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

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Abstract

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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 ordered the conversion of previously non-tradable stocks 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 stock markets on firms' real and financial outcomes. It does so exploiting multiple institutional features of the Chinese equity conversion program. We first examine a 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 policy-makers. We also take advantage of the staggered nature of the conversion schedule used in the second phase of the reform, whereby over one thousand firms converted their shares at different times within a government-dictated window. These various wrinkles produce counterfactuals against which to gauge the economic importance of secondary equity trading. Using a time-varying treatment estimation approach, we identify increases in corporate profitability, investment, value, and productivity as a result of the reform. We also identify changes in firms' propensity to issue new shares and engage in merger deals, as well as changes in their dividend and capital structure policies. Our findings provide insights on the role of stock markets in shaping corporate activity and on the impact of market regulation on economic growth.

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1 Introduction

Firms can issue equity to access external financing, and evidence shows that funds raised via *primary* equity issues (IPOs and SEOs) are used for investment, inventory accumulation, and R&D spending. It is less clear, however, whether *secondary* equity transactions — among market investors — affect firm outcomes. It has long been argued that secondary stock market transactions are largely a "side show to real corporate activity" (see, e.g., 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 (Dow and Gorton, 1997). An active secondary market might also 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 stocks in public markets affects corporate activity. 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 markets. In addition, firms choose when they go public, and that choice is confounded with factors such as underlying firm characteristics, prospects, and financing needs. This makes it difficult to compare firms before and after they go public to learn about the effects of public equity trading. To gauge the effects of trades that take place in organized exchanges, one would like to compare public firms whose stocks are traded with similar public firms whose stocks are not traded. While these types of counterfactuals are rarely observed, recent institutional changes affecting the Chinese stock market may help us identify the effect of secondary equity trading on real corporate activity.

Stock ownership in China is divided into three classes: shares reserved for domestic investors (A-shares), shares available to foreign investors (B-shares), and shares of firms 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. These share categories gave their owners identical cash flow and voting rights, yet the vast majority of stocks in China (over 70% of total shares) could not be traded in the organized exchanges. This unique structure — the cumulative outcome of past reforms — created a number of difficulties as Chinese companies expanded their operations in the early 2000s. Central planners acknowledged the problem and in 2005 put in motion a large-scale reform.

The "split-share reform" swiftly converted non-tradable shares into tradable. The reform started with a pilot trial in May of 2005, with a set of 46 firms selected into conversion. In September of that same year, the pilot unfolded into a fully-fledged program under which all listed firms were mandated to conclude their conversions by December 2006. In this watershed event, a sizeable secondary market emerged within a short window dictated by a top-down governmental program — a far cry from the kinds of endogenous, slow-moving processes in which equity markets typically evolve.

The 2005 market reform in China provides a 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 corporate stockholders should become more closely tied to stock prices (previously, most stockholders could not trade their stocks and only cared about dividends). Bolstering an observable measure of firm performance — liquid securities priced in the marketplace — could, in turn, create incentives to improve corporate efficiency. Second, the increase in liquidity and depth of secondary markets should allow firms to raise more capital in primary equity markets, since investors could now acquire securities that remain liquid after they are issued. As such, the reform could facilitate corporate funding, investment, and growth, potentially affecting firms' policies concerning capital structure and dividend payout. Finally, by making stocks transferable and liquid, the reform could facilitate corporate merger deals, allowing for a more efficient capital reallocation process and even the replacement of inefficient managerial structures.

This paper gauges the impact of the split-share reform on firms' real and financial outcomes using quasi-experimental strategies. We do so exploiting institutional features of the reform in conjunction with a time-varying treatment estimation approach that allows us to measure program effects 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 light on the role of the stock market in the economy.

We use a couple of different strategies to evaluate the effects of the split-share 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 describe 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 — 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 their trial. The matching is based on an extensive set of covariates, including regulatory, geographical, industry, and firm characteristics. 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 share reform.

The main limitation of the pilot-based test is that the number of firms examined is small and could have an idiosyncratic distribution of unobserved characteristics. This makes it difficult to generalize the findings of the pilot. One alternative test strategy is to gauge the impact of the reform on the hundreds of firms that entered the program right after the trial phase. 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 inferences are compromised by biases arising from selection based on expected outcomes or outcome manipulation by the government. The disadvantage is that, after the pilot, firms have some degree of discretion about the timing of program compliance. Moreover, as firms gradually join the reform, it becomes increasingly difficult to identify a control match for each firm that converts its stock (as time evolves, all firms become part of the treatment group). These challenges are interesting in their own right and lead us to use an alternative estimation technique that is worth discussing.

The conversion process required the approval of a super-majority of votes by both tradable and non-tradable shareholders of the firm. Various reports point to difficulties in reaching such a high level of collective agreement, and additional regulatory hurdles (discussed shortly) added noise to firms' conversion timing. Another factor affecting program participation was the government's desire to promote an "orderly conversion process." To avoid downward pressures on stock prices, the CSRC limited the number of firms allowed to convert their shares at any particular point of the reform window (firms were subjected to arbitrary, time-varying "weekly conversion quotas"). Notwithstanding factors that made it difficult for firms to time the conversion of their shares, one could argue that firms monitored market developments during the reform process, anticipated the potential effects of share conversion, and optimally timed their entry into the program.

Standard fixed-effects and difference-in-differences models will not account for firm-specific trends or expected outcomes that affect firms' decisions to join the reform or the timing of those decisions. These approaches will also fail to account for the changing economic environment in China at the time of the reform (with rising stock prices and volatility). Our analysis, instead, utilizes a multi-valued treatment approach that minimizes concerns about these types of problems. In particular, we use a Generalized Propensity Score (GPS) estimator (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 — the estimator is designed so as to make these potential confounders orthogonal to the entry date decision. As we detail below, the GPS estimator uses pre-treatment firm characteristics and outcome dynamics to create multiple counterfactuals for each firm. These counterfactuals, in turn, allow one to compare firms that have an equal probability of complying with the program at a particular point in time, yet enter the program at different times. Differences in these firms' outcomes reveal the impact of the reform across time.[1] Our time-varying treatment approach not only tackles dynamic self-selection issues but also takes into account that: (1) firms spend different periods of time in the reform window (with earlier compliers spending more

[1] 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).

time under the treatment status), (2) the pools of treated and control firms change as the conversion process evolves (implying time-varying composition effects), and (3) the effect of the treatment may not be constant over time, especially in a changing economic environment.

Our estimations suggest that the split-share reform impacted corporate policies and wealth by bolstering the market for secondary equity transactions. The paper's main results can be summarized as follows. First, we find that conversions boosted stock liquidity and reduced ownership concentration. Importantly, we also find that real corporate activity is significantly affected by the trading reform. As an example, relative to the baseline case of no conversion, investment in fixed assets increased 27% two years after a firm's outstanding stocks were allowed to trade. At the same time, stock conversions did not prompt firms to employ more workers. Following conversions, firms also experienced positive effects on their profitability, with net operating revenues growing, on average, 13% 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 (33% of the sample average). Notably, gains in economic performance were accompanied by improvements in productivity, as measured by the ratio of sales to capital. Eighteen months after conversion, sales were 35% higher given the same amount of fixed assets. In the long run, this ratio remained 26% higher than in the case of non-conversion.

Assessing the impact of the reform even further, we find that the effect of conversions on the ratio of market-to-book value of equity was positive and increasing up to two years after reform compliance. That measure of corporate value almost doubled 24 months after a firm's stock started to trade freely in the organized exchanges, remaining well above the baseline in the long run. Firms also altered their financial policies as a result of converting their shares. In particular, conversions prompted firms to issue more stocks, suggesting they gained greater access to equity financing (in primary markets) as a result of the greater liquidity in secondary markets. At the same time, leverage ratios declined steeply following conversions. Finally, as stocks became more liquid firms seemed to put less emphasis on dividend payments.

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

There are several channels by which the reform-induced increase in stock liquidity could affect firm outcomes and we investigate various explanations at the end of our analysis. We find that stock prices become more informative following conversion, potentially explaining the increases in corporate efficiency and value that are associated with the reform. A more liquid stock market should allow firms to more actively engage in merger and acquisition deals, since stocks can be used to finance these transactions. This is what we find in the data. We also examine the effects of stock liquidity on managers. We do not find evidence that managerial compensation packages and turnover rates changed as a result of conversions. Finally, we look at various manifestations of agency problems in Chinese firms (e.g., expropriation via "related party transactions" and "intercorporate loans") and find weak support for a reduction in agency costs associated with share conversions.

There exists a small 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 larger compensation packages 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 related to the bargaining power of non-tradable shareholders. While these studies provide important insights on the reform process, they focus exclusively on the negotiations that characterize the conversion schemes adopted by various firms. To our knowledge, existing papers abstract from the real-side, long-term implications of the share reform on corporate outcomes (such as investment, employment, or productivity).

Our paper relates to a literature that looks at connections between stock markets and real corporate behavior. Existing papers, however, focus on links between stock prices and investment spending. Baker et al. (2003), for example, find that the investment of equity-dependent firms is more sensitive to stock prices. Chen et al. (2007) show that the sensitivity of investment to stock prices is increasing in price informativeness. Campello and Graham (2012) find that constrained manufacturers increased their investment in response to market mispricing led by technology firms in the 1990s.

Finally, a number of 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; 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 dimensions. First, our analysis consists of a firm-level, within-country examination that builds on well-defined institutional features of a structural reform. This, in turn, allows us to describe in detail how economic outcomes are affected by market liberalization (e.g., corporate investment, productivity, 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 and financial institutions. In particular, we gauge the impact of a deregulation initiative that brought into the local equity market an estimated 25 million new domestic investors. Third, we explore various quasi-experimental test strategies 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.

Our inferences are, by design, related to the conditions of one particular economy during one context-specific market reform. We believe, however, that the results we report provide 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 as well as investor wealth. More broadly, our findings may help better understand the impact of governmental interventions and the trend towards capital market liberalization.

The remainder of the paper is organized as follows. Section 2 describes the split-share reform, explaining 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 moves toward market liberalization in China are seen by some 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, child care, and other benefits), SOEs had very few incentives to improve their operating efficiency (see Bai et al., 2006). 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 broadly-defined "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 set by state agencies using accounting information — not market values — and were set deliberately low to avoid transfers. Even so, the government retained the ultimate say on any transactions involving those shares. Proposed transactions had to be submitted in writing, with central and local governments commonly taking months (sometimes years) before issuing a decision.

While non-tradable and tradable shares had the same voting and cash flow rights, non-tradable shares accounted for about two-thirds of all shares. Cross-firm variation in the proportion of these share classes was determined according to interests within an intricate web of bureaucracies, including central-government asset management committees, central finance and industry ministries, local governments (various layers), and local-government asset management committees. All of these parties retained some power in determining which shares would be deemed as state-related.

A myriad of conflicting forces determined the assignment of firms' stock tradability status during the privatization process. Not surprisingly, firms came out of that process displaying a wide degree of variation in the proportion of non-tradable shares in their books. Figure 1 shows a histogram of the proportion of non-tradable stocks across A-share firms listed at the end of 2004. Out of 1,378 firms, 1,350 (or 98%) had anywhere between 20% and 80% of their stocks under the non-tradable category. A feature of those original "tradability assignments" is that they could not be easily changed. In fact, firms were expected to maintain the proportional tradability status of their stocks going forward. Among other hurdles, multiple layers of government had a say over issuance of new shares that could be intended to alter the proportion of tradable and non-tradable shares in a firm. Evidence suggests that markets had reacted negatively to past issuance programs that could alter the proportion of tradable and non-tradable stocks (see Inoue, 2005) and tradable shareholders had been able to block large programs since these required a high level of shareholder approval.

<u>FIGURE 1 ABOUT HERE</u>

Research shows that the split-share structure created a number of governance problems (see 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. Since the wealth of these shareholders was largely insulated from changes in stock market prices, conflicts between controlling and minority (tradable) shareholders quickly 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 (Cheung et al., 2006; Deng et al., 2008). As top managers and 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 want in the equity markets (Tenev et al., 2002).

2.2 The Timeline of the Reform

By early 2005 it was clear that the split-share structure created an illiquid stock market, with the better Chinese companies choosing to list abroad. 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."[2] Within days, the Shanghai 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 shares 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. Only 2% of the firms failed to comply by the December 2006 (most of those complied in January 2007). Figure 2 shows the number of firms complying with the program over time.

The pilot firms had to start their share conversion process immediately after the government announced their selection. Materials published by the CSRC and government-run media provide the guidelines used by central planners for selecting firms in to the pilot program. The criteria used for selection considered four general firm attributes: profitability, representativeness, geographic location, and industry of operation.

In short, according to the government's guidelines, 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 firm performance, 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 firm in a particular province, or the best-known company in an 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

[2] 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 as saying the following about the reform: "An arrow that has left the bow can never be taken back."

for firms in competitive industries, since concentrated industries were often associated with national interests or state monopolies.

All of the above selection criteria can be observed using publicly available data. Indeed, as we explain below, we are able to conduct our analysis using information from the same data provider that was commissioned by the Chinese government to implement the reform. One of our tests builds on a matching-based strategy in which we identify a set of control firms that planners 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, thus excluding 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 would propose 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, it was formally 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.[3]

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 shareholders 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 extraneous noise to compliance timing was the fact that the CSRC arbitrarily 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 situation, they imposed caps on the number of conversions, precluding firms from converting their shares at will. Specifically, before voting on a 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

[3]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.

converting firms to about twenty per week (down to eight per week later in the reform). 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.

In sum, various institutional elements of the reform made it difficult for firms to "optimally time" the conversion of their shares. Additional evidence further suggests that shareholders were unsure about the outcomes they should bargain over, making negotiations often unpredictable. Indeed, the initial market reaction to the pilot program was ambiguous so it was not clear that firms should jump into the program (see Inoue, 2005). These features of our setting are helpful in minimizing worries about endogenous biases in our tests (in particular, self-selection). Even so, as 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 ratio of the number of shares traded on the Shanghai Stock Exchange over the total number of shares outstanding. Inspection of Figure 2 suggests that stock turnover in the Chinese equity markets moved in tandem with firm adherence to the share reform program. Indeed, the figure suggests that overall market liquidity increased nearly three-fold from April 2005 to December 2006. 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 in Chinese organized stock exchanges. Government planners alluded — often vaguely — to easier access to capital and increases in firm efficiency as likely consequences of the reform. In this section, we lay out a set of priors concerning the potential consequences of the split-share reform and 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 stocks at a later date if they wish. With these priors in mind, we expect firms to issue equity more actively after their shares become tradable.

One could expect firms to improve their performance under better incentives and more flexible financing opportunities potentially generated by the 2005 reform.[4] Enhanced liquidity brought about by the reform could also lower the cost of equity and broaden the pool of feasible investments (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 performance in corporate finance, and if the share reform was relevant these variables should plausibly respond to the conversion process. We also look at employment as one of our real outcome variables. Given the 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 hard to predict the effect of the reform on employment. Yet, we measure the reform's impact on employment demand as an additional way to gauge potential links between the stock markets and the real economy.

While our estimations focus primarily on the effect of the reform on real-side activities, it is also 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, owners of non-tradable shares could only benefit from their holdings via dividend payments. The reform, however, could change the preferences of those investors. In particular, after shares become tradable, all shareholders would be able to profit from capital gains. As a result, firms could place relatively less emphasis on dividend payments — which were more heavily taxed — as a way to reward investors.[5] In addition to equity issuance, we assess changes in firms' financial policies by looking at their dividend payments. Moreover, we study whether greater access to equity financing has implications for capital structure policy by looking at changes in leverage ratios.

In China, conflicts of interests between minority (tradable) and majority (non-tradable) shareholders are known to be associated with mismanagement and even fraud. These problems became acute in recent years, with minority shareholder expropriation conducted primarily by way of "related party transactions" and "intercorporate loans."[6] We measure the incidence of these fraud-laden transactions in firms converting their shares to see if market liquidity has an impact on these activ-

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

[5] As of 2005, dividends were taxed as ordinary income at a 20% rate, while capital gains were not taxed.

[6] 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.

ities. 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 a decline in those transactions following share conversions. Another mechanism through which existing governance structures could change is via the replacement of corporate managers. Accordingly, we also examine the frequency with which firms replace their CEOs after shares are traded in secondary markets. We also consider the effects of the reform on managerial incentives by looking at changes in CEO stock-based compensation following conversions.

Finally, one could conjecture that the ability to freely trade shares could boost the market for corporate mergers. Newly-converted shares could even be used as a currency to acquire other firms. Accordingly, a potential outcome of the reform is an increase in the number of firms engaging in M&As after converting their shares. Our investigation thus also looks at corporate mergers and the use of stocks in financing those transactions.

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,[7] and companies with previous fraud-related court cases as indicated by the CSRC. That is, we exclude companies that were ineligible for stock conversion according to the CSRC's reform principles. We also exclude financial firms. Data on the share reform come from WIND Financial Information Systems, which was commissioned by the CSRC to conduct the 2005 conversion program. The 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 over 80% of the A-share firms. Our tests use detailed data for these firms from the first quarter of 2002 through the last quarter of 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. 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

[7] 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).

($NetIncome$) 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 study a number of financial outcomes associated with the reform. We first look at stock liquidity, since this is central to our identification. Our benchmark measure of liquidity is the liquidity ratio (*LiqRatio*). This 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). An alternative measure of liquidity is share turnover ($ShareTurnover$), defined as the log ratio of the number of shares traded divided by the number of shares outstanding. For each of these two measures, we obtain data from 2002 to 2009. We also look at firms' issuance and dividend policies. To measure equity issuance ($Issuance$), we collect data on issuance activity (including SEOs and rights offerings) from 2002 to 2009. Firm capital structure is assessed through the debt-to-asset ratio (*Leverage*). Firm dividend policy is examined through the ratio of cash dividends over net income (*Dividend*).

We consider a number of additional outcomes that help us characterize the effect of the reform. While we detail the computation of those outcomes later in the analysis, one line of inquiry we pursue is whether prices become more informative after stocks become tradable. On that front, we use a proxy for price informativeness that is based on the synchronicity of a firm's stock returns and the returns on the aggregate market (*PriceInfo*). Relatedly, we also measure the number of individuals trading on the firm's stock ($ShareHolders$). In addition, we consider proxies for managerial incentives and agency problems. On that dimension, we examine the effect of the reform on the proportion of shares owned by the top managers ($ManagerShares$) and whether firms replace their CEOs ($CEOTurnover$). We use the Herfindahl index of top 5 shareholders ($OwnerConcent$) to gauge ownership concentration. Furthermore, we investigate activities that are known to be associated with shareholder expropriation in China. In particular, we look at firms' accounting statements and identify "related party transactions" ($RPTs$) and "intercorporate loans" ($InterLoans$). Finally, we measure the impact of the reform on firms' propensity to initiate merger and acquisition deals ($M\&A$), and further examine if those deals were financed by — now tradable — stocks.

Besides the outcomes described above, we use a comprehensive set of control variables in our matching procedures. Regarding ownership structure, we account for the proportion of non-tradable shares ($NonTradable$), the log of number of shares ($Shares$), whether a firm is ultimately controlled by the state (*StateControl*), the proportion of shares owned by the State (*StateShares*), and the proportion of shares held by institutions (*InstShares*). Other firm characteristics include age (*Age*), the log of total assets (*Assets*), the log of total sales (*Sales*), the ratio of cash flow over assets ($CF/Assets$), the ratio of fixed assets over number of employees (K/L), bank loans over assets

($Loans$), and cash-to-assets ratio ($Cash$). Two forward looking variables we use in our matching are the price-earnings ratio (P/E) and the market-to-book asset ratio (q). The importance of firms in their industry and region is proxied by the ratio of firm sales over industry sales ($IndRep$) and the ratio of firm sales over provincial GDP ($ProvRep$). Additionally, we include characteristics associated with firms' geographical location, such as the log per capita GDP of the province in which the firm is established ($ProvGDP$), the log of industry sales ($IndSales$), and the industry concentration index ($IndConcent$). Since the CSRC required firms to reduce their intercorporate loans prior to the reform, we also use this variable ($InterLoans$) as a matching covariate.

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 most 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 schedule of 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) "complying before June 2006" comprises 821 non-pilot firms that converted their shares at or before June 2006; and (3) "complying after June 2006" comprises 190 firms that converted their shares after June 2006. We detail shortly how our binary-treatment tests use these groups of firms.

Table 2 About Here

Table 2 suggests that pilot firms, as well as firms that converted their shares up to June 2006, are different from firms that joined the reform later for most observables as of 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 they had more concentrated ownership. Moreover, these firms had grown faster than those that joined the reform later. These differences suggest that the timing of the reform compliance 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 and Methodology

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.

4.1 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, one can compare firms that have already joined the reform with firms that have yet to do so. One must take into account, however, that the timing of compliance might be endogenous. Firms could potentially choose when to join the conversion process based on expected outcomes. Another potential concern is that idiosyncratic dynamics in firm outcomes could confound inferences, leading one 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 advance in the treatment window. 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.[8] The treatment spell is the length between the date 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 treatment 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 these time-varying effects. We do so using large sets of covariates under a Generalized

[8] 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.

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. After 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 matched 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. The last part presents the econometric model used to estimate the dose-response function of non-pilot firms.

4.2 Notation

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 they allow us to assess possible anticipation effects (more on this below).

TABLE 3 ABOUT HERE

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, it is difficult to obtain an average estimate for each potential outcome (or each cell in Table 3).[9] 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 being under treatment for a longer period 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

[9] In our application, for example, non-pilot firms can comply with the reform at point in time during the Sept. 2005 to Dec. 2006 window.

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.

With the basic notation in place, we can easily map our methods and empirical setting. In our tests the threshold k is set to be June 2005, which corresponds to the pilot period. The threshold for the control group, k', is July 2006. This is to say that we extract our counterfactuals 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. 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 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.[10]

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 evaluation schedule, in December 2005 we assess the short-run effects of the program on firms between 6 and 7 months in the reform. In December 2006, we compare firms with 18-19 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 with 30-31 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.

[10] 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.

4.3 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 for treatment at d. This condition is strong and might be violated in the data. The estimation approach used for cases in which the treatment 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 empirical 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).[11] 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.

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), all information in X_0 that is contained in the GPS function can be summarized by a 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.

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 allows us to estimate the GPS using the Cox's proportional hazard model (Cox, 1972). Note that 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.

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 (Sauerbrei and Royston, 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:

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

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

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.

4.4 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 approach 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.

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).[12] Our matching estimations use overlap regions defined as follows (Dehejia and Wahba, 1999, 2002):

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

4.5 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:

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

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

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, $k_n = 6, 12, 18, 24, 30$. This allows us to identify nonlinear patterns in the dose-response function in a way that is less computationally intensive than alternative nonparametric methods.

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

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

This method is 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.

Our multi-valued treatment experiments report the results from of the following equation:

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

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.[13]

From any estimate for the dose-response function, we can obtain an estimate for the average time-varying treatment effect of the following form:

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

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 include firm idiosyncratic characteristics (e.g., location, industry, ownership), pre-reform policies (such as dividend payments and capital investments), and pre-reform outcomes (e.g., firm performance and value).[14] 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 the binary propensity score and the GPS index for each firm, we can identify the relevant overlap samples that we use for our comparisons. Figure 4 shows the overlap between pilot and control groups' distributions in the binary comparison. 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

[13] We also defined a common support with $\varepsilon = 12$, but there was no significant change in terms of balance.
[14] The full set of control variables is discussed in Section 3 and listed in Table 1.

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

Table 4 About Here

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 19 out of $1,011$ non-pilot firms, the GPS cannot find a similar control match. For this reason, we exclude those firms when implementing the GPS-based approach.

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.[15] Table 5 reports the Imai-van Dyk regression coefficients and associated 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 Effects of the Reform on Pilot Firms

This section uses a standard treated–control assignment approach to measure the impact of the share conversion program on pilot firms. For ease of exposition, we focus on tests related to a set of real

[15] 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 $b_1 = 0$.

outcomes and financial policies: corporate investment, employment, productivity, profitability, equity issuance, leverage, and dividend payments. Our main, fully-fledged set of results is presented in the next section, where we use a time-varying, multi-valued treatment approach to evaluate a wide range of outcomes.

Estimates for the 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. In addition to outcome effects, 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 under 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 productivity ($Sales/K$), profits ($NetIncome$), and returns (ROE), but these differences decline over time. After 15 months, the probability of equity issuance rises some 16 percentage points for pilot firms relative to their non-pilot counterfactuals. This is a notable increase when compared to the average issuance probability of only 3% for those same pilot firms over the period that preceded the program (see Table 4). After linearly controlling for covariates (column 2), the differences in employment, fixed assets, and share turnover 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

Some 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 on equity. After about six months, fixed assets grew 21% more for pilot firms than for their counterfactuals (the sample average annual asset growth for the period preceding the pilot is 19.5%). Accounting equity returns (ROE) also increased some 9 percentage points more for pilot firms six months after the reform (the pre-reform sample average ROE is 4.5%). 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. A pilot firm's market-to-book ratio, in particular, increased 0.9 above that of a control by the 15^{th} month (the sample average is 2.1). Notably, that ratio declined in the following year, which as we explain below is due to the increase in equity issuance. By December 2007, pilot firms' employment growth was 35% higher than that of matched control firms. Share conversions have a positive, significant effect on equity issuance across all specifications.

As a robustness check, we replaced the pilot firms by non-pilot firms that joined the program early in the reform process (sometime between September and December 2005). The pool of counterfactual firms is similar to that used in the tests of Table 6, that is, 190 firms that joined the reform in the second half of 2006. This gives more testing power (372 non-pilot firms converted their stocks in 2005) and ameliorates concerns that the government may have manipulated the outcomes of pilot firms to showcase the reform. The results are not tabulated to save space, but we find that even though "near pilot" firms were slightly less exposed to the reform, they observe similar growth effects on investment, employment, and equity issuance.

Finally, 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. This is exactly what we find in the data. Similar patterns are found when we consider longer trends in pre-treatment variables and outcomes; e.g., using data going back to 2002 (these results are omitted to save space, but are readily available).

5.3 Time-Varying Effects of the Reform

This section presents our paper's central results. Because we want to describe the impact of the reform across time we present estimated effects by graphing them on a time line. These estimates are computed 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 firm real performance (such as investment, employment, and productivity), those related to financial policy (stock issuance, leverage ratios, and dividend payments), and other outcomes (such as merger deals, managerial compensation, 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 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 log liquidity ratio measure ($LiqRatio$), there is an immediate and persistent positive conversion effect. Thirty months after the reform, that ratio increases 30% above the baseline case of non-conversion. The effect on the share turnover measure

($ShareTurnover$) is less persistent, but it, too, increases up to two years after conversion. In the long run, share turnover becomes about 10% higher due to share conversion.

FIGURE 6 ABOUT HERE

The evidence in Figure 6 confirms our basic prior that corporate shares become significantly more liquid after conversion into tradable status. In turn, we investigate the impact of the stock trading reform 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 examined in our pilot-based tests: ΔK, ΔL, $Sales/K$, $NetIncome$, ROE, and M/B.

FIGURE 7 ABOUT HERE

The first panel of Figure 7 suggests that corporate investment, ΔK (measured in log changes), responds markedly to share conversions. Indeed, the growth in fixed assets is increasingly impacted by the reform. By the 24^{th} month, the investment growth rate is almost 30% higher than in the case of non-conversion. In the longer run, the effect remains at around 20%. Noteworthy, the growth in investment happens without a relative 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 the first 24 months and decreases somewhat afterwards. These two results suggest that firms adjusted their capital-to-labor ratios — appearing to be more productive — after their shares begin to trade freely 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 to 20 months after compliance, when $Sales/K$ becomes almost 40% higher than the counterfactual case of non-compliance. In the longer run, 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 consistent with theoretical priors presented in Section 3.

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 program compliance. By then, operating revenues grow 15% above expenses. 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 pre-reform 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 — the scaler of ROE — following the reform, 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 of 2.1. After 30 months, M/B is 1.1 higher than in the case of no share conversion (about 50% higher than the baseline sample 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 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 appears to have distorted firm policies and hurt private sector growth. The effects of the split-share reform 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 ($Issuance$), capital structure ($Leverage$), 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>

As discussed in Section 3, 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 join the conversion program.[16] In particular, the first panel of Figure 8 shows that the probability that a firm issues new stocks grows steadily after its shares become tradable. Thirty months after conversion the likelihood of issuance is at least 70% 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 listed firms issued equity in 2004, while in 2007 this figure was 13%.

[16] 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.

The increase in equity issues seems to be associated with a decline in corporate leverage following conversions. In particular, the second panel of Figure 8 shows that firms reduce their debt-to-asset ratios by 4 percentage points 24 months after their stocks become tradable (compare to the average benchmark of 48%). Finally, the last panel of Figure 8 suggests that the reform is responsible for a small reduction in dividend payments. Despite the larger error bands associated with tests using financial policy variables, we find that payout ratios fall by about 5 percentage points 24 months after a firm's shares become tradable. This decrease is economically significant if one considers that the average payout prior to conversion was 35%.

5.3.4 Characterizing the Liquidity Channel

There are several, non-exclusive ways by which greater stock liquidity may drive the effects we document in Figures 7 and 8. Increased liquidity in secondary market transactions might, for example, help firm managers make more informed decisions. Increased liquidity might also influence managerial incentives and strengthen links between real and stock market performance if it allows for a greater use of stock-based compensation packages. Furthermore, liquidity could jump start the market for corporate control, reallocating capital where it can be used most efficiently. Finally, higher liquidity allows minority investors to more quickly respond — by selling their shares — to detrimental actions by controlling shareholders (a rampant problem in China). In this section, we provide more direct evidence for these channels.

Price Informativeness One potential explanation for our findings is that greater liquidity might allow for better, more informed decisions by managers. If a price discovery channel is at work, we would expect to see stock prices becoming more informative about firm fundamentals after stocks start to trade. Following previous literature (e.g., Morck et al., 2000), we use stock price synchronicity as a proxy for informativeness. To wit, when the information environment surrounding a firm improves and more firm-relevant information is incorporated in the firm's price discovery process, market factors should explain a bigger proportion of the observed variation in stock returns. In other words, the synchronicity between a firm's stock return and that of the overall stock market (and other indicators, such as industry-wide returns) would reflect the informativeness of the firm's stock price. If increased liquidity improves the information environment surrounding firms' stocks, we would expect to see price synchronicity to be positively associated with reform compliance.

Following Gul et al. (2010), we measure a firm's stock price synchronicity by the R^2 from a regression of the individual firm returns on market and industry returns. We also include lagged industry and market returns to alleviate concerns about potential non-synchronous trading biases that may arise from the use of daily returns for estimating the market model.[17] As Figure 9 suggests, the reform has a positive effect on price informativeness. Converting firms' stock prices become more synchronous with the market up to the 24^{th} month following conversion, when it is at least 60% higher than the case of non-conversion. To gauge the effect of this estimate, note that the pre-reform sample average R^2 is about 10%, which implies that R^2 rose, on average, to 16% as firms converted their shares. Our results suggest that the stock prices incorporate more firm-relevant information as a result of having a more liquid equity market.

Figure 9 About Here

Merger Activity A deeper, more liquid equity market should facilitate corporate control transactions, which are often made possible through the use of shares as a means of exchange (see Bhide, 1993; Maug, 1998). Results depicted in Figure 10 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 percentage 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.

Figure 10 About Here

If our conjecture about the link between stock liquidity and M&A activity is correct, one would expect that firms not only engage in more M&As, but also more often use stocks as a payment method. The evidence we gather suggests that this is indeed the case. For the M&A transactions for which we could collect information on payment methods (nearly half of the total number of M&As), the percentage of deals using cash-only payments dropped from 96% for the period before the reform, to 59% after the reform.

Managerial Incentives To assess the degree of performance-related incentives given to corporate managers around the reform, we collected ownership data for the top three executives for each firm in the sample. We also tracked down CEO departures over our sample period by manually checking firms' annual reports. One would expect the proportion of shares held by top managers ($ManagerShares$) to increase after the reform if firms are more likely to adopt stock-based compensation packages. Likewise, CEO departures ($CEOTurnover$) could increase if poor stock performance would become more relevant in the evaluation of CEO performance and tenure. At the same time,

[17] The A-share market return is based on the composite A-share index of the Shanghai and Shenzhen stock exchanges. The industry return is created using all firms within the same industry (we use the CSRC's industry classification system). To circumvent the bounded nature of R^2, we follow previous literature and use a logistic transformation: $PriceInfo = \log(R^2/(1 - R^2))$.

one has to bear in mind the context in which tests are conducted. Chinese CEOs are often politically connected to the central government apparatus and their employment terms may be isolated from their firms' outcomes.

<u>FIGURE 11 ABOUT HERE</u>

Results in Figure 11 do not point to significant changes in the proportion of shares held by top managers following conversion. At the same time, one observes a decline in the probability of CEO turnover. Our results do not reveal a significant link between changes in managerial incentives and reform outcomes.

Conflicts of Interests and Fraudulent Activities By abolishing distinctions between tradable and non-tradable shares, the reform could potentially ameliorate conflicts between majority and minority shareholders. In this context, the elimination of "share classes" relates to a somewhat different notion of liquidity and we examine whether the reform may have implications for conflicts of interests and agency issues inside firms.

We start with ownership concentration. Concentrated ownership provides controlling shareholders with the opportunity to divert firm resources at the expense of minority shareholders (Morck et al. 2000, Claessens et al. 2002, Fan and Wong 2002). The first panel of Figure 12 shows that ownership concentration among top-5 shareholders (*OwnerConcent*) dropped substantially after firms converted their stocks. 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% of the average concentration index. The reform thus had significant effects on ownership concentration among large shareholders.

<u>FIGURE 12 ABOUT HERE</u>

We note, however, that the effect of the reform on concentration could be somewhat mechanical. Recall, owners of tradable shares were usually compensated with extra shares. As a result, the fraction of the firm owned by majority (non-tradable) shareholders would mechanically decline following conversion. The reform could thus appear to dilute stock ownership of top shareholders, and yet not necessarily imply that there is greater (new) individual investor participation in ownership. While it is impossible to collect detailed information of proportional ownership of each investor in each firm, we are able to gather data on the number of individual shareholders. The second panel of Figure 12 indicates that the number of shareholders increases following the reform, and particularly so when the probability of new equity issuance spikes up. Thirty months after conversion, the number of firm shareholders is 40% higher. Not only there are more transactions on the firm's stock, but also more investors participating in those transactions. This evidence is consistent with the argument that more heterogeneous investors trade in corporate shares after the reform making stock price formation more informative (cf. Figure 10).

In China, conflicts of interests between majority and minority shareholders are known to be associated with corporate mismanagement and even fraud. These problems became acute in recent years, with shareholder expropriation conducted primarily by way of related party transactions.[18] Following prior literature (Cheung et al., 2006; Deng et al., 2008), we classify as related party transactions 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 ($RPTs$) for each firm in our sample. We also look at additional ways in which expropriation takes place in China. In particular, controlling shareholders reportedly often make loans across different parts of their companies (say, from a public held affiliate to a privately controlled subdivision) as a way to siphon off resources. Following Jiang et al. (2010), we collected information on these "intercorporate loans," which are measured by the log amount of other receivables ($InterLoans$).

To test whether the reduction in agency costs is a plausible channel for our results, we examine the effect of reform on changes in related party transactions and intercorporate loans. 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 last two panels of Figure 12 suggest that $RPTs$ declined slightly following conversions, and that this decline is long-lasting. Intercorporate loans show a decline in the short run; however, this decline does not persist in the long run.

5.3.5 Treatment Heterogeneity

Confirming our basic priors, we found evidence that the split-share reform had an immediate and persistent positive effect on stock liquidity. By the same token, equity ownership 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 positive 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 tests of the reform mechanism by entering a couple of interaction terms in our multivariate model (equation (15)). In particular, in a first examination, we interact a firm's

[18] 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.

pre-reform liquidity level ($ShareTurnover$) 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 conversion "treatment;" i.e., we expect a negative interaction term between $ShareTurnover$ and Z. In a similar vein, we interact a firm's pre-reform concentration index ($OwnerConcent$) and Z, and expect a positive interaction effect. Additionally, we interact Z with the lagged proportion of shares held by the top managers ($ManagerShares$), which represents the managerial incentives previously given by the firm. Finally, we interact Z with the pre-reform amount of intercorporate loans ($InterLoans$), which captures the previous level of agency problems and tunneling.

The results from these interactive models are in Table 7, which present the marginal increase in the treatment effect as a function of changes in lagged stock liquidity, ownership concentration, managerial incentives, and intercorporate loans 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).

TABLE 7 ABOUT HERE

Results in Table 7 suggest that the impact of stock conversions on firms' investment (ΔK) is more pronounced for firms that were less liquid, that had more concentrated ownership, and that provided more managerial incentives prior to share conversion. Estimates of these marginal impacts are, however, not statistically significant. The same can be said about employment growth (ΔL). The effects of liquidity, concentration, managerial incentives, and tunneling activity on productivity outcomes (captured by $Sales/K$) are, however, very significant and consistent with our priors. 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 0.24% higher. This estimated sensitivity is sizeable if one considers that, at its peak, the average response of $Sales/K$ to the conversion process is 0.30%. In a similar fashion, the result from the second column indicates that the effect of share conversion on $Sales/K$ is 0.28% 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). In the third column, we see that for firms where the top managers held one percentage point less of the shares, the effect on $Sales/K$ is 0.60% higher. The last column shows the effect on $Sales/K$ is 0.12% higher in firms where intercorporate loans were 10% higher prior to conversion.

The average effects of the reform on firms' profitability and value also change with respect to pre-conversion liquidity, concentration, and agency problems (tunneling activity). 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 0.06% higher and the effect on ROE is 0.02% higher. For firms whose intercorporate loans were 10% higher, the effect of conversion on $NetIncome$ is 0.08% higher and the effect on ROE is 0.03% higher. All of these estimates are economically important (see corresponding panels in Figure 7) and are statistically significant at better than the 10% test level.

In all, the evidence of this section suggests that firms that had the most to gain from the split-share reform — for example, firms whose stocks were illiquid and concentrated before conversion — 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 firms in China.

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 with 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 8 reports estimates for the conditional relation between current outcome variation and reform compliance in the next period for firms who have yet to comply with the reform. That is, we test whether those firms who are about to convert their shares are different from those who do not convert. The table contrasts the performance of a conventional fixed-effect model (FE) with that of our estimation approach (GPS plus FE) in terms of the potential for outcome dynamics endogeneity.

TABLE 8 ABOUT HERE

Table 8 shows that the fixed-effect model fails to account for the fact that investment and liquidity (ΔK and $LiqRatio$) increase in the year before a firm chooses to convert its shares. Moreover, other characteristics such as stock price synchronization (*PriceInfo*), number of shareholders ($ShareHolders$), probability of merger and acquisition deals ($M\&A$), and number of related party transactions ($RPTs$) seem to have changed just before the firm decided to convert its shares. 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 8 confirms that idiosyncratic changes in firms' behavior and performance do not affect our inferences.

6 Concluding Remarks

The 2005 split-share reform allowed for 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 process that took place within a 16-month window. These features are unique and present both opportunities and challenges for empirical testing. It is possible, for example, that better-managed firms were chosen to participate in the pilot trial because of political motivation to showcase the reform. In addition, after the pilot stage, firms were largely free to join the reform at the time of their choosing. As such, the treatment assignment might also be endogenous due to self-selection. To address 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, had significant effects on corporate performance and shareholder wealth. 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 reduce their leverage ratios and 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 economy, we believe our findings have broad implications for understanding the impact of governmental interventions and the trend towards capital market liberalization. Our study indicates that trading in secondary equity markets have significant connections with outcomes observed in the real economy. Our tests show that policies that ease restrictions on these markets may have measurable, positive implications.

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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	
$LiqRatio$	Log of daily trading volume over absolute value of daily return
$ShareTurnover$	Log of number of shares traded over number of shares outstanding
$Issuance$	Dummy for equity issuance activity
$Leverage$	Total debt over total assets
$Dividend$	Cash dividend over net income
Other outcomes	
$PriceInfo$	Log R^2 of daily stock return on market and industry daily returns
$M\&A$	Dummy for merger and acquisition deals in the last 12 months
$ManagerShares$	Proportion of shares owned by the top 3 managers
$CEOTurnover$	Dummy for CEO turnover in the last 12 months
$OwnerConcent$	Herfindahl index of top 5 shareholder ownership
$ShareHolders$	Log of number of shareholders
$RPTs$	Number of related party transactions in the last 12 months
$InterLoans$	Log of other receivables
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
$InstShares$	Proportion of institutional shares
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
$Loans$	Ratio of bank loans over assets
$Cash$	Cash to asset ratio
P/E	Ratio of price to earning per share
q	Tobin's q, market value of assets over book value of assets
$IndRep$	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)

			Compliance Date									
	Total		Pilot		before June 2006		after June 2006		Difference		Difference	
			(1)		(2)		(3)		(1)-(3)	p-value	(2)-(3)	p-value
Real outcomes												
K	20.17	(0.039)	20.53	(0.267)	20.21	(0.044)	19.92	(0.081)	0.605	0.005	0.290	0.003
ΔK	0.195	(0.010)	0.318	(0.039)	0.206	(0.011)	0.120	(0.024)	0.198	0.000	0.086	0.001
L	7.262	(0.039)	7.340	(0.225)	7.306	(0.044)	7.057	(0.087)	0.283	0.185	0.249	0.013
ΔL	0.037	(0.012)	0.148	(0.040)	0.048	(0.014)	-0.032	(0.031)	0.180	0.009	0.080	0.015
$Sales/K$	0.343	(0.034)	0.574	(0.184)	0.366	(0.037)	0.188	(0.089)	0.387	0.061	0.179	0.043
$\Delta Sales/K$	0.044	(0.015)	0.017	(0.054)	0.060	(0.017)	-0.014	(0.038)	0.030	0.753	0.074	0.060
$NetIncome$	0.080	(0.009)	0.203	(0.034)	0.103	(0.007)	-0.049	(0.040)	0.252	0.004	0.152	0.000
$\Delta NetIncome$	-0.047	(0.010)	-0.007	(0.011)	-0.027	(0.007)	-0.134	(0.042)	0.127	0.220	0.107	0.000
ROE	0.045	(0.005)	0.142	(0.011)	0.062	(0.004)	-0.057	(0.019)	0.199	0.000	0.119	0.000
ΔROE	-0.023	(0.005)	0.005	(0.011)	-0.009	(0.004)	-0.083	(0.019)	0.088	0.063	0.075	0.000
M/B	2.114	(0.044)	2.294	(0.145)	2.002	(0.037)	2.558	(0.176)	-0.264	0.488	-0.556	0.000
$\Delta M/B$	-0.561	(0.047)	-0.462	(0.120)	-0.511	(0.037)	-0.774	(0.194)	0.313	0.527	0.263	0.031
Financial outcomes												
$LiqRatio$	8.677	(0.011)	8.948	(0.067)	8.687	(0.012)	8.570	(0.025)	0.378	0.000	0.117	0.000
$ShareTurnover$	-1.384	(0.019)	-0.977	(0.095)	-1.388	(0.022)	-1.458	(0.042)	0.481	0.000	0.070	0.160
$Issuance$	0.009	(0.003)	0.023	(0.023)	0.010	(0.003)	0.000	(0.000)	0.023	0.035	0.010	0.172
$Leverage$	0.480	(0.006)	0.451	(0.028)	0.464	(0.006)	0.554	(0.016)	-0.104	0.004	-0.091	0.000
$Dividend$	0.351	(0.012)	0.355	(0.034)	0.363	(0.014)	0.275	(0.034)	0.080	0.197	0.088	0.015
Other outcomes												
$PriceInfo$	-0.236	(0.018)	-0.236	(0.076)	-0.192	(0.019)	-0.425	(0.051)	0.189	0.095	0.232	0.000
$M\&A$	0.372	(0.015)	0.512	(0.077)	0.368	(0.017)	0.358	(0.035)	0.154	0.062	0.010	0.798
$ManagerShares$	0.007	(0.001)	0.035	(0.016)	0.006	(0.001)	0.000	(0.000)	0.035	0.000	0.006	0.026
$CEOTurnover$	0.181	(0.012)	0.163	(0.057)	0.174	(0.013)	0.216	(0.030)	-0.053	0.440	-0.042	0.181
$OwnerConcent$	0.232	(0.004)	0.273	(0.026)	0.238	(0.005)	0.200	(0.010)	0.073	0.003	0.037	0.001
$ShareHolders$	10.35	(0.024)	10.10	(0.148)	10.38	(0.027)	10.26	(0.056)	-0.157	0.253	0.118	0.061
$RPTs$	5.688	(0.343)	5.605	(1.569)	5.706	(0.348)	5.626	(1.116)	-0.022	0.993	0.080	0.929
$InterLoans$	17.36	(0.050)	16.72	(0.236)	17.24	(0.056)	18.02	(0.113)	-1.296	0.000	-0.773	0.000
Control variables												
$NonTradable$	0.617	(0.003)	0.678	(0.016)	0.619	(0.004)	0.598	(0.008)	0.080	0.000	0.021	0.013
$StateControl$	0.704	(0.014)	0.465	(0.077)	0.720	(0.016)	0.689	(0.034)	-0.224	0.005	0.030	0.404
$Shares$	19.38	(0.023)	19.48	(0.183)	19.40	(0.026)	19.29	(0.046)	0.197	0.132	0.112	0.057
$StateShares$	0.369	(0.008)	0.279	(0.048)	0.380	(0.009)	0.338	(0.019)	-0.059	0.195	0.042	0.042
$InstShares$	0.032	(0.002)	0.055	(0.008)	0.032	(0.002)	0.024	(0.005)	0.031	0.003	0.008	0.072
Age	8.777	(0.116)	7.116	(0.578)	8.504	(0.125)	10.33	(0.293)	-3.215	0.000	-1.827	0.000
$Assets$	21.19	(0.027)	21.56	(0.188)	21.23	(0.030)	20.95	(0.058)	0.606	0.000	0.282	0.000
$Sales$	20.52	(0.037)	21.10	(0.198)	20.58	(0.040)	20.11	(0.095)	0.991	0.000	0.472	0.000
$CF/Assets$	0.048	(0.003)	0.072	(0.011)	0.051	(0.003)	0.031	(0.008)	0.041	0.015	0.020	0.004
K/L	12.91	(0.034)	13.12	(0.279)	12.91	(0.037)	12.87	(0.070)	0.252	0.206	0.043	0.615
$Loans$	0.060	(0.003)	0.076	(0.016)	0.060	(0.003)	0.053	(0.006)	0.022	0.139	0.007	0.321
$Cash$	0.165	(0.004)	0.207	(0.020)	0.172	(0.004)	0.128	(0.008)	0.079	0.000	0.044	0.000
P/E	56.58	(2.770)	26.39	(3.157)	56.42	(2.947)	64.14	(8.511)	-37.75	0.037	-7.72	0.295
q	1.575	(0.020)	1.718	(0.106)	1.553	(0.022)	1.639	(0.050)	0.080	0.492	-0.085	0.094
$IndRep$	0.001	(0.000)	0.001	(0.001)	0.001	(0.000)	0.000	(0.000)	0.001	0.006	0.001	0.177
$ProvRep$	0.003	(0.000)	0.005	(0.002)	0.004	(0.000)	0.002	(0.000)	0.003	0.003	0.001	0.025
$ProvGDP$	9.597	(0.017)	9.836	(0.082)	9.608	(0.020)	9.495	(0.038)	0.341	0.000	0.113	0.013
$IndSales$	26.62	(0.046)	26.67	(0.198)	26.63	(0.052)	26.55	(0.105)	0.116	0.631	0.077	0.518
$IndConcent$	0.046	(0.002)	0.048	(0.011)	0.047	(0.003)	0.041	(0.004)	0.007	0.483	0.006	0.282
# of obs.	1,054		43		821		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, in column (1), are those that jointed the reform in May-June 2005. Column (2) shows statistics for non-pilot firms that jointed the reform before June 2006. Column (3) shows statistics for firms that joint the reform after June 2006. We also report the difference in sample average between pilot firms and firms complying after June 2006, as well as between non-pilot firms complying before June 2006 and firms complying after June 2006. 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	$\dots$	$T-1$	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 Pilot Firms and Control Firms After Matching

	Pilot	Control	Difference		p-value
Real outcomes					
K	20.70	20.67	0.028	(0.459)	0.952
ΔK	0.319	0.221	0.099	(0.121)	0.416
L	7.378	7.821	-0.443	(0.381)	0.245
ΔL	0.153	0.034	0.119	(0.168)	0.480
$Sales/K$	0.479	0.680	-0.201	(0.392)	0.608
$\Delta Sales/K$	0.011	0.177	-0.166	(0.134)	0.215
$NetIncome$	0.165	0.104	0.062	(0.033)	0.063
$\Delta NetIncome$	-0.011	0.012	-0.023	(0.036)	0.519
ROE	0.131	0.117	0.014	(0.020)	0.477
ΔROE	0.011	0.015	-0.003	(0.019)	0.866
M/B	2.030	2.394	-0.364	(0.563)	0.519
$\Delta M/B$	-0.501	-0.670	0.169	(0.253)	0.504
Financial outcomes					
$LiqRatio$	8.915	8.770	0.145	(0.116)	0.209
$ShareTurnover$	-1.092	-1.413	0.321	(0.210)	0.126
$Issuance$	0.030	0.000	0.030	(0.030)	0.317
$Leverage$	0.493	0.529	-0.036	(0.055)	0.505
$Dividend$	0.345	0.330	0.015	(0.099)	0.878
Other outcomes					
$PriceInfo$	-0.193	-0.533	0.340	(0.293)	0.246
$M\&A$	0.515	0.424	0.091	(0.159)	0.567
$ManagerShares$	0.031	0.000	0.031	(0.018)	0.085
$CEOTurnover$	0.152	0.242	-0.091	(0.128)	0.478
$OwnerConcent$	0.269	0.310	-0.041	(0.059)	0.483
$ShareHolders$	10.30	10.19	0.107	(0.291)	0.712
$RPTs$	5.727	10.33	-4.606	(3.580)	0.198
$InterLoans$	17.05	17.21	-0.160	(0.476)	0.737
Control variables					
$NonTradable$	0.662	0.634	0.028	(0.045)	0.530
$StateControl$	0.545	0.576	-0.030	(0.158)	0.848
$Shares$	19.58	19.54	0.048	(0.256)	0.850
$StateShares$	0.317	0.318	-0.001	(0.096)	0.993
$InstShares$	0.047	0.042	0.006	(0.022)	0.796
Age	7.91	8.55	-0.636	(1.144)	0.578
$Assets$	21.67	21.64	0.030	(0.302)	0.922
$Sales$	21.18	21.35	-0.174	(0.437)	0.691
$CF/Assets$	0.066	0.063	0.003	(0.022)	0.882
K/L	13.32	12.85	0.471	(0.391)	0.229
$Loans$	0.085	0.111	-0.026	(0.031)	0.409
$Cash$	0.183	0.170	0.013	(0.034)	0.702
P/E	26.61	40.77	-14.160	(18.111)	0.434
q	1.520	1.615	-0.10	(0.26)	0.713
$IndRep$	0.001	0.001	0.000	(0.001)	0.740
$ProvRep$	0.006	0.004	0.002	(0.003)	0.529
$ProvGDP$	9.820	9.767	0.053	(0.183)	0.774
$IndSales$	26.55	26.92	-0.37	(0.429)	0.388
$IndConcent$	0.054	0.031	0.023	(0.017)	0.167

This table shows the average difference in pre-reform covariates between pilot firms and their matched control firms. Pilot firms are those that jointed the reform in May-June 2005. Control firms are those that joint the reform after June 2006. 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	-0.017	0.077	0.005	0.760
ΔK	-0.010	0.000	0.001	0.866
L	-0.019	0.045	0.014	0.258
ΔL	-0.007	0.011	-0.001	0.781
$Sales/K$	-0.030	0.002	-0.005	0.693
$\Delta Sales/K$	-0.005	0.229	-0.003	0.414
$NetIncome$	-0.016	0.000	0.001	0.838
$\Delta NetIncome$	-0.012	0.011	0.000	0.869
ROE	-0.013	0.000	0.000	0.819
ΔROE	-0.006	0.000	0.000	0.697
M/B	0.020	0.121	0.011	0.366
$\Delta M/B$	-0.021	0.127	-0.010	0.404
Financial outcomes				
$LiqRatio$	-0.016	0.000	0.000	0.921
$ShareTurnover$	-0.021	0.000	-0.004	0.437
$Issuance$	0.000	0.464	0.000	0.337
$Leverage$	0.008	0.000	0.000	0.831
$Dividend$	-0.011	0.000	-0.005	0.198
Other outcomes				
$PriceInfo$	-0.018	0.000	-0.007	0.271
$M\&A$	-0.006	0.108	0.001	0.837
$ManagerShares$	-0.001	0.000	0.000	0.659
$CEOTurnover$	0.006	0.045	-0.001	0.697
$OwnerConcent$	-0.004	0.000	0.001	0.612
$ShareHolders$	0.002	0.711	-0.005	0.573
$RPTs$	-0.082	0.370	0.117	0.407
$InterLoans$	0.091	0.000	-0.006	0.690
Control variables				
$NonTradable$	-0.004	0.000	0.001	0.601
$StateControl$	0.008	0.040	0.004	0.455
$Shares$	-0.007	0.217	0.002	0.803
$StateShares$	0.000	0.873	0.002	0.568
$InstShares$	-0.002	0.000	0.001	0.476
Age	0.188	0.000	-0.016	0.659
$Assets$	-0.023	0.000	0.002	0.842
$Sales$	-0.047	0.000	0.000	0.974
$CF/Assets$	-0.002	0.001	0.001	0.453
K/L	0.001	0.857	-0.008	0.425
$Loans$	-0.001	0.047	0.000	0.932
$Cash$	-0.005	0.000	-0.001	0.506
P/E	2.501	0.002	-0.115	0.879
q	-0.002	0.689	0.002	0.730
$IndRep$	0.000	0.114	0.000	0.640
$ProvRep$	0.000	0.005	0.000	0.955
$ProvGDP$	-0.019	0.000	-0.004	0.581
$IndSales$	-0.005	0.662	0.017	0.259
$IndConcent$	-0.001	0.261	-0.001	0.167

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)***	0.170	(0.063)***	0.212	(0.116)*
ΔL	0.247	(0.045)***	0.202	(0.072)***	0.150	(0.099)
$Sales/K$	0.188	(0.098)*	-0.096	(0.148)	-0.119	(0.124)
$NetIncome$	0.123	(0.068)*	-0.021	(0.123)	-0.006	(0.023)
ROE	0.119	(0.022)***	0.139	(0.042)***	0.086	(0.035)**
M/B	0.156	(0.267)	0.193	(0.396)	-0.120	(0.237)
$Issuance$	-0.005	(0.033)	-0.060	(0.045)	-0.120	(0.096)
$Leverage$	-0.062	(0.017)***	-0.060	(0.028)**	-0.012	(0.032)
$Dividend$	0.001	(0.062)	-0.017	(0.090)	0.010	(0.097)
Z	5.930	(0.145)***	5.913	(0.147)***	5.879	(0.137)***
2006						
ΔK	0.526	(0.087)***	0.242	(0.174)	0.693	(0.290)**
ΔL	0.406	(0.065)***	0.281	(0.100)***	0.414	(0.135)***
$Sales/K$	0.026	(0.101)	0.040	(0.166)	-0.516	(0.289)*
$NetIncome$	0.038	(0.072)	0.295	(0.189)	-0.013	(0.096)
ROE	0.005	(0.027)	0.087	(0.048)*	0.034	(0.029)
M/B	0.904	(0.386)**	0.779	(0.477)	0.906	(0.459)**
$Issuance$	0.163	(0.065)**	0.160	(0.068)**	0.152	(0.074)**
$Leverage$	-0.082	(0.024)***	-0.129	(0.047)***	-0.031	(0.042)
$Dividend$	0.062	(0.063)	0.046	(0.076)	-0.095	(0.074)
Z	15.830	(0.208)***	15.747	(0.308)***	14.909	(0.442)***
2007						
ΔK	0.674	(0.141)***	0.255	(0.222)	0.580	(0.300)*
ΔL	0.553	(0.090)***	0.302	(0.132)**	0.346	(0.141)**
$Sales/K$	0.133	(0.137)	0.231	(0.209)	-0.228	(0.207)
$NetIncome$	-0.107	(0.061)*	-0.074	(0.085)	-0.101	(0.080)
ROE	-0.106	(0.039)***	-0.034	(0.049)	-0.083	(0.047)*
M/B	-0.308	(0.683)	-0.198	(0.890)	-1.214	(1.326)
$LiqRatio$	0.068	(0.053)	0.104	(0.071)	0.076	(0.116)
$ShareTurnover$	-0.781	(0.139)***	-0.650	(0.164)***	-0.469	(0.282)*
$Issuance$	0.201	(0.074)***	0.181	(0.081)**	0.212	(0.091)**
$Leverage$	-0.081	(0.027)***	-0.104	(0.044)***	-0.054	(0.041)
$Dividend$	-0.085	(0.056)	-0.032	(0.086)	0.061	(0.120)
Z	17.299	(0.388)***	17.484	(0.725)***	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: Heterogeneity of the Marginal Effect on Real Outcomes

	Lagged $ShareTurnover$		Lagged $OwnerConcent$		Lagged $ManagerShares$		Lagged $InterLoans$	
ΔK	-0.019	(0.016)	0.064	(0.059)	0.225	(0.295)	0.005	(0.004)
ΔL	-0.024	(0.023)	0.074	(0.050)	-0.005	(0.157)	-0.002	(0.003)
$Sales/K$	-0.024	(0.014)*	0.280	(0.097)***	-0.602	(0.213)***	0.012	(0.005)**
$NetIncome$	-0.010	(0.004)**	0.064	(0.038)*	-0.007	(0.074)	0.008	(0.004)**
ROE	-0.014	(0.003)***	0.020	(0.018)	-0.012	(0.068)	0.003	(0.001)***
M/B	0.162	(0.066)**	0.023	(0.247)	-0.016	(0.496)	0.025	(0.029)

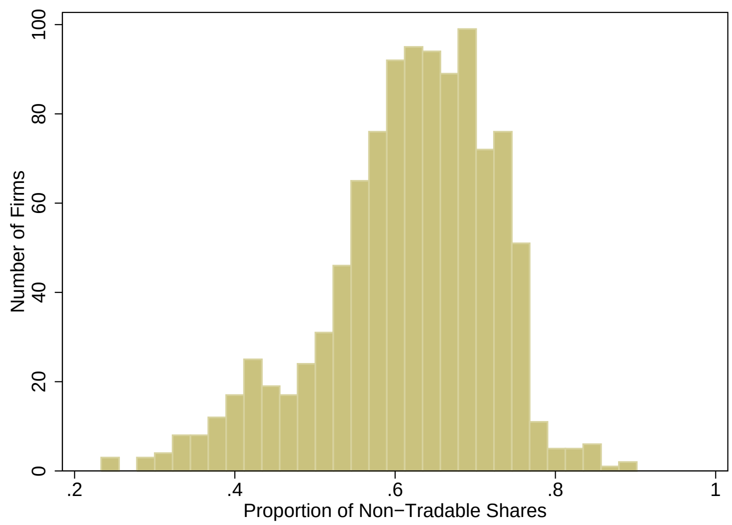
This table shows the estimated coefficient of the interaction between months since the reform started (Z) and the following lagged variables: share turnover ($ShareTurnover$), Herfindahl index of top 5 shareholder ownership ($OwnerConcent$), proportion of shares owned by the top 3 managers ($ManagerShares$), and log of intercorporate loans ($InterLoans$). 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). Robust standard errors are in parentheses and p-value are reported in separate columns.

Table 8: 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	0.133	(0.033)	0.056	0.082	(0.060)	0.309
ΔL	0.061	(0.057)	0.396	0.074	(0.081)	0.455
$Sales/K$	0.140	(0.081)	0.227	0.064	(0.030)	0.164
$NetIncome$	0.178	(0.128)	0.297	0.107	(0.059)	0.214
ROE	0.064	(0.030)	0.167	0.070	(0.046)	0.269
M/B	0.052	(0.027)	0.190	-0.033	(0.093)	0.757
Financial outcomes						
$LiqRatio$	0.014	(0.003)	0.058	-0.019	(0.011)	0.225
$ShareTurnover$	-0.128	(0.047)	0.113	-0.038	(0.030)	0.333
$Issuance$	0.001	(0.003)	0.798	0.000	(0.001)	0.792
$Leverage$	-0.011	(0.031)	0.752	-0.011	(0.017)	0.575
$Dividend$	0.001	(0.030)	0.978	0.015	(0.031)	0.683
Other outcomes						
$PriceInfo$	0.068	(0.013)	0.033	0.012	(0.027)	0.707
$M\&A$	0.044	(0.007)	0.027	0.040	(0.032)	0.333
$ManagerShares$	-0.002	(0.001)	0.119	-0.001	(0.000)	0.063
$CEOTurnover$	-0.016	(0.009)	0.218	-0.046	(0.048)	0.444
$OwnerConcent$	0.001	(0.003)	0.680	0.001	(0.003)	0.770
$ShareHolders$	-0.033	(0.006)	0.033	-0.027	(0.024)	0.374
$RPTs$	1.034	(0.317)	0.083	-0.072	(0.907)	0.944
$InterLoans$	-0.081	(0.088)	0.455	-0.176	(0.160)	0.386

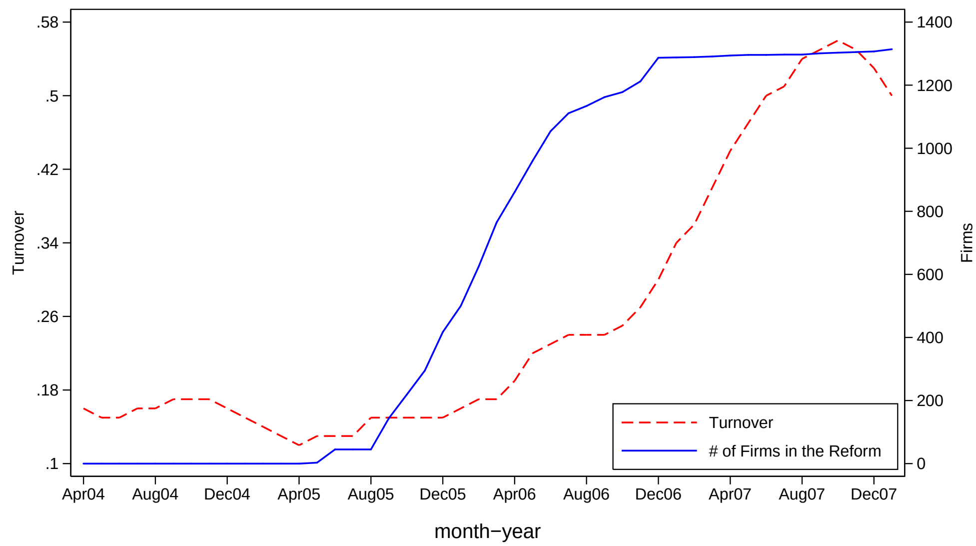
This table shows the estimated coefficient of the lead dummy that indicates whether the firm complies with the reform in the next period or not. The sample only includes observations up to the date when the firm joined the reform, i.e., firm-year observations whose $Z = 0$. The estimates in column 1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

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



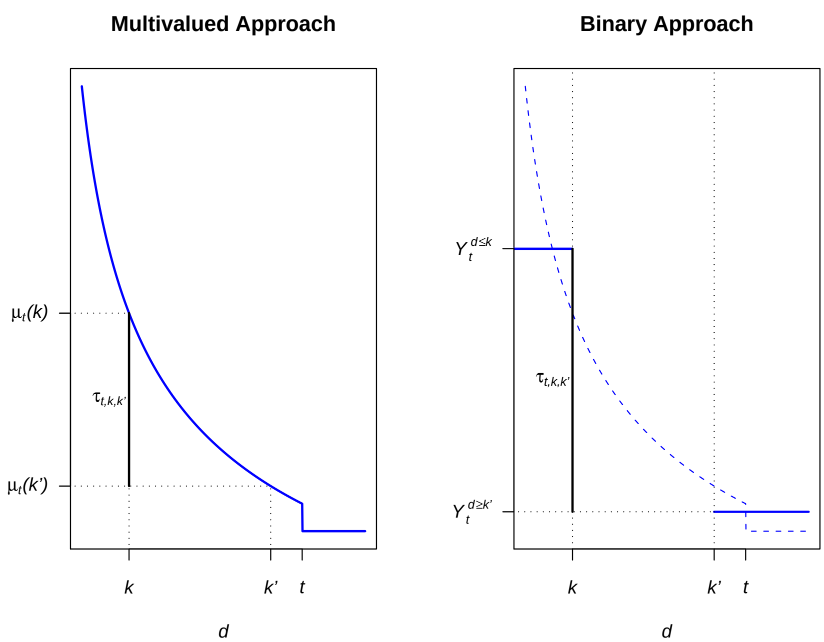
The x-axis represents the proportion of non-trabable shares in December 2004. The y-axis represents the frequency of firms.

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



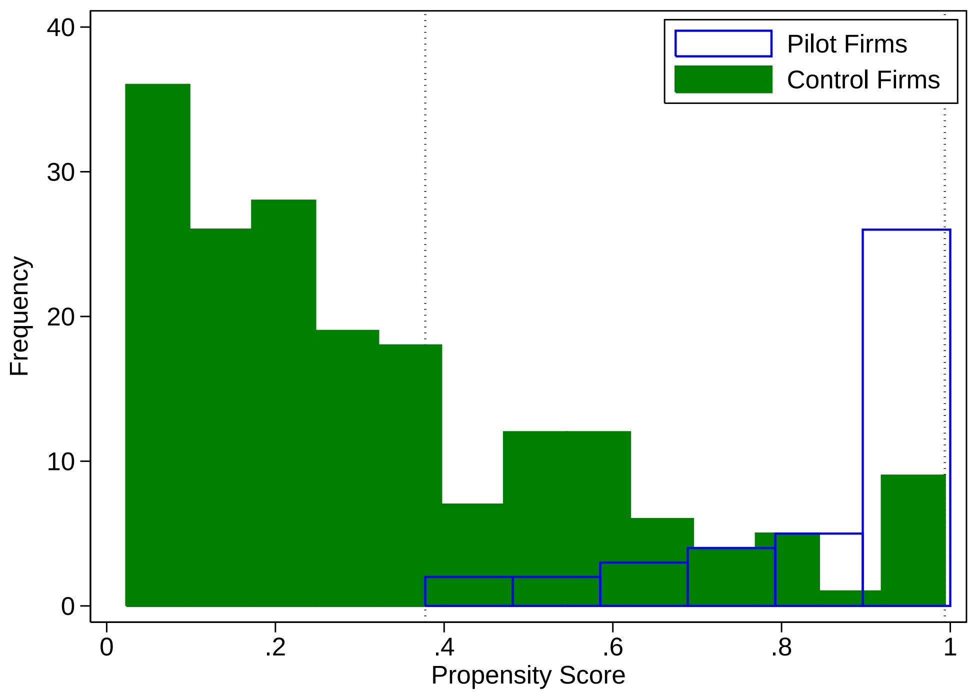
The y-axis (right side) represents the number of firms in the reform. The y-axis (left side) measures the market liquidity (turnover). We compute the market liquidity (turnover) as a 12-month moving average of the ratio of number of shares traded on the Shanghai Stock Exchange over the total number of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

Figure 3: Multivalued and Binary Approaches for ATE



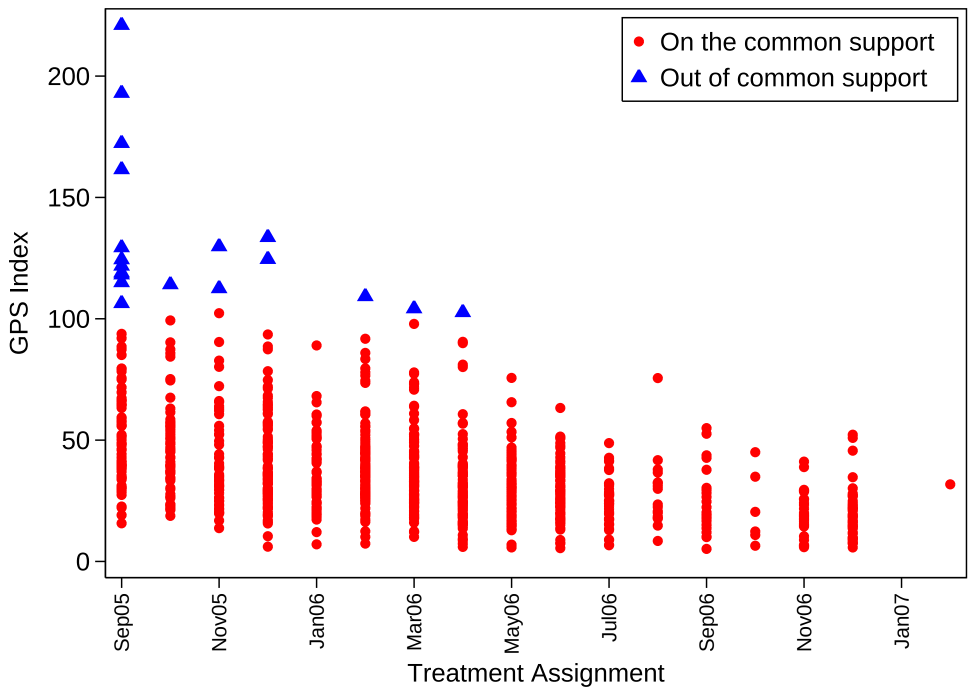
The left graph gives an example of Average Treatment Effect (ATE) estimated from a dose-response function. The point on the curve represents the expected outcome at time t if the firm joins the reform at a particular time k. The right graph depicts the estimates for a mirrored binary-treatment framework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

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



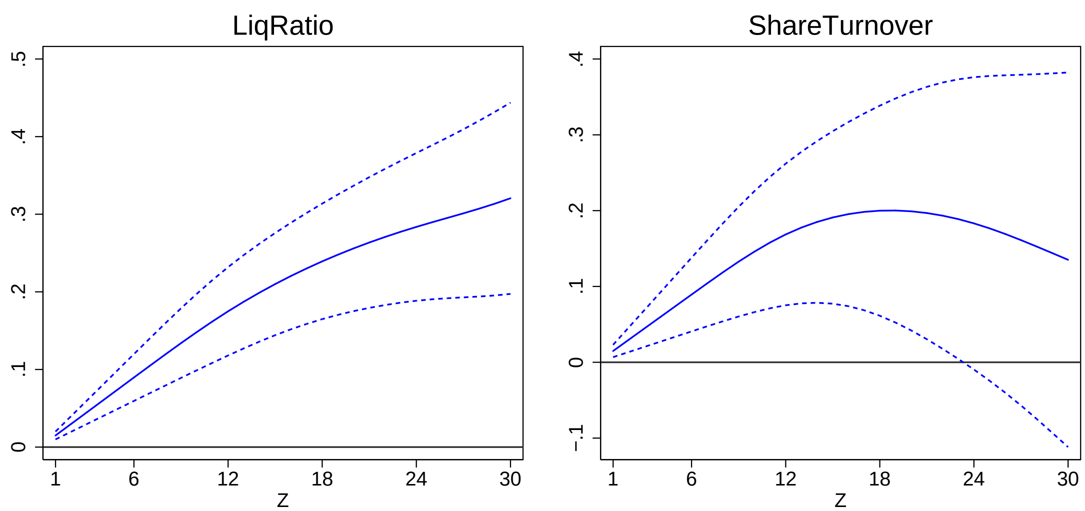
This figure shows the overlap between pilot and control firms' distributions in the binary comparison. We compare these groups in terms of the estimated propensity score. The dotted lines delimit the common support of both groups as defined in equation (13). Pilot firms are those that jointed the reform in May-June 2005. Control firms are those that joint the reform after June 2006.

Figure 5: Relationship between Treatment Assignment and the GPS Index



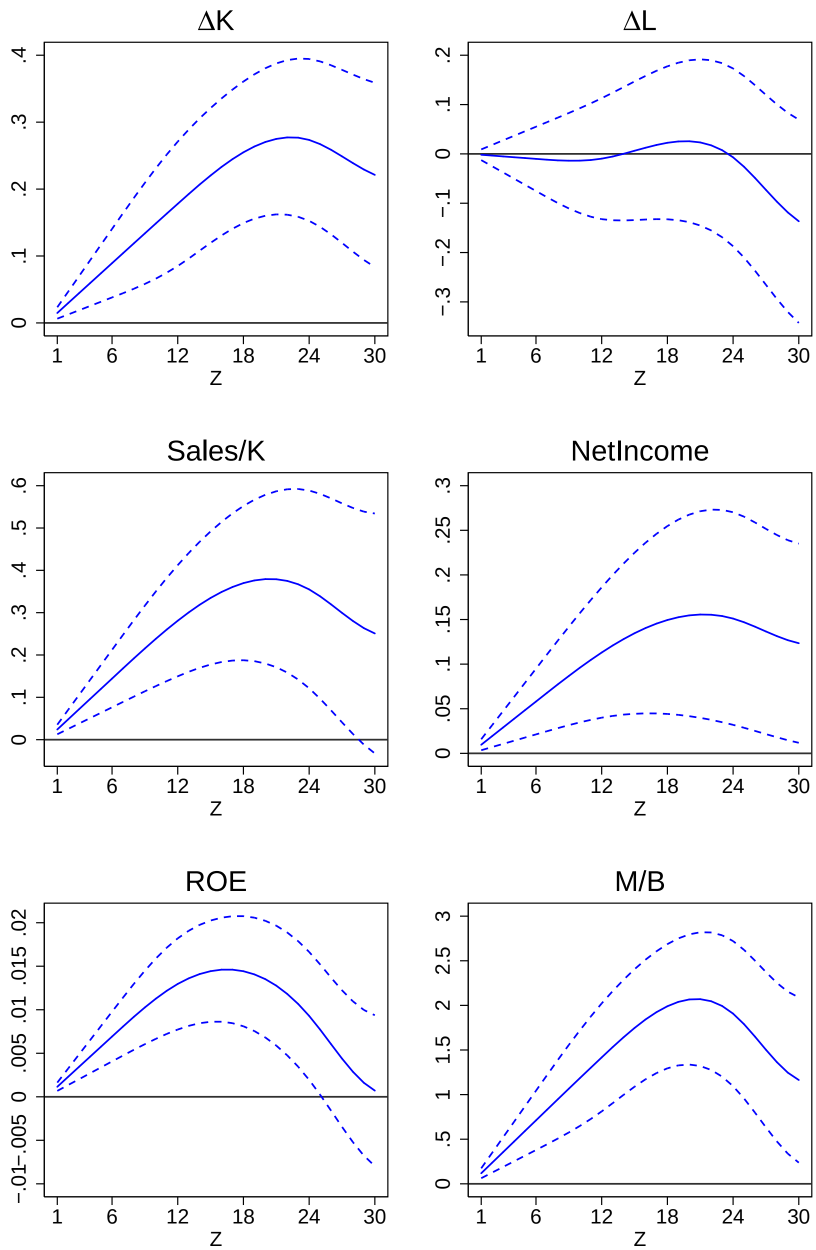
This figure shows the difference between firms outside the common support and those inside the common support in terms of Generalized Propensity Score (GPS) index, which represents observable pre-treatment characteristics. The common support, as defined by equation (16), is the region where each firm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

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



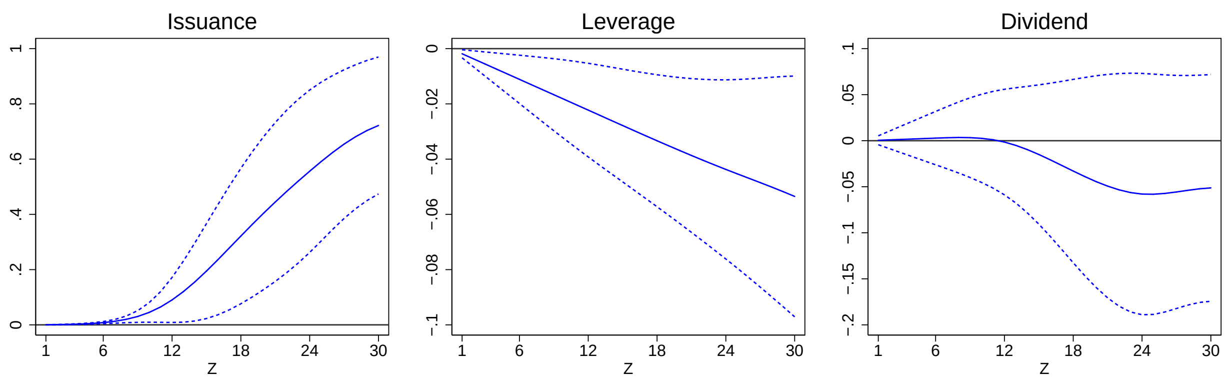
This figure presents time-varying reform effects on liquidity measures: $LiqRatio$ and $ShareTurnover$. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). All regressions are performed using annual 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, $\mu(Z) - \mu(0)$



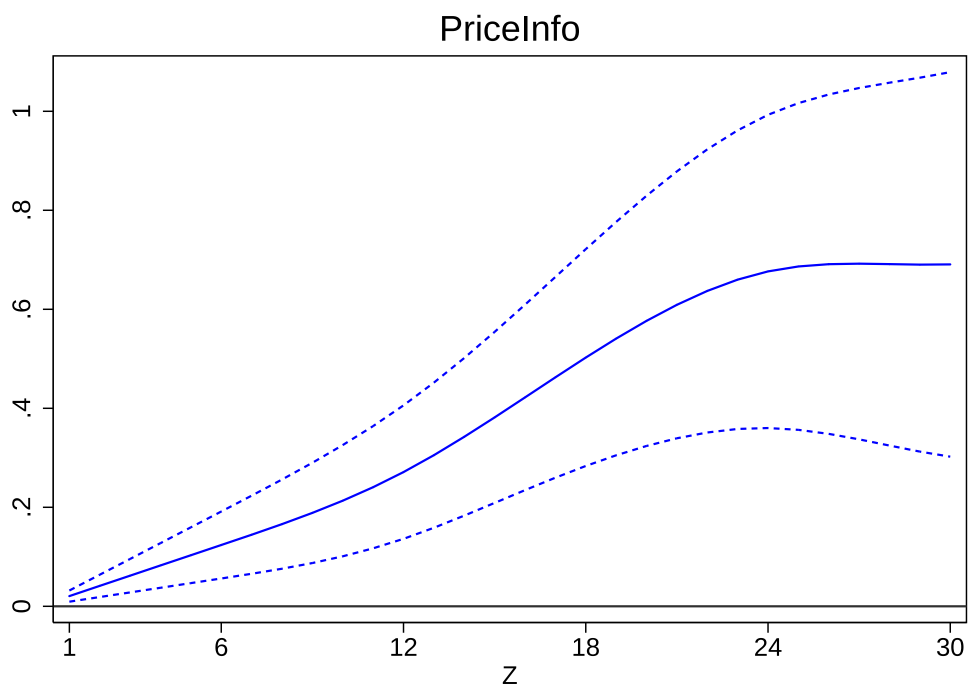
This figure presents time-varying reform effects on each of the business performance measures: ΔK, ΔL, $Sales/K$, $NetIncome$, ROE, and M/B, where Δ indicates the difference between the 12-month forward value and the current value. The regressions for ΔK, $NetIncome$, $Sales/K$, M/B and ROE are estimated 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, $\mu(Z) - \mu(0)$



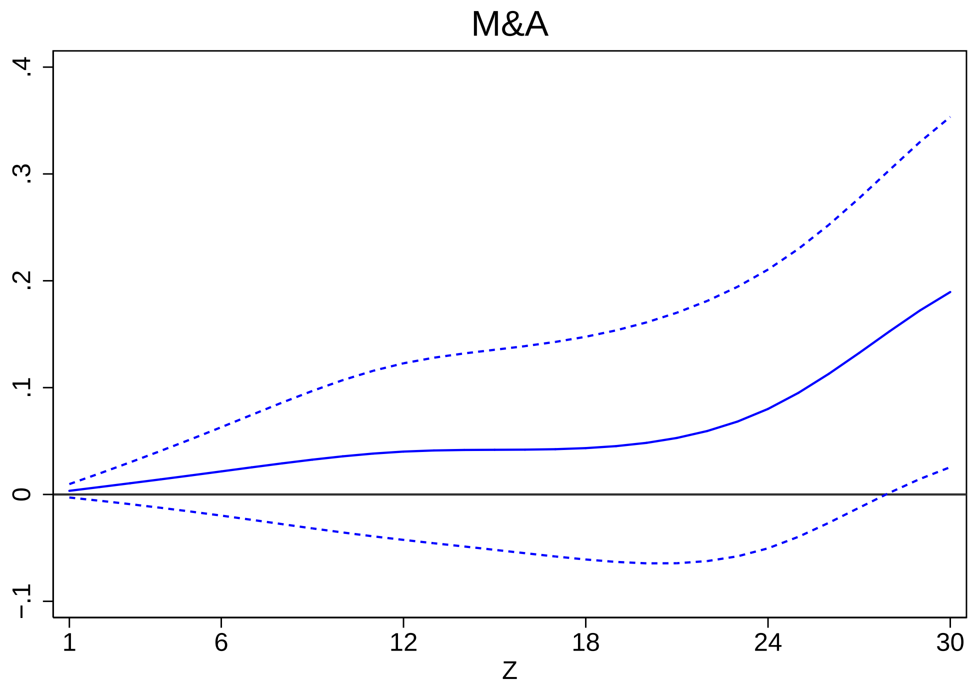
This figure presents time-varying reform effects on financial measures: *Dividend*, *Leverage*, and *Issuance*. For each of the variables, the dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). All regressions are performed using annual data. Dose-response function for *Issuance* is estimated using a Probit model. Dose-response functions for *Dividend* and *Leverage* are 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 Price Informativeness, $\mu(Z) - \mu(0)$



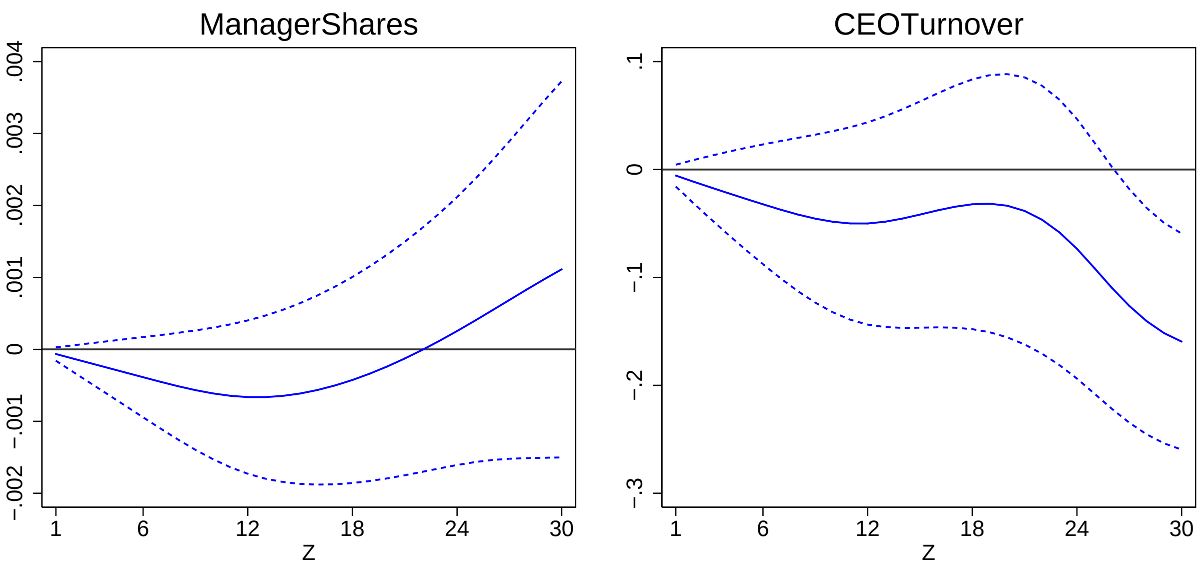
This figure presents time-varying reform effects on $PriceInfo$. The dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). Regression is performed using annual data. The plot represents the expected difference between being in the reform for Z months, $\mu(Z)$, and the counterfactual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% confidence interval for the estimates.

Figure 10: Time-Varying Treatment Effect on Merger Activity, $\mu(Z) - \mu(0)$



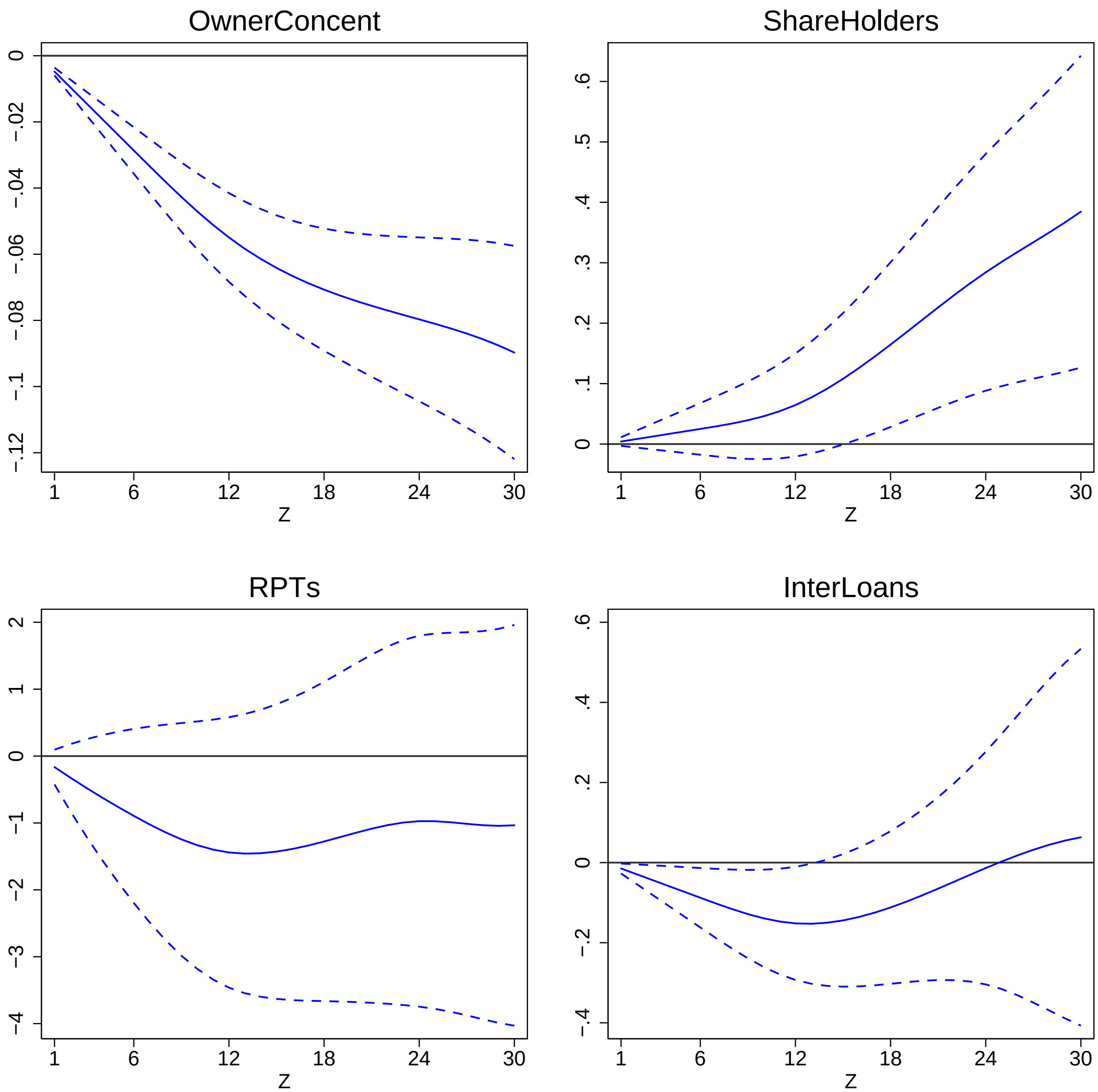
This figure presents time-varying reform effects on $M\&A$. The dose-response function is estimated using Inverse Probability Weighing (IPW) and regression adjustment based on equation (15). The Probit model is estimated using annual data. The plot represents the expected difference between being in 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 11: Time-Varying Treatment Effect on Managerial Incentives, $\mu(Z) - \mu(0)$



This figure presents time-varying reform effects on $ManagerShares$ and $CEOTurnover$. 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 $CEOTurnover$ is estimated using a Probit model. Dose-response functions for $ManagerShares$ 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 12: Time-Varying Treatment Effect on Ownership and Agency Problems, $\mu(Z) - \mu(0)$



This figure presents time-varying reform effects on $OwnerConcent$, $ShareHolders$, $RPTs$, and $InterLoans$. 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 $RPTs$ is estimated using a Poisson model. Dose-response functions for $OwnerConcent$, $ShareHolders$, and $InterLoans$ are 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.