Effects of Credit Expansions on Stock Market Booms and Busts *

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Abstract

Household credit expansions often coincide with high stock-market valuations, but identifying a causal relationship remains challenging. Because unconstrained arbitrageurs play a pivotal role in stock-pricing, the impact of credit is not obvious (despite evidence in less-liquid housing markets). Further, given margin restrictions, credit may leak into stock prices in difficult-to-measure ways. We address these issues using an unprecedented 2010-2015 regulatory expansion of margin lending in China. Institutional fund trades and regression discontinuity evidence suggest a positive impact on prices that was largely anticipated by unconstrained investors. We develop a dynamic panel model of stock prices and recover large causal estimates.

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1 Introduction

Financial liberalizations and household credit expansions are often associated with asset market booms and financial crises (e.g., Kaminsky & Reinhart, 1999; Borio & Lowe, 2002; Schularick & Taylor, 2012). Overheated stock markets play a prominent role in many recent episodes, including the Japanese asset price bubble of the late 1980s and the 1997 Southeast Asian financial crisis. Despite this, well-identified approaches to estimating the causal effect of credit expansions on asset prices have typically focused on housing markets, particularly in the run-up to the Great Recession of 2008.

An important reason for this gap in the literature on stock markets is given by Allen & Gale (1999) in their review of work on bubbles, crises and policy:

> *The relationship between credit and asset prices is relatively straightforward in real estate markets. An expansion of credit reduces the interest rate at which investors can borrow and this in turn increases the prices they are willing to pay. In stock markets, the relationship is more subtle. Margin restrictions imply that only a proportion of the total investment can be financed with borrowed funds. However, if credit expands, investors may be willing to borrow a greater amount against the houses, cars and other assets they buy, and put more into ... [the stock market].*

Indeed, cleanly identifying a link between loosening credit and stock market movements is challenging. Leverage restrictions may blunt straightforward pass-through, but easy credit can leak into asset prices in unexpected and hard to measure ways.[1] Further, despite convincing evidence that credit expansions increase housing prices,[2] the impact in stock markets is not obvious ex-ante. Deep pocketed investors—e.g. hedge funds or other relatively unconstrained actors—play a more pivotal role in price setting in stock markets relative to real estate (Stein, 1995). Their trades may pre-empt or offset the impact of household credit. Unfortunately, the holdings and strategies of such agents are typically difficult to observe.

We address these challenges by examining a major Chinese credit liberalization—the deregula-

[1]Many commentators attribute the stock market collapse during the Great Recession of 2008 to loose credit during the housing boom making its way into stock markets even as regulations kept margin lending in check. For example, Bill Gross, the manager of the world's biggest bond fund at the time, pointed to deleveraging as the primary reason for the crash (*Reuters Newswire* reported on October 24, 2008).

[2]There is a large literature on the direct effects of credit in housing, e.g. Favara & Imbs (2015); Di Maggio & Kermani (2017); Mian & Sufi (2009); Dell'Ariccia & Marquez (2006); Keys *et al.* (2012); Adelino *et al.* (2016); Albanesi *et al.* (2017); Kaplan *et al.* (2020).

tion of margin lending—that occurred between 2010-2015. The liberalization fed a wave of margin debt, which rose from virtually nothing to a peak of roughly 4.5% of total market capitalization and nearly 10% for many individual stocks.[3] A boom in stock prices followed: the Shanghai Composite index rose from about 2000 in mid-2014 to a high of 5166 in June, 2015.

The Chinese episode provides an ideal setting in which to analyze the impacts of credit on stock prices for several reasons. The first is the scale and targeted nature of the deregulation. The liberalization narrowly focused on stock market leverage, and was not part of a larger shift in credit supply. This stands in contrast to more general credit market expansions, which may influence stock prices in ways that are difficult to identify. Further, the quantity of margin lending dwarfs that used in earlier, well-identified studies on margin restrictions (e.g. Foucault *et al.*, 2011; Kahraman & Tookes, 2017), which analyzed relatively small increases in margin debt, and focused on stock price volatility but not the level of stock prices.[4] The second is the availability of data on the institutional investors in the Chinese market that are likely to speculate on credit expansions, which allows us to account for their role in the boom and bust cycle.

Our paper has two contributions. The first is to provide direct evidence on the impacts of credit expansions on stock prices, and on the associated dynamics generated by deep-pocketed investors. We find that the Chinese deregulation increased prices on average, but that these effects were anticipated and priced in by institutional investors. The second is a simple method—based on a dynamic competitive stock-pricing model—to recover and quantify the average impacts of an expansion in the presence of such anticipation. Given the pervasive nature of anticipation in equity markets, this approach may be valuable in more general contexts outside of our study.

We begin by exploiting the unique implementation of the deregulation to establish a series of reduced form facts on the impacts of margin debt on stock prices. Beginning in early 2010, Chinese regulators began to gradually introduce margin debt for different sets of stocks—which we call vintages—over the course of several years. For each vintage, qualification was determined on the basis of a published formula which incorporated real-time data on market capitalization

[3]While the deregulation technically also allowed shorting, there was negligible short interest during this period since the securities lending market was not well developed. See Appendix Figure A.I for more detail.

[4]There is a related literature on changed in Fed margin requirements. Excessive margin lending was widely blamed for the bubble that preceded the 1929 crash (Galbraith, 1961), and between 1934 and 1974 margin requirements ranged from 45% to 100%. Since 1974 they have been 50%. As noted in the review by Fortune (2001), a large fraction of these studies find mixed results (e.g. Eckardt & Rogoff, 1976; Hsieh & Miller, 1990; Hardouvelis & Peristiani, 1992; Jylhä, 2018).

and trading volume. This formula was publicly available before margin debt was introduced for the vintages we study.

Using standard event-study approaches, we find no evidence of an increase in stock prices, on average, after margin lending was introduced. However, across vintages, we find strong evidence that prices rose gradually but consistently for soon-to-be marginable stocks in the months leading up to deregulation. As the criteria used to determine inclusion was public, the set of to-be marginable stocks was at least partially predictable in advance (although the reliance on real-time data meant that meaningful uncertainty existed until the formal announcement). This suggests that the pre-trends we observe are consistent with a large causal effect of margin lending, albeit one that was anticipated and priced in by deep-pocketed investors.

Indeed, we next show direct evidence that the deregulation increased stock prices using a regression discontinuity approach that is plausibly untainted by anticipation. Focusing on the formula that determined eligibility, we compare stocks that barely qualified for margin lending to those that just failed to qualify. High frequency variation in the inputs to this formula generated ex-ante uncertainty about the specific set of eligible stocks in a neighborhood around the qualifying threshold for each vintage. Because slight movements in market capitalization or turnover might cause one stock to qualify or another to be disqualified, investors could not perfectly predict the set of stocks in each vintage. We find that stocks just above the threshold saw a sharp influx of margin debt in the months following the liberalization. This, in turn, corresponded to a non-trivial increase in asset prices. Our estimates suggest that 3-month cumulative returns were roughly 18 percent higher relative to stocks that just failed to quality. Of course, as with all regression discontinuity approaches, these results are based on a relatively small set of stocks close to the qualifying threshold.

Institutional holdings data further confirms our conjecture that large direct-effects were present but priced-in by relatively unconstrained investors. We show that institutional buyers (e.g., Chinese mutual funds) anticipated and profited from the roll out. These buyers overweighted stocks that they could predict were likely to become marginable (based on contemporaneously available information). Our tests, implemented using panel regressions with stock, mutual fund, and time fixed effects, show that funds increased the weight of stocks with a high impending likelihood of marginability by roughly 5% as the next vintage drew near. We then conduct a test of excess risk-adjusted performance (i.e. an alpha test) to show that institutions indeed profited from this strategy. For a typical fund portfolio, stocks with a high likelihood of inclusion outperformed stocks with a low likelihood by 3 to 3.5% over a six-month holding period.

In light of these reduced form findings, we turn to our second contribution, a simple method for estimating and quantifying the causal effects of credit expansions in the presence of anticipation (which, crucially, is not limited to a narrow subset of stocks around the marginability cut-offs). Given the staggered roll-out of margin lending, a difference-in-difference design would, at first glance, be a natural fit, and similar approaches have been used successfully in a real estate context (see Favara & Imbs, 2015; Di Maggio & Kermani, 2017). However, the pre-trends generated by anticipatory buying rule out a straightforward parallel trends assumption. Indeed, the challenges of anticipation for difference-in-difference or event based designs have been noted at least since Ashenfelter (1978). While more recent work (e.g. Freyaldenhoven *et al.*, 2019) has developed econometric models that incorporate pre-trends in estimation, these methods cannot be easily applied to stock market prices, which are forward looking and capture time-varying risk premia.

We propose a framework for asset prices derived from a competitive equilibrium model of stock trading—in the vein of Summers (1986) and De Long *et al.* (1990)—in which deep-pocketed investors receive signals about the size of a coming credit expansion and its impact on household demand for stocks. We show that our model can be easily estimated using a simple linear dynamic panel regressions with a forward looking instrumental variables strategy following Anderson & Hsiao (1981), Arellano & Bond (1991), and Malani & Reif (2015). The exclusion restriction is analogous to a relatively standard difference-in-difference assumption, that the timing of deregulation is orthogonal to vintage-specific demand shocks for stocks.

Using our model, we estimate sizable impacts of the margin deregulation on stock prices—a causal direct effect of around 18%—in line with our regression discontinuity estimates. Furthermore, a key advantage our model is that we are able to estimate underlying parameters governing the *rate* of anticipation. We find that the impacts of margin lending were anticipated gradually as information became available, but were largely incorporated in advance of the introduction date. Even six months before the actual event, more than 60 percent of the direct effect had already been impounded into stock prices.

A natural context for understanding the large causal effect we estimate comes from recent work on demand-based asset pricing and the elasticity of demand for stocks. Considering the ex-post influx of margin debt as a demand shock, we can retrieve a back-of-the-envelope estimate of the demand elasticity by combining our estimated causal effect on stock prices (around 18%) with the quantity of margin lending (around 3% of market cap for the stocks in question, within 3 months). This translates to an elasticity of roughly -0.17, indicating that demand for stocks in the Chinese context we study is more inelastic than what typical findings in the literature on stock-market micro-elasticities (typically based on the US market) would suggest. The order of magnitude of these estimates is generally around -1, whether using an indexing design as in Kaul *et al.* (2000); Greenwood (2005); Chang *et al.* (2014), a demand systems approach (Koijen & Yogo, 2019) or other methods.[5] The difference is perhaps unsurprising, given substantial frictions in the Chinese market, and the aggregate flow into stocks generated by the deregulation.[6]

These elasticities enable us, as a final step, to take a first pass at evaluating the role of credit in the Chinese stock market boom as a whole (which reached its most intense period from mid-2014 to early-2015). While our causal estimates are primarily based on the period prior to the peak, we perform a simple extrapolation using the level of margin debt in 2015, which stood at roughly 8% of market cap for the set of stocks we study. Given our elasticity, this indicates that the credit expansion was responsible for a run up of approximately 47%. This is close to a quarter of the aggregate increase of 185% that was observed between June 2014 and June 2015 in the three vintages we study. We expect this is a lower bound for two reasons. First, because we likely underestimate the total quantity of margin debt, due to the rise of shadow margin during the boom (Bian *et al.*, 2017), and second, because the relevant macro-elasticity to capture the aggregate consequences of a large inflow of margin debt is likely even smaller than our regression discontinuity and panel based estimates (Gabaix & Koijen, 2021). However, we leave a more systematic analysis to future research. Regardless, our results—and these back-of-the-envelope calculations—suggest that credit had a large causal impact on stock prices, and hence was a meaningful driver of the Chinese stock market boom.

[5]See Gabaix & Koijen (2021) for a discussion of the range of micro-elasticities.

[6]Note also that the stock-level elasticities estimated in Haddad *et al.* (2021) are closer in magnitude to those that we find.

2 Background and Data

2.1 The staggered deregulation of margin lending

Between 2010 and 2015, Chinese regulators gradually began to allow margin lending for certain stocks listed on the Shanghai and Shenzhen Stock Exchanges. The deregulation occurred in two overall phases. In the first phase, which we refer to as the pilot, regulators allowed stocks belonging to major market indexes to be purchased on margin. In the second phase, regulators progressively expanded margin lending, selecting stocks on the basis of a published formula that incorporated market capitalization and share turnover. Because our empirical strategies utilize the details of this formula, we focus our analysis on the second phase.

Throughout both phases, retail investors with at least 500,000 RMB of assets in their brokerage account and six months or more of trading experience qualified for margin—provided by their brokerage firms—with an initial margin requirement of 50%. Interest rates on margin loans from brokerage firms were generally around 8 to 9% annualized, significantly lower than the rates on shadow margin loans through informal channels (which typically ranged from 11 to 14%).[7]

The first phase began on February 13th, 2010, when the 90 stocks included in the two major stock indexes— the Shanghai 50 Index(50 stocks) and the Shenzhen Component index (40 stocks)—were opened to margin lending. We refer to this as *Pilot A*. On November 25th, 2011, the Chinese government extended the list of marginable stocks based on membership in two broader market indices. The extended list included 278 stocks: 180 from the Shanghai 180 Index and 98 from the Shenzhen 100 Index. We refer to this extension as *Pilot B*.

The plan for a second phase, the focus of our analysis, was announced in late 2011. Official regulations were released explicitly stating that the list of marginable stocks would be extended in a staggered manner in a series of waves, which we call *Vintages*.[8] To determine the set of qualifying stocks for each vintage, the regulatory agency published a screening-and-ranking rule. This procedure had three steps: (i) screening out stocks that did not satisfy a set criteria intended to disqualify particularly small, volatile, illiquid, and newly listed stocks—the so called Article 24 for Shanghai and Rule 3.2 for Shenzhen;[9] (ii) ranking the remaining stocks according to the formula

[7]See Bian *et al.* (2017) for more details.

[8]See Article 28 in the rule released by the Shanghai Stock Exchange.

[9]The criteria for both exchanges are the same: they require that stocks: (1) have been traded for more than three

shown in Equation 1 below; and (iii) selecting the top candidates in each exchange (with some discretion).[10]

$$\text{Inclusion Index}_i = 2 * \frac{\text{Average Tradable Market Value of Stock } i}{\text{Average Tradable Market Value of All Stocks in SH/SZ}} + \frac{\text{Average Trading Volume in yuan of Stock } i}{\text{Average Trading Volume in yuan of All Stocks in SH/SZ}}. \tag{1}$$

This ranking rule, effectively a value weighted average of a stock's size and trading volume, was conducted separately in the Shanghai and Shenzhen Stock Exchanges. Margin lending was ultimately expanded to three vintages using this procedure.

Table 1 summarizes the timeline of deregulation and the number of newly marginable stocks for each extension. The specific set of stocks included in *Vintage 1* was announced January 25th, 2013, and margin lending for these stocks was implemented on January 31st, 2013. Similarly, *Vintage 2* was announced on September 6th, 2013 and implemented on September 16th. *Vintage 3* was announced on September 12th, 2014, and implemented on September 22nd, 2014. For the purposes of our analysis, which is at the monthly level, there is no distinction between announcement and implementation dates. By the time Vintage 3 was implemented, roughly 900 stocks in total could be bought on margin across the two exchanges.

Figure 1 shows the key focus of our study: the massive expansion of margin debt following this liberalization. The blue line displays margin debt as a fraction of total market capitalization in our sample (described in detail below). Over the course of the liberalization, margin debt rose from a negligible amount to roughly 4.5% of market cap. The black line shows the level of total market capitalization in our sample, which mirrored the influx of margin debt and spiked in mid-2015. Figure 2 plots the rise of margin debt relative to market capitalization separately for each of the three vintages we analyze, with announcement dates denoted by vertical lines. For each

months; (2) have either more than 100 million tradable shares or a market value of tradable shares over 500 million; (3) have more than 4,000 shareholders; (4) have not experienced any of the following in the previous three months: (a) daily turnover less than 20% of the turnover rate of the market index; (b) the average of the absolute value price changes more than 4% off of the market index; (c) market volatility higher than the market volatility by 500%; (5) have completed the share reform; (6) are not specially treated stocks; and (7) other conditions (the official documentation does not specify what these other conditions refer to). See rules on stock trading with margin loans on each stock exchange's website.

[10] Roughly 100 to 150 stocks were newly included in each vintage for each exchange, although the actual number varied slightly, often because certain formerly marginable stocks became non-marginable due to the screening rule and had to be replaced.

vintage, the quantity of margin debt reached 3-5% of market capitalization within a few months and ultimately peaked between 8 and 10%.

It is worth mentioning that the stocks that qualified for margin buying were technically also eligible for short selling. However, as Appendix Figure A.I shows, a negligible amount of shorting took place, with aggregate short interest peaking at 0.02% of market cap in 2013. Given that the aggregate value of shorted shares was orders of magnitude below the quantity of margin debt, we abstract from the effect of shorting in our analysis.

2.2 China's Mutual Fund Industry

China started to develop its mutual fund industry in 1998. Unlike the case in other developed markets, mutual funds in China own a much smaller fraction of the stock market and are known for their active and speculative trading style. Between 2010 and 2015, mutual funds in total held 4–5% of the Chinese stock market, compared to about 30% in the U.S. (Jiang, 2020). Furthermore, Chinese mutual funds charge high fees; the average expense ratio for actively managed mutual funds was 1.2% in 2017 (versus about 0.8% in the U.S.). Jiang (2020) also finds that despite the high fees, Chinese mutual funds' net returns do not underperform the market index, suggesting an ability to generate superior returns. In this sense, mutual funds in China are similar to hedge funds in other markets. On the other hand, the regulatory reporting framework for mutual funds in China is similar to that in other markets. For example, mutual funds are required to disclose their full equity holdings on a semi-annual frequency.[11]

2.3 Data

We use stock price, trading, and financial information from CSMAR, excluding stocks on the Growth Enterprise Board (GEB) and stocks newly listed. Stock level margin debt outstanding is available at a daily frequency from the Shanghai and Shenzhen Stock Exchanges. We focus primarily on the period between March 2009 (roughly a year before Pilot A) and May 2015. Mutual fund data are also from CSMAR and include funds' complete stock holdings on a semi-annual basis.

[11] See Jiang (2020) for more detail on China's mutual fund industry.

3 Reduced Form Evidence on the Reaction of Stock Prices

In this section we provide reduced form evidence that the liberalization of margin lending increased stock prices, but that this increase was largely anticipated and priced in by deep-pocketed investors. We begin with two standard strategies. First, event studies show no indication that prices increased for treated stocks at or after the point of deregulation. However, we do find that prices rose consistently for these stocks in the months preceding deregulation. Second, regression discontinuity evidence, which is plausibly uncontaminated by anticipation, indicates the presence of a large causal effect on stock prices. We then present evidence based in the holdings of relatively unconstrained investors that reconciles these two approaches. Specifically, we show that institutional investors shifted their portfolios toward soon-to-be-marginable stocks—as if they were anticipating a causal effect on prices—and that these investors profited from this strategy.

3.1 Event Studies Comparing Marginable to Non-marginable Stocks.

We begin with simple event studies that compare marginable and non-marginable stocks before versus after deregulation to test whether there is an observable impact of the introduction of margin debt itself on stock prices. We construct our estimates as follows. For each of Vintages 1, 2 and 3, we consider the cross section of all stocks that are either (i) included in the corresponding vintage or (ii) never marginable.[12] We pool these together and consider cumulative returns in the period immediately following the official announcement/implementation of margin debt.[13] Specifically, we consider regressions of the form:

$$Ret_i^k = \beta_0 \text{Marginable}_i^k + \theta_k + \varepsilon_i^k. \tag{2}$$

Here Ret_i^k is the cumulative market-adjusted return in the 1, 3 or 12 month window following the announcement/implementation month for vintage k. Marginable$_i^k$ is an indicator equal to one if stock i becomes marginable in vintage k. θ_k is an indicator equal to one if the observation is included in the cross-section corresponding to vintage k, and captures the average return for non-

[12] We also consider robustness tests that compare the set of stocks in the corresponding vintage to all stocks that are not marginable at the time margin debt was introduced, including those that became marginable in later vintages, with similar results. See Appendix Table A.I.

[13] In our monthly data, there is no distinction between announcement and implementation.

marginable stocks in the relevant window. Our coefficient of interest is then β_0, which captures the deviation in cumulative returns from the average for non-marginable stocks. We cluster our standard errors at the stock level.

The first three columns of Table 2, labelled "Following Marginability," show that the relative returns for newly marginable stocks were virtually zero in the period immediately following marginability. We see no significant differential return through 1, 3, or 12 months following marginability. These results are highly inconsistent with an unexpected direct effect of margin debt on the level of asset prices, which would generate positive returns. The fact that these coefficients are effectively zero suggests that for the average stock, either (i) any direct effect of margin debt was already priced in by the time of the deregulation or (ii) there was no average impact of margin debt on asset prices.

Columns 4-6 of Table 2, labelled "Preceding Marginability," provide evidence for the former explanation, showing that the prices of soon-to-be eligible stocks rose in advance of the margin lending deregulation. In these specifications, we repeat the analysis shown in Equation 2 but consider cumulative market-adjusted returns in the 1, 3 or 12 months prior to the announcement/implementation. We see strong evidence of *positive* returns in the period preceding marginability. We estimate significant differential market-adjusted returns of roughly 1.8% in the month just prior to implementation, of 5.6 percent in the 3 months preceding implementation, and of over 20% in the year before implementation. Furthermore, these returns did not dissipate following implementation. Column 7 shows that cumulative returns from 12 months prior to 12 months after the announcement/implementation month were approximately 20%. Similar results hold when considering each vintage independently, suggesting that these patterns are not driven by outsized returns for a single vintage.

Placebo Tests: One potential concern is that our event study findings of positive returns in the months preceding marginability might be in part mechanical, driven by the ranking procedure used to select marginable stocks (i.e., Equation 1). To address this possibility, we use the same ranking formula to construct and implement a series of placebo tests and confirm that these results are not the mechanical consequence of the ranking criteria. Our basic approach is to randomly select placebo event dates 10,000 times, re-implement the ranking procedure for all placebo dates, and re-estimate our event studies. We then compare our estimated coefficients to the distribution of placebo coefficients and construct p-values. As a summary, we show one sided placebo p-values in square brackets in Table 2. Our results suggest that neither our ex-ante nor ex-post effects are mechanically driven. We provide more detail on our placebo tests in Appendix B.

Taken together, our event studies provide suggestive evidence that the introduction of margin debt had a sizable impact on the level of asset prices, but that these impacts were largely anticipated and priced in. Of course, these patterns are not conclusive. It is feasible that the ex-ante positive returns were driven by some factor unrelated to margin debt itself, and that the lack of impact at the moment of deregulation reflects a minimal influence of margin lending on equity prices. To rule out this possibility, we now turn to a regression discontinuity approach that plausibly identifies direct impacts of the deregulation while accounting for anticipation.

3.2 Regression Discontinuity Estimates

Our regression discontinuity approach focuses on the the set of stocks close to the cut-off in the formula used to determine which stocks would qualify for margin lending (we refer to the output of this formula, which is shown in Equation 1, as the inclusion index). Only a fixed number of stocks could be included in each vintage, meaning a discontinuity existed at the value of the index held by the lowest ranking eligible stock. In principle, stocks above this value qualified for margin debt while stocks below did not. Furthermore, because the formula is based on real time inputs that vary at high frequency, and because both the date at which the stocks in each vintage were to be chosen and the precise number of stocks included in each vintage was unknown ex-ante, investors could not perfectly predict the set of stocks included in each vintage. As a result, the introduction of margin debt can be viewed as an un-anticipatable shock to credit for stocks in a small neighborhood around the cut-off. This allows us to isolate the effect of the deregulation from local cross-sectional comparisons.

Defining the inclusion index and marginability threshold. For each stock i, we denote the value of the inclusion index for Vintage k as $Index_i^k$, where $k = \{1, 2, 3\}$. We construct this following the formula in Equation 1. We next define C_E^k to be the cut-off for vintage k in exchange E. For details on the definition and implementation of each vintage, see Appendix C.1.

No evidence of sorting around the threshold. There is little evidence that investors or insiders were able to manipulate the rankings of particular stocks locally around the threshold C_E^k. While the basic inputs into the index could certainly have been influenced to some extent, uncertainty over the exact number of stocks included in each vintage made precise control around the threshold effectively impossible. To show this formally, we conduct manipulation tests (in the tradition of McCrary, 2008) following Cattaneo *et al.* (2018). We present the results in Figure 3. We find no evidence of bunching around the threshold: the magnitude of the t-statistic is just below 0.5. Further, Appendix Figure A.II shows that there are no discontinuities in ex-ante observable characteristics of stocks above vs. below the threshold.[14]

A discontinuity in marginability and margin debt at the threshold. We begin by showing that the threshold C_E^k is indeed associated with a discontinuity in marginability and the quantity of margin debt. This is displayed most clearly in Panel (a) of Figure 4, which shows sharp jumps in the probability a stock is marginable, the quantity of margin debt, and the ratio of margin debt to market capitalization, for stocks just above versus just below the threshold.[15]

To show this jump more formally, we take a standard regression discontinuity approach. That is, letting Y_i^k be the outcome of interest, we estimate:

$$Y_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + \varepsilon_i^k. \tag{3}$$

Here τ_i^k indicates that stock i is above the marginability threshold, that is, it is equal one if $Index_i^k \geq C_E^k$ and 0 otherwise. θ_k represents a vintage fixed effect. Our coefficient of interest is α_{0r}, representing the discrete change in the probability of marginability at the threshold. In our baseline specification, shown above, we include separate linear slopes on each side of the threshold (local

[14] Appendix Table A.II shows formal tests of covariate smoothness, based on the specification in Equation 3.

[15] In theory, this discontinuity should be sharp and exactly equal to the value of the index for the lowest-ranking included stock (for each vintage and exchange). In practice, the discontinuity is slightly less sharp for two reasons. First, there is some uncertainty over our ability to precisely replicate the procedure used by regulators both because the window we use to collect data on market capitalization may not be perfectly aligned and because of minor ambiguities in the screening procedures used to rule out certain stocks. Second, and more importantly, there was some room at the margin for discretion on the part of the exchanges, with little in the way of published detail. Our definition of C_E^k is designed to keep this discretion from biasing our estimates. See Appendix C.1 for more detail.

linear regressions with a rectangular kernel). For robustness, we also show results that use local linear regressions with a triangular kernel throughout. We include all not-yet-marginable stocks that satisfy the screening rule, and use the covariate adjusted MSE optimal bandwidths described in Calonico *et al.* (2018).[16] We show standard errors based upon the three nearest neighbor variance estimators described in Calonico *et al.* (2014) (CCT).

Our results based upon these specifications, shown in Panel A of Table 3, are consistent with the figures discussed above. We see a jump in the probability a stock becomes marginable of roughly 50% and a differential influx of roughly 13 million yuan within three months, or alternatively, roughly 1.7 percent of market cap. All estimates are statistically significant and together show that our threshold indeed corresponds a differential (and plausibly unpredictable) influx of margin debt in a local area around the threshold. There was a discontinuous increase in the probability of marginability and the use of margin debt for stocks just above the threshold.

Price reactions at the threshold. We next consider the impact of the deregulation on asset prices. We examine whether stocks just above the threshold saw higher cumulative returns in the month, 3 months or 12 months following the announcement and implementation of each vintage. Panel B of Figure 4 shows plots similar to those in Panel A of Figure 4. The inclusion index is displayed on the x-axis (normalized to set the threshold to 0). Cumulative raw returns are shown the y-axis. These plots introduce the basic results we flesh out more formally below. In the first month, returns for stocks just above the threshold are slightly higher than returns for those below the threshold. Further, a large and statistically significant difference is evident for 3 month returns and persists through 12 month returns.

In Panel B of Table 3 we show estimates of these reduced-form effects using a regression discontinuity approach analogous to the one outlined in Equation 3. For these specifications, the outcome of interest Y_{ik} refers to the raw cumulative return for stock i in the 1, 3 or 12 months following the announcement of Vintage k.[17] Our results, presented in Panel B of Table 3, align with the plots

[16] Robustness checks showing alternative bandwidths (we include both Imbens and Kalyanaraman and a fixed bandwidth of 0.5) are shown in Appendix Table A.III. Point estimates are similar in magnitude and standard errors are generally smaller.

[17] We consider raw cumulative returns, although results are similar for market-adjusted returns. For our baseline specifications, we choose bandwidths and estimate standard errors exactly as in Panel A. In Appendix Table A.III we show a series of robustness exercises with varying bandwidths.

shown in Figure 4. In the first column, we see a small and only marginally significant impact of 2.7 percent on one month returns. However, by 3 months, we see highly statistically significant returns of just over 10 percent, and by 12 months we see returns of 9.5 percent.

Fuzzy Regression Discontinuity to Recover the Direct Effect. To quantify the direct effect of becoming marginable, our final step is to implement a fuzzy regression discontinuity approach that accounts for the fact that the threshold does not perfectly predict marginability. Put simply, we report two-stage least squares regressions in which we instrument for marginability with an indicator for being above the threshold, and examine the impact on 1, 3 or 12 month cumulative returns. For more details, see Appendix C.2.

Our results are reported in Panel C of Table 3. Unsurprisingly, the qualitative patterns are in line with those presented in Panel B of Table 3, with smaller returns at 1 month and sizeable and significant returns at 3 and 12 months. We estimate raw cumulative returns of 17-18 percent at 3 months and 25-28 percent by 12 months after the deregulation. Across the board, results are similar when using local linear regressions with a triangular kernel (the last three columns of Table 3), or when considering alternative bandwidths (Appendix Table A.III).

These results indicate the presence of a sizable direct effect of the margin deregulation on asset prices. Across various specifications, we estimate that eligibility for margin lending generated 12 month cumulative returns (a direct effect) of more than 20 percent. While large, this quantity must be considered in light of the sharp increase in the market overall during our sample period, and of the large quantities of margin debt that flowed into these stocks. Further, these results stand in sharp contrast to our event study findings of no impact on average at the deregulation date. Taken together, our evidence suggests that the introduction of margin debt increases equity prices, but that these effects were largely priced in by the time the deregulation occurred. However, this anticipation requires relatively unconstrained—and deep pocketed—investors shifting their portfolios toward soon to be eligible stocks in advance of the deregulation. We next show that this was the case.

3.3 Anticipation by Institutional Investors

In this subsection, we show that the stock price patterns noted above are likely due to anticipatory purchases by relatively unconstrained investors such as Chinese mutual funds (who take strategies that are comparable to hedge funds in many other contries, see Section 2.2). As discussed in Section 2, following Pilots A and B, the government announced to the market that there would be a gradual expansion of margin lending over time to smaller and less liquid stocks. While the exact deregulation dates and sets of stocks to be included were unknown beforehand, we show here that funds could nonetheless profit from a simple trading strategy based on ex-ante public information: how highly ranked each stock was according to contemporaneous values of the inputs to the inclusion index (a measure of the likelihood that stock would be included in the next vintage) and the time elapsed since the most recent vintage (a measure of the likelihood that the next vintage would be announced shortly).

We proceed in two steps. We first show that mutual funds adjusted their portfolios towards this strategy in anticipation of the deregulation. We then show that this was a profitable choice. There were meaningful risk-adjusted returns to buying likely-to-be marginable stocks in advance, even if the precise set of stocks could not be perfectly predicted. Since such anticipatory buying naturally has price impact, it is likely that the rise in prices prior to the deregulation for the average soon-to-be-marginable stock is a reflection of these anticipatory purchases (and similar purchases by other relatively unconstrained investors). As a result, ex-post price effects existed only to the extent these anticipatory purchases could not precisely predict eligibility, e.g., for the subsample of stocks around the threshold used in our regression discontinuity estimates.

Changes in Portfolio Weights. We first examine whether mutual funds increased their investment in likely-to-be marginable stocks as the roll-out of the next vintage approached. Once again, we focus on anticipation of vintages 1-3. Before these vintages (and after the two pilots), it was clear to market participants that Chinese regulators would continue the deregulation process and gradually extend the list of marginable stocks: the screening and ranking procedure and the formula for the inclusion index (i.e., Equation 1) had been announced.[18] While the precise set of

[18]To be precise, the screening procedure was released with Pilot A, and Equation 1 was formally released to the public in January 2013 when Vintage 1 was announced. However, professional investors could likely learn of the formula through various channels before formal publication. Our results are the similar, if not stronger, when we only focus on

marginable stocks depended on the real-time values of the inputs to this formula around the date of deregulation (which was unknown beforehand), the inclusion index could still be used to make ex-ante predictions about which stocks would be included.

All mutual funds in China are required to report their complete stock holdings at the end of June and December each year. Our test uses these semi-annual portfolio disclosures from December 2011 (right after Pilot B was announced) to June 2014 (right before Vintage 3 was rolled out). Our sample covers all (1121) mutual funds who report equity holdings. On average, these funds hold 63.0 stocks in their portfolios (with median of 36). The average weight of a stock in a fund's portfolio is 0.91%. The total market value of stocks held by mutual funds in our sample is about 1 trillion yuan, which is less than 5% of total market capitalization over this period (see Figure 1).

To capture the likelihood that the next vintage was soon-to-be announced, we calculate the number of months between fund reporting and the introduction of the previous vintage, which we denote as $MonthsSinceLastVintage$. For example, for fund holdings reported in June 2014, our variable $MonthsSinceLastVintage$ equals 9 as vintage 2 rolled out in September 2013. Over our sample, $MonthsSinceLastVintage$ ranges from 1 to 13 months. Because the deregulation was expected to roll-out in a relatively standard fashion, a greater time elapsed since the last vintage indicated that the next vintage was soon-to-be announced. Therefore, we expect mutual funds to increase the weight of soon-to-be marginable stocks in their portfolios as $MonthsSinceLastVintage$ increases.

To gauge an individual stock's likelihood of becoming marginable in the next vintage, we calculate the inclusion index ($Index_t$) based on Equation 1 for all not-yet-marginable stocks using the information available for each stock (market cap and trading volume) at time t. This avoids any look-forward bias that might result from using information not available to the funds at the time of investment. We define soon-to-be marginable stocks as the top ranking stocks according to this formula at time t, regardless of whether they ultimately became marginable. Specifically, we define soon-to-be marginable stocks given information at time t as those ranked either in the top 20% of stocks or the top 150, depending on the specification.

To test our hypothesis, we follow the specification in Cohen *et al.* (2008). For each fund j in reporting month t, we calculate stock i's portfolio weight, $w_{i,j,t}$ as the fund's dollar-value investment

anticipation of Vintages 2 and 3.

in stock i scaled by fund total net assets. We conduct the following regression at the semi-annual fund-stock level,

$$w_{i,j,t} = \alpha + \beta Top\_Quintile_{i,t} \times MonthsSinceLastVintage_t + Controls_{i,t} + \sigma_i + \theta_j + \gamma_t + \epsilon_{i,j,t} \quad (4)$$

where $Top\_Quintile_{i,t}$ equals one if stock i is currently non-marginable and ranked in the top 20% based on $Index_{i,t}$ among all non-marginable stocks at t, and zero otherwise. Our coefficient of interest is β, which captures the tilt of portfolios toward soon-to-be marginable stocks. Controls represent the percentile ranking of stock characteristics, including market capitalization, book-to-market, turnover rate, past month return, and past year return, and the interactions between $MonthsSinceLastVintage$ and each of the stock characteristics. The interaction terms allow the effect of $MonthsSinceLastVintage$ to vary with different stock characteristics; for example, as the inclusion index (Equation 1) is based on stock size and volume, one might be concerned that our findings are driven by large and high turnover stocks. We also include fund, stock, and time fixed effects. Standard errors are clustered by fund.

Table 4 shows our results. In column (1), the coefficient on the interaction term between $Top\_Quintile$ and $MonthsSinceLastVintage$ is 0.0034 (with t-stat of 3.2). The point estimate of the intercept equals 0.982, which implies that the average conditional portfolio weight of a stock is 0.98%. In column (2), we use the dummy variable, $Top150$ in place of $Top\_Quintile$, which equals one if the stock in question is ranked within the top 150 within its exchange based on $Index_{i,t}$. The coefficient on the interaction term is 0.0027 (with t-stat of 2.5). In terms of economic magnitude, for example, at the 10th month since the last vintage rolling out, mutual funds tend to overweight the top 150 (based on inclusion index) stocks by 0.037% per stock; that is, overall mutual funds tilt their investment towards likely-to-be marginable stocks by about 11.10% ($150 \times 0.037\% \times 2$ exchanges), which is quite sizeable. In column (6), we consider an alternative specification that replaces the binary $Top150$ or $Top\_Quintile$ with a continuous measure of the stock's ranking—the log of one plus $Index_{i,t}$, with already marginable stocks assigned a value of zero. The coefficient on the interaction term remains significantly positive. Together, these results suggest that mutual funds meaningfully adjusted their portfolios in anticipation of the deregulation.

Performance Tests. We next examine whether mutual funds' soon-to-be marginable holdings earned higher returns compared with the funds' other holdings. We follow the method of Cohen *et al.* (2008), who test if funds' connected holdings generate alpha compared to the fund's other positions. Specifically, at the end of each June or December, stocks in each mutual fund's portfolio are assigned to one of two portfolios: "to-be-marginable" (TBM) and Non-TBM (NTBM). TBM companies are defined as stocks that are ranked within the top quintile or top 150 (following Table 4) within the relevant exchange based on the inclusion index at time t. Again, this distinction is based only on ex-ante available information. We then compute the monthly returns on TBM and NTBM holdings over the next six months, as well as the returns on a long TBM and short NTBM strategy. The six month window is based on the assumption that funds did not change their holdings between semi-annual reports. We rebalance porfolios every six months and, within a given fund portfolio, stocks are value weighted by the fund's dollar holdings. Finally, we calculate calendar time portfolio returns by averaging across funds. We use raw returns, market-adjusted returns, and CAPM alpha. CAPM alpha is the intercept on a regression of monthly portfolio excess returns on the market factor. The sample period is from January 2012 to September 2014. Newey-West standard errors are used with a lag of 11 months.

Table 5 presents the results. Over our period, on average, mutual funds allocated 12.98% of AUM into those $Top\_Quintile$ stocks (or 12.41% defining TBM as Top150). Mutual funds' TBM holdings based on $Top\_Quintile$ earned 1.65% per month. By comparison, the average return of the NTBM holdings was 1.09%. The portfolio that is long TBM and short NTBM generates a sizable abnormal return 0.56% per month which is statistically significant (with a t-statistic of 2.1). We see similar results when using market adjusted returns (the raw return minus the value-weighted market return). The TBM portfolio generates a significantly positive return of 0.83% per month (with t-stat of 2.6), whereas the return of NTBM portfolio is not significantly different from zero. The return on the long-short portfolio remains positive and significant. The next column uses CAPM alpha, and the next row reports the results using Top150 to define TBM stocks. Overall, our main finding is robust: TBM portfolios exhibit significantly positive abnormal returns of about 0.85% per month and earn higher returns than mutual funds' other holdings (NTBM). The results suggest that mutual funds earned significant profits by anticipating the demand of constrained investors that was released by the deregulation.

Robustness Checks. In the Appendix we show a series of alternative specifications that further support our findings. We first show, in Appendix Table A.IV, that the proportion of shares of soon-to-be marginable stocks held by mutual funds and other institutional owners (relative to all floating shares) increased in the period just prior to deregulation. We then show, in Appendix Table A.V that the most highly ranked stocks *within* each vintage—that is, the set of stocks that investors could have most easily predicted would become marginable—experienced a differentially strong anticipatory increase in prices before deregulation.

The role of deep-pocketed investors reconciles the most basic event-study results—which provides no evidence of a price effect—with the large positive effects from our regression discontinuity approach. However, the local effect from our regression discontinuity may not reflect the average effect for all stocks, and it is natural to consider the concordance between these two effects. In the next section, we develop a model to recover the causal effect for a broader set of stocks (away from the threshold) in the presence of anticipation.

4 A Dynamic Panel Model of Stock Prices

The previous section shows evidence that the margin lending deregulation increased stock prices, and that relatively unconstrained investors anticipated and priced in the impacts of this liberalization. In this section, we introduce a parsimonious competitive stock pricing model that allows us to better understand price dynamics in markets anticipating liberalizations. Under a set of reasonable assumptions, this model translates into an easy-to-estimate linear dynamic panel model, enabling us to recover direct effects in the presence of anticipatory pre-trends with just a slight twist on a standard event-study design. Crucially, this model allows us to estimate the impact of the deregulation for our full sample—going beyond the small set of stocks near the threshold that were used in our regression discontinuity approach—while additionally providing a direct parameterization of the *rate* of anticipation itself. Our model based estimates align closely with those from our regression discontinuity approach.

4.1 A General Information Revelation Model

As a benchmark, consider a market for a stock with shares outstanding of Q. The stock pays a dividend at terminal date T, and we consider periods t from $-n < 0$ to $T > 0$. The dividend π is normally distributed with mean zero and variance σ_π: $\pi \sim N(0, \sigma_\pi^2)$. For simplicity, we set the interest rate to zero. There is a unit mass of unconstrained risk averse investors with CARA utility $-e^{-\gamma W}$ who are price takers.

The equilibrium price is given by, for all $t < T$:

$$p^* = -\gamma\sigma_\pi^2 Q.$$

Since there are Q shares outstanding, risk-averse investors require a risk-discount of $-\gamma\sigma_\pi^2 Q$ to own these shares at $T-1$. For all t from $-n$ to $T-1$ the stock price is simply equal to the price at $T-1$ since there are no further risks to owning the shares.

An Unexpected Shock. Now suppose that at time $t = 0$ there is a shock to credit available to a set of previously constrained investors. In our context, time $t = 0$ can be interpreted as the deregulatory date at which margin lending becomes available. We model this, in reduced form, as a permanent price inelastic demand shock of Δ shares similar to De Long *et al.* (1990). If this shock was entirely unanticipatable, price would jump discretely at time $t = 0$ from p^* to

$$p_0 = -\gamma\sigma_\pi^2 (Q - \Delta)$$

for all $t \geq 0$.

In this context, it is natural to view $m = \Delta\gamma\sigma_\pi^2$ as the direct effect of the credit expansion. The unanticipated demand shock of Δ leaves effectively $(Q - \Delta)$ shares for risk-averse unconstrained investors to own. If $\Delta > 0$, this leads to a lower required rate of return or higher prices. The black line in Panel (a) of Figure 5 shows a stylized example of the price path following this sort of surprise. The x-axis represents time, with the vertical line at 0 corresponding to the date of the credit supply expansion. The y-axis represents the price of an asset that can be purchased with credit after the event date. In a world with no anticipation, we would expect the path of prices to follow the black line: flat before the event date, with a sharp jump to a higher price immediately or shortly after the deregulatory event. The difference in the two prices represents the direct effect m in our model, which is the degree to which prices jump after the event date. If the shock were totally unanticipated, a simple difference-in-difference or event study design would identify this effect.

An Anticipated Shock. Suppose instead that unconstrained investors begin to receive signals about the inelastic demand shock Δ in period $t = -n < 0$. Specifically, we assume that in each period $t \leq 0$ they receive signals m_t about $m = \gamma\sigma_\pi^2\Delta$, which we model as[19]

$$m = \sum_{-n}^{0} m_t. \tag{5}$$

In other words, investors progressively learn about m as it is realized over time. We assume that the signals m_t's are independent normal with mean zero and variance σ_t^2, which may vary across periods.

The equilibrium price for any t between $-n$ and 0 (the event itself) is given by:

$$p_t = p^* + \sum_{j=-n}^{t} m_j - \gamma\bigg(\sum_{k=t+1}^{0} \sigma_k^2\bigg)Q. \tag{6}$$

At $t = 0$, the price is simply $p_0 = p^* + m$.

In our setting, we are interested in studying the prices of stocks that ultimately receive a *positive* credit supply shock at time $t = 0$. In our model, this translates to stocks with $m > 0$.[20] We refer to these throughout as *treated* stocks. In a cross-section of such ex-post treated stocks, the expected price at any time t is given by:

$$E[p_t|m > 0] = p^* + E\bigg[\sum_{j=-n}^{t} m_j|m > 0\bigg] - \gamma\bigg(\sum_{k=t+1}^{0} \sigma_k^2\bigg)Q. \tag{7}$$

Notice that this equation shows two distinct sources of pre-trends in prices for treated stocks. First, to the extent there is anticipation, we expect prices will start to rise in advance beginning at $t = -n$.

[19] This dividend structure is first used in Grundy & McNichols (1989) and He & Wang (1995).

[20] In this stylized model all stocks receive a shock and treatment is defined as a positive realization. One can write an analogous model in which m acts as a latent index determining the subset of treated firms receiving a binary credit supply shock, but such a model is less tractable with similar intuition.

This is captured by the expectation on the righthand side of Equation 7. Given our assumptions, this expectation term is positive and grows as t increases towards 0. This captures the gradual introduction of information about the shock.

Second, there is a risk discount effect. Risk-averse investors recognize the variance associated with the Δ shock and must be compensated to own shares Q before the deregulation date 0. As time progresses, there is less uncertainty regarding the size of the credit-supply driven demand shock, so the risk discount falls and the stock price rises. This is captured by the third term on the righthand side of Equation 7. Note that, in this context, the "ex-post" effect—the change in prices at or after deregulation—is not the direct effect of credit, but simply the realization of the final signal m_0.

4.2 The Exponential Decay Information Structure

To take our model to data, we propose an information structure that does not require the econometrician to observe when investors begin to receive signals regarding the direct effect. Specifically, we allow unconstrained investors to receive signals about m into the infinite past (formally, we set n from Equation (5) to ∞ so that $m = \sum_{t=-\infty}^{0} m_t$).

We next place some parametric structure on the signals m_t to enable a straightforward estimation strategy. Specifically, we assume that the variance of each m_t is given by

$$\sigma_t^2 = \beta(\theta)^t$$

for some $\theta > 1$. In other words, the variances of the signals increase exponentially as the event date approaches (or uncertainty about m reduces exponentially). We view this as a reasonable assumption in a broad set of contexts, including our own, as it is relatively flexible (depending on the value of θ, which parameterizes the rate of anticipation), and captures the intuition that more information is likely to be revealed as the event date approaches.

Given this assumption, note that the unconditional variance of m can be written as

$$\sigma_m^2 = \frac{\beta}{1 - \frac{1}{\theta}}.$$

A Simple Expression for the Expected Price of Treated Stocks. With this information structure, the expected price for treated shocks shown in Equation 7 takes a simple form:

Result 1: Under our exponential decay information structure, the expected price of a treated stock at any time t may be written as

$$E[p_t|m>0] = \tilde{p} + \underbrace{\beta(\lambda+\gamma)}_{\delta_1} \sum_{j=-\infty}^{0} \theta^j D_{t-j}, \tag{8}$$

where D_{t-j} is an indicator equal to one for $j \leq t$ (equivalently, $t-j \geq 0$) and zero otherwise, $\lambda = \frac{\phi(0)}{\Phi(0)} \frac{1}{\sigma_m}$, and $\tilde{p} = p^* - \frac{\gamma\beta}{1-\frac{1}{\theta}}$.

Proof: See Appendix D.1.

Implications for Stock Prices. The presence of anticipatory pre-trends is immediately obvious from the expression in Equation 8. In each period $t<0$, the average price of treated stocks rises by $\beta(\lambda+\gamma)\theta^t$. The parameter θ captures the exponential rate at which prices rise. The red and blue lines to the left of the event date in Panel (a) of Figure 5 show this pattern, displaying the expected price path for treated stocks—incorporating anticipation on the part of unconstrained investors—given this information structure. We show two values of θ (holding the size of the direct effect fixed). The blue line displays a relatively high value of θ—effectively a very high rate of decay prior to the event. In this case, anticipation only begins to meaningfully impact prices in the last few periods prior to the event. The red line shows a lower value of θ. In this case, prices begin to rise noticeably much earlier.

Panel (b) of Figure 5 shows simulated price paths for individual stocks based on our model. The average, captured by the dark blue line, highlights the key feature of our model: anticipatory pre-trends in prices can be captured by fitting an exponential curve. Furthermore, the variation in individual level stock prices around this average captures an important auxiliary implication of our model for the behavior of stock markets during liberalization episodes: anticipation generates time-varying stock price volatility. The variance of stock market prices rises as the liberalization date approaches and more information is revealed.

4.3 Panel Estimation Strategy

We now show that Equation 8 translates naturally into a simple panel regression model that we can use to (i) estimate the direct effect using our full sample of margin-eligible stocks (including stocks far from the cut-off) and (ii) recover the parameters governing the rate of anticipation.

To see this, first consider the price realization for an individual "treated" stock i at time t. Recall that we define a stock to be treated if it receives a positive shock ($m^i = \sum_{j=-\infty}^{0} m_j^i > 0$). Given Equation 8, we have:

$$p_{it}^{treated} = \tilde{p} + \delta_1 \sum_{j=-\infty}^{0} \theta^{-j} D_{it-j} + \varepsilon_{it}, \tag{9}$$

where ε_{it} is mean 0 and uncorrelated across stocks. Taken literally, this error term represents the difference between the realized stream of messages for stock i and the conditional expectation for the treated group.[21] Of course, in practice ε_{it} will also include any unmodeled stock and time specific factors not captured by the expression in Equation 8. Iterating Equation 9 forward one period and rearranging gives a straightforward linear expression for $p_{it}^{treated}$ as a function of future prices and a treatment indicator.

Result 2: For treated stocks, the price at time t can be written as

$$p_{it}^{treated} = \underbrace{\left(1 - \frac{1}{\theta}\right)}_{\delta_0} \tilde{p} + \delta_1 D_{it} + \underbrace{\frac{1}{\theta}}_{\delta_2} p_{it+1}^{treated} + \underbrace{\varepsilon_{it} - \frac{1}{\theta}\varepsilon_{it+1}}_{e_{it}}. \tag{10}$$

where D_{it} is an indicator equal to one if $t \geq 0$, as described above.

Proof: See Appendix D.2.

One could consider taking this equation directly to the data using only a panel of treated firms. However, doing so risks conflating market movements or trends with the coefficients of interest. This concern can be avoided with access to a control group—ideally a set of stocks that generally experience the same aggregate movements as treated stocks, but that have no ex-ante possibility

[21] $\varepsilon_{it} = \sum_{j=-\infty}^{t} m_j^i - E\left[\sum_{j=-\infty}^{t} m_j^k | m^k > 0\right]$.

of receiving a credit supply shock.

With such a control group, a natural generalization is a difference-in-difference version of Equation 10 using a broader sample that includes both treated and control stocks:

Result 3: For all stocks, the price at time t can be written as

$$p_{it} = \delta_1 D_{it} + \delta_2 p_{it+1} + \alpha_i + \eta_t + e_{it}, \tag{11}$$

Here, D_{it} is an indicator equal to one *only* for treated stocks when $t \geq 0$) and α_i and η_t represent stock and period fixed effects.

Proof: See Appendix D.3.

This simple equation with two parameters relates the price to one-period-ahead prices and an indicator equal to one after credit formally rolls out. $\delta_1 = \beta(\lambda + \gamma)$ captures the average price increase for treated stocks on date of the credit supply roll out itself. $\delta_2 = \frac{1}{\theta}$ captures the speed of information revelation. For larger θ anticipation is less important, as investors have less information about the existence or size of the credit supply shock far away from the event date.

The direct impact of the deregulation in an economy with anticipation is the average change in prices from $t = -\infty$ to 0. This is given by:

$$\Delta p_{-\infty} = E[p_0 | m > 0] - \tilde{p} = \frac{\beta(\lambda + \gamma)}{1 - \frac{1}{\theta}} = \frac{\delta_1}{1 - \delta_2}.$$

Note that, because of anticipation, the direct effect will be greater than the price increase in the period of the roll out itself (which is captured by $\beta(\lambda + \gamma)$).

4.4 Instruments

Estimation of Equation 11 has known issues that are analogous to those in the literature on dynamic panel models with lagged dependent variables (Arellano & Bond, 1991). Most simply, because the error term e_{it} contains ε_{it+1} we generically have

$$Corr(p_{it+1}, e_{it}) \neq 0.$$

However, the panel structure of the data provide a natural set of instruments. Specifically, following a logic similar to that in Malani & Reif (2015), we may construct a forward looking instrument set by allowing leads of D_{it} to act as instruments for p_{it+1}, for example, $D_{it+2}, D_{it+3}, \cdots$. Equation 11 can then be estimated via two stage least squares or through system GMM approaches in the vein of Arellano & Bover (1995).

Instrument relevance follows directly from Equation 9, which shows that p_{it+1} is a function of all future leads of D_{it}. Because treatment (and hence D_{it}) are defined on an ex-post basis, this holds despite the fact that messages m_t^i are a martingale from the perspective of market participants. A sufficient exclusion restriction is:

$$E[\varepsilon_{it}|D_{it-1}, D_{it}, D_{it+1}, D_{it+2}\cdots] = 0. \tag{12}$$

Note that this restriction implies that e_{it} will be mean independent of D_{it} and its leads. In other words, in any given periods, the stock specific error term must not correlate with future treatment status.[22] If Equation 9 is taken literally (i.e., signals m_{it} are the only source of idiosyncratic price fluctuations), then this restriction is satisfied given the rational expectation assumptions of our model. More generally, this restriction is analogous to the assumptions in a standard difference-in-difference setting: that the control allows us to construct a reasonable counterfactual for the price of the treatment group in the absence of any credit supply shock. This would be violated if, for example, the prices of stocks in the treated group were trending differently for reasons unrelated to the shock (e.g., because of differential exposure to some underlying factor), or if some unrelated shock hit the treated group during the sample period.

4.5 Implementation and Results

We implement our estimation strategy using our sample of stocks in vintages 1-3 and those that were never marginable. The stocks in vintages 1-3 here serve as the treated stocks in our model. The set of never marginable stocks—largely composed of stocks very far from the threshold for inclusion according to the screening-and-ranking rule (and therefore with little ex-ante probability of becoming marginable)—serve as control stocks. We consider monthly data covering March

[22] Note that the primary instruments proposed in Malani & Reif (2015), further leads and lags of the dependent variable itself, will not work in our context because there is inherent autocorrelation in ε_{it}.

2009-October 2015. To account for scale effects, we normalize p_{it}, the price of stock i in month t by the price of that stock in March 2009. Here $D_{it} = 1$ if margin lending is available for stock i in month t and 0 otherwise. We consider OLS estimates and several versions of our IV specifications, including standard two-stage least squares and system GMM style approaches.

The first column of Table 6 shows an OLS version of Equation 11.[23] The OLS estimate of δ_2, which we expect to be biased due to the endogeneity concern described above, is $\hat{\delta}_2^{OLS} = 0.883$. Taken literally, this would suggest that the direct effect was anticipated gradually, with the equivalent of a 12 percent monthly discount rate. We also have $\delta_1^{OLS} = 0.013$, which translates to a direct effect ($\frac{\delta_1}{1-\delta_2}$) of roughly 0.11, i.e. that treated stocks cumulatively experienced a differential increase of 11 percent of the March 2009 price once margin lending was fully rolled out.

The second column of Table 6 implements our instrument based estimation strategy using a standard two-stage least squares approach. In the first stage, we instrument for p_{it+1} using leads of D_{it}. Specifically, we use leads 2 through 4 and estimate first stage:

$$p_{it+1} = \mu_1 D_{it} + \mu_2 D_{it+2} + \mu_3 D_{it+3} + \mu_4 D_{it+4} + \iota_i + \kappa_t + u_{it}.$$

We then use predicted $\hat{p}_{it+1}$ in Equation 11. The results from this approach, which resolve the bias in the OLS, suggest a substantially smaller effective discount rate—or that more information was available to market participants ex-ante. Specifically, $\hat{\delta}_2^{2SLS} = 0.939$, which implies that the direct effect of margin lending was anticipated and impounded into prices with a discount of 6 percent monthly. Furthermore, the estimate $\hat{\delta}_1^{2SLS} = 0.011$ suggests a direct effect of 0.18.

The final two columns of Table 6 show the results of implementing our strategy using an Arellano and Bond style one-stage GMM approach with leads of D_{it} as instruments. In column 3 we use 2-4 leads for comparability with our two stage least squares approach. In column 4 we use a much broader set of instruments, employing 2-10 leads. We follow Malani & Reif (2015) and transform the data using forward orthogonal deviations instead of first differences.[24]

Our results are consistent with the two-stage least squares approach. $\hat{\delta}_2^{AB}$ ranges from 0.92 to 0.94, suggesting that the ultimate price effects of margin lending were anticipated gradually—

[23] In all specifications we cluster standard errors at the stock level.

[24] Our panel is not entirely balanced as some firms experience long-period trading suspensions from time to time. See, e.g. Huang *et al.* (2019).

with an effective discount rate of 6-8 percent—as information slowly became available. This rate of anticipation suggests that more than 60 percent of the direct effect of credit supply was already priced in even 6 months prior to deregulation.

The direct causal effect implied by these estimates ranges from 0.18-0.24. In other words, treated stocks cumulatively experienced a differential increase of 18-24 percent due to the introduction of margin lending, almost perfectly in line with the estimates found using our RD approach. Note that our estimates also align with the magnitudes from event-study specifications examining cumulative returns in the year prior to marginability. Put simply, our results show that the level of stock prices is meaningfully impacted by an inflow of credit.

4.6 Implications of Our Estimates for the 2014-2015 Stock Market Boom

A natural set of comparisons for the large causal effects of credit that we estimate comes from recent work on the elasticity of demand for stocks, and from work on demand-based asset pricing more generally. Given our estimates, the effect of the deregulation (18% in our more conservative specifications), and the observed quantity of margin debt flowing into the vintages we study (roughly 3% of market cap within 3 months), we can recover a basic estimate of the elasticity using the following formula:

$$Return = -\frac{1}{\epsilon}\%\Delta Demand. \tag{13}$$

Here, $Return$ is the return associated with the event (i.e. our causal effect), $\Delta Demand$ is the increase in margin debt relative to market cap driven from buying by previously constrained retail investors when margin debt becomes available, and ϵ is the elasticity of demand of the unconstrained buyers or arbitrageurs in the economy. ϵ approaching $-\infty$ corresponds to infinitely elastic demand curve, while a small negative number corresponds to an inelastic demand curve. Estimates from the literature focused on the micro-elasticity for individual stocks are generally on the order of -1, whether using a demand systems approach as in Koijen & Yogo (2019) or an indexing design as in Chang *et al.* (2014) (although some recent estimates have been meaningfully below 1 in magnitude, e.g., Haddad *et al.*, 2021).

Combing our estimated direct effect and the influx of margin debt within 3 months using Equation 13 gives an estimate of 0.167, indicating less elastic demand for stocks in the Chinese context we study relative to the literature (which is typically based on US data). This is perhaps unsurprising, given greater frictions and the lack of a meaningful market for short selling in the Chinese context. Furthermore, the fact that the deregulation we study corresponded to a large aggregate inflow into a broader set of stocks may be an additional explanation for our relatively inelastic estimates (although this is unlikely to be a factor in the cross-sectional estimation strategy used in our regression discontinuity design).

We can use our elasticities to provide a back-of-the-envelope evaluation the importance of the deregulation of margin lending—and the sizeable inflow of margin debt—in the stock market boom that came to a head in the latter half of 2014 and the beginning of 2015. While our estimates are largely focused on the period prior to the peak of this boom, a simple extrapolation is possible. By 2015, the ratio of margin debt to market cap for the three vintages we study reached roughly 8 percent (see Figure 2). Combining this with our estimated elasticity suggests that the introduction of margin debt was responsible for a price increase of 47% for the three vintages we study. This represents approximately one-fourth of the increase in prices for these stocks between mid-2014 and mid-2015. In other words, this suggests that a quarter of the boom can be attributed to margin debt for these stocks.

Further, we believe this calculation may understate the aggregate effects of margin lending on the boom for two reasons. First, our measure of margin debt does not take into account the role of shadow margin, which became a major factor at the peak of the boom, and may have injected substantially more credit into the system (Bian *et al.*, 2017). Second, the deregulation of margin lending inherently generated a large flow of money into the stock market from other sectors. As such, assessing the aggregate consequences depends on the macro-elasticity of the Chinese market, which is likely even smaller in magnitude than the elasticities we recover using our regression-discontinuity and panel-based designs (Gabaix & Koijen, 2021). Given this, we leave a more formal evaluation to future work.

5 Conclusion

Credit expansions often coincide with increased valuations in both housing and stock markets. While a broad set of theories has linked credit to asset prices in both contexts,[25] well identified studies have typically focused on housing markets. There are at least two reason why empirically capturing the impact of credit on stock prices is particularly challenging. First, the flow of easy credit into stock prices can be complex and difficult-to-account for (in comparison to real estate, where mortgages are more readily observable). Furthermore, deep-pocketed investors are more pivotal in pricing for stock markets, and their trades may offset or pre-empt the influence of credit.

We confront these challenges using the Chinese deregulation of margin lending that took place in the first half of the 2010s. This liberalization narrowly focused on stock market leverage, and the influx of margin debt that followed far-exceeded margin lending deregulations studied in other work. Furthermore, Chinese data on the relevant institutional investors allows us to observe the role of deep-pocked agents during a credit expansion.

Reduced form evidence indicates the presence of a postive causal effect of margin lending, albeit one that was anticipated and priced in by institutions and other deep-pocketed investors. We develop and estimate a dynamic panel model of stock prices that incorporates anticipation, which allows us to quantify the impact of the expansion and recover the parameters governing the rate of anticipation. As credit expansions are likely to be foreseen by sophisticated and relatively unconstrained agents in more general stock market contexts, this approach may be valuable in other settings.

Ultimately, our results suggest that the expansion of credit generated by the deregulation of margin lending had large positive impacts on stock prices. This highlights the importance of a demand based view of stock markets: if demand for stocks slopes downwards, we should expect credit expansions to raise equity prices. Clear views on the (micro and macro) elasticities are necessary for regulators to understand the impacts of credit liberalizations on stock market booms.

[25] A non-exhaustive list includes political economy considerations Herrera *et al.* (2020); agency and risk-shifting (Allen *et al.*, 2022; Allen & Gale, 2000); complacent or neglectful creditors underestimating downside or tail risk (Minsky, 1977; Gennaioli *et al.*, 2012); contracting and leverage constraints (Geanakoplos, 2010; Simsek, 2013); and intermediary frictions or balance sheets (Bernanke & Gertler, 1989; Kiyotaki & Moore, 1997; Adrian & Shin, 2010; He & Krishnamurthy, 2013).

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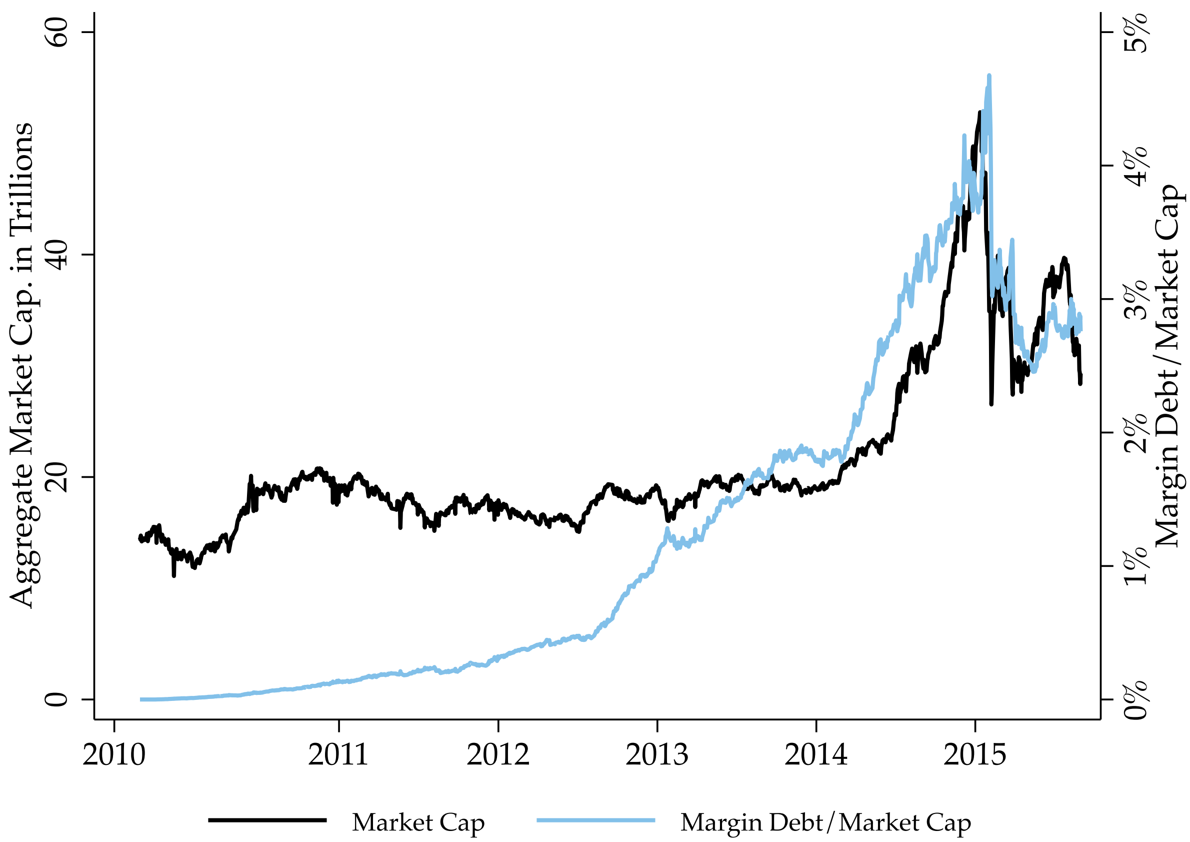
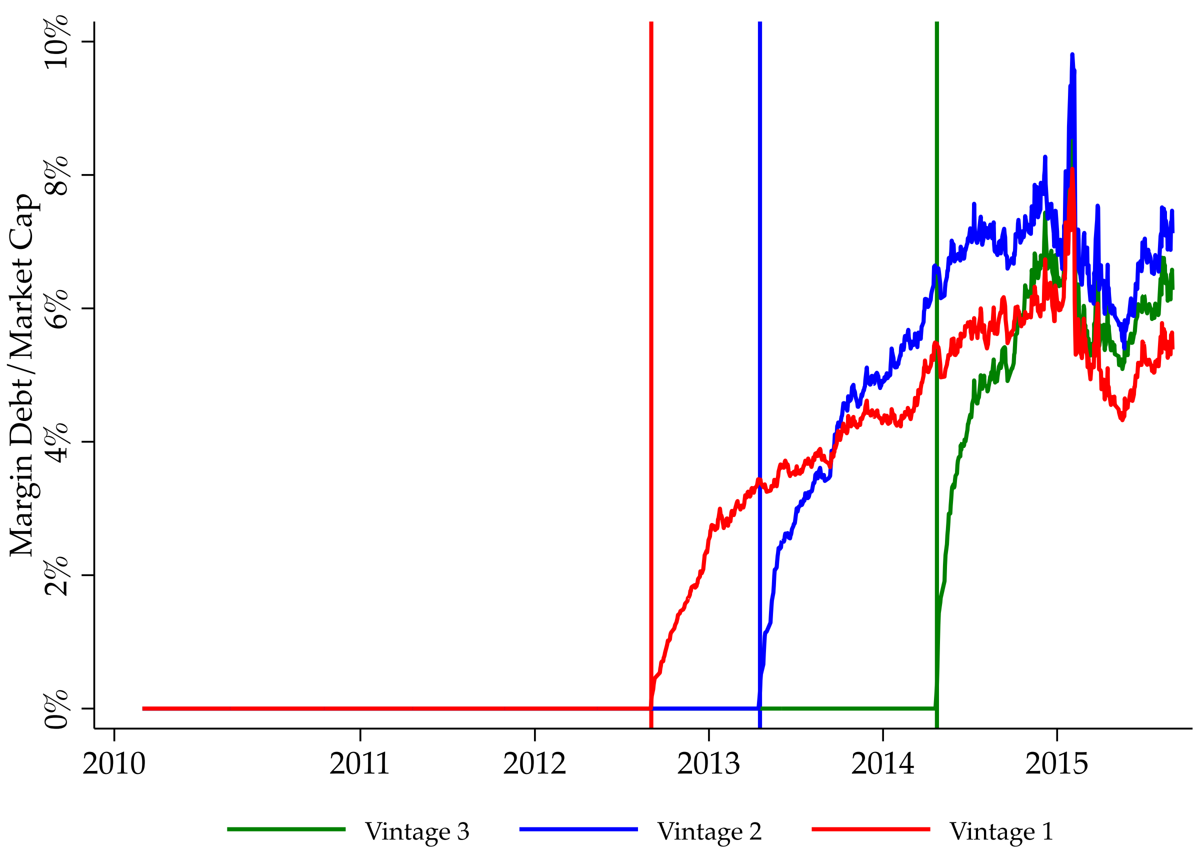
Figure 1: Aggregate Market Cap. and Margin Debt/Market Cap. Over Time



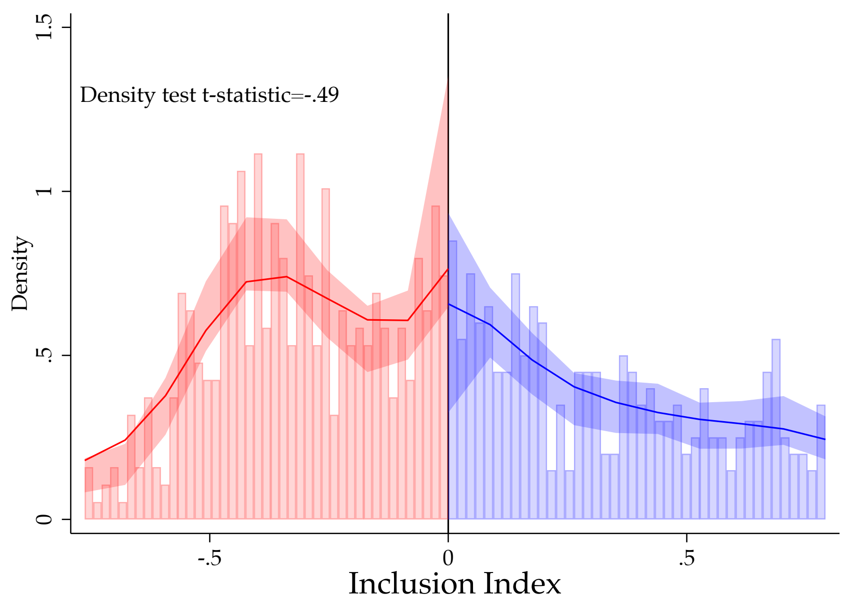
Notes: This plot shows daily aggregate market cap (in black) and the ratio of margin debt to market cap (in blue) for all stocks in sample, including those that are never marginable. Market cap is measured in trillions of yuan.

Figure 2: Margin Debt/Market Cap. by Vintage



Notes: This plot shows the daily ratio of total margin debt to total market cap for each of the three vintages we study. Vertical lines denote starting dates of each vintage.

FIGURE 3: NO EVIDENCE OF BUNCHING AT THRESHOLD

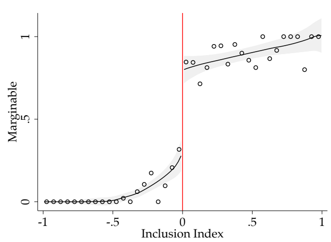


Notes: This plot shows the results of the manipulation tests outlined in Cattaneo *et al.* (2018). Red and blue bars show a histogram of the underlying data, while red and blue lines show and local polynomial estimates of the density.

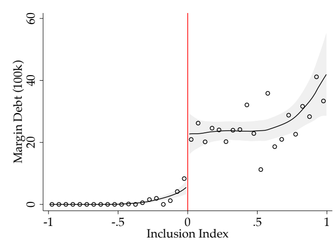
Figure 4: Regression Discontinuity Evidence of Impacts of Margin Lending on Equity Prices

Panel A: Inclusion Index Determines Marginability

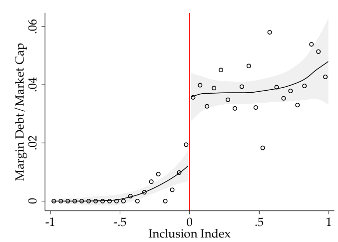
Marginability



Margin Debt

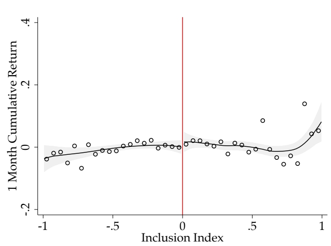


Margin Debt/Market Cap

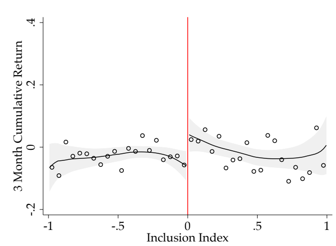


Panel B: Positive Returns to Crossing Marginability Threshold

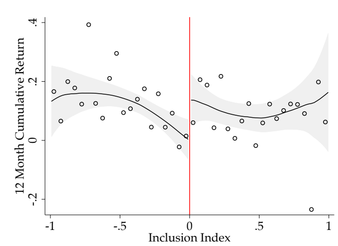
1 Month Cumulative Returns



3 Month Cumulative Returns



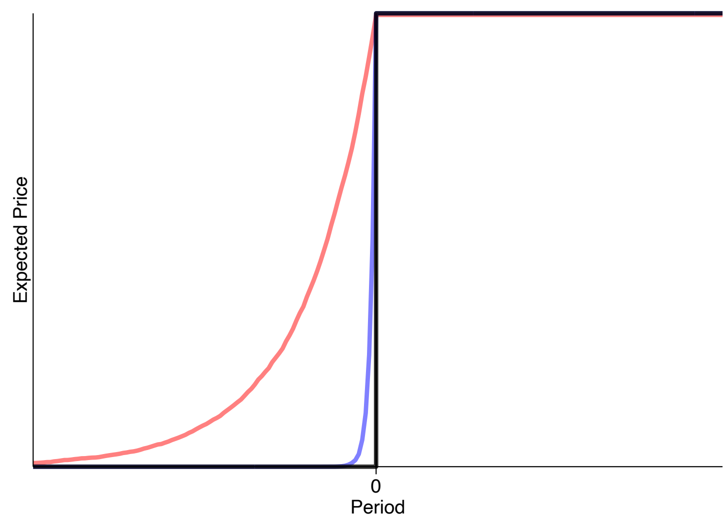
12 Month Cumulative Returns



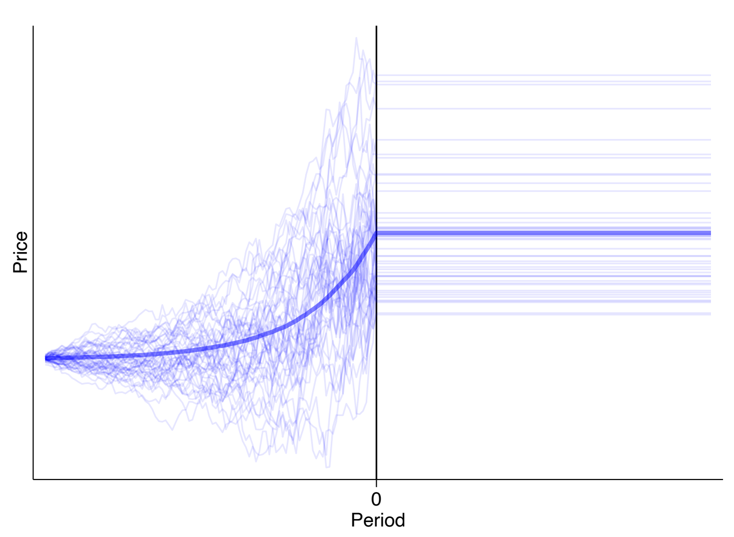
Notes: Panel (a) shows an indicator for marginability, stock level margin debt, and the stock level ratio of margin debt to market cap plotted against the inclusion index. Panel (b) shows raw cumulative returns. Inclusion index normalized to set vintage specific threshold equal to 0. For each vintage, all not-yet marginable stocks with inclusion index within 1 of the threshold at the time marginability was determined are included. Marginability, market cap, and margin debt are measured in the third calendar month following the start of each vintage. Points show averages within bins of width 0.05 in the index. Lines shows local linear fits with 95% confidence intervals on either side of the threshold.

FIGURE 5: ANTICIPATION EFFECTS OF AN INCREASE IN CREDIT SUPPLY

(a) Varying the Rate of Anticipation



(b) Simulated Price Paths for Treated Stocks



Notes: In both figures the y-axis displays price, the x-axis displays time, and the vertical line indicates the event date of a credit supply shock. Panel (a) shows the expected price path for stocks receiving a credit supply shock at time 0 under three regimes (holding the total price effect constant). The black line shows the expected price path from a model with no anticipation. The blue line shows the expected price path from a version of our model with a large value of θ. The red line shows the expected price path from a low value of θ. Panel B shows price realizations for treated stocks from simulations based on our model. Each blue line represents the price path for an individual stock. The thicker blue line represents the average price for all treated stocks in each period. For these simulations, we set $\gamma = 0.2$, $\theta = 1.05$, and $\beta = 1.43$.

TABLE 1: NUMBER OF MARGINABLE STOCKS BY VINTAGE

Number of marginable stocks by vintage				
		# of newly marginable		
Vintage #	Announcement date	Shanghai	Shenzhen	% of total cap
Pilot A	February 13th, 2010	50	40	51.74%
Pilot B	November 25th, 2011	131	60	66.31%
1	January 25th, 2013	163	113	75.23%
2	September 6th, 2013	104	102	77.95%
3	September 12th, 2014	104	114	78.48%

TABLE 2: EVENT STUDY OF MARGINABILITY

	Following Marginability			Preceding Marginability			Before vs. After
	0 to 1	0 to 3	0 to 12	-1 to 0	-3 to 0	-12 to 0	-12 to 12
Marginable	0.001	-0.008	0.001	0.018^{***}	0.062^{***}	0.232^{***}	0.232^{***}
	(0.004)	(0.007)	(0.015)	(0.005)	(0.008)	(0.014)	(0.021)
	[0.512]	[0.854]	[0.635]	[0.000]	[0.000]	[0.000]	[0.000]
Mean of Dep. Var.	-0.00257	-0.0185	-0.0521	-0.00239	-0.00956	-0.0316	-0.0821
N	4026	3906	3677	3944	3863	3784	3554

First three columns show results from regressions of cumulative market adjusted returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the introduction of margin debt. Columns 4-6 show results from regressions of cumulative market adjusted returns at the stock level from 1, 3, and 12 months preceding the introduction to the month of the introduction itself on an indicator for newly marginable stocks. Column 7 shows cumulative returns from 12 months before to 12 months after introduction. For each of the three vintages we consider only the newly marginable stocks in that vintage as well as the set of never marginable stocks. All specifications include dummy variables for vintage as controls. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. One sided p-values from placebo exercise shown in square brackets based on 10000 recreations of each regression using the period of July, 2001 to September 2007. P-values represent the fraction of placebo regressions with larger (for ex-ante effects) or smaller (for ex-post effects) values of the relevant coefficient.

TABLE 3: REGRESSION DISCONTINUITY EVIDENCE OF THE IMPACT OF MARGIN DEBT ON STOCK PRICES

Panel A: Crossing Marginability Threshold Predicts Margin Debt

	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.509***	13.129***	0.017**	0.496***	11.242***	0.016**
	(0.077)	(3.462)	(0.007)	(0.080)	(3.808)	(0.007)
P-Value	0.000	0.000	0.011	0.000	0.003	0.024
CCT Robust P-Value	0.000	0.002	0.053	0.000	0.022	0.093
Bandwidth	0.289	0.264	0.274	0.326	0.294	0.315
N	350	323	329	400	351	383

Panel B: Positive Returns to Crossing Marginability Threshold – Reduced Form

	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.028*	0.102***	0.095*	0.020	0.085**	0.126**
	(0.015)	(0.036)	(0.056)	(0.014)	(0.036)	(0.049)
P-Value	0.062	0.005	0.088	0.149	0.017	0.010
CCT Robust P-Value	0.068	0.003	0.151	0.155	0.019	0.015
Bandwidth	0.359	0.308	0.293	0.476	0.394	0.458
N	437	368	323	590	472	516

Panel C: Positive Returns to Crossing Marginability Threshold – Fuzzy RD

	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.048*	0.179**	0.280***	0.039	0.168**	0.246**
	(0.028)	(0.071)	(0.109)	(0.027)	(0.073)	(0.105)
P-Value	0.082	0.012	0.010	0.145	0.021	0.019
CCT Robust P-Value	0.088	0.010	0.007	0.141	0.022	0.017
Bandwidth	0.347	0.327	0.362	0.434	0.408	0.486
N	424	395	403	532	495	546

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with inclusion index value within the specified bandwidth of the threshold at the time marginability was determined. In panel A, we consider outcomes in the third month after marginability. For panels B and C, we consider raw cumulative returns 1,3, and 12 months after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns are derived from local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use the covariate adjusted MSE optimal bandwidths described in Calonico, Cattaneo, Farrell, and Titiunik (2017). Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Marginable is a dummy variable equal to one if the stock became marginable in the relevant vintage. Margin debt refers to stock level quantity of margin debt in millions of yuan. $\frac{\text{Margin}}{\text{Market Cap}}$ refers to the ratio of margin debt to market capitalization. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4: MUTUAL FUND PORTFOLIO WEIGHTS

	(1)	(2)	(3)
Top_Quintile*MonthsSinceLastVintage	0.0034***		
	(0.0011)		
Top150*MonthsSinceLastVintage		0.0027**	
		(0.0011)	
log(Index)*MonthsSinceLastVintage			0.0016**
			(0.0007)
Constant	0.9822***	0.9846***	0.9982***
	(0.0414)	(0.0415)	(0.0409)
Controls	Yes	Yes	Yes
Stock Fixed Effect	Yes	Yes	Yes
Fund Fixed Effect	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes
N	324860	324860	324860
adj. R^2	0.446	0.446	0.446

This table reports the results of regressions of Equation 4. Top_Quintile equals one if the stock is non-marginable and ranked as the top 20% based on $Index$ among all non-marginable stocks, and zero otherwise. $Index$ is calculated as Equation 1 using stocks' market cap and volume information. Top150 equals one if stocks are ranked within the top 150 based on $Index$ and zero otherwise. log($Index$) equals the log of $Index$ for non-marginable stocks and zero for marginable stocks. $MonthsSinceLastVintage$ equals the number of months between fund reporting and the previous vintage. Controls represent the percentile ranking of stock characteristics, including market capitalization, book-to-market, turnover rate, past month return, and past year return, and the interactions between $MonthsSinceLastVintage$ and each of the stock characteristics. Fund, stock, and time fixed effects are included. The sample period is from December 2011 to June 2014. Standard errors are clustered by fund and the reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5: RETURNS FOR TO-BE-MARGINABLE (TBM) PORTFOLIOS

	TBM Weight (%)	Raw Return (%)			Mkt-adj. Return (%)			CAPM Alpha (%)		
		TBM	NTBM	L-S	TBM	NTBM	L-S	TBM	NTBM	L-S
Top_Quintile	12.98	1.646***	1.089**	0.556**	0.830**	0.273	0.556**	0.844***	0.285	0.560*
		(0.590)	(0.500)	(0.265)	(0.323)	(0.250)	(0.265)	(0.300)	(0.186)	(0.279)
Top150	12.41	1.656***	1.084**	0.558**	0.840**	0.268	0.558**	0.852***	0.280	0.557*
		(0.590)	(0.498)	(0.264)	(0.332)	(0.248)	(0.264)	(0.308)	(0.184)	(0.281)

This table reports returns of mutual funds' TBM, NTBM, and long-TBM-short-NTBM portfolios. For each semi-annual report, stocks in each mutual fund's portfolio are assigned to one of two portfolios: "to-be-marginable" (TBM) and Non-TBM (NTBM). TBM companies are defined as stocks that are ranked within the Top Quintile or Top150 in its exchange based on the inclusion index. Then, we compute the monthly returns on TBM and NTBM holdings over the next six months and also returns of the strategies that long TBM and short NTBM (L-S). Portfolios are rebalanced every six months, and within a given fund portfolio, stocks are value weighted by the funds dollar holdings. Calendar time portfolio returns are calculated by averaging across funds. We use raw returns, market-adjusted returns, and CAPM alpha. Market-adjusted returns equal raw return minus value-weighted market returns. CAPM alpha is the intercept on a regression of monthly portfolio excess returns on the market factor. TBM Weight (%) is the average portfolio weight of all TBM stocks. The sample period is from January 2012 to September 2014. Newey-West standard errors are used with lag of 11 months and are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 6: INFORMATION REVELATION MODEL OF ANTICIPATION

	OLS	IV: Leads 2-4	AB: Leads 2-4	AB: Leads 2-10
Price_{t+1}	0.883^{***}	0.939^{***}	0.938^{***}	0.924^{***}
	(0.005)	(0.030)	(0.010)	(0.010)
Margin Trading Active	0.013^{***}	0.011^{***}	0.015^{***}	0.015^{***}
	(0.004)	(0.003)	(0.003)	(0.003)
θ	1.133	1.064	1.067	1.083
Direct Effect	0.108	0.181	0.243	0.190
First Stage F-Stat (Kleibergen-Paap)		17.2		
Month $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes

Results from estimation of information revelation model of anticipation of price on marginability and future prices. Specifically we report coefficients and recovered parameters from the following regressions:

$$\text{Price}_{it} = \delta_0 + \delta_1 \text{Margin Trading Active}_{it} + \delta_2 \text{Price}_{it+1} + \gamma_i + \eta_t + e_{it}.$$

Margin Trading Active is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in months after margin trading is active in those stocks. $Price_{it}$ represents the price of stock i in month t, normalized by the price in March 2009, the first month in our sample. Derived parameters are $\theta = \frac{1}{\delta_2}$ and Direct Effect= $\frac{\delta_1}{1-\delta_2}$ The first column shows OLS estimates. The second column shows standard IV estimates with leads from $t+2$ through $t+4$ of Margin Trading Active as instruments. Columns three and four show Arellano and Bond style one-stage GMM estimates using leads of Margin Trading Active from $t+2$ through $t+4$ and $t+2$ through $t+10$ respectively. Data transformed using forward orthogonal deviations instead of first differences. Monthly data from March 2009-October 2015. Standard errors clustered at the stock level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Internet Appendix: For Online Publication

A Appendix Tables and Figures

Table A.I: Event Study of Marginability
Including All Not-Yet Marginable Stocks in Control Group

	Cumulative Returns						
	Following Marginability			Preceding Marginability			Before vs. After
	0 to 1	0 to 3	0 to 12	-1 to 0	-3 to 0	-12 to 0	-12 to 12
Marginable	-0.001	-0.017^{**}	-0.033^{**}	0.015^{***}	0.056^{***}	0.218^{***}	0.184^{***}
	(0.004)	(0.007)	(0.014)	(0.005)	(0.008)	(0.014)	(0.020)
N	4513	4388	4151	4422	4338	4255	4015

First three columns show results from regressions of cumulative market adjusted returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the announcement/introduction of margin debt on an indicator for newly marginable stocks. Columns 4-6 show results from regressions of cumulative market adjusted returns at the stock level from 1, 3, and 12 months preceding the annoucement/introduction to the month of the introduction itself. Column 7 shows cumulative market adjusted returns from 12 months before to 12 months after introduction. All specifications include dummy variables for vintage as a control. For each of the three vintages we consider newly marginable stocks in that vintage as well as the set not-yet marginable stocks. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.II: NO DISCONTINUITY IN STOCK LEVEL COVARIATES ACROSS THRESHOLD
BALANCE SHEET VARIABLES IN T-1

	Total Assets	Fixed Assets	Current Assets	Total Debt	Total Equity	Operating Sales
Above Marginable Threshold	0.348	0.225	-0.386	0.040	0.316	-0.348
	(2.315)	(0.916)	(1.427)	(1.626)	(0.731)	(2.522)
P-Value	0.880	0.806	0.787	0.980	0.666	0.890
CCT Robust P-Value	0.934	0.914	0.844	0.819	0.847	0.952
Bandwidth	0.457	0.483	0.416	0.490	0.440	0.483
N	549	578	499	583	527	578

	Net Profit	Operating Profit	Market-to-Book	Price-to-Earnings	ROE	ROA
Above Marginable Threshold	0.025	0.009	0.061	12.492	-0.029	-0.009
	(0.107)	(0.128)	(0.478)	(23.617)	(0.031)	(0.009)
P-Value	0.813	0.943	0.899	0.597	0.356	0.307
CCT Robust P-Value	0.907	0.993	0.767	0.505	0.320	0.281
Bandwidth	0.343	0.354	0.406	0.296	0.434	0.411
N	408	418	487	344	520	496

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with inclusion index value within the specified bandwidth of the threshold at the time marginability was determined. We consider covariates in the year prior to marginability. All columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. All variables are scaled in billions (RMB). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.III: REGRESSION DISCONTINUITY EVIDENCE OF THE IMPACT OF MARGIN DEBT ON STOCK PRICES–VARYING BANDWIDTHS

	Crossing Marginability Threshold Predicts Margin Debt – IK Bandwidth					
	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.586^{***}	11.212^{***}	0.017^{***}	0.549^{***}	9.544^{***}	0.015^{***}
	(0.054)	(1.498)	(0.003)	(0.061)	(1.666)	(0.003)
P-Value	0.000	0.000	0.000	0.000	0.000	0.000
CCT Robust P-Value	0.000	0.001	0.002	0.000	0.002	0.007
Bandwidth	0.564	0.559	0.718	0.564	0.559	0.718
N	664	662	749	664	662	749

	Crossing Marginability Threshold Predicts Margin Debt – Bandwidth=0.5					
	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.567^{***}	10.025^{***}	0.015^{***}	0.538^{***}	9.143^{***}	0.014^{***}
	(0.057)	(1.594)	(0.003)	(0.064)	(1.740)	(0.004)
P-Value	0.000	0.000	0.000	0.000	0.000	0.000
CCT Robust P-Value	0.000	0.001	0.012	0.000	0.003	0.036
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500
N	610	610	607	610	610	607

	Positive Returns to Crossing Marginability Threshold – IK Bandwidth					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.058^{**}	0.105^{**}	0.019	0.075^{**}	0.121^{***}
	(0.013)	(0.029)	(0.042)	(0.013)	(0.030)	(0.045)
P-Value	0.237	0.044	0.014	0.157	0.014	0.006
CCT Robust P-Value	0.148	0.011	0.015	0.245	0.037	0.063
Bandwidth	0.530	0.597	0.590	0.530	0.597	0.590
N	627	665	623	627	665	623

	Positive Returns to Crossing Marginability Threshold – Bandwidth=0.5					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.068^{**}	0.117^{***}	0.019	0.082^{**}	0.125^{***}
	(0.013)	(0.030)	(0.045)	(0.013)	(0.032)	(0.047)
P-Value	0.241	0.025	0.009	0.152	0.011	0.008
CCT Robust P-Value	0.157	0.016	0.036	0.273	0.088	0.128
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500
N	607	595	557	607	595	557

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with index value within the specified bandwidth of the threshold at the time marginability was determined. We consider outcomes in the first month after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. The first and third panel use the Imbens and Kalyanaraman bandwidth, while the second and fourth panels set the bandwidth to 0.5 for all specifications. Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Marginable is a dummy variable equal to one if the stock became marginable in the relevant vintage. Margin debt refers to stock level quantity of margin debt in millions of yuan. $\frac{\text{Margin}}{\text{Market Cap}}$ refers to the ratio of margin debt to market capitalization. 1, 3 and 12 month cumulative raw returns shown in the bottom two panels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.IV: INSTITUTIONAL OWNERSHIP SURGES BEFORE MARGINABILITY

	Mutual Fund Ownership Share		Top 10 Ownership Share		Turnover	
		Quarterly Lags		Quarterly Lags		Quarterly Lags
Ex-Post Effect	-0.005^{***}	-0.004^{**}	-0.014	-0.004	0.036^{**}	0.081^{***}
	(0.002)	(0.002)	(0.012)	(0.014)	(0.014)	(0.016)
Ex-Ante Effect (t-1)		0.007^{***}		0.043^{***}		0.247^{***}
		(0.002)		(0.014)		(0.019)
Ex-Ante Effect (t-2)		0.007^{***}		0.038^{***}		0.134^{***}
		(0.002)		(0.014)		(0.016)
Ex-Ante Effect (t-3)		0.005^{***}		0.037^{***}		0.098^{***}
		(0.002)		(0.014)		(0.016)
Mean of Dep. Var.	0.0137	0.0137	0.0137	0.0137	0.560	0.560
N	42160	42160	42160	42160	127572	127572

Results from difference-in-difference regressions of ownership by institutions and turnover on marginability. For our difference-in-difference specifications we report coefficients from the following regression

$$\mathrm{y}_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{it} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \gamma_i + \delta_t + \varepsilon_{it}.$$

Margin Trading Active is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in months after margin trading is active in those stocks. $D_{i,t+j}$ is equal to one if margin trading initially becomes active for stock i in period $t+j$, and zero otherwise. $y_{i,t}$ represents the proportion of ownership by mutual funds of each stock, the proportion of ownership by the top 10 investors in each stock, or turnover. The first two are at a quarterly frequency, while turnover is at a monthly frequency. The number of *ex-ante effect* coefficients indicates the value of S for the regression in question. The first, third and fifth columns include no ex-ante effects, and is equivalent to a collapsed difference-in-difference approach. Other specifications include indicators aimed at capturing ex-ante effects for the three quarters leading up to the roll-out for each stock. Sample covers March 2009-May 2015. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.V: MORE ANTICIPATION FOR HIGH RANKED STOCKS

	Unadjusted Returns		Market Adjusted Returns:	
		Quarterly Lags		Quarterly Lags
Ex-Post Effect	-0.018^{***}	-0.015^{***}	-0.011^{***}	-0.008^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Ex-Post Effect × High Rank	-0.007^{***}	-0.005^{***}	-0.004^{**}	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Ex-Ante Effect (t-1)		0.013^{***}		0.014^{***}
		(0.003)		(0.003)
Ex-Ante Effect (t-1) × High Rank		-0.002		0.002
		(0.005)		(0.005)
Ex-Ante Effect (t-2)		0.005		0.003
		(0.003)		(0.003)
Ex-Ante Effect (t-2) × High Rank		0.013^{**}		0.016^{***}
		(0.005)		(0.005)
Ex-Ante Effect (t-3)		0.008^{**}		0.008^{**}
		(0.004)		(0.004)
Ex-Ante Effect (t-3) × High Rank		0.010^{*}		0.006
		(0.005)		(0.005)
Mean of Dep. Var.	0.0144	0.0144	-0.00373	-0.00373
N	126131	126131	126131	126131

Results from triple-difference regressions of returns on marginability and the interaction with "high-rank" defined as the set of marginable stocks in each vintage with an above median value of the marginability index. We report coefficients from the following regression

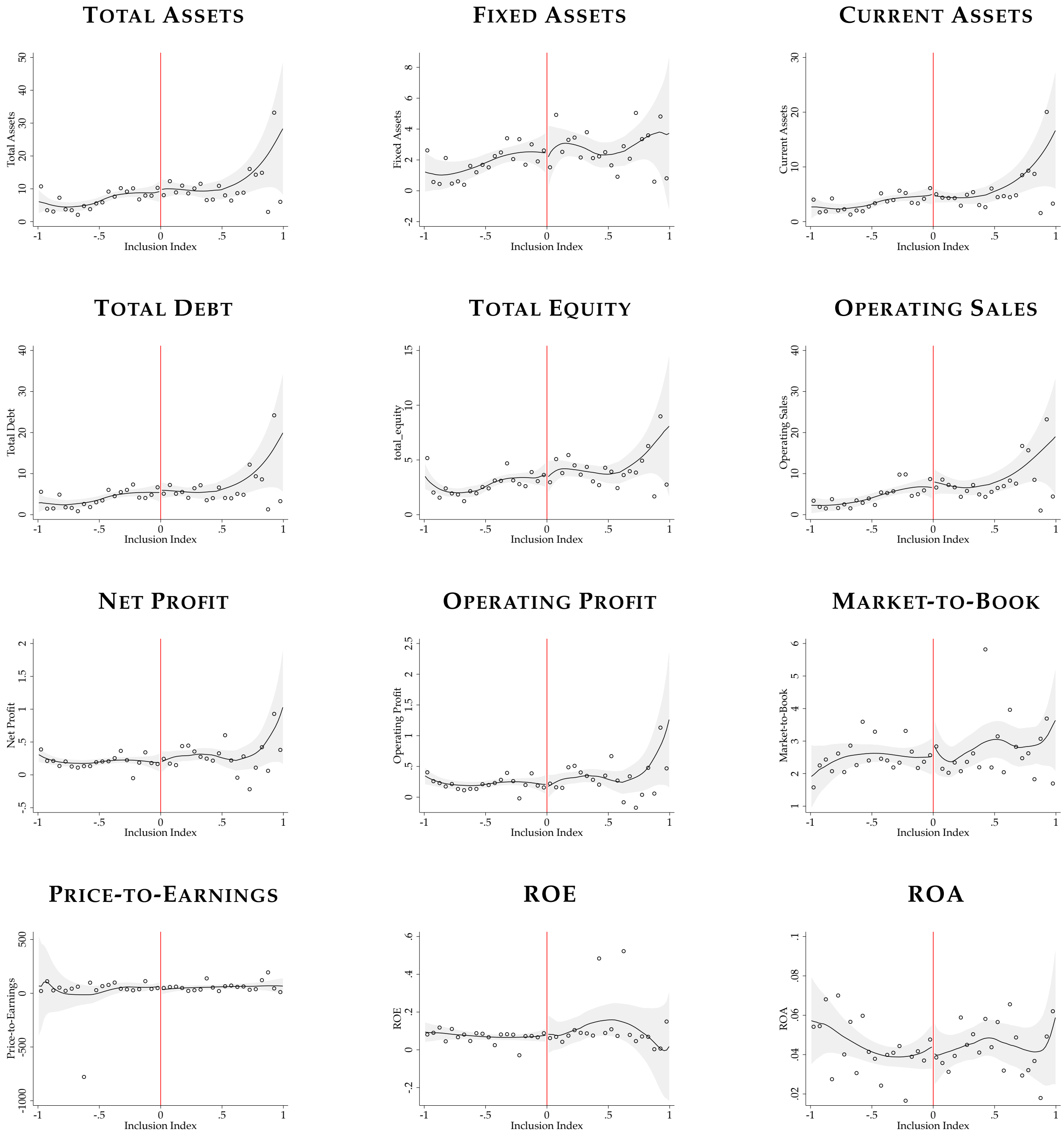
$$\begin{aligned} \mathrm{r}_{i,t} &= \alpha + \beta_0 \text{Margin Trading Active}_{it} + \eta_0 \text{Margin Trading Active}_{it} \times \text{High Rank}_{it} \\ &+ \sum_{j=1}^{S} \left[\beta_j D_{i,t+j} + \eta_j D_{i,t+j} \times \text{High Rank}_{it} \right] + \gamma_i + \delta_t + \varepsilon_{it} \end{aligned}$$

Margin Trading Active is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in months after margin trading is active in those stocks. $D_{i,t+j}$ is equal to one if margin trading initially becomes active for stock i in period $t+j$, and zero otherwise. The first and third columns include no ex-ante effects, and is equivalent to a collapsed triple-difference approach. Other specifications include indicators aimed at capturing ex-ante effects for the three quarters leading up to the roll-out for each stock. Sample covers March 2009-May 2015. The left two columns show cumulative log returns adjusted for splits and dividends but otherwise unadjusted. The right two columns show market adjusted returns. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE A.I: AGGREGATE MARKET CAP. AND SHORTED SHARES/MARKET CAP. OVER TIME

Notes: The plot shows daily aggregate market cap (in black) and the ratio of the value of all shorted shares to market cap (in red) for all stocks in sample. Market cap is measured in trillions of yuan.

FIGURE A.II: COVARIATES ARE SMOOTH CROSS THE RD THRESHOLD



Notes: For each vintage, all not-yet marginable stocks with inclusion index within 1 of the threshold at the time marginability was determined are included. We measure all covariates in the year prior to the rollout of the corresponding vintage. Points show averages within bins of width 0.05 in the index. Lines shows local linear fits with 95% confidence intervals on either side of the threshold. All variables are scaled in billions (RMB).

B Placebo Tests

One potential concern is that the anticipation we find in our event study and difference-in-differences approaches in Section 3 might be in part mechanical, driven by the ranking procedure used to select marginable stocks. In this appendix, we use the same ranking procedure to construct and implement a series of placebo regressions and confirm that this is not the case.

Our basic approach is to randomly select placebo event dates and use the ranking formula outlined in Equation 1 to define a set of treated stocks at those dates. We then repeat the regressions shown in Table 2 and compare our placebo coefficients to those generated using the actual treatment group. Because our sample period is contaminated by the deregulation itself, we implement this approach using an alternative window that matches the broad stock market dynamics of our primary sample. In particular, we consider the previous Chinese stock market bubble, which occurred following share reforms in China and during the lead-up to the Beijing Olympics. We include data from July 2001 to September 2007, the same number of months as included in our primary sample period.

For each of our placebo regressions, we randomly select three event dates. At each of these dates, we calculate the inclusion index for each stock according to Equation 1.[26] For the earliest date, we define the top 100 stocks in each exchange as Placebo Vintage 1. At the next date, we exclude stocks in Placebo Vintage 1, and define the top 100 remaining stocks in each exchange as Placebo Vintage 2. At the final date, we exclude stocks in either of the first two placebo vintages, and define the top 100 remaining stocks in each exchange as Placebo Vintage 3. We do not apply the screening procedure as the relevant criteria are not available in this earlier sample period.

With these vintages and our randomly selected event dates in hand, we re-run the regressions in Table 2 and store the estimated coefficients. We then repeat this process 10,000 times, each time randomly drawing the three event dates (with replacement). To test whether our results in Table 2 are mechanical, we compare the true coefficients to the distribution of placebo coefficients. As a summary, we show one sided placebo p-values in square brackets in Table 2. These p-values show the fraction of our placebo coefficients that are smaller than our ex-post effects or larger than our ex-ante effects.

[26] As in Section 3.2 we use data for the three months prior to the event date. We also exclude all stocks in the indices used to form Pilots A and B.

Our results suggest that neither our ex-ante nor or ex-post effects are mechanically driven. The placebo p-values for our ex-ante effects all equal 0.000, suggesting that the treatment effect was more positive than almost all the placebo estimates. The placebo p-values for our ex-post effects ranges from 0.5–0.8, suggesting insignificant ex-post effects.

C Details of Regression Discontinuity

C.1 Defining the Inclusion Index and Marginability Threshold

To construct the inclusion index, we use public stock market data and follow the screening and ranking procedure discussed in Section 2.1. We begin by removing the set of stocks that failed to satisfy the screening criteria. To construct the index itself, we must choose the window in which to measure the key inputs: market capitalization and turnover. While the exact window used by regulators was not published, industry sources suggest that the exchanges used a three-month period before the formal announcement of each vintage. Assuming that there was at least some small gap between data collection and the formal announcement, we take this to mean the three calendar months prior to the announcement date. For each of the three vintages we calculate the inclusion index for the full set of stocks that had not yet qualified for margin (and satisfied the screening criteria). We denote stock i's index for Vintage k as $Index_i^k$, where $k = \{1, 2, 3\}$.

We then identify the relevant discontinuity in the inclusion index. In theory, this discontinuity should be sharp and exactly equal to the value of the index for the lowest-ranking included stock (for each vintage and exchange). In practice the discontinuity is slightly less sharp for two reasons. First, there is some uncertainty over our ability to precisely replicate the procedure used by regulators both because the window we use to collect data on market capitalization may not be perfectly aligned and because of minor ambiguities in the screening procedures used to rule out certain stocks. Second, and more importantly, there was some room at the margin for discretion on the part of the exchanges, with little in the way of published detail.

To prevent this discretion from contaminating our discontinuity, we define our threshold as follows: (i) for each exchange and each vintage, we rank the full set of not-yet-marginable stocks that satisfy the screening criteria; and (ii) we then take the realized number of stocks actually included and set the threshold to be the index value of the stock with a ranking equal to that number. For example, if 113 stocks were included, the threshold is defined to be the index value for the 113th ranked stock, whether or not it was actually the lowest ranking stock included. We define C^k_E to be the threshold for vintage k in exchange E.

C.2 Fuzzy Regression Discontinuity

We quantify the direct effect of becoming marginable using a fuzzy regression discontinuity approach that accounts for the fact that the threshold does not perfectly predict marginability. We report two-stage least squares estimates, where the first stage is given by Equation 3 and the second stage is given by:

$$Ret^k_i = \gamma_{0l} + \gamma_{1l}(Index^k_i - C^k_E) + \gamma_{0r}Y^k_i + \gamma_{1r}[\tau^k_i \times (Index^k_i - C^k_E)] + \theta_k + v^k_i. \tag{14}$$

In words, we instrument for marginability (Y^k_i) with an indicator for being above the threshold (τ^k_i). Ret^k_i here represents 1, 3 or 12 month cumulative returns. Our coefficient of interest is γ_{0r}, which represents the direct impact of marginability on returns.

D Model Derivations

D.1 Result 1: A Simple Expression for the Price of Treated Stocks

Given the assumption of an exponential decay information structure, the price at time $t < 0$ (normalizing $Q = 1$ for simplicity) is [27]

$$p_t = p^* + \sum_{j=-\infty}^{t} m_j - \gamma\beta \sum_{j=t+1}^{0} \theta^j.$$

[27] The price for $t \geq 0$ is simply $p_0 = p^* + m$.

Recalling the normality and independence of the signals m_t, the expected price for treated stocks at any point $t \leq 0$ is:

$$\begin{aligned} E[p_t|m>0] &= p^* + E\left[\sum_{j=-\infty}^{t} m_j|m>0\right] - \gamma\beta \sum_{j=t+1}^{0} \theta^j \\ &= p^* + \beta \underbrace{\frac{\phi(0)}{\Phi(0)}\frac{1}{\sigma_m}}_{\lambda} \sum_{j=-\infty}^{t} \theta^j - \gamma\beta \sum_{j=t+1}^{0} \theta^j. \end{aligned}$$

If we define $\tilde{p} = p^* - \frac{\gamma\beta}{1-\frac{1}{\theta}}$, we may rewrite this as[28]

$$E[p_t|m>0] = \tilde{p} + \beta(\lambda+\gamma) \sum_{j=-\infty}^{t} \theta^j.$$

Finally, we can generalize the above so that it holds for all t (whether greater or less than 0):

$$E[p_t|m>0] = \tilde{p} + \underbrace{\beta(\lambda+\gamma)}_{\delta_1} \sum_{j=-\infty}^{0} \theta^j D_{t-j}, \tag{15}$$

where D_{t-j} is indicator equal to one for $j \leq t$ (equivalently, $t - j \geq 0$) and zero otherwise.

D.2 Result 2: A Panel Estimation Equation for Treated Stocks

Equation 10 follows because we may write:

$$p_{it}^{treated} = \tilde{p} + \delta_1 D_{it} + \delta_1 \sum_{j=-\infty}^{-1} \theta^{-j} D_{it-j} + \varepsilon_{it}.$$

Furthermore,

$$\begin{aligned} p_{it+1}^{treated} &= \tilde{p} + \delta_1 \sum_{j=-\infty}^{0} \theta^{-j} D_{it+1-j} + \varepsilon_{it+1} \\ &= \tilde{p} + \delta_1 \theta \sum_{j=-\infty}^{-1} \theta^j D_{it-j} + \varepsilon_{it+1}. \end{aligned}$$

[28] This follows from: $\gamma\beta \sum_{j=-\infty}^{0} \theta^j = \frac{\gamma\beta}{1-\frac{1}{\theta}}$.

Therefore:

$$\delta_1 \sum_{j=-\infty}^{-1} \theta^{-j} D_{it-j} = \frac{1}{\theta}(p_{it+1}^{treated} - \tilde{p} - \varepsilon_{it+1}).$$

Substituting this in the original expression gives the result.

D.3 Result 3: A Panel Estimation Equation for All Stocks

To see how control stocks can be incorporated to generate Equation 11 note first that we may write an analogue of Equation 9 for any t (with p^c representing the price in the control group):

$$p_{it}^{control} = p^c + \varepsilon_{it}.$$

Subtracting and adding $\frac{1}{\theta} p_{it}^{control}$ gives:

$$p_{it}^{control} = \left(1 - \frac{1}{\theta}\right) p^c + \underbrace{\frac{1}{\theta}}_{\delta_2} p_{it+1}^{control} + \underbrace{\varepsilon_{it} - \frac{1}{\theta}\varepsilon_{it+1}}_{e_{it}}.$$

Considering this alongside Equation 10 and letting α_i and η_t absorb the constant term and any individual or time-specific fixed effects gives Equation 11. Importantly, this should not suggest that the parameter θ has a meaningful structural interpretation in the context of control stocks. Given the IV strategy described in Subsection 4.4, θ is identified strictly off of variation *within* the treatment group.