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

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The 2005 split-share reform in China mandated the conversion of nontradable stocks into tradable status. This paper examines the effects of stock markets on corporate outcomes exploiting multiple institutional features of the Chinese conversion program. Using a generalized propensity score matching approach, we identify increases in corporate profitability, investment, value, and productivity as a result of the reform. We also identify changes in firms' likelihood to issue shares and engage in mergers, as well as changes in dividend and capital structure policies. Our findings provide insights on the role of stock markets in shaping corporate activity and on the impact of regulation on economic growth. (*JEL* G31, C21, O16, D21)

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—those among market investors—affect corporate outcomes. It has been long argued that secondary stock market transactions are largely a "side show" to the real economy (e.g., Bosworth 1975). At the same time, there are reasons to believe that those transactions might matter. In the presence of agency problems, for example, secondary market transactions are important to

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the extent that they allow for changes in corporate control (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).

It is difficult to test 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, a choice confounded with 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. Although these types of counterfactuals are rarely observed, recent institutional changes affecting the Chinese stock market help us to 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 listed overseas (H-shares, for firms listed in Hong Kong). A-shares represent over 90% of the market and were, until recently, split into tradable and nontradable categories. These share categories gave their owners identical cash flow and voting rights, yet over 70% of outstanding A-shares could not be traded in the organized exchanges. This unique structure—the cumulative result of past reforms—created a number of difficulties when 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 nontradable shares into tradable. The reform started with a pilot trial in May of 2005, with a set of 46 firms forced into immediate 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.

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, financial policy, 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. 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 immediately 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.

Conversions required the approval of a super-majority of votes by tradable and nontradable shareholders. Various reports pointed to difficulties in reaching such a high level of agreement, and countless regulatory hurdles added noise to firms' conversion timing. Another factor affecting program participation was the CSRC's desire to promote an "orderly conversion process." To avoid downward pressure on stock prices, the agency limited the number of firms allowed to convert their shares at any point of the reform window (firms were subjected to arbitrary "weekly conversion quotas"). Despite these considerations, 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.

Our paper employs a multivalued treatment approach that minimizes concerns about these types of selection 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 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. 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 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.[1]

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 noncompliance. 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 nonconversion.

Further assessing the impact of the reform, we find that the effect of conversions on the ratio of market-to-book value of equity was positive

[1] Under this approach, treatment is not defined as a constant indicator variable (treated *versus* untreated), but rather is defined as the number of months since joining the reform (length of treatment exposure).

and increasing until 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 after 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. Moreover, as stocks became more liquid, firms put less emphasis on dividend payments.

Our 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 characterize the proposed mechanism, we 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 conversion process. Using pre-conversion distributions of stock liquidity and ownership concentration, 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. 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, because stocks can be used to finance these transactions. This is what we find in the data. We also study the effect 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 find only weak evidence of a reduction in agency costs following the reform.

1. The 2005 Share Reform

1.1 Institutional background

Starting in the late 1990s, Chinese central planners implemented a series of privatization programs to recapitalize state-owned enterprises (SOEs). To keep some degree of control over the privatized firms, the government established *share classes* based on firms' relationship with the state, with

all broadly defined "state-related" shares becoming nontradable in the organized exchanges. Under that arrangement, owners of nontradable shares could only sell their shares under strict government control. Prices were set by state agencies at deliberately low levels to avoid transfers. Proposed transactions had to be submitted in writing, with central and local governments commonly taking months to issue a decision.

Whereas nontradable and tradable shares had similar voting and cash flow rights, nontradables accounted for about two-thirds of all shares. Cross-firm variation in the proportion of share classes was determined according to interests within an intricate web of bureaucracies, including central-government asset management committees, central finance and industry ministries, and local governments. All of these parties had power to determine 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 nontradable shares in their books. Figure 1 shows a histogram of the proportion of nontradable 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 nontradable category. A feature of those original "tradability assignments" is that they could not be easily changed.

1.2 Time line 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 directive titled "Circular on Issues Related to the Pilot Program of Non-Tradable Share Reform in Listed Companies."[2] Within days, the Shanghai and Shenzhen Stock Exchanges issued a joint circular formalizing the overhaul of the split-share system. The reform contained a "pilot program," and on May 9 four firms were selected into the pilot. On June 20, a final batch of 42 companies was added to the pilot. On September 4, the CSRC issued "Administrative Measures on the Split Share Structure Reform in Listed Companies," a document determining that the conversion of nontradable shares into tradable shares should be adopted by *all* A-share firms by December 2006. Figure 2 shows the number of firms that complied with the program over time.

Materials published by the CSRC and government-run media provide the guidelines used to select firms for the pilot. Those criteria contained

[2] The directive was issued Friday night before a prolonged May 1st holiday and was interpreted by newspapers as a signal that the government intended to push the reform without consulting firms, investors, or the organized exchanges.

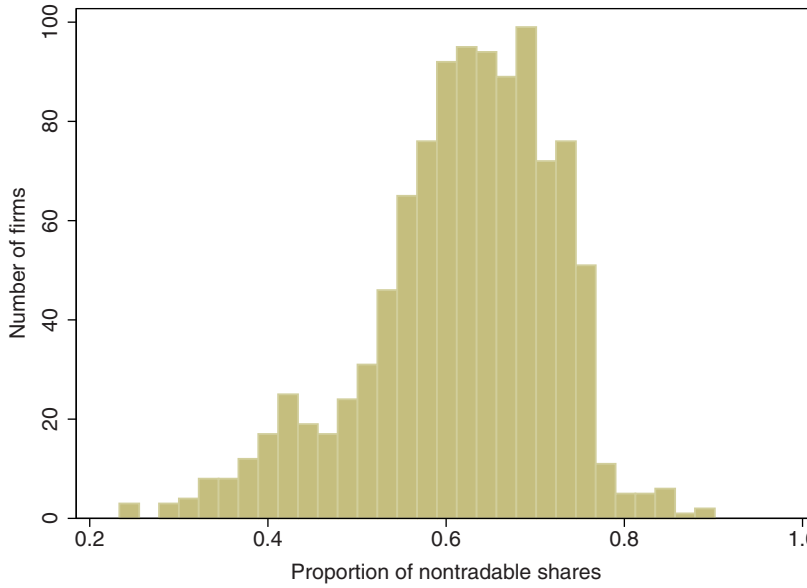

Figure 1
Distribution of nontradable shares before the reform.
The *x*-axis represents the proportion of nontrabable shares in December 2004. The *y*-axis represents the frequency of firms. The sample comprises 1,378 A-share firms.

four main attributes: profitability, representativeness, geographic location, and industry of operation. In short, a profitable firm should be able to afford a conversion proposal with a high compensation package (explained shortly), making it easier to receive the approval of tradable shareholders. Representativeness was associated with characteristics such as firm size and history (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, avoiding a concentration in the large provinces ("geographical balance"). Accordingly, 17 of the 31 Chinese provinces had firms selected into the pilot. Finally, the government had a preference for firms in competitive industries, because concentrated industries were often associated with national interests or monopolies.

1.3 The conversion process

Share conversions involved nontradable 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

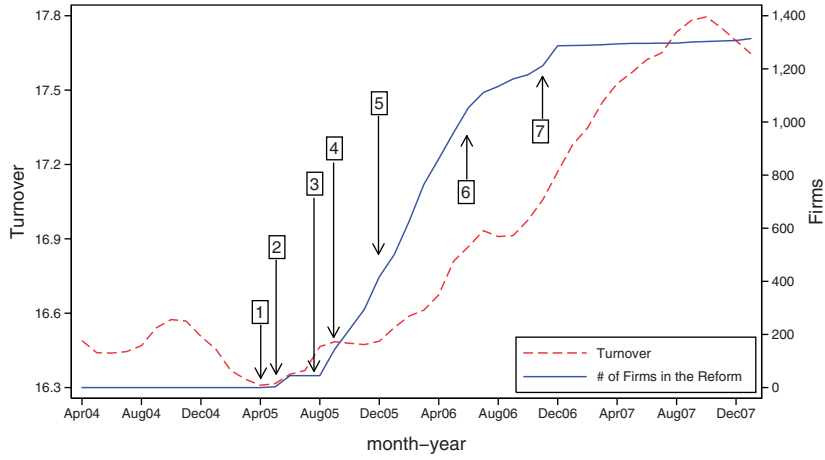

Figure 2
Time line of events, number of reformed firms, and aggregate market liquidity.
The right side vertical axis measures the number of firms joining the reform. The left side vertical axis measures the market liquidity (turnover). We compute turnover as a 12-month moving average of the ratio of number of shares traded on the Shanghai Stock Exchange over the total number of tradable shares outstanding. The vertical axis captures the calendar time from April 2004 to December 2007. The major events of the reform window are as follows:

1. April 24, 2005: The CSRC issues "Circular on Issues Relating to Pilot Program of Split Share Reform in Listed Companies."
2. May 9, 2005: The CSRC announces first batch of four pilot firms. The second batch of 42 firms is announced on June 20, 2005.
3. August 19, 2005: Pilot program of the Split Share Reform is completed.
4. September 4: The CSRC announces "Administrative Measures on the Split Share Structure Reform in Listed Firms," marking the official start of the reform for the rest of the A-share firms.
5. December 31, 2005: There were 434 firms in the reform, accounting for 37% of the total market capitalization.
6. June 30, 2006: There were 1,054 firms in the reform, accounting for 88% of the total market capitalization.
7. December 31, 2006: There were 1,287 firms in the reform, accounting for 98% of the total market capitalization.

investors. Payments to tradable shareholders were made following a vote. Afterwards, a lockup period applied under which nontradable shareholders could not immediately sell all of their shares at once.[3]

Reaching agreements on conversions was notoriously difficult (see Firth, Lin, and Zhou 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 nontradable shareholders. More often than not, there were disagreements between (and within) the two share classes about the conversion process. From an identification standpoint, the

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

upshot of this feature is the noise that is added to firms' 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 receiving 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, 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 added to the CSRC's "approval lists." These lists limited the number of converting firms to about 20 per week (down to eight per week later in the reform). Although various institutional elements of the reform made it difficult for firms to "optimally time" conversions, we explicitly tackle potential sources of endogeneity (e.g., self-selection) in our tests.

1.4 Share conversions and aggregate stock market liquidity

Our working hypothesis is that conversions increased liquidity in secondary markets. We provide general evidence in support of this in Figure 2, where we plot 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. Figure 2 suggests that stock turnover in the Chinese equity markets moved in tandem with firms' adherence to the share reform program.

1.5 Potential effects of the reform

We hypothesize that stocks would become more liquid after conversion into tradable status. Accordingly, we consider increases in stock liquidity as a primary outcome 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. Access to primary equity markets—IPOs and SEOs—also might be facilitated when investors are able to easily resell their stocks. 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 after the reform. Enhanced liquidity brought about by the reform could also lower the cost of equity and broaden the pool of feasible investments. Focusing on real side effects of the reform, our analysis considers measures of firm investment, profitability, productivity, and value as outcome variables. We also look at employment. 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. This makes it hard to predict the effect of the reform on employment.

Our tests also consider firms' financial policies. Historically, owners of nontradable shares could only benefit from their holdings via dividends. 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 less emphasis on dividends—which were more heavily taxed—as a way to reward investors.[4] 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.

One could also conjecture that the ability to trade shares may boost the market for corporate mergers. Newly converted shares even could be used as a currency to acquire other firms. Accordingly, a final outcome we consider is the probability that firms engage in mergers after converting their shares.

In China, conflicts of interests between minority (tradable) and majority (nontradable) shareholders are known to be associated with mismanagement and fraud. These problems became acute in recent years, with shareholder expropriation conducted primarily by way of "related-party transactions" and "intercompany loans" (Deng, Gan, and Jia 2008). We measure the incidence of these fraud-laden transactions in firms converting their shares to see if market liquidity has an impact on these activities. To the extent that market prices might more quickly respond to illicit activities by corporate controllers after stocks become liquid, we would expect a decline in those activities following 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.

2. Data

2.1 Data sources and sampling

Our raw dataset comprises all A-share companies listed in the Chinese public exchanges at the end of 2004. Our tests exclude companies that were ineligible for stock conversion according to the CSRC's reform

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

principles. That is, they exclude companies with B-/H-shares, companies assigned to ST/PT status,[5] and companies with previous fraud-related court cases as indicated by the CSRC. 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. 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.

2.2 Variable construction

We consider an extensive list of real and financial outcomes in our analysis (all variables are listed in the Appendix). 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 ($NetIncome$) 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, because 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 (Amihud and Mendelson 1988; Amihud, Mendelson, and Lauterback 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. 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 ($Dividend$).

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

We consider a number of additional outcomes to characterize the effects 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*). 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 top managers (*ManagerShares*) and whether firms replace their CEOs (*CEOTurnover*). We use the Herfindahl index of the top five 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 "intercompany loans" (*InterLoans*). Finally, we measure the impact of the reform on firms' propensity to initiate merger and acquisition deals (*M&A*).

Besides the outcomes described above, we use a comprehensive set of controls in our matching procedures. Firstly, we account for the proportion of nontradable shares (*NonTradable*) across firms. Regarding ownership structure, we account for 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 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 are the price-earnings ratio (*P / E*) and the market-to-book asset ratio (*q*). The relevance of firms in their industry and region are proxied by the ratio of firm sales over industry sales (*IndRep*) and the ratio of firm sales over provincial GDP (*ProvRep*). We also include information about 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 industry concentration (*IndConcent*). Because the CSRC required firms to reduce their intercompany loans prior to the reform, we also use *InterLoans* as a matching covariate.

2.3 Descriptive statistics

Summary statistics for our sample firms in 2004 are presented in Table 1. Column 1 (full sample) indicates that firms had, on average, nine years of

Table 1
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
Δ*K*	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
Δ*L*	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
Δ*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
Δ*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
Δ*ROE*	−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
Δ*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
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
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
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 A1 for all 1,054 A-share firms listed in 2004. Pilot firms, in Column (1), are those that joined the reform in May–June 2005. Column (2) shows statistics for nonpilot firms that joined the reform before June 2006. Column (3) shows statistics for firms that joined the reform after June 2006. We also report the difference in sample average between pilot firms and firms that comply after June 2006, as well as between nonpilot firms that comply before June 2006 and firms that comply after June 2006. Standard errors are in the parentheses. Δ indicates the difference between December 2003 and December 2004.

operation under their current charter (recall most were privatized in the 1990s). Sixty-two percent of their shares were nontradable in 2004 and 37% of shares were owned by the state. Firms seemed to be profitable (average *ROE* of 4.5%) and have positive prospects (average $M \mid B$ of 2.1). These and other summary statistics are similar to those found in contemporary papers on Chinese firms (e.g., Li, Wang, Cheung, and Jiang 2011; Jiang, Charles, and Heng 2010). We omit their discussion for brevity.

Following the schedule of the reform process, we divide our sample into three groups: (1) the "pilot group" includes 43 nonfinancial firms in the May/June-2005 pilot program; (2) "complying before June 2006" comprises 821 nonpilot firms that converted their shares during 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 1 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, and 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.

3. Estimation Strategy and Methodology

Our goal is to compare outcomes that accrue to firms that join the split-share reform (at the time they join it) to the counterfactual situation of joining it at a different time. This section discusses our quasi-experimental strategy to estimate those effects.

3.1 Strategy

Even though all A-share firms were forced to change their trading status, they did not comply with the reform all at the same time. This is important for identifying causal effects in that, for each point in time, one can compare firms that have already joined the reform with firms that have not yet done so. One must take into account, however, that 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-differences model combined with a time-varying propensity score matching estimator to address these issues.

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"). Accordingly, the treatment assignment is defined according to the date at which the firm joins the reform, and the treatment spell is defined as the length between this date and the date at which the effect is assessed. To calculate the effect of the reform, we estimate a nonparametric dose-response function that maps treatment spells into potential outcomes. Then the difference between two points along this 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. In addition, we also match firms so that they present similar pre-treatment outcome dynamics regardless their compliance date. This matching is performed using a large set of covariates under a generalized propensity score (GPS). The role of the GPS is to identify and compare firms that did not join the reform at the same point in time, despite having similar odds of doing so.

3.2 Notation and the role of GPS

Let $d \in \mathcal{D} = \{1,2,\ldots,K\}$ indicate when the firm may join the reform. For any period $t \in \{0,\ldots,T\}$, each firm has a set of potential outcomes, Y_t^d, which depends on the compliance date, d, as presented in Table 2. 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. The shadowed area under the diagonal represents situations in which the firm has not yet joined the reform.

Given the full set of potential outcomes, the average treatment effect (ATE) is defined as the expected differences between two potential outcomes in the same period (Heckman and Vytlacil 2007):

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

To identify this parameter, we assume that, conditional on pre-treatment covariates, X_0, idiosyncratic changes in firms' potential outcomes,

Table 2
Time-varying potential outcomes

	t				
d	0	1	...	$T-1$	T
1	Y_0^1	Y_1^1	⋯	Y_{T-1}^1	Y_T^1
2	Y_0^2	Y_1^2		Y_{T-1}^2	Y_T^2
⋮	⋮	⋮	⋮	⋮	⋮
$T-1$	Y_0^{T-1}	Y_1^{T-1}	⋯	Y_{T-1}^{T-1}	Y_T^{T-1}
T	Y_0^T	Y_1^T		Y_{T-1}^T	Y_T^T
⋮	⋮	⋮	⋮	⋮	⋮
K	Y_0^K	Y_1^K	⋯	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.

$Y_t^d - Y_{t'}^d$, are independent from complying at date d, $\mathbf{1}(D = d)$, for all possible d (Heckman, Ichimura, and Todd 1997; Imbens 2000). This assumption allows us to use pre-reform characteristics and decisions to predict the part of outcome dynamics that is related to the compliance date.

Conditioning on a large number of covariates can be difficult in practice, especially in small samples. However, if both the "balancing property" and the conditional independence assumption are satisfied, then it suffices to adjust for a generalized propensity score (GPS) to identify treatment effects (Hirano and Imbens 2004; Imbens 2000; Imai and van Dyk 2004). The GPS, $R(X_0)$, is defined as the conditional probability of complying with the reform at the actual date, D:

$$R(X_0) \equiv r(D,X_0) = \Pr(D = d|X_0), \tag{2}$$

where $r(., X_0)$ is the GPS function, which maps each hypothetical date d into a probability value. The balancing condition for the GPS to replace a high dimensional X_0 is that each covariate is independent from the actual compliance date, D, conditional on the GPS function, $r(d, X_0)$ for all $d \in \mathcal{D}$.

In our setting, once the firm joins the reform and becomes "treated," it cannot reverse its decision and become "untreated." Hence, 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 function using Cox's proportional hazard model. In this model, for every set X_0 there exists a unique correspondence $\theta(X_0)$ such that $r(d, X_0) = r(d, \theta(X_0))$ for all $d \in \mathcal{D}$. Accordingly, all information in X_0 that is contained in the GPS function can be summarized by a unique value, $\theta(X_0)$, called GPS index.

To ease the balance of covariates, the GPS index is nonparametrically estimated using a restricted cubic spline in which knots are selected using backward elimination of weak predictors (Sauerbrei and Royston 2007).

3.3 The binary treatment effect estimator

To estimate the effect of the reform on pilot firms, we adopt a binary treatment framework. In this case, pilot firms are those that joined the reform by June 2005, whereas the control group comprises firms that complied with the reform in July 2006 or later. This threshold is set so as to allow for sensible outcome comparisons between treated and control units; that is, exposure to the reform is sufficiently different to produce measurable potential effects.[6] Formally, we estimate the following ATT parameter:

$$ATT: \gamma_{t,k,k'} = E\left[Y_t^{d\leq k} - Y_t^{d\geq k'} \middle| D \leq k\right]. \tag{3}$$

where k represents June 2005 (pilot phase) and k' represents July 2006.

This parameter is estimated using a difference-in-differences (DID) model with propensity score matching (PSM). From the estimated GPS function, we calculate the propensity score, $\hat{p}_{k,k'}$,:

$$\hat{p}_{k,k'}(X_0) = \frac{\widehat{\Pr}(d \leq k|X_0)}{\widehat{\Pr}(d \leq k|X_0) + \widehat{\Pr}(d \geq k'|X_0)} = \frac{1 - \hat{S}_0(k)^{\hat{\theta}}}{1 - \hat{S}_0(k)^{\hat{\theta}} + \hat{S}_0(k')^{\hat{\theta}}}, \tag{4}$$

where $\hat{S}_0(.)$ is the estimated survival function. The estimation is performed by matching the propensity score, $\hat{p}_{k,k'}(X_0)$, between the group of pilot firms and the group of control firms using a nearest neighbor algorithm (NNM).[7] Moreover, the sample of matched firms includes only those within the overlap region (Dehejia and Wahba 2002).

To calculate the ATT effect, we compute the first difference in outcomes over time, $\Delta_{t,t'} Y$, for each firm and the second difference between matched firms. The baseline period, t', is December 2004, well before the reform was publicly discussed. The impact assessment dates, t, are December 2005, December 2006, and December 2007. 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. 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.

[6] To check robustness, we experimented with different values for this threshold of control firms. Our results are similar even when we set the threshold to January 2006.

[7] We also estimate the ATT using kernel matching as a robustness check, and results are similar.

3.4 The multivalued treatment effect estimator

As the starting time, d, can assume many values, it is difficult to obtain an average estimate for each potential outcome (or each cell in Table 2). For this reason, Imbens (2000) and Hirano and Imbens (2004) consider what is called the dose-response function, $\mu_t(d) = E\left[Y_t^d\right]$ with $d \in \mathcal{D}$, namely, a continuous function that smooths the value of potential outcomes. The ATE parameter is defined as

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

And the dose-response function is estimated using the following fixed-effect model:

$$Y_{it} = \mu(Z_{it}) + \mu(Z_{it}) \cdot h\left(\hat{\theta}_i\right) + \varphi_t + \eta_i + \upsilon_{it}, \tag{6}$$

where $Z_{it} = \max(0,\ t - D_i)$ is the time of exposure to the reform, η_i is the firm-specific effect, φ_t is the time-specific effect, and υ_{it} is the error term (clustered at the firm level).

In this regression, $\mu(.)$ is set to be a restricted cubic spline function with five knots, $k_n = 6{,}12{,}18{,}24{,}30$. This allows one to identify nonlinear patterns in the dose-response function in a way that is less computationally intensive than alternative nonparametric methods. To account for the heterogeneity in firms' response, we let $\mu(.)$ change with respect to the GPS index, $\hat{\theta}_i$. The heterogeneity term, $h(.)$, is a mean-centered cubic spline function with four equally spaced knots.

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. Note that $R(X_0)$ is the conditional probability that the firm is assigned to its true treatment status. 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 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). To operationalize this approach, we weight firm observations by the inverse of their estimated GPS.[8]

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

$$C = \left\{ i : \hat{\theta}_i \in \left[\min_j\left(\hat{\theta}_j\right), \max_j\left(\hat{\theta}_j\right) \right], \text{ with } \left|D_i - D_j\right| \geq \varepsilon \right\}, \tag{7}$$

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

[8] The use of inverse probability weighting (IPW) is discussed by Imbens (2000) and Wooldridge (2007).

support, there are comparable firms with sufficiently distinct treatments. We let the width, ε, be equal to six months in our estimations.[9]

4. Results

4.1 Balancing property of the matching approach

To verify the balancing property of the propensity score, we estimate the average difference in pre-treatment covariates between treated and controls firms after matching. These differences are shown in Table 3. Notably, after matching, we find no significant differences between the two groups. This balance is obtained not only for the 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.

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.[10] Table 4 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; that is, 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.

4.2 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. To ease exposition, we focus these tests on a small set of outcomes: investment, employment, productivity, profitability, equity issuance, leverage, and dividends. The next section uses a time-varying, multivalued treatment approach to evaluate a wide range of outcomes.

Estimates for the effects of the reform on pilot firms are shown in Table 5. 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.

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

[10] For each $x_0 \in X_0$, one estimates $x_0 = b_0 + b_1 D + g\left(\hat{\theta}\right) + \xi$, where $g\left(\hat{\theta}\right)$ is a spline function. One then tests if $b_1 = 0$.

Table 3
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
Δ*K*	0.319	0.221	0.099	(0.121)	0.416
L	7.378	7.821	−0.443	(0.381)	0.245
Δ*L*	0.153	0.034	0.119	(0.168)	0.480
Sales / K	0.479	0.680	−0.201	(0.392)	0.608
Δ*Sales/K*	0.011	0.177	−0.166	(0.134)	0.215
NetIncome	0.165	0.104	0.062	(0.033)	0.063
Δ*NetIncome*	−0.011	0.012	−0.023	(0.036)	0.519
ROE	0.131	0.117	0.014	(0.020)	0.477
Δ*ROE*	0.011	0.015	−0.003	(0.019)	0.866
M / B	2.030	2.394	−0.364	(0.563)	0.519
Δ*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
Shares	19.58	19.54	0.048	(0.256)	0.850
StateControl	0.545	0.576	−0.030	(0.158)	0.848
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	−14.16	(18.11)	0.434
q	1.520	1.615	−0.095	(0.257)	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	−0.370	(0.429)	0.388
IndConcent	0.054	0.031	0.023	(0.017)	0.167
# of obs.	42	183			

This table shows the average difference in pre-reform covariates between pilot firms and their matched control firms. Pilot firms are those that joined the reform from May–June 2005. Control firms are those that joined the reform after June 2006. Robust standard errors of the differences are in the parentheses, and the *p*-value is reported in separate columns. Δ indicates the difference between December 2003 and December 2004.

Table 4
GPS balancing property test

	W/O controls	*p*-value	W/ controls	*p*-value
Real outcomes				
K	−0.017	0.077	0.005	0.760
Δ*K*	−0.010	0.000	0.001	0.866
L	−0.019	0.045	0.014	0.258
Δ*L*	−0.007	0.011	−0.001	0.781
Sales / *K*	−0.030	0.002	−0.005	0.693
Δ*Sales*/*K*	−0.005	0.229	−0.003	0.414
NetIncome	−0.016	0.000	0.001	0.838
Δ*NetIncome*	−0.012	0.011	0.000	0.869
ROE	−0.013	0.000	0.000	0.819
Δ*ROE*	−0.006	0.000	0.000	0.697
M / *B*	0.020	0.121	0.011	0.366
Δ*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
Shares	−0.007	0.217	0.002	0.803
StateControl	0.008	0.040	0.004	0.455
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
# of obs.	984		961	

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 robust *p*-values are reported in separate columns. Δ indicates the difference between December 2003 and December 2004.

Table 5
ATT difference-in-differences 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)***
# of obs.	232		232		216	

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 an ordinary least squares (OLS) regression without any control variables. The estimates in Column 2 are from an 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 significance at the 1%, 5%, and 10% levels, respectively.

Table 5 also reports 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. About six months into the reform (end of 2005), we also find significantly positive 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 the pilot firms. This is a notable increase when compared with the average issuance probability of only 3% for pilot firms prior to the program (see Table 3). 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.

Some results become weaker when we use 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 pre-reform average asset growth 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. 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 with nonpilot firms that joined the program early in the reform process (between September and December 2005). The pool of counterfactual firms is similar to that used in the tests of Table 5, that is, 190 firms that joined the reform in the second half of 2006. This gives more testing power (300 nonpilot 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 in Table 6. In general, even though "near pilot" firms were slightly less exposed to the reform, they observe similar growth effects on investment, employment, and equity issuance.

4.3 Time-varying effects of the reform

This section presents our paper's central results. We describe the impact of the reform across time by graphing estimated effects on a time line.

Table 6
ATT difference-in-differences estimates for near pilot firms

	OLS w/o controls		OLS w/ controls		NNM	
2005						
ΔK	0.159	(0.031)***	0.129	(0.042)***	0.042	(0.064)
ΔL	0.164	(0.034)***	0.137	(0.045)***	0.099	(0.060)*
Sales / K	0.183	(0.090)**	0.045	(0.053)	0.028	(0.069)
NetIncome	0.134	(0.068)**	0.091	(0.031)***	0.056	(0.023)**
ROE	0.118	(0.022)***	0.111	(0.020)***	0.063	(0.032)**
M / B	0.181	(0.253)	−0.041	(0.141)	−0.108	(0.243)
Issuance	−0.012	(0.007)*	−0.016	(0.013)	−0.032	(0.018)*
Leverage	−0.061	(0.014)***	−0.058	(0.013)***	−0.004	(0.016)
Dividend	0.018	(0.045)	0.037	(0.047)	0.032	(0.052)
Z	1.430	(0.070)***	1.208	(0.082)***	1.376	(0.072)***
2006						
ΔK	0.415	(0.077)***	0.286	(0.063)***	0.470	(0.234)**
ΔL	0.296	(0.053)***	0.245	(0.053)***	0.303	(0.098)***
Sales / K	0.056	(0.089)	0.023	(0.071)	−0.341	(0.234)
NetIncome	0.048	(0.071)	0.120	(0.068)*	0.044	(0.097)
ROE	−0.001	(0.025)	0.023	(0.023)	0.020	(0.027)
M / B	0.461	(0.281)	0.183	(0.225)	0.087	(0.321)
Issuance	0.083	(0.017)***	0.085	(0.019)***	0.090	(0.019)***
Leverage	−0.061	(0.020)***	−0.077	(0.020)***	0.013	(0.036)
Dividend	0.017	(0.046)	0.057	(0.051)	−0.059	(0.060)
Z	11.330	(0.165)***	10.954	(0.183)***	10.696	(0.356)***
2007						
ΔK	0.666	(0.100)***	0.481	(0.101)***	0.609	(0.229)***
ΔL	0.491	(0.077)***	0.376	(0.081)***	0.318	(0.104)***
Sales / K	0.008	(0.111)	−0.059	(0.093)	−0.291	(0.145)**
NetIncome	−0.154	(0.055)***	−0.129	(0.041)***	−0.070	(0.066)
ROE	−0.113	(0.023)***	−0.076	(0.018)***	−0.033	(0.030)
M / B	−1.262	(0.458)***	−1.427	(0.438)***	−1.554	(1.019)
Issuance	0.172	(0.027)***	0.175	(0.034)***	0.172	(0.048)***
Leverage	−0.048	(0.021)**	−0.060	(0.020)***	0.008	(0.030)
Dividend	−0.080	(0.045)*	−0.019	(0.050)	−0.022	(0.069)
Z	12.798	(0.366)***	12.345	(0.382)***	11.305	(1.094)***
# of obs.	483		483		462	

This table shows the average treatment effect (ATT) estimates for nonpilot firms that joined the reform from September to December 2005. To study the changes brought about by the reform, we consider changes in outcome variables from the end of 2004 (prior to the reform) to (1) the end of 2005 (top panel), (2) the end of 2006 (middle panel), and (3) the end of 2007 (bottom panel). The estimates in Column 1 are from an ordinary least squares (OLS) regression without any control variables. The estimates in Column 2 are from an 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 significance at the 1%, 5%, and 10% levels, respectively.

These estimates are computed from our GPS model (Equation (6)). To ease exposition, we report and discuss separately the outcomes that are related to real firm 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 by evaluating stock liquidity.

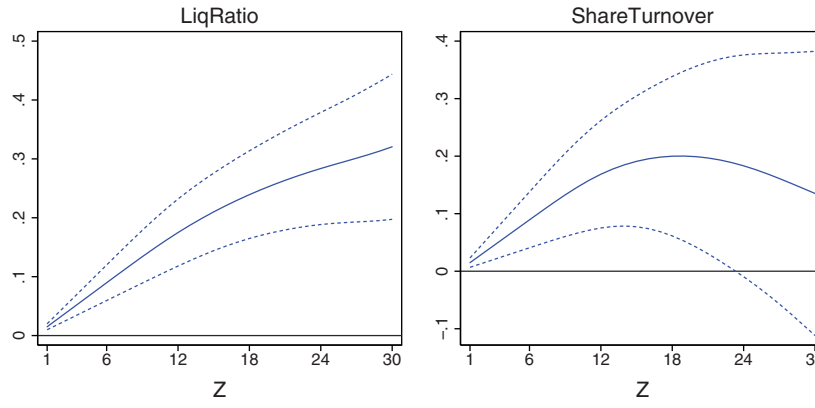

Figure 3
Time-varying treatment effect on liquidity, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on the liquidity measures *LiqRatio* and *ShareTurnover*. For each of the variables, the dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). All regressions are performed using annual data with 7,293 observations and estimated using a linear model. 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% clustered confidence interval for the estimates.

4.3.1 Effects on liquidity. Figure 3 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-visthe counterfactual case of not complying with the reform, $\mu(0)$. Figure 3 shows that stock liquidity increases immediately after a firm converts its shares. For the liquidity ratio measure (*LiqRatio*), there is an immediate and persistent positive conversion effect. Thirty months after the reform, that ratio increases 30% above the baseline case of nonconversion. 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 because of share conversion. Figure 3 confirms our base prior that corporate shares become significantly more liquid after converting into tradable status.

4.3.2 Effects on real outcome. Figure 4 presents time-varying effects of share conversions on each of the real performance measures examined in our pilot-based tests: ΔK, ΔL, $Sales \mid K$, $NetIncome$,ROE, and $M \mid B$.

The first panel of Figure 4 suggests that corporate investment, ΔK, responds markedly well to share conversions. By the 24th 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

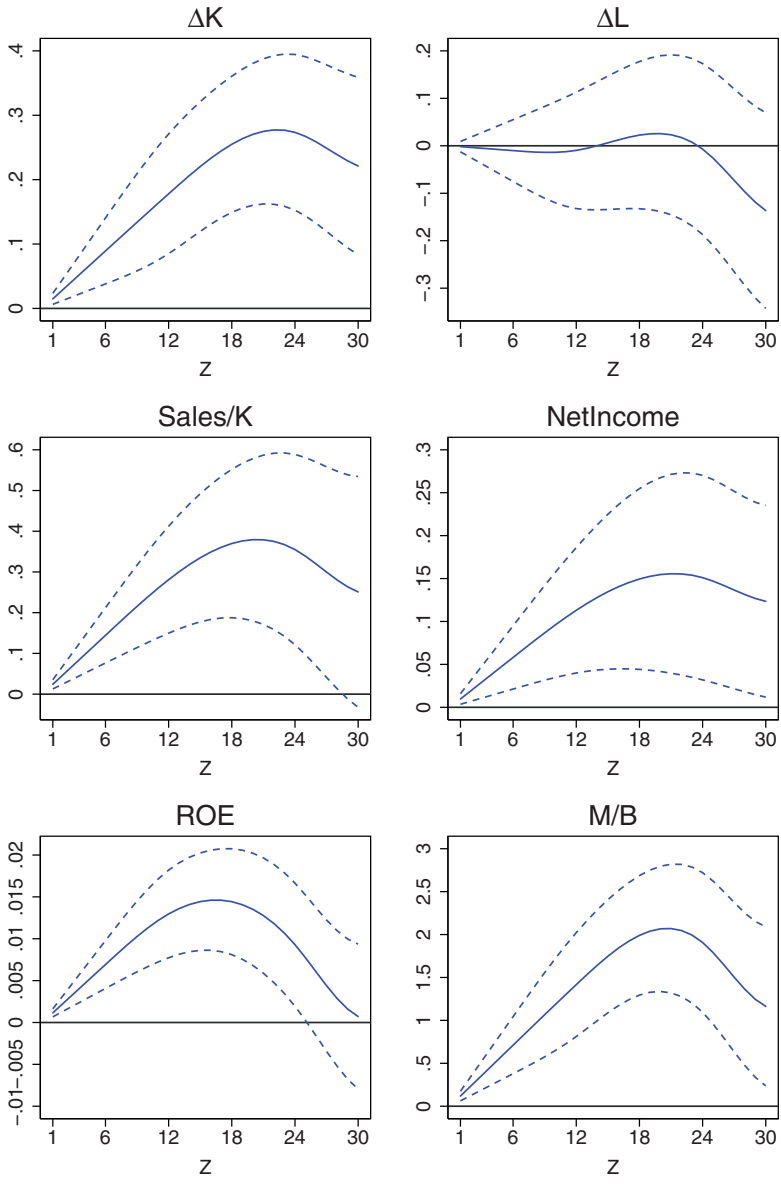

Figure 4
Time-varying treatment effect on real outcomes, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on each of the business performance measures: ΔK, ΔL, *Sales*/*K*, *NetIncome*, *ROE*, and *M* / *B*, where Δ indicates the difference between the 12-month forward value and the current value. The regressions for ΔK, *NetIncome*, *Sales* / *K*, *M* / *B*, and *ROE* are estimated using quarterly data, with 25,760 observations, whereas the regression for ΔL is estimated using annual data, with 6,486 observations. For each of the variables, the dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). 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% clustered confidence interval for the estimates.

increase in the number of employees. In particular, the second panel of Figure 4 shows that labor growth, ΔL, remains flat for complying firms over at least two years. 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 are also implied by the third panel of Figure 4, where we plot the effect of conversions on sales-to-capital ratios, *Sales* / *K*. That effect is immediate and increasing until 20 months after compliance, when *Sales* / *K* becomes almost 40% higher than the counterfactual case. In the longer run, this ratio is about 20% higher due to conversion. Improvements in corporate efficiency following share conversions into tradable status are consistent with the priors discussed in Section 3.

Gains in productivity are consistent with profitability increases that we also observe in Figure 4. In particular, the dose-response function of *NetIncome* increases until the 20th month of conversion. By then, operating revenues grow 15% above expenses. In the long run, the reform leads to an increase of 10% in *NetIncome*. Likewise, *ROE* increases until the 18th month following conversion, when it is about 1.5 percentage points higher than the counterfactual case (this figure represents 33% of the pre-reform average *ROE*). After that point, however, *ROE* declines. Whereas the initial growth is consistent with firms expanding and performing better, the subsequent decline is explained by the higher proportion of firms issuing equity—the scaler of *ROE*—after conversion, as we discuss below.

As the last panel of Figure 4 shows, stock conversions lead to significant increases in corporate valuation. In particular, market-to-book, *M* / *B*, increases for about 20 months after conversion, when it nearly doubles with respect to the sample average of 2.1. After 30 months, *M* / *B* is 1.1 higher than in the nonconversion case. Arguably, equity valuation is the ultimate summary statistic of corporate wealth. Our results suggest that stock conversions were markedly beneficial to equity holders in China.

Our findings 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 distorted firm policies and hurt growth. The effects of the split-share reform point to sizeable gains to Chinese firms and their shareholders, revealing to the importance of secondary stock markets for the real economy.

4.3.3 Effects on financial policies. Figure 5 shows the estimated time-varying reform effects on equity issuance (*Issuance*), capital structure

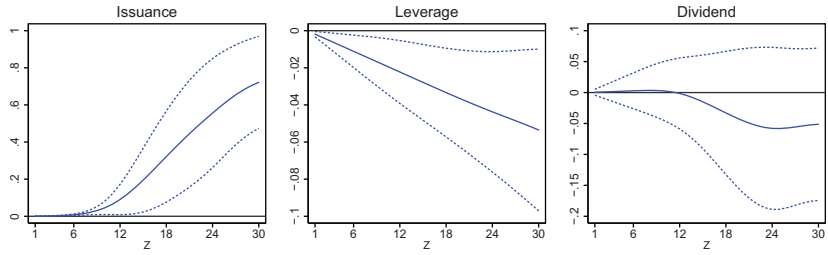

Figure 5
Time-varying treatment effect on financial outcomes, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on financial measures: *Dividend, Leverage*, and *Issuance*. For each of the variables, the dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). All regressions are performed using annual data with 7,688 observations. 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 counterfactual case of not joining the reform, $\mu(0)$. Dashed lines represent the 90% clustered confidence interval for the estimates.

(*Leverage*), and dividend payout (*Dividend*). 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.[11] In particular, the first panel of Figure 5 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 nonconversion baseline. Looking at the aggregate impact of this policy change, we note that only 1% of the listed firms issued equity in 2004, whereas in 2007 this figure was 13%.

The increase in equity issuance is associated with a drop in corporate leverage. In particular, the second panel of Figure 5 shows that firms reduce their debt-to-asset ratios by 4 percentage points 24 months after their stocks become tradable (compare with the sample average of 48%). Finally, the last panel of Figure 5 suggests that the reform is responsible for a small reduction in dividend payments. Noting the large error bands associated with tests using financial policy variables, 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%.

[11] 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.

4.4 Characterizing the liquidity channel

There are several, nonexclusive ways by which greater stock liquidity may drive the effects depicted in Figures 4 and 5. Increased liquidity in secondary market transactions might, for example, help 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.

4.4.1 Price informativeness. A potential explanation for our findings is that greater liquidity allows for better, more-informed decisions by managers. If a price discovery channel is at work, we would expect stock prices to become more informative about firm fundamentals after stocks start to trade. Following previous literature (e.g., Morck, Yeung, and Yu 2000), we use stock price synchronicity as a proxy for informativeness. When the information environment surrounding a firm improves and more firm-relevant information is incorporated in its price discovery process, market factors should explain a bigger proportion of the observed variation in stock returns. If increased liquidity improves the information environment surrounding firms' stocks, we should see price synchronicity to be positively associated with reform compliance.

Following Gul, Kim, and Qui (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.[12] As the first panel of Figure 6 suggests, firms' stock prices become more synchronous with the market up to the 24th month following conversion, when it is at least 60% higher than the case of nonconversion. 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 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.

4.4.2 Merger activity. A deeper, more liquid equity market should facilitate corporate control transactions, which are often made possible

[12] The market return is based on the composite A-share index of the Shanghai and Shenzhen exchanges. Given the bounded nature of R^2, we follow prior literature and use a logistic transformation: $PriceInfo = \log(R^2/(1 - R^2))$.

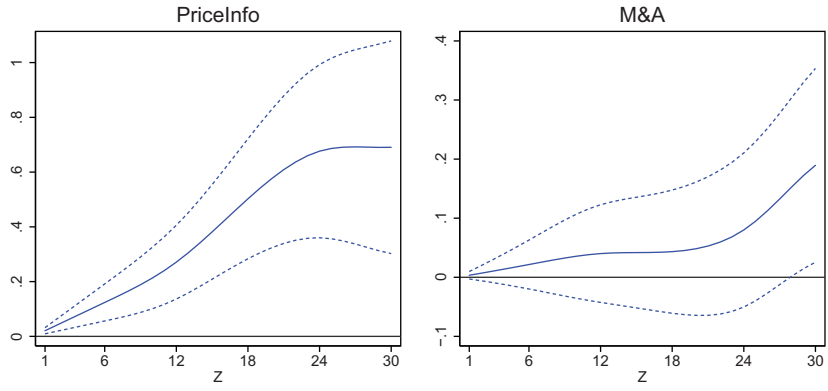

Figure 6
Time-varying treatment effect on price informativeness and merger activity, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on *PriceInfo* and *M&A*. The dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). *PriceInfo* is estimated using a linear model with 7,399 annual observations. *M&A* is estimated using a Probit model with 7,688 observations. 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% clustered confidence interval for the estimates.

through the use of shares as a means of exchange.[13] Results shown in the second panel of Figure 6 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 deals. By the 30th month after conversion, the probability of having a M&A deal per year is 20 percentage points higher than in the case of nonconversion. In aggregate, this effect represents an increase of 60% in the number of firms involved in M&A deals per year.

4.4.3 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 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 became more relevant in the evaluation of CEO performance and tenure. At the same time, one has to bear in mind the context in which our tests are conducted. Chinese CEOs are

[13] We are able to collect information on payment methods for nearly half of the M&As in our sample. The percentage of deals using cash-only payments dropped sharply from 96% for the period before the reform, to 59% after the reform.

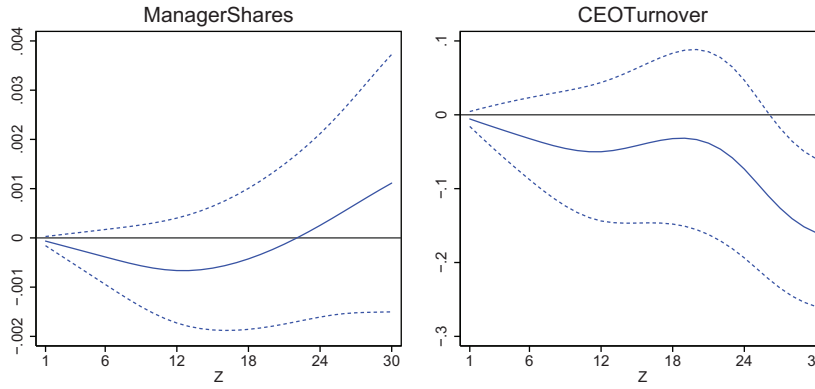

Figure 7
Time-varying treatment effect on managerial incentives, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on *ManagerShares* and *CEOTurnover*. For each of the variables, the dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). All regressions are performed using annual data. Dose-response function for *CEOTurnover* is estimated using a Probit model with 7,688 observations. Dose-response functions for *ManagerShares* is estimated using a linear model with 7,357 observations. 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% clustered confidence interval for the estimates.

often politically connected to the central government apparatus and their employment terms may be isolated from their firms' outcomes.

Results in Figure 7 do not point to 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 tests do not reveal a significant link between changes in managerial incentives and reform outcomes.

4.4.4 Conflicts of interests and fraudulent activities. By abolishing distinctions between tradable and nontradable shares, the reform could ameliorate conflicts between majority and minority shareholders. In this context, the elimination of "share classes" relates to a different notion of liquidity, and we examine if the reform has 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, Yeung, and Yu 2000; Claessens, Djankov, Fan, and Lang 2002). The first panel of Figure 8 shows that ownership concentration among the top five shareholders (*OwnerConcent*) drops substantially after firms converted their stocks. By the 30th month into the program, *OwnerConcent* is about 10 percentage points lower than in the counterfactual case of nonconversion, which represents a reduction of 43% of the average concentration index.

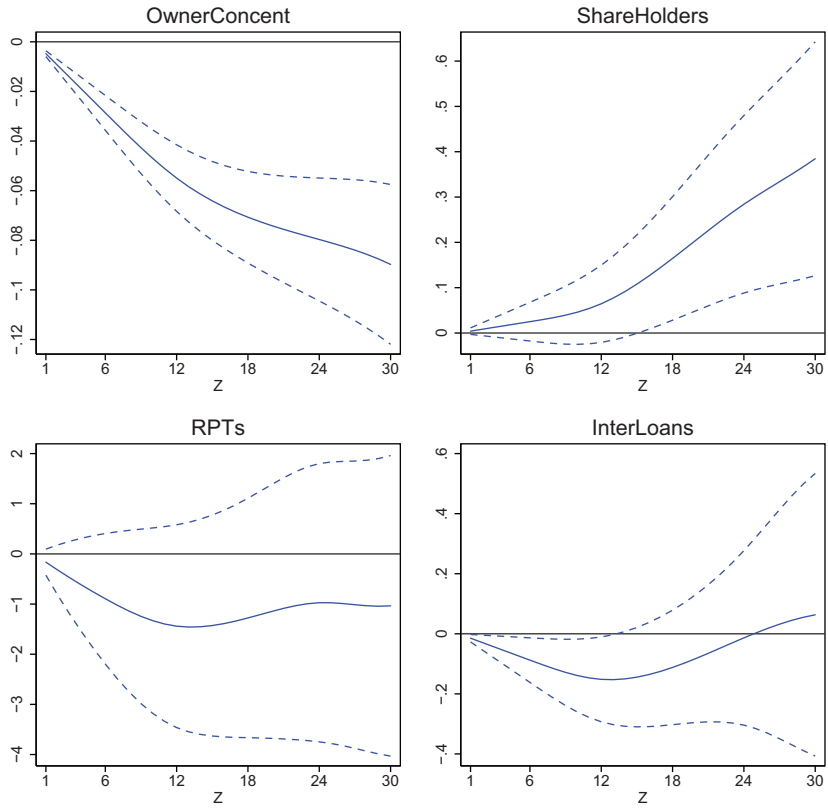

Figure 8
Time-varying treatment effect on ownership and agency problems, $\mu(Z) - \mu(0)$.
This figure presents time-varying reform effects on *OwnerConcent, ShareHolders, RPTs*, and *InterLoans*. For each of the variables, the dose-response function is estimated using inverse probability weighing (IPW) and regression adjustment based on Equation (6). All regressions are performed using annual data. Dose-response function for *RPTs* is estimated using a Poisson model with 7,688 observations. Dose-response functions for *OwnerConcent, ShareHolders*, and *InterLoans* are estimated using a linear model with 7,457 observations. 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% clustered confidence interval for the estimates.

We note that the effect of the reform on concentration could be mechanical. Owners of tradable shares were usually compensated with extra shares. As a result, the fraction of the firm owned by majority (nontradable) shareholders would naturally decline after conversion. The reform could appear to dilute stock ownership of top shareholders and yet not necessarily imply that there was greater (new) individual investor participation in ownership. We gather data on the number of individual shareholders and show in the second panel of Figure 8 that the number of shareholders increases following conversion. Thirty months after conversion, the number of firm shareholders is 40% higher. Not only there are

more trades on the firm's stock but there are also more investors participating in those trades.

Conflicts of interests between majority and minority shareholders are known to be associated with corporate mismanagement and even fraud. In China, these problems became acute in recent years and we follow existing literature in constructing proxies for these issues. Following Cheung, Rau, and Stouraitis (2006) and Deng, Gan, and Jia (2008), we identify suspicious "related-party transactions" in annual reports and count the number of such transactions (*RPTs*) for each firm in our sample.[14] In addition, following Jiang, Charles, and Heng (2010), we collected information on "intercompany loans" (*InterLoans*), which are notorious mechanisms via which controlling shareholders siphon resources from listed firms.

To test whether the reduction in agency costs is a plausible channel for our results, we examine the effect of the reform on related-party transactions and intercompany loans. Our prior is that converting shares into a tradable status makes managers more accountable for their actions and discourage them from engaging in dealings that are detrimental to holders of public stocks. We find only weak support for this hypothesis. The last two panels of Figure 8 suggest that *RPTs* declines slightly, following conversions. *InterLoans* show a decline in the short run, but this decline does not persist in the long run.

4.5 Treatment heterogeneity

Confirming our priors, we found 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. Whereas our results are consistent with this interpretation, one would like to see that mechanism more fully characterized. One way to verify our claims is to check whether firms that had the most to gain from the conversion program did indeed observe the largest gains. 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 show the largest responses to the conversion program.

We operationalize our treatment heterogeneity tests by entering interaction terms in our model. In particular, in a first examination, we interact a firm's pre-reform liquidity level (*ShareTurnover*) and the months since it joined the share reform (Z). This term captures the product between a firm's potential to gain from the treatment (the degree to which

[14] Deng, Gan, and Jia (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.

Table 7
Heterogeneity of the marginal effect on real outcomes

	Lagged		Lagged		Lagged		Lagged	
	ShareTurnover		*OwnerConcent*		*ManagerShares*		*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)
# of obs.	5,407		5,520		5,451		5,527	

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 intercompany 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 (6). Clustered standard errors are in parentheses, and p-values are reported in separate columns.

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;" that is, we expect a negative interaction 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 captures ex ante managerial incentives. Finally, we interact Z with the pre-reform amount of intercompany loans (*InterLoans*), which captures pre-existing agency issues.

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

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 agency problems 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 by 1 percentage point. In the third column, we see that for firms for which 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 intercompany 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 in ways that are consistent with our priors.

The evidence of this section suggests that firms with the highest potential gains from the split-share reform indeed benefitted the most from it. These heterogeneous effects are consistent with our argument that the lifting of trading restrictions had positive welfare implications for the Chinese economy.

5. 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 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 also might be endogenous because of 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. 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.

Our results shed unique insights 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 has 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.

Appendix

Table A1
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 three managers
CEOTurnover	Dummy for CEO turnover in the last 12 months
OwnerConcent	Herfindahl index of top five shareholder ownership
ShareHolders	Log of number of shareholders
RPTs	Number of related party transactions in the last 12 months
InterLoans	Log of intercompany loans
Control variables	
NonTradable	Proportion of nontradable shares
Shares	Log of total shares
StateControl	Dummy for firms ultimately controlled by the state
StateShares	Proportion of shares owned by the state

(*continued*)

Table A1
Continued

Variable	Description
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.

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