant differences in profits ($NetIncome$), productivity ($Sales/K$) and return on equity (ROE), but these differences decline over time. After linearly controlling for covariates (column 2), the differences in employment and fixed assets become smaller. In other words, part of the observed differences between pilot and control firms can be explained by pre-treatment characteristics.

TABLE 6 ABOUT HERE

Results become somewhat weaker when we restrict attention to estimations using matching (column 3). The NNM estimates suggest that the reform only had an immediate effect on fixed assets and return of equity. After about six months, assets grew 21% more for pilot firms, while ROE was 8.6 points higher for those same firms. Interestingly, the former effect is persistent over time, while the latter effect disappears. At the end of 2006, with an average 15-month difference in exposures between pilot and control firms ($Z = 14.9$), fixed assets in the pilot group grew by about 69% more than in the control group. One year later, in 2007, there is still a significant differential increase of 58%. Market-to-book and employment were also positively affected by the reform, but results only became economically and statistically significant in 2006. Market-to-book ratio, in particular, increased 0.9 by the 15^{th} month (December 2006), but declined over the following year. By December 2007, pilot firms' employment growth was 35% higher than that of matched control firms. We observe no significant effects on $NetIncome$ and $Sales/K$.

5.2.2 Effect of the Reform on Firms Converting Shares in 2005

Table 7 presents treatment effect estimates for non-pilot firms that complied with the reform at or before December 2005. Examining this treatment group has the two advantages; namely, a larger sample size and the fact that these outcomes are less likely to be manipulated by the government (no "showcase" effect). On the other hand, these firms opted for joining the reform earlier. Thus the treatment assignment is now endogenous due to self-selection (a problem we address in the next section).

Table 7 About Here

The OLS estimates reported in the first column of Table 7 resemble those of Table 6. The results suggest that firms that chose to comply with the reform just after the pilot phase had outcome variations that are similar to firms in the pilot. Furthermore, the point estimates obtained by matching (NNM), in the third column, are also very close to those of the pilot firms. To some extent, these findings minimize concerns about biases caused by "optimal timing" of program compliance (self-selection). That is, whether selected by the government into a pilot trial or voluntarily complying with the reform program, firms that converted their shares in 2005 observed changes in real outcomes that were pronounced and persistent for years after conversion vis-à-vis similar firms that only converted their shares later in the reform program.

Even though treated non-pilot firms were slightly less exposed to the reform, we verify that the estimated effects on fixed assets and employment growth are robust to changes in the treatment group. Estimates for 2006 in Table 7, for example, show that the fixed assets of firms that entered the reform in 2005 grew 47% more than the assets of firms that entered the reform, on average, about a year later. The differential growth in employment rates is about 30% in favor of firms that entered the reform in 2005. These estimates show that while both treatment and control firms had been exposed to treatment by December 2006, the firms exposed for a longer time span registered more substantial (positive) outcome variations.

5.2.3 Falsification Test

We perform a falsification test to check if our matching procedures are effective in controlling for trends in outcome variation. We do so by estimating ATT effects in 2004; that is, *before* the reform takes place. If the matching estimates are unbiased, outcome variation from 2003 to 2004 should *not* point to significant treated–control group differences.

Results in Table 8 are interesting in highlighting that the simple OLS-DID model does not address the treatment selection problem. The numbers in the table suggest, for example, that the pilot firms (as well as those firms complying in 2005) were experiencing, on average, higher growth in fixed assets and employment even before the reform was announced. With the NNM, however, it is possible to eliminate these biases. Indeed, Table 8 reports that no cross-group differences are found after matching (third column). Similar patterns are found when we consider longer trends in pre-treatment variables and outcomes; e.g., using data going back to 2002 (results available upon request).

TABLE 8 ABOUT HERE

5.3 Time-Varying Effects of the Reform

This section presents our paper's central results, which are based on time-varying treatment effects estimations. Because we want to describe the effects of the reform in the short-, mid-, and long-runs, we condense the presentation by graphing (as opposed to tabulating) our estimates. These estimates are taken from the fixed-effect model with IPW and regression adjustments for the GPS (equation (15)). For ease of exposition, we report and discuss separately the outcomes that are related to real firm performance (such as investment, employment, and productivity), those related with financial policy (dividend payments and stock issuance), and other outcomes (such as merger deals and related party transactions). We start with an evaluation of stock liquidity.

5.3.1 Effects on Liquidity

Figure 6 presents estimated time-varying effects of stock conversions (into tradable status) on stock liquidity. The plots represent the expected difference between being in the reform for Z months, $\mu(Z)$, vis-à-vis the counterfactual case of not complying with the reform, $\mu(0)$. Figure 6 shows that stock liquidity increases immediately after a firm converts its shares. For the liquidity ratio measure ($LiqRatio$), there is an immediate and persistent positive reform effect. Thirty months after the reform, that ratio increases over 40% compared to the baseline case of non-conversion. The effect on the share turnover measure ($ShareTurnover$) is less immediate, but it increases up to two years after conversion. In the long run, share turnover becomes about 20% higher due to the share conversion.

FIGURE 6 ABOUT HERE

The evidence in Figure 6 is robust and confirms our base prior that corporate shares become significantly more liquid after conversion into tradable status. In turn, we investigate the impact of this significant increase in secondary market liquidity on key corporate outcomes.

5.3.2 Effects on Real Outcomes

Figure 7 presents time-varying effects of share conversions on each of the business performance measures we have previously discussed: ΔK, ΔL, $Sales/K$, $NetIncome$, ROE, and M/B.

FIGURE 7 ABOUT HERE

The first panel of Figure 7 suggests that corporate investment, ΔK, does not respond to share conversions in the short run (if anything, there is a small decline in investment spending). After about

18 months, however, the converting firm invests more than in the case of non-conversion, and keeps expanding its capital base afterwards. Thirty months after its shares become tradable, the firm's annual investment rate is 11% higher than in the counterfactual case of non-conversion. Noteworthy, the growth in investment happens without an increase in the number of employees. In particular, the second panel of Figure 7 shows that labor growth, ΔL, remains flat for complying firms over our 30-month horizon. These two results suggest that firms adjust their capital-to-labor ratios (make their workforce more productive) after a larger fraction of their equity shares become traded in the organized exchanges.

Gains in productivity following conversions are also implied by the third panel of Figure 7, where we plot the effect of the share conversion program on the firm's sales-to-capital ratio, $Sales/K$. The effect of conversions on $Sales/K$ is immediate and increasing up 20 months after compliance, when $Sales/K$ becomes nearly 30% higher than in the counterfactual case of non-compliance. In the longer run (30 months), this ratio is about 20% higher due to conversion. These results add to existing evidence that previous market-oriented reforms in China led to measurable gains in productivity (e.g., Li, 1997; Groves et al., 1994). Improvements in corporate efficiency following share conversions into tradable status are also consistent with theoretical priors that managers will want to increase efficiency once their firms' shares become liquid and priced by the market.

The results just described suggest that the share reform had a positive impact on corporate growth and productivity. Those gains to business fundamentals are consistent with the gains in profitability that we also observe in Figure 7. In particular, the dose-response function of $NetIncome$ increases up to the 20^{th} month of compliance. By then, operating revenues grow 14% more than expenses, increasing the firm's profitability. In the long run, the reform leads to an increase of 10% on $NetIncome$. In a similar fashion, ROE increases up to the 18^{th} month following conversion, when it is about 1.5 percentage points higher than the counterfactual benchmark (this figure represents 33% of the average ROE). After that point, however, ROE declines. While the initial growth is consistent with firms expanding and performing better, the subsequent decline can be explained by the higher proportion of firms issuing equity overtime, which we discuss below.

As the last panel of Figure 7 shows, stock conversions also lead to significant increases in corporate valuation. In particular, market-to-book, M/B, increases for about 20 months following conversion, when it almost doubles with respect to the baseline average. After 30 months, M/B is 1.5 higher than in the case of no share conversion (about 71% higher than the baseline average). These immediate, strong effects of stock liquidity on corporate values are notable. Arguably, equity valuation is the ultimate summary statistic of corporate wealth. Our valuation results suggest that stock conversions were very beneficial to equityholders in China.

The findings we report on corporate investment, employment, productivity, profitability, and value invite further discussion on the effects of market-oriented reforms in countries like China. More broadly, they reveal the costs of imposing restrictions on the functioning of stock markets. By hindering investors' ability to trade their claims on corporate cash flows, the dual-share class system used in China seems to have distorted firm policies and hurt private sector growth. Our results show that the repeal of restrictions on stock trading unleashed a secondary market for securities that quickly helped shape corporate policies and outcomes. The effects we observed point to sizeable gains to Chinese firms and their shareholders, highlighting to the importance of secondary stock market transactions for real economic activity.

5.3.3 Effects on Financial Policies

Figure 8 shows the estimated time-varying reform effects on equity issuance ($Issuance$) and dividend payout ($Dividend$). Like the results on liquidity and real performance, the plots represent the expected difference between complying with the reform for Z months, $\mu(Z)$, versus the baseline case of non-compliance, $\mu(0)$.

<u>Figure 8 About Here</u>

The left panel of Figure 8 suggests that the reform is responsible for a significant reduction in cash dividend payments. Despite the larger error bands associated with tests using financial policy variables, we find that payout ratios fall by about 10 percentage points 24 months after a firm's shares become tradable in the market. This difference represents nearly one-third of the pre-reform average payout ratio. As hypothesized, this change in distribution policy seems to capture a shift in the preferences of shareholders, from high to low cash dividend payouts.

As discussed above, a sharp increase in stock liquidity should renew firms' interest in equity issuance as a source of funding. Accordingly, we find that firms are more likely to issue new shares after they comply with the conversion program.[21] In particular, the right panel of Figure 8 shows that the probability that a firm issues equity grows steadily after all of its shares start to trade. Indeed, 30 months after conversion the likelihood of issuance is 40% higher than the baseline case of non-conversion. Looking at the aggregate effect of this shift in the propensity to issue equity, we note that only 1% of the Chinese listed firms issued equity in 2004, while in 2007 this figure was 13%.

5.3.4 Effects on Other Firm Variables

We deepen our analysis of the reform by looking at other outcomes that speak to the efficiency of the capital allocation process and corporate governance in China. Figure 9 considers the effect of share conversions on merger activity ($M\&A$), related party transactions (RPT), equity ownership concentration ($OwnerConcent$), and CEO turnover ($CEOTurnover$).

[21] The 12-month delay is to be expected given various CSRC policies that made it difficult for firms to issue new securities during the first few months following conversion.

Figure 9 About Here

A deeper, more liquid market for stocks should facilitate the emergence of corporate control transactions that are made possible through the use of shares as a means of exchange (see Bhide, 1993; Maug, 1998). Results depicted in the first panel of Figure 9 are consistent with this conjecture. After converting its shares, and following the subsequent increase in issuance activity, a firm is more likely to engage in M&A transactions. By the 30^{th} month after conversion, the probability of having a M&A deal per year is 20 points higher than in the case of non-conversion. In aggregate, this effect represents an increase of 60% in the number of firms making M&A deals per year.

We also look at the effect of share conversion on firms' propensity to engage in related party transactions. Our prior is that converting shares into a tradable status may make managers more accountable for their actions and discourage them from engaging in dealings that are detrimental to most holders of public stocks. We find only weak support for this hypothesis. The second panel of Figure 9 suggests that $RPTs$ declined slightly following conversions, and that this decline is long-lasting. The estimates, however, are too noisy to allow us to conclude that firms are less inclined to engage in RPTs after a large fraction of their shares start to trade in secondary markets.

Finally, we examine outcome variables that capture changes in control structure as a result of equity conversions. Figure 9 suggests that ownership concentration among top-5 shareholders ($OwnerConcent$) drops substantially following conversions. By the 30^{th} month into the program, $OwnerConcent$ is about 10 percentage points lower than in the counterfactual case of non-conversion, which represents a reduction of 43% in the average concentration index. This finding is consistent with the idea that the reform represented a shock to governance structures under which a small fraction of shareholders had ultimate control of the firm. In the same vein, one could expect to see poor managers being forced out of their jobs more often when shareholders start emphasizing performance with a market-based benchmark (the price of liquid corporate securities). Notably, however, converting firms became more profitable and valuable. Perhaps not surprisingly, we find that CEOs are slightly less likely to be replaced following stock conversions. The probability of turnover ($CEOTurnover$) drops by 10 percentage points after 30 months into the reform. That estimate, however, is statistically barely indistinguishable from zero.

5.3.5 Treatment Heterogeneity

We have discussed the impact of the 2005 split-share reform on stock market liquidity and breath of ownership in China. Confirming our basic priors, we found evidence that the reform had an immediate and persistent positive effect on stock liquidity. By the same token, the ownership of those shares became less concentrated. We argued that a more liquid, deeper stock market has in turn led to significant changes in firms' real and financial policies. While our results are consistent with this interpretation, one would like to see that mechanism more fully characterized. One way to better characterize our main claims is to check whether firms that had the most to gain from the conversion program indeed observed the largest responses to the reform. In this section, we identify heterogeneity in treatment outcomes by examining whether firms whose stocks were less liquid and more concentrated prior to the reform present the largest responses to the share conversion program.

We operationalize our test of the reform mechanism by entering a couple of interaction terms in our multi-variate model (equation (15)). In particular, in a first examination, we interact a firm's pre-reform liquidity ($ShareTurnover$) and the months since it joined the share reform (Z). This interaction term captures the product between a firm's potential to gain from the treatment (the degree to which the firm stock was liquid before the conversion program) and the firm's exposure to the treatment (number of months since conversion). We expect firms with less liquid stocks prior to conversion to observe the most pronounced responses to the reform; i.e., we expect a negative interaction term between $ShareTurnover$ and Z. In a similar vein, we interact a firm's pre-conversion concentration index ($OwnerConcent$) and Z, and expect a positive interaction effect. The results from these interactive models are in Table 9, which present the marginal increase in the treatment effect as a function of changes in lagged stock liquidity (first half of the table) and ownership concentration (second half of the table) for compliant firms. For brevity, these tests focus only on the six real-side variables previously examined (ΔK, ΔL, $Sales/K$, $NetIncome$, ROE, and M/B).

<u>Table 9 About Here</u>

Results in Table 9 suggest that the impact of stock conversions on firms' investment (ΔK) and employment growth (ΔL) are more pronounced for firms that were less liquid and that had more concentrated ownership prior to conversion. Estimates of these marginal impacts are, however, not statistically significant. The effects of liquidity and concentration on productivity outcomes (captured by $Sales/K$), however, are very significant. The estimate reported in the first column implies that for firms whose stocks were 10% less liquid than the average prior to the reform, the effect of share conversion on $Sales/K$ is 0.2% higher. This estimated sensitivity is sizeable if one considers that, at its peak, the average response of $Sales/K$ to the conversion process is 0.3%. In a similar fashion, the result from the second column indicates that the effect of share conversion on $Sales/K$ is 0.25% higher when we increase the firm's ownership concentration index slightly by 1 percentage point (the standard deviation of the concentration measure is 0.13).

The average effects of the reform on firms' profitability and value also change with respect to pre-conversion liquidity and concentration characteristics. Consistent with our proposed liquidity mechanism, for firms whose stocks were 10% less liquid than the average, the effects of stock conversion on $NetIncome$ and on ROE are 0.1% higher. Likewise, for firms whose concentration index was 1 point higher, the effect of the reform on $NetIncome$ is 0.05% higher and the effect on ROE is

0.02% higher. All of these estimates are economically important (see corresponding panels in Figure 7) and are statistically significant at better than the 5% test level. Finally, we find that firms with highest pre-reform ownership concentration gain the most value with conversions (as measured by M/B). Counter to previous results, however, our estimates imply that firms with less liquid stocks gain relatively less value as a result of stock conversions.

In all, the evidence of this section suggests that firms that had the most to gain from the split-share reform — firms whose stocks were illiquid and concentrated before converting their shares — benefitted the most from the reform program. Evidence of these heterogeneous effects is consistent with our argument that the lifting of trading restrictions had positive implications for Chinese firms.

5.3.6 Falsification Test

Finally, one could still be concerned that firm compliance might be encouraged by idiosyncratic shocks or that firms who complied in a specific period are marginally distinct from the rest in terms of outcome variation. The types of endogeneity dynamics one could still be concerned about imply that, if our strategy does not eliminate these sorts of selection biases, reform compliance in the next period would be related to current outcome variation.

Table 10 reports estimates for the conditional relation between current outcome variation and reform compliance in the next period for firms who have not yet complied. That is, we test whether those firms who are about to convert their shares are significantly different from those who keep their shares under the non-tradable status. The table contrasts the performance of a conventional fixed-effect model (FE) with that of our estimation approach (GPS + FE) in terms of the potential for outcome dynamics endogeneity.

TABLE 10 ABOUT HERE

The table shows that the fixed-effect model fails to account for the fact that profitability ($NetIncome$ and ROE) increases in the year before a firm chooses to convert its shares. In other words, reform compliance seems to be encouraged by positive innovations to firm profitability. Firms that comply earlier also have higher ownership concentration ($OwnerConcent$) that is unaccounted for under the FE model. Our estimation model, which combines inverse GPS weighting with fixed effects, weakens biases stemming from shocks to firm profits and ownership by making them irrelevant for the timing of program compliance. Table 10 confirms that idiosyncratic changes in firms' behavior and performance are an unlikely source of bias for our inferences.

6 Concluding Remarks

The 2005 split-share reform allowed for restricted stocks worth hundreds of billions of dollars to become tradable over a short period, sharply increasing liquidity in the Chinese stock market. Our paper uses this episode as a way to flesh out links between stock market activity and real business activity.

We evaluate the impact of the 2005 reform exploiting various institutional features associated with its implementation. One of such feature is a pilot experiment conducted at the beginning of the reform schedule. Another is the gradual, large-scale share conversion that took place within a 16-month window. These features are unique and present both opportunities and challenges for our empirical tests. It is possible, for example, that better-managed firms were chosen to participate in the pilot trial that initiates the conversion program because of political motivation to showcase the reform. In addition, after the pilot stage, firms were free to join the reform at the time of their choosing. Thus, the treatment assignment might also be endogenous due to self-selection. To minimize these concerns, our analysis employs quasi-experimental methods that make the outcome variation before and after conversion conditionally independent from the compliance date.

We find that 2005 Chinese split-share reform had largely positive effects on corporate outcomes. Unlike previous reforms, the state loosened its control over local companies by allowing all of their shares to be traded in organized secondary markets. The elimination of dual-structure ownership, as well as the easier access to financing, gave new incentives for shareholders and managers to increase firm performance. Our results suggest that sales, profitability, and value increase because of the reform. The increase in business performance is accompanied by an expansion of capital investment, followed by improvements in productivity. The reform also allowed firms to have greater access to equity financing and prompted them to engage in more corporate acquisition deals.

The results we report shed a unique perspective on the role of public stock markets in the economy. In particular, they reveal the extent to which restrictions on secondary equity transactions can be detrimental to corporate growth. While our tests build on features that are particular to the Chinese equity markets, we believe our findings have broad implications for understanding the impact of governmental interventions and the trend towards capital markets liberalization. Our study indicates that trading in secondary equity markets have significant connections with outcomes observed in the real economy. Policies that ease restrictions on these markets may have positive effects.

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Table 1: List of Variables

Variable	Description
Real outcomes	
K	Log of fixed assets
L	Log of number of employees
$Sales/K$	Log of annual sales over fixed assets
$NetIncome$	Log of operating revenue over operating expenses
ROE	Return on equity
M/B	Market value of equity over book value of equity
Financial outcomes	
$Dividend$	Cash dividend over net income
$Issuance$	Dummy for equity issuance activity
$LiqRatio$	Log of daily trading volume over absolute value of daily return
$ShareTurnover$	Log of number of shares traded over number of shares outstanding
Other outcomes	
$M\&A$	Dummy for merger and acquisition deals in the last 12 months
RPT	Number of related party transactions in the last 12 months
$OwnerConcent$	Herfindahl index of top 5 shareholder ownership
$CEOTurnover$	Dummy for CEO turnover in the last 12 months
Control variables	
$NonTradable$	Proportion of non-tradable shares
$StateControl$	Dummy for firms ultimately controlled by the State
$Shares$	Log of total shares
$StateShares$	Proportion of shares owned by the State
Age	Firm's age in years
$Assets$	Log of total assets
$Sales$	Log of annual total sales
$CF/Assets$	Cash flow from operations over total assets
K/L	Log of fixed assets over number of employees
$Debt$	Total debt over total assets
$Loans$	Ratio of bank loans over assets
$Cash$	Cash to asset ratio
P/E	Ratio of price to earning per share
$IndRep$	Annual firm's sales over industry sales
$ProvRep$	Annual firm's sales over province GDP
$ProvGDP$	Log of province per capital GDP
$IndSales$	Log of annual industry sales
$IndConcent$	Industry Herfindahl index
Treatment assignment	
D	date (in months) when the reform started
Z	months since the reform started

This table describes the variables used in the paper (see Section 3 for definitions). Data are annual from 2002 to 2009. For real outcomes, except for L, data are also available by quarter.

Table 2: Summary Statistics for Pre-Reform Period (2004)

	Total		Pilot (1)		Converting in 2005 (2)		Control (3)		Difference (1)-(3)	*p*-value	Difference (2)-(3)	*p*-value
Real outcomes												
K	20.17	(0.039)	20.53	(0.267)	20.13	(0.079)	19.92	(0.081)	0.605	0.005	0.208	0.079
ΔK	0.195	(0.010)	0.318	(0.039)	0.243	(0.021)	0.120	(0.024)	0.198	0.000	0.124	0.000
L	7.262	(0.039)	7.340	(0.225)	7.316	(0.076)	7.057	(0.087)	0.283	0.185	0.259	0.029
ΔL	0.037	(0.012)	0.148	(0.040)	0.059	(0.024)	-0.032	(0.031)	0.180	0.009	0.091	0.020
$Sales/K$	0.343	(0.034)	0.574	(0.184)	0.482	(0.061)	0.188	(0.089)	0.387	0.061	0.295	0.005
$\Delta Sales/K$	0.044	(0.015)	0.017	(0.054)	0.061	(0.028)	-0.014	(0.038)	0.030	0.753	0.075	0.102
$NetIncome$	0.080	(0.009)	0.203	(0.034)	0.144	(0.008)	-0.049	(0.040)	0.252	0.004	0.193	0.000
$\Delta NetIncome$	-0.047	(0.010)	-0.007	(0.011)	0.006	(0.008)	-0.134	(0.042)	0.127	0.220	0.140	0.000
ROE	0.045	(0.005)	0.142	(0.011)	0.099	(0.004)	-0.057	(0.019)	0.199	0.000	0.156	0.000
ΔROE	-0.023	(0.005)	0.005	(0.011)	0.009	(0.004)	-0.083	(0.019)	0.088	0.063	0.092	0.000
M/B	2.114	(0.044)	2.294	(0.145)	2.152	(0.052)	2.558	(0.176)	-0.264	0.488	-0.406	0.009
$\Delta M/B$	-0.561	(0.047)	-0.462	(0.120)	-0.557	(0.075)	-0.774	(0.194)	0.313	0.527	0.217	0.248
Financial outcomes												
$Dividend$	0.351	(0.012)	0.355	(0.034)	0.389	(0.020)	0.275	(0.034)	0.080	0.197	0.114	0.003
$Issuance$	0.009	(0.003)	0.023	(0.023)	0.007	(0.005)	0.000	(0.000)	0.023	0.035	0.007	0.260
$LiqRatio$	8.677	(0.011)	8.948	(0.067)	8.734	(0.020)	8.570	(0.025)	0.378	0.000	0.164	0.000
$ShareTurnover$	-1.384	(0.019)	-0.977	(0.095)	-1.252	(0.040)	-1.458	(0.042)	0.481	0.000	0.206	0.001
Other outcomes												
$M\&A$	0.372	(0.015)	0.512	(0.077)	0.410	(0.028)	0.358	(0.035)	0.154	0.062	0.052	0.250
RPT	5.688	(0.343)	5.605	(1.569)	5.997	(0.609)	5.626	(1.116)	-0.022	0.993	0.370	0.752
$OwnerConcent$	0.232	(0.004)	0.273	(0.026)	0.250	(0.008)	0.200	(0.010)	0.073	0.003	0.050	0.000
$CEOTurnover$	0.181	(0.012)	0.163	(0.057)	0.173	(0.022)	0.216	(0.030)	-0.053	0.440	-0.042	0.244
Control variables												
$NonTradable$	0.617	(0.003)	0.678	(0.016)	0.637	(0.005)	0.598	(0.008)	0.080	0.000	0.039	0.000
$StateControl$	0.704	(0.014)	0.465	(0.077)	0.653	(0.028)	0.689	(0.034)	-0.224	0.005	-0.036	0.409
$Shares$	19.38	(0.023)	19.48	(0.183)	19.36	(0.046)	19.29	(0.046)	0.197	0.132	0.071	0.299
$StateShares$	0.369	(0.008)	0.279	(0.048)	0.358	(0.016)	0.338	(0.019)	-0.059	0.195	0.020	0.433
Age	8.777	(0.116)	7.116	(0.578)	8.100	(0.215)	10.33	(0.293)	-3.215	0.000	-2.232	0.000
$Assets$	21.19	(0.027)	21.56	(0.188)	21.21	(0.051)	20.95	(0.058)	0.606	0.000	0.266	0.001
$Sales$	20.52	(0.037)	21.10	(0.198)	20.61	(0.065)	20.11	(0.095)	0.991	0.000	0.503	0.000
$CF/Assets$	0.048	(0.003)	0.072	(0.011)	0.058	(0.005)	0.031	(0.008)	0.041	0.015	0.028	0.002
K/L	12.91	(0.034)	13.12	(0.279)	12.82	(0.062)	12.87	(0.070)	0.252	0.206	-0.050	0.598
$Debt$	0.480	(0.006)	0.451	(0.028)	0.461	(0.010)	0.554	(0.016)	-0.104	0.004	-0.094	0.000
$Loans$	0.060	(0.003)	0.076	(0.016)	0.069	(0.006)	0.053	(0.006)	0.022	0.139	0.016	0.065
$Cash$	0.165	(0.004)	0.207	(0.020)	0.183	(0.008)	0.128	(0.008)	0.079	0.000	0.055	0.000
P/E	56.58	(2.770)	26.39	(3.157)	44.33	(3.844)	64.14	(8.511)	-37.75	0.037	-19.81	0.018
$IndRep$	0.001	(0.000)	0.001	(0.001)	0.001	(0.000)	0.000	(0.000)	0.001	0.006	0.001	0.098
$ProvRep$	0.003	(0.000)	0.005	(0.002)	0.003	(0.000)	0.002	(0.000)	0.003	0.003	0.001	0.022
$ProvGDP$	9.597	(0.017)	9.836	(0.082)	9.643	(0.034)	9.495	(0.038)	0.341	0.000	0.147	0.005
$IndSales$	26.62	(0.046)	26.67	(0.198)	26.63	(0.090)	26.55	(0.105)	0.116	0.631	0.082	0.558
$IndConcent$	0.046	(0.002)	0.048	(0.011)	0.049	(0.005)	0.041	(0.004)	0.007	0.483	0.008	0.259
# of obs.	1054		43		300		190					

This table shows the sample averages of the variables listed in Table 1 for all 1,054 A-share firms listed in 2004. Pilot firms are those that jointed the reform in May-June 2005. Converting in 2005 firms are those that jointed the reform by the end of 2005. Control firms are those that joint the reform after June 2006. We also report the difference in sample average between pilot firms and control firms, as well as between firms converting in 2005 and their controls. Standard errors are in the parentheses. Δ indicates difference between December 2003 and December 2004.

Table 3: Time-Varying Potential Outcomes

d	t = 0	1	...	$T-1$	T
1	Y_0^1	Y_1^1	$\cdots$	Y_{T-1}^1	Y_T^1
2	Y_0^2	Y_1^2		Y_{T-1}^2	Y_T^2
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
$T-1$	Y_0^{T-1}	Y_1^{T-1}	$\cdots$	Y_{T-1}^{T-1}	Y_T^{T-1}
T	Y_0^T	Y_1^T		Y_{T-1}^T	Y_T^T
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
K	Y_0^K	Y_1^K	$\cdots$	Y_{T-1}^K	Y_T^K

This table shows the potential outcomes of treatment for the time-varying approach. d represents the treatment value, indicating when the firm may join the reform. t represents the real time horizon. Y_t^d is an ordinary variable (or vector) that maps a particular treatment value, d, to a potential outcome at time t. Each cell in the matrix indicates the potential outcome for a given firm with a particular treatment value in a specific period. For example, at period T (column T), $Y_T^2 - Y_T^T$ is the effect of being in the reform for $T-2$ months with respect to joining the reform in period T.

Table 4: Pre-Reform Difference Between Treated Firms and Control Firms After Matching

	Panel A					**Panel B**				
	Pilot	Control	Difference		p-value	Converting in 2005	Control	Difference		p-value
Real outcomes										
K	20.70	20.67	0.028	(0.459)	0.952	20.17	20.47	-0.303	(0.234)	0.202
ΔK	0.301	0.134	0.168	(0.140)	0.237	0.209	0.089	0.120	(0.060)	0.053
L	7.378	7.821	-0.443	(0.381)	0.252	7.298	7.573	-0.275	(0.202)	0.182
ΔL	0.149	0.061	0.088	(0.169)	0.605	0.040	0.052	-0.012	(0.067)	0.857
$Sales/K$	0.479	0.680	-0.201	(0.392)	0.610	0.442	0.383	0.059	(0.198)	0.766
$\Delta Sales/K$	0.011	0.177	-0.166	(0.134)	0.222	0.061	0.172	-0.110	(0.074)	0.141
$NetIncome$	0.165	0.104	0.062	(0.033)	0.070	0.141	0.132	0.010	(0.021)	0.649
$\Delta NetIncome$	-0.011	0.012	-0.023	(0.036)	0.522	0.006	-0.007	0.012	(0.021)	0.558
ROE	0.131	0.117	0.014	(0.020)	0.481	0.095	0.098	-0.002	(0.018)	0.900
ΔROE	0.011	0.015	-0.003	(0.019)	0.867	0.009	0.012	-0.003	(0.029)	0.908
M/B	2.030	2.394	-0.364	(0.563)	0.522	2.128	2.285	-0.157	(0.301)	0.606
$\Delta M/B$	-0.501	-0.670	0.169	(0.253)	0.507	-0.575	-0.818	0.244	(0.292)	0.409
Financial outcomes										
$Dividend$	0.345	0.330	0.015	(0.099)	0.878	0.373	0.302	0.071	(0.069)	0.308
$Issuance$	0.030	0.000	0.030	(0.030)	0.323	0.007	0.000	0.007	(0.005)	0.164
$LiqRatio$	8.915	8.770	0.145	(0.116)	0.216	8.719	8.728	-0.009	(0.067)	0.892
$ShareTurnover$	-1.092	-1.413	0.321	(0.210)	0.134	-1.322	-1.467	0.145	(0.110)	0.197
Other outcomes										
$M\&A$	0.515	0.424	0.091	(0.159)	0.570	0.407	0.433	-0.025	(0.089)	0.777
RPT	5.727	10.33	-4.606	(3.580)	0.205	6.215	11.09	-4.876	(3.869)	0.214
$OwnerConcent$	0.269	0.310	-0.041	(0.059)	0.486	0.254	0.266	-0.013	(0.029)	0.665
$CEOTurnover$	0.152	0.24	-0.091	(0.128)	0.482	0.182	0.19	-0.007	(0.070)	0.918
Control variables										
$NonTradable$	0.662	0.634	0.028	(0.045)	0.533	0.632	0.618	0.014	(0.024)	0.564
$StateControl$	0.545	0.576	-0.030	(0.158)	0.849	0.676	0.673	0.004	(0.088)	0.967
$Shares$	19.58	19.54	0.048	(0.256)	0.850	19.391	19.543	-0.151	(0.133)	0.260
$StateShares$	0.317	0.318	-0.001	(0.096)	0.994	0.369	0.381	-0.012	(0.049)	0.803
Age	7.909	8.545	-0.636	(1.144)	0.581	8.356	8.891	-0.535	(0.705)	0.452
$Assets$	21.67	21.64	0.030	(0.302)	0.922	21.23	21.42	-0.190	(0.165)	0.255
$Sales$	21.18	21.35	-0.174	(0.437)	0.693	20.61	20.85	-0.243	(0.213)	0.260
$CF/Assets$	0.066	0.063	0.003	(0.022)	0.882	0.058	0.060	-0.002	(0.012)	0.869
K/L	13.32	12.85	0.471	(0.391)	0.235	12.87	12.90	-0.028	(0.182)	0.878
$Debt$	0.493	0.529	-0.036	(0.055)	0.509	0.467	0.491	-0.024	(0.034)	0.486
$Loans$	0.085	0.111	-0.026	(0.031)	0.413	0.072	0.092	-0.021	(0.017)	0.235
$Cash$	0.183	0.170	0.013	(0.034)	0.703	0.173	0.152	0.022	(0.019)	0.257
P/E	26.61	40.77	-14.16	(18.11)	0.439	44.60	49.50	-4.900	(20.26)	0.810
$IndRep$	0.001	0.001	0.000	(0.001)	0.741	0.001	0.001	0.001	(0.001)	0.340
$ProvRep$	0.006	0.004	0.002	(0.003)	0.533	0.003	0.003	0.000	(0.001)	0.838
$ProvGDP$	9.820	9.767	0.053	(0.183)	0.776	9.623	9.627	-0.005	(0.100)	0.963
$IndSales$	26.55	26.92	-0.37	(0.429)	0.393	26.60	26.71	-0.110	(0.284)	0.701
$IndConcent$	0.054	0.031	0.023	(0.017)	0.174	0.046	0.036	0.011	(0.011)	0.358

This table shows the average difference in pre-reform covariates between treated firms and their matched control firms. Pilot firms are those that jointed the reform in May-June 2005. Converting in 2005 firms are those that jointed the reform by the end of 2005. Control firms are those that joint the reform after June 2006. Panel A represents the average differences between pilot firms and matched control firms. Panel B represents the average differences between firms converting in 2005 and their matched controls. Standard errors of the differences are in the parentheses and the p-value is reported in separate columns. Δ indicates difference between December 2003 and December 2004.

Table 5: GPS Balancing Property Test

	W/O controls	p-value	W/ controls	p-value
Real outcomes				
K	-0.016	0.006	-0.005	0.420
ΔK	-0.006	0.000	0.000	0.890
L	-0.022	0.001	-0.002	0.793
ΔL	-0.006	0.003	0.001	0.543
$Sales/K$	-0.017	0.003	0.007	0.276
$\Delta Sales/K$	-0.006	0.006	-0.004	0.162
$NetIncome$	-0.012	0.000	0.000	0.785
$\Delta NetIncome$	-0.009	0.000	-0.001	0.713
ROE	-0.010	0.000	-0.001	0.433
ΔROE	-0.007	0.000	-0.001	0.367
M/B	0.022	0.057	0.017	0.102
$\Delta M/B$	0.003	0.789	0.009	0.370
Financial outcomes				
$Dividend$	-0.008	0.001	-0.002	0.576
$Issuance$	0.000	0.158	0.000	0.718
$LiqRatio$	-0.011	0.000	0.001	0.587
$ShareTurnover$	-0.012	0.001	0.003	0.375
Other outcomes				
$M\&A$	-0.004	0.137	0.001	0.718
RPT	-0.066	0.144	-0.009	0.870
$OwnerConcent$	-0.003	0.000	0.000	0.625
$CEOTurnover$	0.003	0.173	-0.003	0.127
Control variables				
$NonTradable$	-0.003	0.000	0.000	0.647
$StateControl$	0.002	0.374	-0.001	0.622
$Shares$	-0.005	0.175	-0.003	0.438
$StateShares$	-0.001	0.303	0.000	0.848
Age	0.129	0.000	-0.003	0.900
$Assets$	-0.017	0.000	-0.002	0.739
$Sales$	-0.033	0.000	0.001	0.846
$CF/Assets$	-0.002	0.000	-0.001	0.153
K/L	0.006	0.301	-0.003	0.686
$Debt$	0.005	0.000	0.000	0.816
$Loans$	-0.001	0.027	0.000	0.691
$Cash$	-0.003	0.000	0.001	0.243
P/E	0.665	0.213	-0.609	0.306
$ProvGDP$	-0.011	0.000	0.001	0.724
$IndSales$	-0.007	0.286	-0.003	0.732
$IndConcent$	0.000	0.869	0.000	0.971
$IndRep$	0.000	0.021	0.000	0.851
$ProvRep$	0.000	0.003	0.000	0.901

This table shows the regression results for Generalized Propensity Score (GPS) Balancing Property Test based on Imai and van Dyk (2004). In the regression, each pre-reform covariate is regressed on the treatment assignment, before and after controlling for the estimated GPS. Regression coefficients and associated p-values are reported in separate columns. Δ indicates difference between December 2003 and December 2004.

Table 6: ATT Difference-in-Difference Estimates for Pilot Firms

	OLS w/o controls		OLS w/ controls		NNM	
2005						
ΔK	0.211	$(0.043)^{***}$	0.198	$(0.062)^{***}$	0.212	$(0.116)^{*}$
ΔL	0.247	$(0.045)^{***}$	0.241	$(0.071)^{***}$	0.150	(0.099)
$Sales/K$	0.188	$(0.098)^{*}$	-0.103	(0.136)	-0.119	(0.124)
$NetIncome$	0.123	$(0.068)^{*}$	-0.008	(0.121)	-0.006	(0.023)
ROE	0.119	$(0.022)^{***}$	0.141	$(0.042)^{***}$	0.086	$(0.035)^{**}$
M/B	0.156	(0.267)	-0.058	(0.395)	-0.120	(0.237)
Z	5.930	$(0.145)^{***}$	5.902	$(0.155)^{***}$	5.879	$(0.137)^{***}$
2006						
ΔK	0.526	$(0.087)^{***}$	0.341	$(0.120)^{***}$	0.693	$(0.290)^{**}$
ΔL	0.406	$(0.065)^{***}$	0.335	$(0.100)^{***}$	0.414	$(0.135)^{***}$
$Sales/K$	0.026	(0.101)	-0.071	(0.146)	-0.516	$(0.289)^{*}$
$NetIncome$	0.038	(0.072)	0.229	(0.142)	-0.013	(0.096)
ROE	0.005	(0.027)	0.061	(0.047)	0.034	(0.029)
M/B	0.904	$(0.386)^{**}$	0.425	(0.468)	0.906	$(0.459)^{**}$
Z	15.830	$(0.208)^{***}$	15.788	$(0.307)^{***}$	14.909	$(0.442)^{***}$
2007						
ΔK	0.674	$(0.141)^{***}$	0.409	$(0.188)^{**}$	0.580	$(0.300)^{*}$
ΔL	0.553	$(0.090)^{***}$	0.348	$(0.117)^{***}$	0.346	$(0.141)^{**}$
$Sales/K$	0.133	(0.137)	-0.023	(0.207)	-0.228	(0.207)
$NetIncome$	-0.107	$(0.061)^{*}$	0.021	(0.088)	-0.101	(0.080)
ROE	-0.106	$(0.039)^{***}$	-0.038	(0.045)	-0.083	$(0.047)^{*}$
M/B	-0.308	(0.683)	-0.450	(0.845)	-1.214	(1.326)
Z	17.299	$(0.388)^{***}$	17.199	$(0.640)^{***}$	14.909	$(1.285)^{***}$

This table shows the average treatment effect (ATT) estimates for pilot firms. To study the changes brought about by the reform, we consider changes in outcome variables from the end of 2004 (prior to the reform) to: (1) the end of 2005 (top panel); (2) the end of 2006 (middle panel); and (3) the end of 2007 (bottom panel). The estimates in column 1 are from Ordinary Least Squares (OLS) regression without any control variables. The estimates in column 2 are from Ordinary Least Squares regression with control variables. The estimates in column 3 are from the Nearest Neighbor Matching (NNM) estimator. Robust standard errors are in parentheses. The symbols ***, **, and * represent statistical significant at the 1%, 5%, and 10% levels, respectively.

Table 7: ATT Difference-in-Difference Estimates for Firms Converting in 2005

	OLS w/o controls		OLS w/ controls		NNM	
2005						
ΔK	0.159	$(0.031)^{***}$	0.139	$(0.040)^{***}$	0.042	(0.064)
ΔL	0.164	$(0.034)^{***}$	0.143	$(0.043)^{***}$	0.099	$(0.060)^{*}$
$Sales/K$	0.183	$(0.090)^{**}$	0.063	(0.055)	0.028	(0.069)
$NetIncome$	0.134	$(0.068)^{**}$	0.092	$(0.035)^{***}$	0.056	$(0.023)^{**}$
ROE	0.118	$(0.022)^{***}$	0.112	$(0.021)^{***}$	0.063	$(0.032)^{*}$
M/B	0.181	(0.253)	-0.069	(0.145)	-0.108	(0.243)
Z	1.430	$(0.070)^{***}$	1.284	$(0.079)^{***}$	1.376	$(0.072)^{***}$
2006						
ΔK	0.415	$(0.077)^{***}$	0.303	$(0.062)^{***}$	0.470	$(0.234)^{**}$
ΔL	0.296	$(0.053)^{***}$	0.246	$(0.052)^{***}$	0.303	$(0.098)^{***}$
$Sales/K$	0.056	(0.089)	0.031	(0.072)	-0.341	(0.234)
$NetIncome$	0.048	(0.071)	0.091	(0.067)	0.044	(0.097)
ROE	-0.001	(0.025)	0.018	(0.023)	0.020	(0.027)
M/B	0.461	(0.281)	0.157	(0.216)	0.087	(0.321)
Z	11.330	$(0.165)^{***}$	11.123	$(0.180)^{***}$	10.696	$(0.356)^{***}$
2007						
ΔK	0.666	$(0.100)^{***}$	0.570	$(0.104)^{***}$	0.609	$(0.229)^{***}$
ΔL	0.491	$(0.077)^{***}$	0.402	$(0.081)^{***}$	0.318	$(0.104)^{***}$
$Sales/K$	0.008	(0.111)	-0.098	(0.099)	-0.291	$(0.145)^{**}$
$NetIncome$	-0.154	$(0.055)^{***}$	-0.131	$(0.043)^{***}$	-0.070	(0.066)
ROE	-0.113	$(0.023)^{***}$	-0.083	$(0.019)^{***}$	-0.033	(0.030)
M/B	-1.262	$(0.458)^{***}$	-1.407	$(0.428)^{***}$	-1.553	(1.019)
Z	12.798	$(0.366)^{***}$	12.537	$(0.391)^{***}$	11.306	$(1.094)^{***}$

This table shows the average treatment effect (ATT) estimates for firms converting in 2005. To study the changes brought about by the reform, we consider changes in outcome variables from the end of 2004 (prior to the reform) to: (1) the end of 2005 (top panel); (2) the end of 2006 (middle panel); and (3) the end of 2007 (bottom panel). The estimates in column 1 are from Ordinary Least Squares (OLS) regression without any control variables. The estimates in column 2 are from Ordinary Least Squares regression with control variables. The estimates in column 3 are from the Nearest Neighbor Matching (NNM) estimator. Robust standard errors are in parentheses. The symbols ***, **, and * represent statistical significant at the 1%, 5%, and 10% levels, respectively.

Table 8: Falsification Test, ATT Difference-in-Difference Estimates Before the Reform (2003-2004)

	OLS w/o controls		OLS w/ controls		NNM	
Pilot Firms						
ΔK	0.198	$(0.046)^{***}$	0.164	$(0.073)^{**}$	0.100	(0.146)
ΔL	0.180	$(0.050)^{***}$	0.128	$(0.076)^{*}$	0.118	(0.101)
$Sales/K$	0.030	(0.065)	0.010	(0.078)	-0.166	(0.142)
$NetIncome$	0.127	$(0.043)^{***}$	0.118	$(0.050)^{**}$	-0.024	(0.019)
ROE	0.088	$(0.022)^{***}$	0.116	$(0.036)^{***}$	-0.003	(0.016)
M/B	0.313	(0.228)	-0.255	(0.275)	0.171	(0.223)
Firms Converting in 2005						
ΔK	0.124	$(0.032)^{***}$	0.054	(0.033)	0.090	(0.103)
ΔL	0.091	$(0.039)^{**}$	0.060	(0.050)	0.006	(0.067)
$Sales/K$	0.075	(0.047)	0.096	$(0.051)^{*}$	-0.109	(0.094)
$NetIncome$	0.140	$(0.042)^{***}$	0.135	$(0.037)^{***}$	0.013	(0.013)
ROE	0.092	$(0.019)^{***}$	0.088	$(0.017)^{***}$	-0.002	(0.018)
M/B	0.217	(0.208)	-0.135	(0.198)	0.245	(0.207)

This table shows the average treatment effect (ATT) estimates for firms in 2004, before the reform takes place. We consider changes in outcome variables from the end of 2003 to the end of 2004 for pilot firms (top panel) and firms converting in 2005 (bottom panel). The estimates in column 1 are from Ordinary Least Squares (OLS) regression without any control variables. The estimates in column 2 are from Ordinary Least Squares regression with control variables. The estimates in column 3 are from the Nearest Neighbor Matching (NNM) estimator. Robust standard errors are in parentheses. The symbols ***, **, and * represent statistical significant at the 1%, 5%, and 10% levels, respectively.

Table 9: Heterogeneity of the Marginal Effect on Real Outcomes

	Lagged $ShareTurnover$			Lagged $OwnerConcent$		
	Coefficient		p-value	Coefficient		p-value
ΔK	-0.002	(0.008)	0.782	0.039	(0.035)	0.263
ΔL	-0.008	(0.007)	0.263	0.034	(0.031)	0.274
$Sales/K$	-0.024	(0.010)	0.019	0.251	(0.044)	0.000
$NetIncome$	-0.013	(0.005)	0.009	0.051	(0.022)	0.018
ROE	-0.012	(0.003)	0.000	0.022	(0.012)	0.058
M/B	0.173	(0.037)	0.000	0.208	(0.160)	0.196

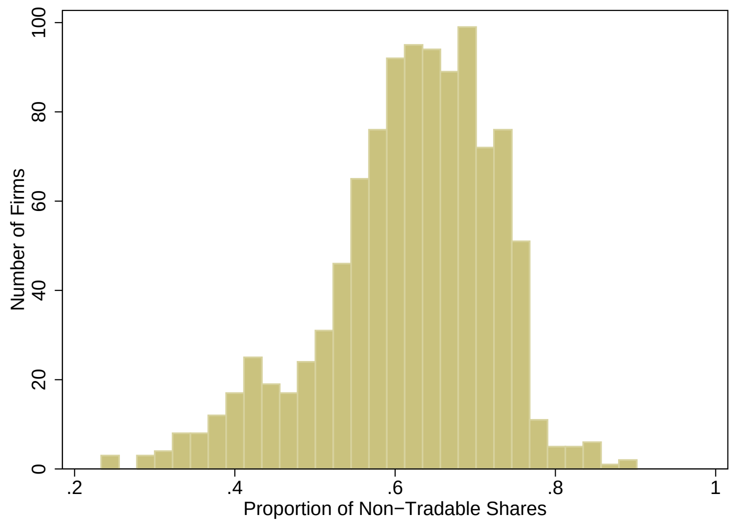
This table shows the estimated coefficient of the interaction between months since the reform started (Z) and the following lagged variables: Herfindahl index of top 5 shareholder ownership ($OwnerConcent$) and share turnover ($ShareTurnover$). Each row in this table comes from a different regression and the reported coefficients are multiplied by 12 to represent annual effects. The regressions are estimated using Inverse Probability Weighting (IPW) and GPS adjustment as in equation (15). Standard Errors are in parentheses and p-value are reported in separate columns.

Table 10: Falsification Test, Effect of Joining the Reform Next Period on the Current Outcome

	FE Model			GPS + FE Model		
	Coefficient		p-value	Coefficient		p-value
Real outcomes						
ΔK	0.088	(0.081)	0.274	0.034	(0.065)	0.602
ΔL	0.002	(0.085)	0.980	-0.073	(0.071)	0.307
$Sales/K$	0.062	(0.053)	0.245	-0.003	(0.048)	0.946
$NetIncome$	0.084	(0.033)	0.012	0.038	(0.029)	0.181
ROE	0.036	(0.012)	0.002	-0.003	(0.011)	0.814
M/B	0.152	(0.135)	0.260	0.138	(0.119)	0.248
Financial outcomes						
$Dividend$	-0.003	(0.033)	0.927	0.033	(0.030)	0.278
$Issuance$	0.009	(0.011)	0.424	0.014	(0.010)	0.178
$LiqRatio$	0.014	(0.017)	0.432	-0.007	(0.016)	0.674
Other outcomes						
$ShareTurnover$	-0.047	(0.042)	0.272	-0.056	(0.039)	0.157
$M\&A$	0.017	(0.034)	0.628	0.004	(0.033)	0.910
RPT	0.187	(0.466)	0.688	-0.015	(0.476)	0.975
$OwnerConcent$	0.005	(0.003)	0.106	0.004	(0.003)	0.215
$CEOTurnover$	0.003	(0.032)	0.931	0.025	(0.032)	0.429

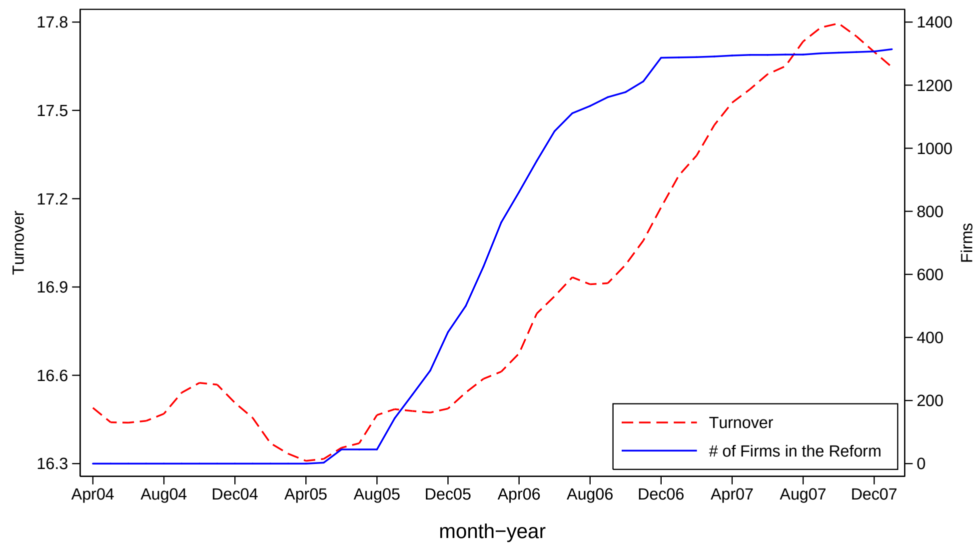
This table shows the estimated coefficient of the lead dummy that indicates whether the firm complies with the reform in the next period or not. The sample only includes observations up to the date when the firm joined the reform, i.e., firm-year observations whose $Z = 0$. The estimates in column 1 are for a fixed-effect (FE) model as shown in equation (14). The estimates in column 3 are obtained from the same model but adjusting for the Generalized Propensity Score (GPS) using Inverse Probability Weighting (IPW). Standard Errors are in parentheses and p-values are reported in separate columns.

Figure 1: Distribution of Non-Tradable Shares Before the Reform



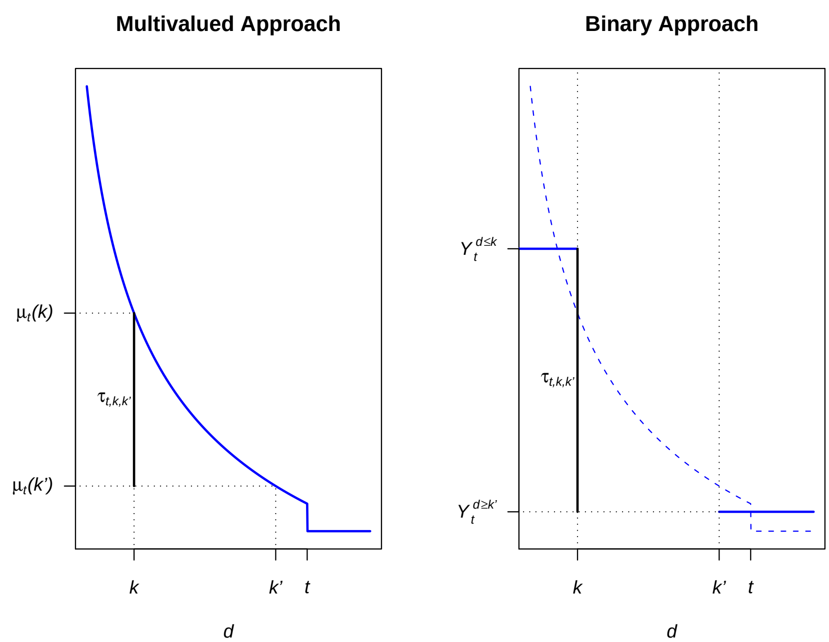
The x-axis represents the proportion of non-trabable shares in December 2004. The y-axis represents the frequency of firms.

Figure 2: Number of Firms in the Reform and Market Liquidity (Turnover) by Month-Year



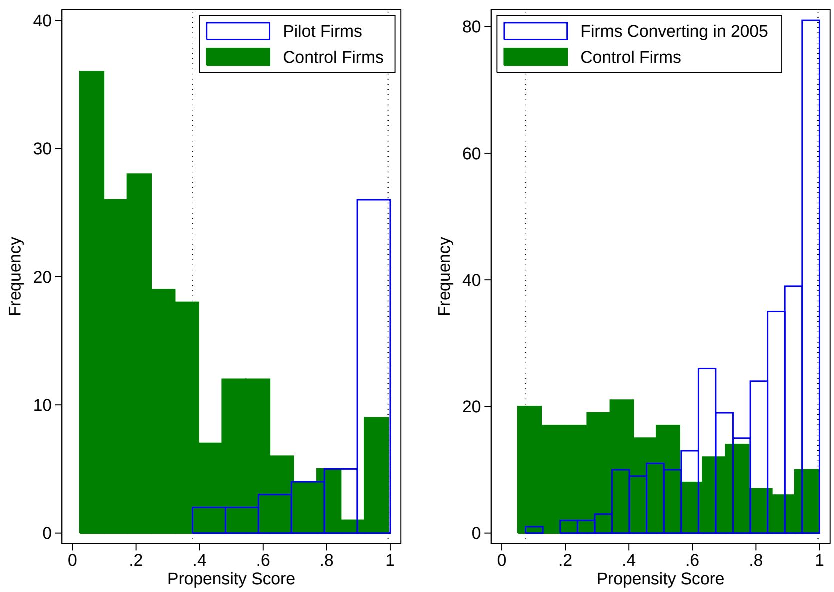
The y-axis (right side) represents the number of firms in the reform. The y-axis (left side) measures the market liquidity (turnover). We compute the market liquidity (turnover) as a 12-month moving average of the log ratio of number of shares traded on the Shanghai Stock Exchange over the total number of shares outstanding. The x-axis represents the year-month from April 2004 to June 2007.

Figure 3: Multivalued and Binary Approaches for ATE



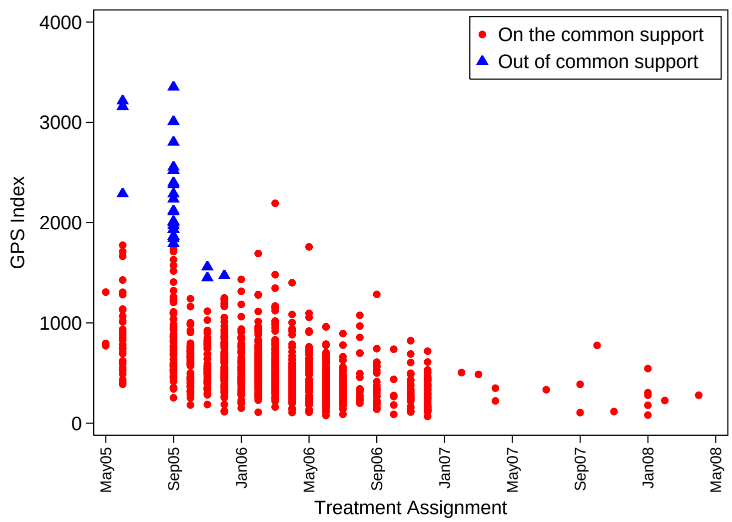
The left graph gives an example of Average Treatment Effect (ATE) estimated from a dose-response function. The point on the curve represents the expected outcome at time t if the firm joins the reform at a particular time k. The right graph depicts the estimates for a mirrored binary-treatment framework under the assumption that the dose-response function is locally constant.

Figure 4: Distribution of Binary Propensity Score for Treated and Control Firms



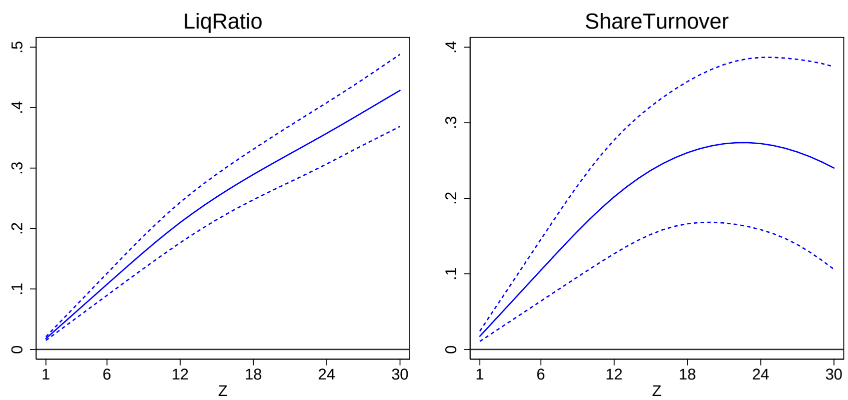
This figure shows the overlap between treated and control firms' distributions in the binary comparison. On the left panel, we compare the pilot and control firms in terms of the estimated propensity score. The panel on the right shows the overlap between firms converting in 2005 and control firms. The dotted lines delimit the common support of both groups as defined in equation (13). Pilot firms are those that jointed the reform in May-June 2005. Converting in 2005 firms are those that jointed the reform by the end of 2005. Control firms are those that joint the reform after June 2006.

Figure 5: Relationship between Treatment Assignment and the GPS Index



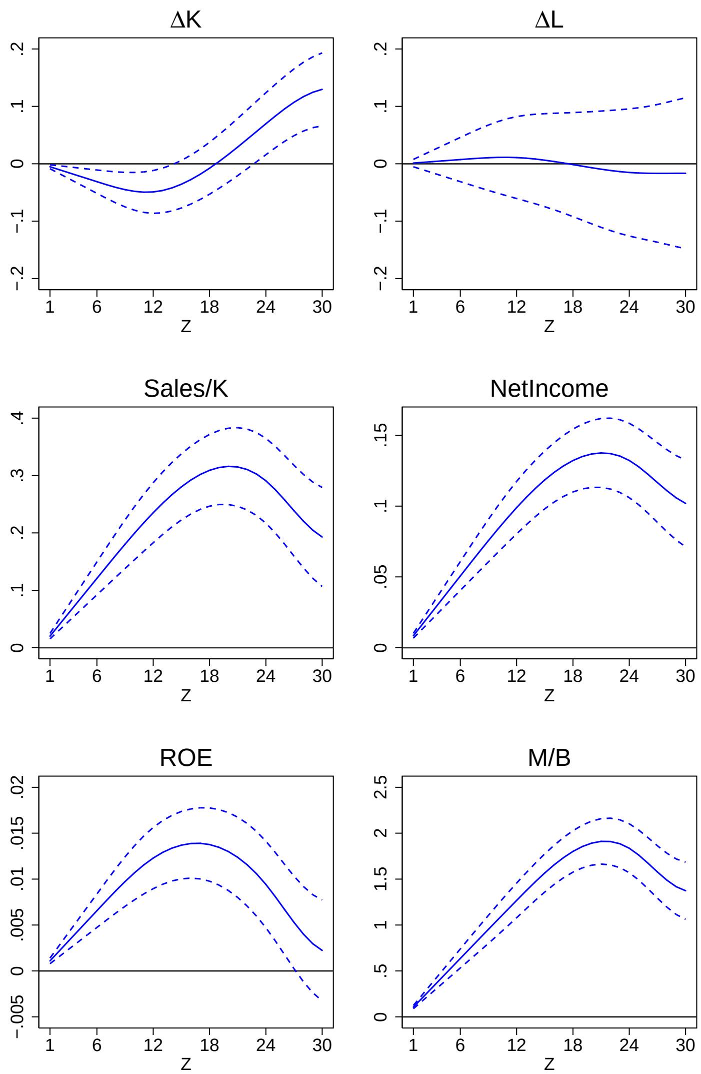
This figure shows the difference between firms outside the common support and those inside the common support in terms of Generalized Propensity Score (GPS) index, which represents observable pre-treatment characteristics. The common support, as defined by equation (16), is the region where each firm is always between two other firms with sufficiently distinct treatment values.

Figure 6: Time-Varying Treatment Effect on Liquidity, $\mu(Z) - \mu(0)$



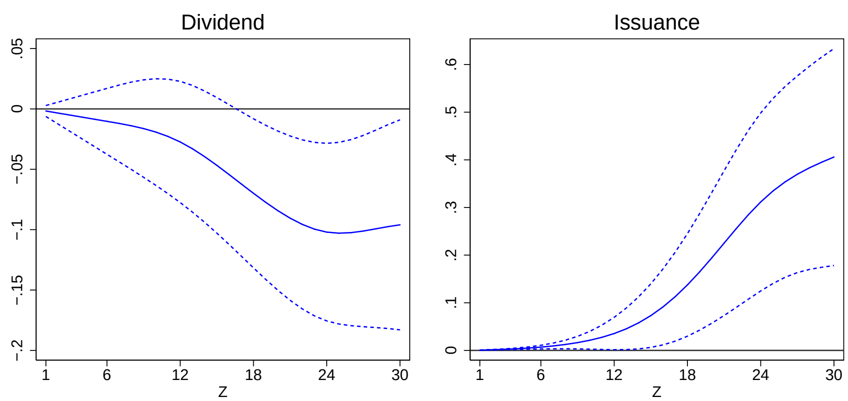
This figure presents time-varying reform effects on liquidity measures: $LiqRatio$ and $ShareTurnover$. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). All regressions are performed using annual data and estimated using a linear model. The plot represents the expected difference between being in the reform for Z months, $\mu(Z)$, and the counter-factual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% confidence interval for the estimates.

Figure 7: Time-Varying Treatment Effect on Real Outcomes, $\mu(Z) - \mu(0)$



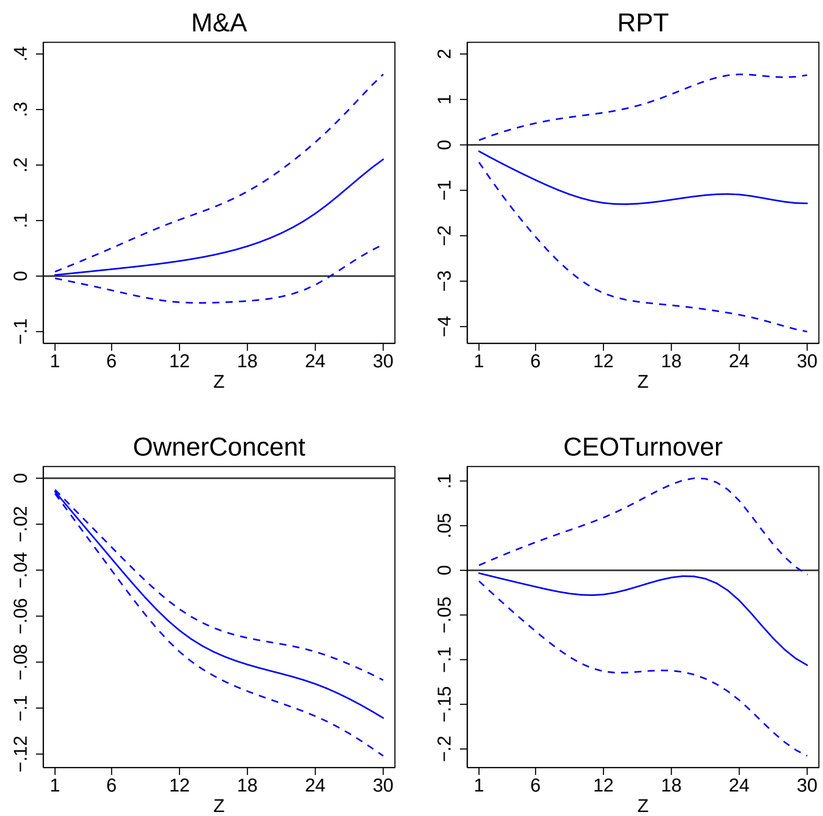
This figure presents time-varying reform effects on each of the business performance measures: ΔK, ΔL, $NetIncome$, $Sales/K$, M/B and ROE, where Δ indicates 12-month variation. The regressions for ΔK, $NetIncome$, $Sales/K$, M/B and ROE are estimated using quarterly data, while the regression for ΔL is estimated using annual data. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). The plot represents the expected difference between being in the reform for Z months, $\mu(Z)$, and the counter-factual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% confidence interval for the estimates.

Figure 8: Time-Varying Treatment Effect on Financial Outcomes, $\mu(Z) - \mu(0)$



This figure presents time-varying reform effects on financial measures: *Dividend* and *Issuance*. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). All regressions are performed using annual data. Dose-response function for *Issuance* is estimated using a Probit model. Dose-response function for *Dividend* is estimated using a linear model. The plot represents the expected difference between being in the reform for Z months, $\mu(Z)$, and the counter-factual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% confidence interval for the estimates.

Figure 9: Time-Varying Treatment Effect on Other Outcomes, $\mu(Z) - \mu(0)$



This figure presents time-varying reform effects on each of the equity trading measures: $M\&A$, RPT, $OwnerConcent$, $CEOTurnover$. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). All regressions are performed using annual data. Dose-response function for RPT is estimated using a Poisson model. Dose-response functions for $M\&A$ and $CEOTurnover$ are estimated using a Probit model. Dose-response function for $OwnerConcent$ is estimated using a linear model. The plot represents the expected difference between being in the reform for Z months, $\mu(Z)$, and the counter-factual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% confidence interval for the estimates.