Effects of Credit Expansions on Stock Market Booms and Busts *

Christopher Hansman
Imperial College London

Harrison Hong
Columbia University

Wenxi Jiang
Chinese University of Hong Kong

Yu-Jane Liu
Peking University

Juan-Juan Meng
Peking University

March 10, 2022

Abstract

Credit expansions are commonly associated with elevated stock market valuations. However, relative to other markets, isolating the causal relationship is challenging. Due to margin restrictions, easy credit often leaks into stock prices in difficult-to-measure ways. We address this by examining a large-scale deregulation in China that explicitly liberalized margin lending. Regression discontinuity and event study estimates show that this deregulation caused a sizable increase in the level of asset prices, which was largely anticipated by unconstrained investors. We develop an easy-to-estimate model of stock prices with anticipation to quantify the importance of expectations regarding financial liberalizations and credit expansions.

*We thank Jeremy Stein, Jiang Wang, Jiangze Bian, Zhi Da, Dong Lou, Emi Nakamura, Haizhou Huang, Matthieu Gomez, Yu Qin, Baolian Wang, Paymon Khorrami, Savitar Sundaresan and seminar participants at Seoul National University, CICF, Cavalcade Asia-Pacific meeting, Norges Bank, Aalto, INSEAD, the Summer Institute for Finance at SAIF, CIFFP at UIBES, the NBER Conference on the Chinese Economy at Tsinghua University and Columbia University for helpful comments, and Jingxuan Chen for excellent research assistance. This paper subsumes work previously circulated as *Riding the Credit Boom* and *Anticipating the Direct Effects of Credit Supply*. Please email inquiries to: c.hansman@imperial.ac.uk.

1 Introduction

Financial liberalizations and credit expansions are often associated with asset market booms and financial crises (e.g. Kaminsky & Reinhart, 1999; Borio & Lowe, 2002; Schularick & Taylor, 2012). Theories emphasize a causal "direct" effect: an expansion of lending to financially constrained households leading to asset price bubbles, the bursting of which contributes to financial fragility.[1] 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.[2]

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 ascribing stock market booms to loosening credit is challenging. Leverage restrictions may blunt straightforward pass-through, but easy credit can leak into asset prices in unexpected and difficult to measure ways.[3] Furthermore, given the relatively liquid nature of equity markets, foreseeable expansions may begin to impact prices well in advance of any change in credit supply. To credibly isolate causal impacts, we need a liberalization that explicitly targets leverage in the stock market—margin debt—and a strategy to deal with anticipation by unconstrained investors.

[1] A non-exhaustive list includes 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); optimism 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).

[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).

[3] Many commentators attribute the stock market collapse during the Great Recession 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).

We address this challenge by examining a recent credit-boom-gone-wrong in China, which was triggered by an unprecedented deregulation of margin debt. Starting in early 2010, regulators gradually allowed trading on margin for a series of successive *vintages* or sets of stocks. The liberalization was paired with a subsidy of bank lending to brokerages that effectively lowered the cost of margin lending. This fed a wave of margin, which rose from virtually nothing to a peak of roughly 4.5 percent of total market capitalization—more than 2 trillion yuan—in June of 2015. A boom in stock prices followed: the Shanghai Composite index rose from about 2000 in mid-2014 to a peak of 5166 in June, 2015 before crashing to 3709 within three weeks after the government unexpectedly halted the roll-out due to concerns about bubbles and financial fragility.

The Chinese episode is an ideal setting for our study for several reasons. The first is the historic size and targeted nature of the expansion of margin lending. The deregulation narrowly focused on the stock market, and was not part of a larger shift in credit supply. In general, there were not other obvious macroeconomics reasons for a dramatic appreciation in stock prices.[4] The second is the unique implementation of the liberalization. Margin lending was introduced for different vintages of stocks gradually over several years. For each vintage, qualification was determined on the basis of a published formula incorporating real-time data on market capitalization and trading volume. These features allow us to combine regression discontinuity and event-based strategies to isolate the direct impacts of the expansion and highlight the associated dynamics.

This paper has two key purposes. The first is to provide estimates of the direct impacts of the margin lending deregulation on the level of stock prices. The second is to emphasize—and structurally model—pervasive anticipation of these effects. As we show, failing to account for anticipation may lead researchers to overlook the role of credit expansions (particularly using event-based strategies), or to falsely attribute credit driven booms to demand by unconstrained investors.

Our analysis proceeds in three parts. We begin by providing descriptive evidence, based on standard event-study and difference-in-difference approaches, that the market anticipated sizable direct impacts of the introduction of margin lending. Across all vintages, asset prices rose consistently for soon-to-be marginable stocks in the months leading up to deregulation. There was little change after the formal start date. Anticipation was concentrated in high-ranked stocks within

[4] There can be speculative bubbles even in the absence of leverage when there is enough disagreement and speculation about growth prospects, as in the dot-com bubble (Scheinkman & Xiong, 2003; Hong *et al.*, 2006).

each vintage (those that investors could confidently predict would qualify for margin lending), with substantial purchasing by relatively liquid institutional investors and mutual funds. In other words, investors appear to have preempted the expansion of margin debt, exactly as theory would predict in a world where changes in credit are not entirely unexpected. Notably, the trends are moderate instead of a sharp step, as would be expected in a world where investors are not learning from a single announcement, but rather gradually resolving uncertainty about the existence, size, targeting, and extent of the deregulation. While suggestive of large effects, these anticipatory trends limit our ability to quantify the direct impact using event-based approaches.

In the second part, we isolate the direct effects themselves 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 year following the liberalization. This, in turn, corresponded to a non-trivial increase in asset prices. Our estimates suggest that 12 month cumulative returns were roughly 20 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.

Our final step is to build and estimate a competitive stock-pricing model with anticipation. This model allows us to better understand the effects of liberalizations for stocks away from the threshold, and to explicitly measure and account for anticipation by unconstrained investors. We extend the canonical dynamic model of persistent mispricing—the fads or noise trader model of Summers (1986) and De Long *et al.* (1990)—to allow for time-varying participation of otherwise constrained households. The purchases of these households capture the loosening of margin regulations and buying that drive up stock prices. Importantly, to capture anticipation, we allow unconstrained arbitrageurs to receive messages about the size of a coming credit expansion in advance.

We then show that with a reasonably flexible parametric assumption on the nature of these messages, our framework can be estimated with a straightforward linear dynamic panel model.

We address a generic endogeneity concern with a forward looking instrumental variables strategy in the vein of Malani & Reif (2015), Anderson & Hsiao (1981), and Arellano & Bond (1991). Estimates of the direct effect from our model match the results from our regression discontinuity approach: the margin lending deregulation led to a roughly 20 percent increase for treated stocks.

A key advantage of our model is that we are able to estimate underlying parameters governing the *rate* of anticipation. Our results suggest 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. This anticipation generates pronounced time-varying stock market volatility—variance rises as the date of the expansion draws near.

Furthermore, the estimated parameters allow us to construct counterfactuals that depend on the *timing* of a regulators action. As an example, we examine the unexpected halt of the margin lending expansion in 2015—due to financial fragility concerns—and consider the consequences of earlier or later interventions. Our counterfactuals suggest that more timely regulatory action could have prevented a significant anticipatory run-up in the set of stocks expected to next qualify for margin lending. This analysis underscores the importance of market expectations of credit expansions and shows that accounting for anticipation can be crucial for the design and implementation of prudential policy.

Our paper contributes to three strands of the literature. The first is to work on bubbles and crises, as reviewed in Allen & Gale (1999). As outlined above, isolating the relationship between financial liberalizations and stock market bubbles is particularly challenging. Our analysis of the Chinese boom between 2010-2015 provides causal evidence on the impacts of credit expansions on stock market valuations. We also contribute by showing the importance of anticipation in the context of these expansions. In stylized models with a binding constraint for all investors, prices may indeed not change until credit becomes available. This is not the case in a more heterogeneous economy with unconstrained agents.[5] Anticipation of credit supply shocks may be a valuable consideration in theoretical models of credit cycles (e.g. those following Kiyotaki & Moore, 1997; Geanakoplos, 2010).

[5]In this sense, our work complements recent research (e.g. Adelino *et al.*, 2016; Albanesi *et al.*, 2017; Kaplan *et al.*, 2020) that highlights the importance of beliefs and relatively unconstrained agents in the US housing boom that preceded the great recession.

Our second contribution is more narrowly to the literature on margin and stock prices (e.g. Eckardt & Rogoff, 1976; Hsieh & Miller, 1990; Hardouvelis & Peristiani, 1992; Jylhä, 2018). As noted in the review by Fortune (2001), a large fraction of studies have focused on changes in Fed margin requirements in the US, in general finding mixed results.[6] We contribute to this literature by providing clean regression-discontinuity based evidence in the context of a massive expansion in margin lending. This complements a series of other well identified empirical papers that have directly analyzed stock margin lending using regression discontinuity approaches (Kahraman & Tookes, 2017, 2020) or other credible designs (Foucault *et al.*, 2011). However, this research has not focused on the direct impacts on the level of asset prices, instead addressing effects on other features of the trading environment including liquidity, volatility, and stock comovement. A primary reason we are able to measure level effects on asset prices is the fact that the scale of margin debt in our context dwarfs that studied in previous work.

The third comes in in developing an easy-to-estimate model of anticipation that captures direct effects. The challenges of anticipation for difference-in-difference or event based designs have been noted at least since Ashenfelter (1978). More recent work (e.g. Malani & Reif, 2015; Freyaldenhoven *et al.*, 2019) has developed procedures that are robust to or incorporate pre-trends in estimation. We build on past work by proposing and estimating an information revelation based asset pricing model of anticipation that integrates pre-trends. This complements Borochin *et al.* (2021) which incorporates information from options prices to deal with anticipation in equity markets. Our approach can be applied to assets beyond the stock market, including housing and bonds.[7]

Our paper proceeds as follows. In Section 2 we provide background on the Chinese deregulation of margin lending and describe our data. In Section 3 we provide stylized facts regarding anticipation and the impact of the deregulation on asset prices. In Section 4 we implement our regression discontinuity approach. In Section 5 we introduce and estimate our information revelation based model of anticipation and provide counterfactuals. We conclude in Section 6.

[6]Excessive margin lending was widely blamed for the bubble that preceded the 1929 crash (Galbraith, 1961), and between 1934 and 1974 margins ranged from 45% to 100%. Since 1974 they have been 50%.

[7]In housing markets, investment home buyers play the analog of relatively unconstrained buyers in our model and have been implicated in the US housing bubble (Glaeser *et al.*, 2008; Haughwout *et al.*, 2011; DeFusco *et al.*, 2017; Nathanson & Zwick, 2018). In bond markets, trading by large investment firms are thought to play a role in the anticipation of asset purchase programs by central banks following the financial crisis, such as European Central Bank's 2015 Public Sector Purchase Programme. See `https://bankunderground.co.uk/2015/08/14/very-much-anticipated-ecb-qe-had-a-big-impact-on-asset-prices-even-before-it-was-officially-announced.`

2 Background and Data

2.1 China's 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 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 percent. Interest rates on margin loans from brokerage firms were generally around 8 to 9 percent annualized, significantly lower than the rates on shadow margin loans through informal channels (which typically ranged from 11 to 14 percent).[8]

The pilot itself was implemented in two stages. On February 13th, 2010, 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 as *Pilot B*.

The 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*.[9] 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

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

[9] See Article 28 in the rule released by the Shanghai Stock Exchanges.

and Rule 3.2 for Shenzhen;[10] (ii) ranking the remaining stocks according to the formula shown in Equation 1 below and (iii) selecting the top candidates in each exchange (with some discretion).[11]

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

This ranking rule, effectively a value weighted average of a stock's size and trading volume, was conducted separately in the Shanghai (SH) and Shenzhen (SZ) 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 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. 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 percent 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

[10] The criteria for both exchanges are the same: they require that stocks: (1) have been traded for more than three 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 percent of the turnover rate of the market index; (b) the average of the absolute value price changes more than 4 percent off of the market index; (c) market volatility higher than the market volatility by 500 percent; (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.

[11] Roughly 100 stocks were 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.

each of the three vintages, with announcement dates denoted by vertical lines. For each vintage, the quantity of margin debt reached 3-5 percent of the each vintage's market capitalization within a few months and ultimately peaked between 8 and 10 percent.

2.2 Data

We use stock price, trading, and financial information from CSMAR, excluding stocks on the Growth Enterprise Board (GEB). 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 (just before the crash). The majority of our analysis is conducted at the monthly level.

While the margin lending deregulation targeted households facing financial constraints, there are many institutional investors in China with relatively easy access to capital. We rely on two datasets to get at the trading behavior of these investors. The first is an analog of the 13-F quarterly institutional ownership filings in US markets used in studies of trading by institutional investors. While the data on institutional ownership in China is not as high quality as the data in the US, public companies in China do have to disclose the largest ten shareholders and their ownership in quarterly financial reports.

Our second measure of the holdings of unconstrained investors is based on mutual fund data from CSMAR. In China, mutual funds are required to report their stock holdings on a quarterly basis. For each stock, we calculate a Mutual Fund Ownership Share, which is the fraction of floating shares held by all mutual funds.

3 Stylized Facts on Anticipation of Direct Effects

We begin by providing reduced form evidence on the impact—and anticipation—of China's margin lending deregulation.

3.1 Anticipatory Pre-Trends in The Time Series

We use a series of standard event-study and difference-in-difference approaches to examine whether the impacts of the deregulation are evident in the time series for a broad class of stocks, including stocks whose eligibility could easily have been predicted in advance.

We show that unconstrained investors anticipated the introduction of margin lending. There were substantial returns, on average, for soon-to-be marginable stocks in the months prior to the introduction. We find no discernible increase in prices at or after the introduction of margin lending. In other words, the impact of margin lending appears to have been gradually incorporated in the form of strong pre-trends in stock prices. While these trends suggest that margin debt had a large impact on asset prices, they limit our ability to explicitly quantify the size of this effect using standard event-based approaches.

Event Study Comparing Marginable to Non-marginable Stocks. We first present simple event studies that compare marginable and non-marginable stocks following deregulation. 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) not marginable at the time margin debt was introduced for that vintage. We pool these together and consider cumulative returns in the period immediately following the official announcement/implementation of margin debt.[12] 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 DGTW adjusted return in the 1, 3 or 12 month window following the announcement/implementation month for vintage k.[13] 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-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 other non-marginable stocks. We cluster our standard errors at the stock level.

The first three columns of Table 2 show that the relative returns for newly marginable stocks were, if anything, slightly negative in the period immediately following marginability. We see no

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

[13]We follow a Daniel *et al.* (1997) (DGTW) style adjustment using independent sorts of quintiles of size and book-to-market to get 25 portfolios. Each stock is assigned to one of these 25 bins. The equal-weighted returns in each bin then serve as the benchmark for that stock's adjustment.

significant differential return in the first month following marginability, a marginally significant negative return of roughly 1.5 percent in the first three months, and a negative return of roughly 3 percent in the first 12 months.

These results are highly inconsistent with an *unexpected* direct effect caused by margin debt, which would generate positive returns. The fact that the coefficient is non-positive suggests that, for most stocks, either (i) the direct effect was priced in at the time of marginability or, (ii) there was no average impact of margin debt on asset prices (which does not seem plausible given our regression discontinuity estimates below and the large quantity of margin debt entering the market).

Indeed, Columns 4-6 in Table 2 suggest the former: the direct effect was anticipated and priced in by the time of the official announcement. In these specifications, we repeat the analysis shown in Equation 2 but consider cumulative DGTW 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 DGTW returns of roughly 1.5 percent in the month just prior to implementation, of 4.5 percent in the 3 months preceding implementation, and of over 20 percent 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 percent. In other words, the returns on soon-to-be marginable stocks were positive on average in the year leading up to marginability, suggesting that unconstrained investors differentially purchased these stocks in anticipation of the introduction of margin debt.

The results in Table 2 can be visualized vintage by vintage in Figure 3. Panel (a) plots the log of monthly market cap—after netting out stock, month, and book-equity decile fixed effects— and displays evidence of sustained increases in market cap for Vintages 1, 2 and 3 in anticipation of the introduction of margin debt. Panel (b) plots cumulative DGTW returns from March 2011 onwards, and displays virtually the same pattern.

One potential concern is that our results showing negative or flat returns in the period following marginability might be mechanically driven by anticipation in the set of not-yet-marginable stocks. Specifically, our effective control group for vintage k includes stocks in vintage $k+1$. Therefore, what we interpret as a negative or flat relative return for stocks in vintage k might simply be an artifact of a *positive* relative return for stocks in vintage $k+1$ due to anticipation. To rule out this concern, Appendix Table A.I repeats our analysis, but includes for each vintage k only (i) the set of stocks in vintage k and (ii) the set of stocks that never become marginable. As a result of this sample restriction, the control group does not include soon-to-become marginable stocks. The results for returns prior to marginability align with our main specifications. Furthermore, we find a null effect after marginability, reinforcing the conclusion that the impact of margin lending was largely priced in by the time of deregulation.

Difference-in-difference approach. We next verify the findings of our event study approach using a slightly generalized difference-in-difference approach. This allows us to first confirm that the effects of margin lending were largely anticipated and priced in—a standard difference-in-difference approach recovers no evidence of a direct effect—and to quantify and identify the presence of anticipatory pre-trends. The basic approach is to consider the period well before the roll-out took place as a pre-period, and to estimate separate difference-in-difference coefficients for (i) the months just before the roll-out took place (anticipatory effects), and (ii) the actual treatment period in which margin lending was active (ex-post effects).

This strategy can be seen most clearly in the following, which utilizes a monthly panel of all stocks (excluding those in the pilot programs) over our sample period:

$$Return_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (3)$$

Here Margin Trading Active$_{i,t}$ is an indicator equal to one if stock i is eligible for margin trading in month t. γ_i and δ_t represent stock i and month t fixed effects, respectively. The key to this approach is the inclusion of a series of dummies to allow differential effects for treated stocks in the period just before deregulation. These are captured by the indicators $D_{i,t+j}$, which are equal to one if margin trading initially becomes active for stock i in period $t+j$, and zero otherwise. Put more simply, $D_{i,t+j}$ is variable that, for a specific stock i, indicates that margin lending is about to roll-out. S captures the number of periods in advance investors might feasibly speculate upon the coming introduction of margin lending. The standard difference-in-difference approach is simply a special case in which we constrain $\beta_j = 0$ for $j > 0$.

As a dependent variable, we use monthly returns. As such, $\beta_1, ..., \beta_S$, which we refer to as *Ex-Ante Effects*, capture the average monthly return on soon-to-be-marginable stocks in the period leading up to marginability.[14] Similarly, β_0 captures the average differential return in the period *after* margin debt is available, which we refer to as the *Ex-Post Effect*. We consider both raw and DGTW returns.

The results shown in Table 3 are largely consistent with our event studies and Figure 3. As a baseline, the first and fourth columns show standard difference-in-difference approaches (with no allowance for anticipation). As before, we see no evidence of positive returns immediately following marginability, and actually find significant negative coefficients. In other words, a naive application of a difference-in-difference would suggest that there is no direct effect of margin debt.

The remaining columns account for anticipation: the second and fifth columns allow for three months of anticipation while the third and sixth columns allow for three quarters of anticipation.[15] In these specifications we again find no evidence of positive ex-post effects. Our estimated coefficients range from -0.7 percent to -2 percent per month.

Our estimated ex-ante effects indicate the presence of anticipation. Whether considering returns in the three months or three quarters leading up to marginability, we see consistently positive differential returns for soon-to-be marginable stocks. We estimate differential monthly returns as large as 2.0 percent (using raw returns) or 2.5 percent (using DGTW returns). One notable exception is that we see no impact in the month just prior to the introduction of margin debt, suggesting that the direct effect had already been priced in by this point. In general, the patterns here reaffirm the evidence for anticipatory pre-trends, i.e. that prices were driven up in advance of the introduction of margin debt by unconstrained investors.

Placebo Tests One potential concern is that the anticipation we estimate might be in part mechanical, driven by the ranking procedure used to select marginable stocks. To address this possibility, we use the same ranking procedure to construct and implement a series of placebo regressions and confirm that our results are not the mechanical consequence of the ranking criteria. Our basic

[14]More specifically, this is the average after differencing out the stock's average return in the period well before deregulation, and the average return of non-marginable stocks in the same period.

[15]For specifications that allow for three quarters of anticipation, we constrain β_j to be equal for observations within the same quarter.

approach is to randomly select placebo event dates 10,000 times, re-implement the ranking procedure for all placebo dates, and re-estimate our regression specifications from Table 3 for each. 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 3. Our results suggest that neither our ex-ante nor or ex-post effects are mechanically driven. We provide more detail on our placebo tests in Appendix B.

3.2 Further Evidence of Anticipation

We conclude this section by providing two additional pieces of evidence that the impacts of margin debt were anticipated by unconstrained investors and priced in.

Larger stock price pre-trends for higher ranked stocks. Some portion of the ex-ante uncertainty regarding the implementation of margin lending was over the precise set of stocks that would be included in each vintage. Given the rule used to select stocks, the inclusion of the highest ranking qualifiers should have been *relatively* more predictable ex-ante. If the patterns we observe are indeed driven by anticipatory speculation, we should therefore expect greater anticipation ex-ante for higher ranking stocks (among those that ultimately became marginable).

To capture this, we conduct a triple-difference version of Equation 3, further interacting Margin Trading Active$_{i,t}$ and all $D_{i,t+j}$ with an indicator equal to one if stock i is highly ranked within its vintage. We define highly ranked stocks to be those with above median rank among the set that ultimately qualified within each vintage and exchange. The results, presented in Appendix Table A.II, show significant positive coefficients on the interaction between ex-ante effects and our dummy variable for highly ranked stocks. In other words, high ranking stocks saw significantly greater pre-trends in the months prior to marginability. This suggests that unconstrained investors were indeed anticipating the introduction of margin lending (and that they were better able to anticipate the inclusion of high- versus low-ranking stocks).

Unconstrained-investor holdings and trades. We next ask whether unconstrained investors actually purchased soon-to-be marginable stocks in advance. We focus on the behavior of two groups of investors that we expect were relatively unconstrained even prior to the introduction of margin lending: (i) mutual funds and (ii) the largest holders of each stock—defined as the top ten investors by quantity of shares at the stock-quarter level.

There is strong evidence that these unconstrained investors increased their holdings in anticipation of the roll-out of margin lending. In Table 4 we display regression results following Equation 3, but replace the dependent variable with the share of ownership by unconstrained investors (defined as either mutual funds or the top 10 investors). The regressions are estimated at the quarterly level, corresponding to the frequency of our data on these investors. In Columns 1 and 3 we show traditional difference-in-difference specifications with no ex-ante effects. In Columns 2 and 4 we allow for three quarters of ex-ante effects. We find positive ex-ante effects in each of these three quarters, suggesting that mutual funds differentially increased their holdings in soon-to-be marginable stocks by 0.5-0.7 percentage points per quarter in the period leading up to deregulation. Similarly, the top 10 ownership share differentially increased by 3.7-4.3 percentage points per quarter. Furthermore, we find negative effects in the quarters after deregulation for both outcomes—on the order of 0.4 percentage points per quarter—suggesting that unconstrained investors sold once margin debt was available.

In the final two columns of Table 4 we repeat the exercise but include monthly stock level turnover as our dependent variable. We find elevated levels of trading in the period preceding marginability for soon-to-be marginable stocks, consistent with elevated trading levels due to anticipatory buying. In contrast to our results on unconstrained investors, we find positive and significant ex-post effects, suggesting that there was also differentially high turnover once margin debt became available. This too is consistent with formerly constrained investors buying from ex-ante unconstrained investors.

4 Regression Discontinuity Estimates of the Direct Effect

The stylized facts presented in Section 3 provide evidence that unconstrained investors anticipated a large impact of the margin lending deregulation. This anticipation makes it difficult to recover the magnitude of the effect using event-study or difference in difference approaches.

In this section we explicitly estimate the impact of the liberalization on stock prices using a regression discontinuity approach, focusing on the set of stocks close to a cut-off in the formula used to determine marginability (we refer to the output of this formula, which is shown in Equation 1, as the inclusion index). This allows us to isolate the effect of the deregulation from local cross-sectional comparisons that are arguably not subject to bias from anticipation. Because only a fixed number of stocks could be included in each vintage, a discontinuity exists at the value of the inclusion index held by the lowest ranking eligible stock. In principle, stocks to the right of this value qualified for margin debt, and stocks to the left did not. Furthermore, 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 to qualifying stocks can be plausibly viewed as an unexpected credit expansion in a small neighborhood around the cut-off.

Defining the inclusion index and marginability threshold. We start by recreating the inclusion index used by the regulators to determine marginability. 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 $k = \{1, 2, 3\}$.

The second step is to 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. This meant that stocks ranking well above the lowest included stock were occasionally excluded, and, similarly, stocks ranked low enough to be disqualified were occasionally included.

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; (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 100 stocks were included, the threshold is defined to be the index value for the 100th ranked stock, whether or not it was actually the lowest ranking stock included. We define C_E^k to be the threshold for vintage k in exchange E.

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. In Figure 4, we plot histograms of the inclusion index around C_E^k, which we normalize to 0 for both exchanges and all vintages. In Panel (a) we include the closest 100 stocks on each side of the threshold for each vintage and exchange and in Panel (b) we restrict the sample to values of the index within 1 of the threshold. Both panels show that the distribution of stocks is relatively smooth across the threshold. McCrary (2008) tests fail to reject the null hypothesis of no bunching around the cut-off.

A discontinuity in marginability and margin debt at the threshold. We now turn to showing that the threshold C_E^k is associated with a discontinuity in the probability that margin lending is introduced for a given stock. This is displayed most clearly in Panel (a) of Figure 5. In this figure, we once again include data from all vintages and exchanges with an index value within 1 of the threshold, normalizing C_E^k to 0. The x-axis represents the inclusion index, our running variable. On the y-axis, we display the probability that a stock becomes marginable. The scatter plot shows 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. A sharp jump in the probability of marginability is evident at the threshold.

To show this jump more formally, we take a standard regression discontinuity approach. That is, letting D_i^k be a dummy variable equal to one if stock i becomes marginable in vintage k, we estimate:

$$D_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + \varepsilon_i^k. \tag{4}$$

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 linear regressions with a rectangular kernel). 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 include standard errors based upon the three nearest neighbor variance estimators described in Calonico *et al.* (2014) (CCT).

In line with the pattern shown in Panel (a) of Figure 5, our regression results show a large and significant jump at the threshold. In column (1) of Table 5, $\hat{\alpha}_{0r}$ is 0.509 and significant at the 1% level, suggesting that being just to the right of the threshold is associated with a roughly 50 percentage point jump in the probability of marginability. Our estimates using a triangular kernel are nearly identical, at 0.496. The fact that these coefficients are smaller than 1 indicates the importance of discretion in the deregulation process.

Corresponding to the sharp increase in marginability, Panel (b) of Figure 5 shows that there is also a sharp increase in the total quantity of margin debt used to purchase stocks just above the threshold. This figure repeats the exercise shown in Panel (a), but includes the stock level quantity of margin debt—measured three months after implementation—on the y-axis. Once again, while there is some margin debt provided for stocks just below the threshold, we see a discrete jump at the threshold itself. Panel (c) shows the same pattern, this time scaling margin debt by market capitalization at the stock level.

To formalize these figures, columns (2) and (3) of Table 5 estimate the specification shown in

[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.

Equation 4, but replace the dependent variable with stock level margin debt (in (2)) and the ratio of margin debt to market capitalization (in (3)). Column (2) shows that crossing the marginability threshold generates a discrete jump of roughly 13 million yuan of margin debt, on average. Similarly, column (3) suggests that this jump is equivalent to about 1.7 percent of market capitalization, on average. Columns (5) and (6) repeat this exercise with a triangular kernel, showing similar results. All estimates are statistically significant, and together show that our threshold indeed corresponds to a credit supply shock. There was a discontinuous increase in the probability of marginability and the use of margin debt for stocks just above the threshold.

Direct Impacts: Price effects 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. Figure 6 shows plots similar to those in Figure 5. 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 only slightly higher than returns for those below the threshold. However, a large and statistically significant difference is evident for 3 month returns and persists through 12 month returns.

We provide reduced-form estimates of these effects using a regression discontinuity approach analogous to the one outlined in Equation 4:

$$Ret_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + e_i^k. \quad (5)$$

Here, Ret_i^k refers to the cumulative return for stock i in the 1, 3 or 12 months following the announcement of Vintage k. We consider both raw cumulative returns and DGTW adjusted returns. For our baseline specifications, we choose bandwidths and estimate standard errors exactly as in Table 5. In the Appendix we show a series of robustness exercises with varying bandwidths.

Our results, presented in Table 6, align with the plots shown in Figure 6. In the first column of the top panel, we see a small and only marginally significant impact of 2.7 percent on one month raw cumulative returns. However, by 3 months, we see highly statistically significant returns of just over 10 percent, suggesting a large price impact for stocks just above the threshold. These effects appear to persist through a year, as we see an impact on 12 month returns of 9.5 percent.

The remaining columns of the top panel show that these results are not dependent on the linear spline specification shown in Equation 5. We see statistically indistinguishable results allowing for local linear regressions with a triangular kernel on either side of the threshold, although the point estimates are marginally smaller (8.5 percent) at three months, and marginally larger (14.7 percent) at 12 months. Our estimates are similar when using DGTW adjusted returns rather than raw returns, as shown in the bottom panel. In Appendix Tables A.IV and A.V we show that these results are also not sensitive to the choice of bandwidth. For example, to using either the bandwidth selection procedure suggested by Imbens & Kalyanaraman (2012) or setting the bandwidth to 0.5 to allow comparability across specifications.

We next 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 4 and the second stage is given by:

$$Ret_i^k = \gamma_{0l} + \gamma_{1l}(Index_i^k - C_E^k) + \gamma_{0r}D_i^k + \gamma_{1r}[\tau_i^k \times (Index_i^k - C_E^k)] + \theta_k + v_i^k. \tag{6}$$

In words, we instrument for marginability (D_i^k) with an indicator for being above the threshold (τ_i^k). Ret_i^k continues to represent 1, 3 or 12 month cumulative returns. Our coefficient of interest is γ_{0r}, which represents the direct impact of marginability on returns.

Our results are reported in Table 7. Unsurprisingly, the qualitative patterns are in line with those presented in Table 6, with smaller returns at 1 month and sizeable and significant returns at 3 and 12 months. We see DGTW adjusted returns of 12-13 percent at three months, and 21-23 percent at 12 months. Similarly, we see raw cumulative returns of 17-18 percent at 3 months and 25-28 percent at 12 months.

These results indicate the presence of a sizeable 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 more than tripling of the market during our sample period, and of the large quantites of margin debt that flowed into these stocks.[17]

5 A Stock Pricing Model with Anticipation

The previous sections show evidence that the market anticipated the impact of the liberalization on stock prices and provide estimates of these direct effects in a small neighborhood around the eligibility threshold. In this section, we introduce a parsimonious competitive stock pricing model that allows us to capture the direct impacts for stocks away from the threshold, and 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. Furthermore, it provides a parameterization of the *rate* of anticipation itself. We show that our model-based estimates align closely with those from our regression discontinuity approach and highlight the importance of anticipation for market behavior and prudential policy during liberalization episodes.

5.1 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

[17] The demand elasticity of Chinese stocks implied from this RD estimate is similar to that retrieved from studies of how exogenous changes in passive indexing demand move stock prices (Chang *et al.* (2014), Wurgler & Zhuravskaya (2002)). Stocks in Vintages 1-3 were mid-cap stocks encompassing roughly 12 percent of total market capitalization—comparable to Russell 2000 stocks in the US market.

$T - 1$ since there are no further risks to owning the shares.

5.1.1 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 the 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 7 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 which 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 ($\gamma\sigma_\pi\Delta$ in our model) which is the degree to which prices jump after the event date. If the shock is totally unanticipated, a simple difference-in-difference or event study design would pick up the direct effect.

5.1.2 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[18]

$$m = \sum_{-n}^{0} m_t. \tag{7}$$

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:

$$p_t = p^* + \sum_{j=-n}^{t} m_j - \gamma\Big(\sum_{k=t+1}^{0} \sigma_k^2\Big)Q. \tag{8}$$

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 $m > 0$.[19] 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:

$$E[p_t|m > 0] = p^* + E\Big[\sum_{j=-n}^{t} m_j|m > 0\Big] - \gamma\Big(\sum_{k=t+1}^{0} \sigma_k^2\Big)Q. \tag{9}$$

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 to begin to rise in advance beginning at $t = -n$. This is captured by the expectation on the righthand side of Equation 9. Based on 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

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

[19] In this stylized continuous 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.

righthand side of Equation 9. Note that, in this context, the "ex-post" effect—the change in prices at or after deregulation—is not the direct effect, but simply the realization of the final signal m_0.

5.2 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 (7) to ∞ so that $m = \sum_{t=-\infty}^{0} m_t$).

We next place some 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

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

The price at time $t < 0$ (normalizing $Q = 1$ for simplicity) is then [20]

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

5.2.1 A Simple Expression for the Price of Treated Stocks

Now let us again consider anticipation and the price path for a set of treated stocks: those that ex-post were found to have received a positive margin lending shock. To model this, we again

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

consider a stock to be treated with a positive supply shock if $m > 0$.

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

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

If we define $\tilde{p} = p^* - \frac{\gamma\beta}{1-\frac{1}{\theta}}$, we may rewrite this as[21]

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

The presence of anticipatory pre-trends is immediately obvious from this expression. In each period t<0, prices of treated stocks rise by $\beta(\lambda+\gamma)\theta^t$. Naturally, the parameter θ captures the exponential rate at which prices rise.

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

$$E[p_t|m>0] = \tilde{p} + \underbrace{\beta(\lambda+\gamma)}_{\delta_1} \sum_{j=-\infty}^{0} \theta^j D_{t-j}, \tag{10}$$

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

5.2.2 Implications for Stock Prices: Time-varying Volatility

The red and blue lines to the left of the event date in Panel (a) of Figure 7 show 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

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

much earlier.

Panel (b) of Figure 7 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 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.

5.3 Panel Estimation Strategy

We now show that Equation 10 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 a given "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 10, we have:

$$p_{it}^{treated} = \tilde{p} + \delta_1 \sum_{j=-\infty}^{0} \theta^{-j} D_{it-j} + \varepsilon_{it}, \tag{11}$$

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:

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

Of course, in practice ε_{it} will also include any unmodeled stock and time specific factors not captured by the expression in Equation 10. Iterating Equation 11 forward one period and rearranging, we may rewrite this as:[22]

$$p_{it}^{treated} = \underbrace{\left(1 - \frac{1}{\theta}\right)\tilde{p}}_{\delta_0} + \delta_1 D_{it} + \underbrace{\frac{1}{\theta}}_{\delta_2} p_{it+1}^{treated} + \underbrace{\varepsilon_{it} - \frac{1}{\theta}\varepsilon_{it+1}}_{e_{it}}. \tag{12}$$

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

One could estimate this equation directly using only panel data on 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 of receiving a credit supply shock.

With such a control group, a natural generalization is a difference-in-difference version of Equa-

[22]Equation 12 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,

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

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.

tion 12 using a broader sample that includes both treated and control stocks:[23]

$$p_{it} = \delta_1 D_{it} + \delta_2 p_{it+1} + \alpha_i + \eta_t + e_{it}, \tag{13}$$

Here, D_{it} is an indicator equal to one *only* for treated stocks in the period after the credit supply shock occurs ($t \geq 0$). In other words, a fairly standard difference-in-difference treatment indicator. α_i and η_t represent stock and period fixed effects, respectively, allowing us to account for level differences across stocks and, crucially, for broader market movements.

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

[23]To see how control stocks can be incorporated to generate Equation 13 note first that we may write an analogue of Equation 11 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 12 and letting α_i and η_t absorb the constant term and any individual or time-specific fixed effects gives Equation 13. 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 5.4, θ is identified strictly off of variation *within* the treatment group.

5.4 Instruments

Estimation of Equation 13 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 13 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 11, 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:

$$E[\varepsilon_{it}|D_{it-1}, D_{it}, D_{it+1}, D_{it+2} \cdots] = 0. \tag{14}$$

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.[24] If Equation 11 is taken literally (i.e. signals m_{it} are the only source of idiosyncratic price fluctuations) then this restriction is be satisfied given the rational expectations assumptions of our model. More generally, this restriction is analogous to the assumptions in a standard difference-in-difference: 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.

[24] 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}.

5.5 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 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 8 shows an OLS version of Equation 13.[25] The OLS estimate of δ_1, 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 8 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 13. 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 of 0.18.

The final two columns of Table 8 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

[25] In all specifications we cluster at the stock level.

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.[26]

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—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 effect implied by these estimates ranges from 0.19-0.24. In other words, treated stocks cumulatively experienced a differential increase of 19-24 percent due to the introduction of margin lending, almost perfectly in line with the estimates of over 20 percent found using our RD approach. Note that our estimates also align with the magnitudes from back-of-the-envelope event-study specifications examining cumulative returns in the year prior to marginability (see the last column of Table 2).

5.6 Counterfactual: The Timing of A Crackdown on Margin Lending

As a final step, we conduct a set of simple counterfactual exercises that highlight the value of explicitly accounting for and parameterizing anticipation in our model. We consider the *timing* of the Chinese regulator's unexpected 2015 decision to halt the expansion of margin lending due to the growing stock market bubble. While an explosion in stock prices was evident in late 2014, margin lending continued unabated and the market continued to boom through the first half of 2015 (perhaps due to political expediency, as in the model of Herrera *et al.*, 2020). We argue that a portion of this boom was due to anticipation of a continued expansion. Consequently, accurately evaluating the impact of an earlier or later intervention requires an estimate of the rate of anticipation (as provided by our model).

Expectations of a Continued Expansion. After the stocks in Vintage 3 became marginable in September of 2014, market participants generally expected that the liberalization would continue

[26]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).

in a staggered fashion. While we cannot be certain, our assumption is that the expected date of the next round of liberalization was September of 2015, one year after the last liberalization. However, in mid-2015 regulators unexpectedly put a halt to any further expansion of margin lending—and simultaneously cracked down on both existing and shadow margin through informal exchanges (Bian *et al.*, 2017)—citing concerns over instability and the rapid increase in equity prices.

Given the evidence for widespread anticipation of earlier vintages from our reduced form analysis and model, it is natural to expect similar price patterns for stocks predicted to be in the next round, which we term *Expected Vintage 4*. Specifically, if the halt by regulators was a surprise, we would expect prices of stocks in Expected Vintage 4 to increase in anticipation of the continued roll out (before dropping sharply following the June 2015 halt to future expansions). Panel A of Figure 8 shows that this is indeed the case. To approximate the set of stocks in Expected Vintage 4, we sort all not-yet-marginable stocks according to the screening and ranking rule as of June 2015. We then select the top 100 stocks in each exchange, again excluding those on the growth enterprise board. The red line plots the average log market cap of these firms month-by-month (after residualizing all stocks in the data with respect to stock and month). The black line plots residualized log market cap for the full set of never marginable stocks (excluding those in Expected Vintage 4). As predicted, we see a relatively rapid increase in market cap for Expected Vintage 4 in the early part of 2015, with a sharp drop coinciding with the announcement. This drop precipitated a wider stock-market bust.

Counterfactual Timings of the Crackdown. We conduct two counterfactual exercises regarding this anticipatory path using our model estimates. We assume, for the sake of the exercise, that the parameters estimated in Subsection 5.5 (based on the first three vintages) continue to apply to the pace of information revelation in advance of Expected Vintage 4.

For our first exercise, we consider a counterfactual in which regulators halted expansion immediately after Vintage 3 stocks became marginable in September, 2014. We ask two questions: (i) what fraction of the anticipatory price impact had already occurred, and (ii) what fraction of the subsequent increase could have been avoided. Applying our estimates from column 2 of Table 9 ($\theta = 1.064$, $\delta_1 = 0.011$ Direct Effect$= 0.181$), the model indicates that 45 percent of the direct effect had already been priced in as of September 2014, and that 82 percent had been priced in by the time expansion was actually halted in June 2015. This suggests that roughly 45 percent of the total run-up for these stocks (1-45/82) could have been avoided by halting expansions of marginability in September 2014. Given the total market cap of the firms in Expected Vintage 4 of 1.22 trillion yuan at that point, this amounts to more than 70 billion yuan.

Second, we consider a counterfactual in which Expected Vintage 4 went ahead as planned, and the included stocks became marginable. We estimate that the market cap would have risen from 1.29 Trillion Yuan to nearly 1.33, implying that the early announcement avoided an additional increase of market cap of nearly 40 billion yuan. On the whole, our model indicates that the total direct effect of the roll-out of vintage 4, had it gone ahead un-impeded, would have been an increase from 1.13 Trillion yuan to 1.33 trillion yuan, a roughly 200 billion yuan increase.

Figure 8 Panel B displays the price path for a scenario in which expansions were halted in September 2014 (shown in black), a scenario in which they were halted in June of 2015 (shown in red), and a scenario in which they went ahead as planned (shown in blue). Alternatively, if the expansion was entirely unanticipated, we would expect flat lines for both the black and red scenario, and a sharp jump at the deregulation date in the blue scenario. These patterns highlight the potential costs of delaying action in the presence of anticipation and show one example of the value of quantifying the rate of anticipation.

6 Conclusion

We analyze the impact of China's 2010-2015 stock margin lending deregulation on the level of asset prices. We present reduced-form evidence of significant direct effects using a regression-discontinuity design based on the formula used by the Chinese government to determine eligibility for margin lending at the stock level. For stocks away from this threshold (for which the market could easily predict eligibility) we show that this direct effect was largely anticipated by unconstrained investors. We propose an information-revelation based model of anticipatory buying that can be used to recover direct effects from asset price trends before a deregulatory event. The model can be estimated using a simple linear dynamic panel approach. We use our model to highlight key properties of stock markets anticipating the credit effects of financial liberalizations.

References

ADELINO, MANUEL, SCHOAR, ANTOINETTE, & SEVERINO, FELIPE. 2016. Loan originations and defaults in the mortgage crisis: The role of the middle class. *The Review of Financial Studies*, **29**(7), 1635–1670.

ADRIAN, TOBIAS, & SHIN, HYUN SONG. 2010. Liquidity and leverage. *Journal of Financial Intermediation*, **19**(3), 418–437.

ALBANESI, STEFANIA, DE GIORGI, GIACOMO, & NOSAL, JAROMIR. 2017. *Credit growth and the financial crisis: A new narrative*. Tech. rept. National Bureau of Economic Research.

ALLEN, FRANKLIN, & GALE, DOUGLAS. 1999. Bubbles, crises, and policy. *Oxford Review of Economic Policy*, **15**(3), 9–18.

ALLEN, FRANKLIN, & GALE, DOUGLAS. 2000. Bubbles and crises. *Economic Journal*, **110**(460), 236–255.

ALLEN, FRANKLIN, BARLEVY, GADI, & GALE, DOUGLAS. 2022. Asset price booms and macroeconomic policy: A risk-shifting approach. *American Economic Journal: Macroeconomics*, **forthcoming**.

ANDERSON, THEODORE WILBUR, & HSIAO, CHENG. 1981. Estimation of dynamic models with error components. *Journal of the American statistical Association*, **76**(375), 598–606.

ARELLANO, MANUEL, & BOND, STEPHEN. 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *The review of economic studies*, **58**(2), 277–297.

ARELLANO, MANUEL, & BOVER, OLYMPIA. 1995. Another look at the instrumental variable estimation of error-components models. *Journal of econometrics*, **68**(1), 29–51.

ASHENFELTER, ORLEY. 1978. Estimating the effect of training programs on earnings. *The Review of Economics and Statistics*, 47–57.

BERNANKE, BEN, & GERTLER, MARK. 1989. Agency costs, net worth, and business fluctuations. *American Economic Review*, 14–31.

BIAN, JIANGZE, HE, ZHIGUO, SHUE, KELLY, & ZHOU, HAO. 2017. Leverage-Induced Fire Sales and Stock Market Crashes. *Working Paper*.

BORIO, CLAUDIO EV, & LOWE, PHILIP WILLIAM. 2002. Asset prices, financial and monetary stability: exploring the nexus. *Bank for International Settlements.*

BOROCHIN, PAUL, CELIK, MURAT ALP, TIAN, XU, & WHITED, TONI M. 2021. Identifying the heterogeneous impact of highly anticipated events: Evidence from the Tax Cuts and Jobs Act. *Available at SSRN 3806560*.

CALONICO, SEBASTIAN, CATTANEO, MATIAS D, & TITIUNIK, ROCIO. 2014. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, **82**(6), 2295–2326.

CALONICO, SEBASTIAN, CATTANEO, MATIAS D, FARRELL, MAX H, & TITIUNIK, ROCIO. 2018. Regression discontinuity designs using covariates. *Review of Economics and Statistics*.

Chang, Yen-Cheng, Hong, Harrison, & Liskovich, Inessa. 2014. Regression discontinuity and the price effects of stock market indexing. *The Review of Financial Studies*, **28**(1), 212–246.

Daniel, Kent, Grinblatt, Mark, Titman, Sheridan, & Wermers, Russ. 1997. Measuring mutual fund performance with characteristic-based benchmarks. *The Journal of finance*, **52**(3), 1035–1058.

De Long, J Bradford, Shleifer, Andrei, Summers, Lawrence H, & Waldmann, Robert J. 1990. Noise trader risk in financial markets. *Journal of political Economy*, **98**(4), 703–738.

DeFusco, Anthony A, Nathanson, Charles G, & Zwick, Eric. 2017. *Speculative dynamics of prices and volume*. Tech. rept. National Bureau of Economic Research.

Dell'Ariccia, Giovanni, & Marquez, Robert. 2006. Lending booms and lending standards. *The Journal of Finance*, **61**(5), 2511–2546.

Di Maggio, Marco, & Kermani, Amir. 2017. Credit-induced boom and bust. *The Review of Financial Studies*, **30**(11), 3711–3758.

Eckardt, Walter L, & Rogoff, Donald L. 1976. 100% margins revisited. *The Journal of Finance*, **31**(3), 995–1000.

Favara, Giovanni, & Imbs, Jean. 2015. Credit supply and the price of housing. *American Economic Review*, **105**(3), 958–92.

Fortune, Peter. 2001. Margin lending and stock market volatility. *New England Economic Review*, 3–25.

Foucault, Thierry, Sraer, David, & Thesmar, David J. 2011. Individual investors and volatility. *The Journal of Finance*, **66**(4), 1369–1406.

Freyaldenhoven, Simon, Hansen, Christian, & Shapiro, Jesse M. 2019. Pre-event trends in the panel event-study design. *American Economic Review*, **109**(9), 3307–38.

Galbraith, John Kenneth. 1961. *The great crash 1929*. Houghton Mifflin.

Geanakoplos, John. 2010. The leverage cycle. *NBER macroeconomics annual*, **24**(1), 1–66.

Gennaioli, Nicola, Shleifer, Andrei, & Vishny, Robert. 2012. Neglected risks, financial innovation, and financial fragility. *Journal of Financial Economics*, **104**(3), 452–468.

Glaeser, Edward L, Gyourko, Joseph, & Saiz, Albert. 2008. Housing supply and housing bubbles. *Journal of urban Economics*, **64**(2), 198–217.

Grundy, Bruce D, & McNichols, Maureen. 1989. Trade and the revelation of information through prices and direct disclosure. *The Review of Financial Studies*, **2**(4), 495–526.

Hardouvelis, Gikas A., & Peristiani, Stavros. 1992. Margin Requirements, Speculative Trading, and Stock Price Fluctuations: The Case of Japan. *The Quarterly Journal of Economics*, **107**(4), 1333–1370.

Haughwout, Andrew, Lee, Donghoon, Tracy, Joseph S, & Van der Klaauw, Wilbert. 2011. Real estate investors, the leverage cycle, and the housing market crisis. *Working Paper*.

HE, HUA, & WANG, JIANG. 1995. Differential information and dynamic behavior of stock trading volume. *The Review of Financial Studies*, **8**(4), 919–972.

HE, ZHIGUO, & KRISHNAMURTHY, ARVIND. 2013. Intermediary asset pricing. *American Economic Review*, **103**(2), 732–70.

HERRERA, HELIOS, ORDOÑEZ, GUILLERMO, & TREBESCH, CHRISTOPH. 2020. Political booms, financial crises. *Journal of Political economy*, **128**(2), 507–543.

HONG, HARRISON, SCHEINKMAN, JOSE, & XIONG, WEI. 2006. Asset float and speculative bubbles. *The journal of finance*, **61**(3), 1073–1117.

HSIEH, DAVID A, & MILLER, MERTON H. 1990. Margin regulation and stock market volatility. *The Journal of Finance*, **45**(1), 3–29.

HUANG, J., SHI, D., SONG, ZHONGZHI, & ZHAO, BIN. 2019. Discretionary Stock Trading Suspension.

IMBENS, GUIDO, & KALYANARAMAN, KARTHIK. 2012. Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *The Review of Economic Studies*, **79**(3), 933–959.

JYLHÄ, PETRI. 2018. Margin requirements and the security market line. *The journal of finance*, **73**(3), 1281–1321.

KAHRAMAN, BIGE, & TOOKES, HEATHER. 2020. Margin trading and comovement during crises. *Review of Finance*, **24**(4), 813–846.

KAHRAMAN, BIGE, & TOOKES, HEATHER E. 2017. Trader leverage and liquidity. *The Journal of Finance*, **72**(4), 1567–1610.

KAMINSKY, GRACIELA L, & REINHART, CARMEN M. 1999. The twin crises: the causes of banking and balance-of-payments problems. *American Economic Review*, **89**(3), 473–500.

KAPLAN, GREG, MITMAN, KURT, & VIOLANTE, GIOVANNI L. 2020. The housing boom and bust: Model meets evidence. *Journal of Political Economy*, **128**(9), 3285–3345.

KEYS, BENJAMIN J, SERU, AMIT, & VIG, VIKRANT. 2012. Lender screening and the role of securitization: evidence from prime and subprime mortgage markets. *The Review of Financial Studies*, **25**(7), 2071–2108.

KIYOTAKI, NOBUHIRO, & MOORE, JOHN. 1997. Credit Cycles. *Journal of Political Economy*, **105**(2), 211–248.

MALANI, ANUP, & REIF, JULIAN. 2015. Interpreting pre-trends as anticipation: Impact on estimated treatment effects from tort reform. *Journal of Public Economics*, **124**, 1–17.

MCCRARY, JUSTIN. 2008. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics*, **142**(2), 698–714.

MIAN, ATIF, & SUFI, AMIR. 2009. The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis. *The Quarterly Journal of Economics*, **124**(4), 1449–1496.

MINSKY, HYMAN P. 1977. The financial instability hypothesis: An interpretation of Keynes and an alternative to standard theory. *Challenge*, **20**(1), 20–27.

NATHANSON, CHARLES G, & ZWICK, ERIC. 2018. Arrested Development: Theory and Evidence of Supply-Side Speculation in the Housing Market. *The Journal of Finance*, **73**(6), 2587–2633.

SCHEINKMAN, JOSE A, & XIONG, WEI. 2003. Overconfidence and speculative bubbles. *Journal of political Economy*, **111**(6), 1183–1220.

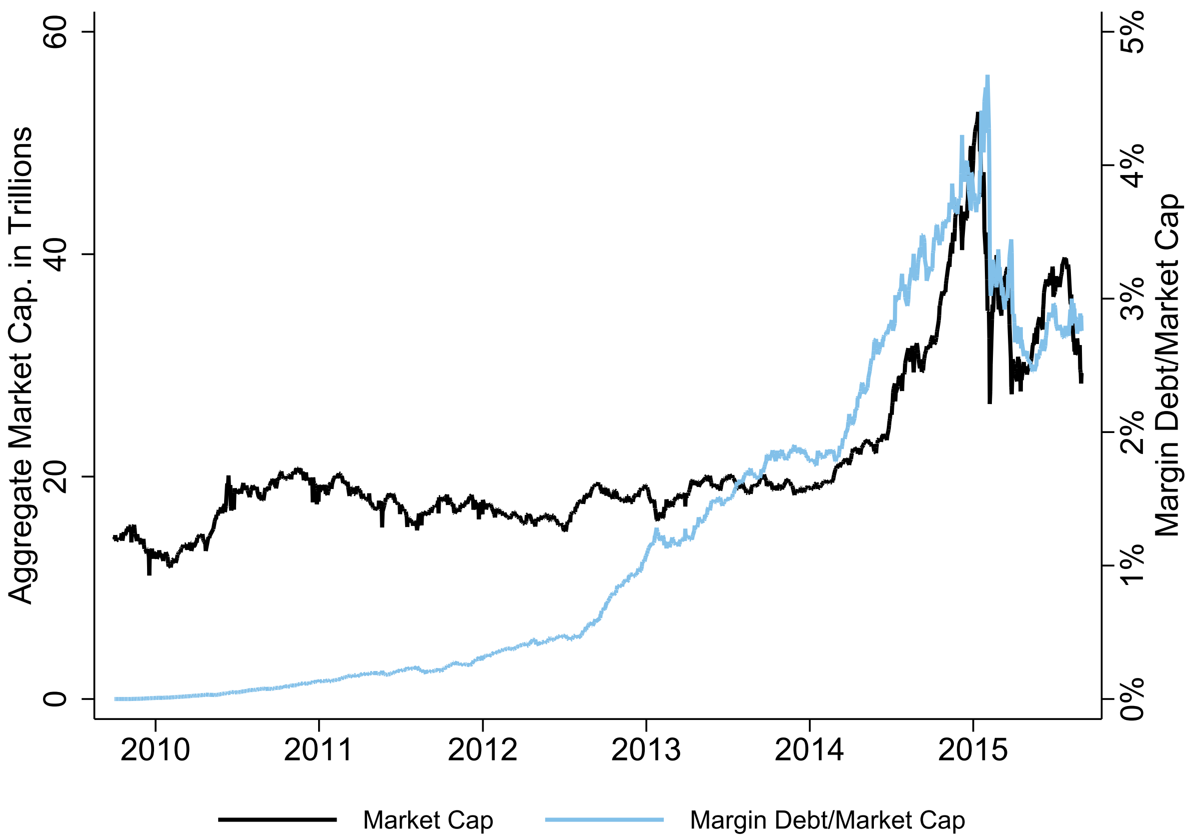
SCHULARICK, MORITZ, & TAYLOR, ALAN M. 2012. Credit booms gone bust: Monetary policy, leverage cycles, and financial crises, 1870-2008. *American Economic Review*, **102**(2), 1029–61.

SIMSEK, ALP. 2013. Belief disagreements and collateral constraints. *Econometrica*, **81**(1), 1–53.

SUMMERS, LAWRENCE H. 1986. Does the stock market rationally reflect fundamental values? *The Journal of Finance*, **41**(3), 591–601.

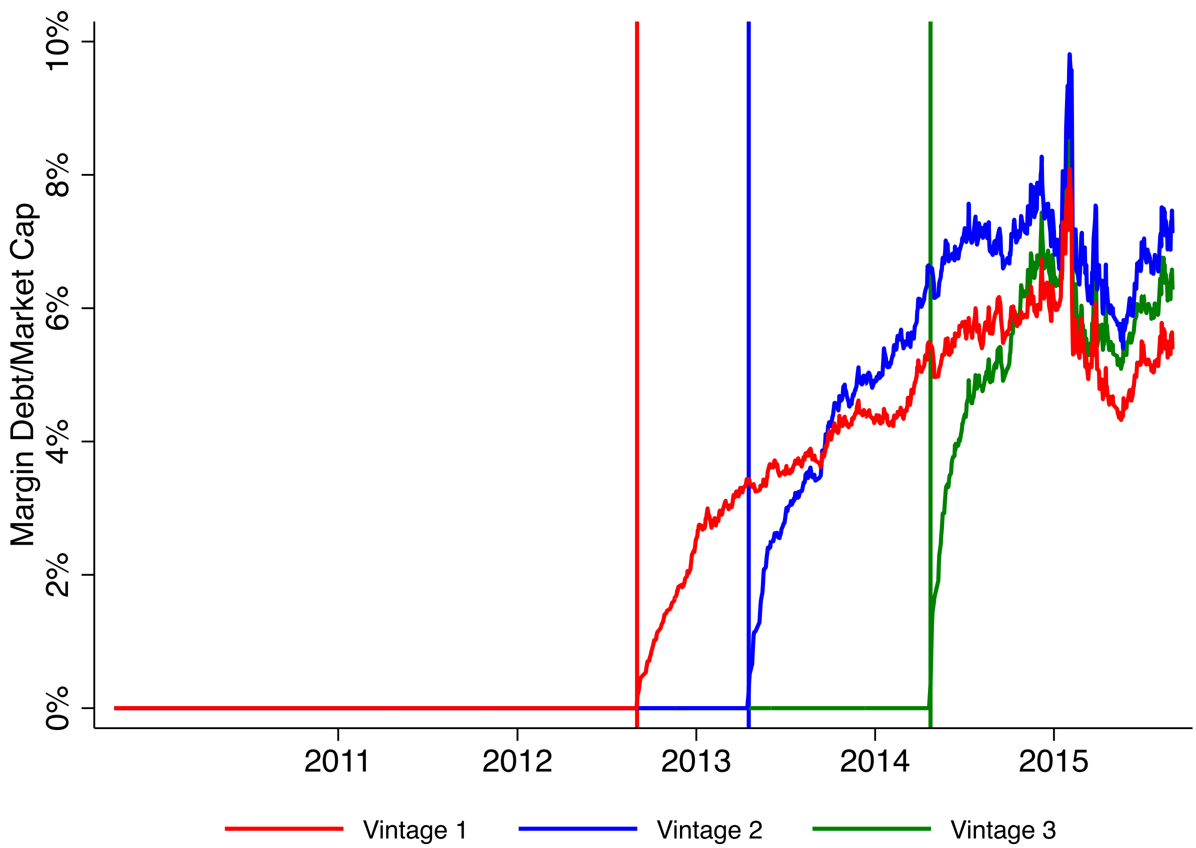
WURGLER, JEFFREY, & ZHURAVSKAYA, EKATERINA. 2002. Does arbitrage flatten demand curves for stocks? *The Journal of Business*, **75**(4), 583–608.

FIGURE 1: AGGREGATE MARKET CAP. AND MARGIN DEBT/MARKET CAP. OVER TIME



Notes: Plot shows daily aggregate market cap (in black) and the ratio of margin debt to market cap (in blue) for all stocks in sample. Both market cap and margin debt are measured in trillions of yuan.

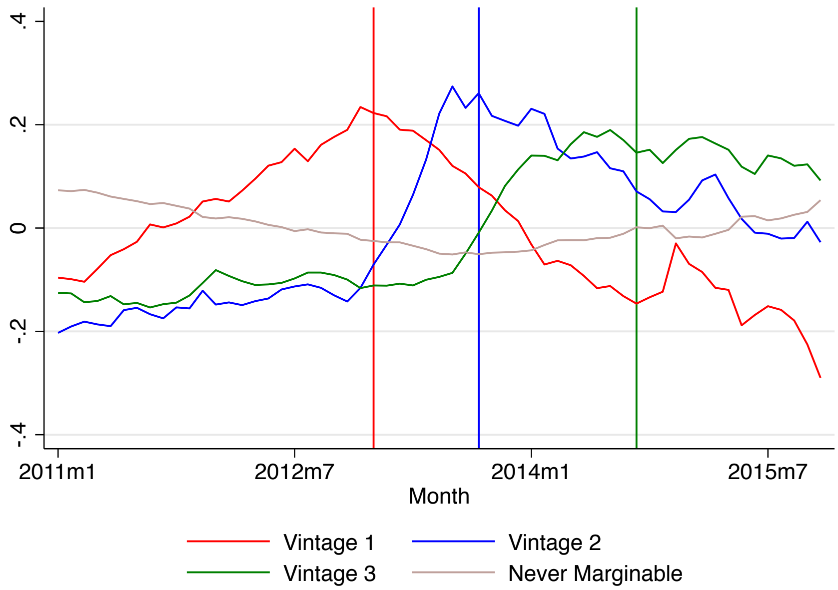
FIGURE 2: MARGIN DEBT/MARKET CAP. BY VINTAGE



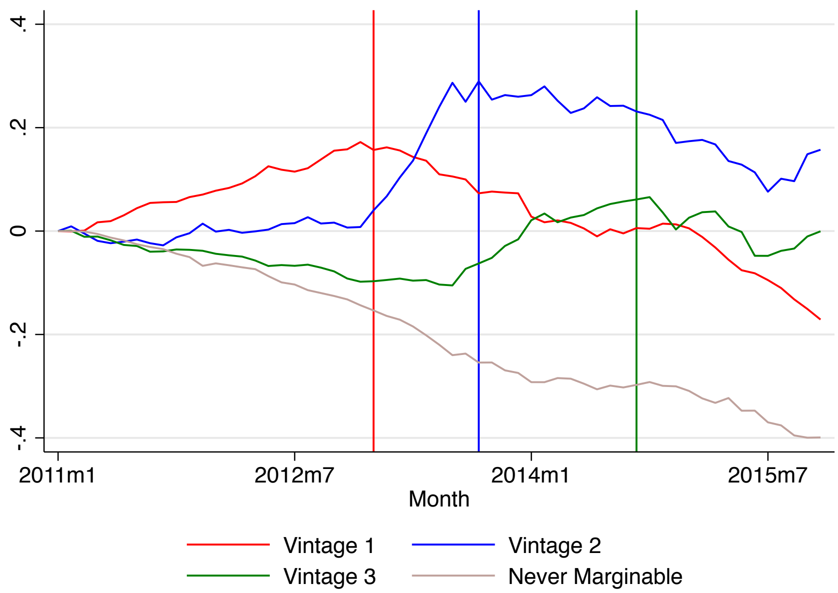
Notes: Plot shows the daily ratio of total margin debt to total market cap for each of the three vintages we study. Vertical lines denote starting dates of each vintage.

FIGURE 3: MARKET ANTICIPATION OF MARGIN LENDING ROLLOUT

(a) Residualized Log(Market Cap)



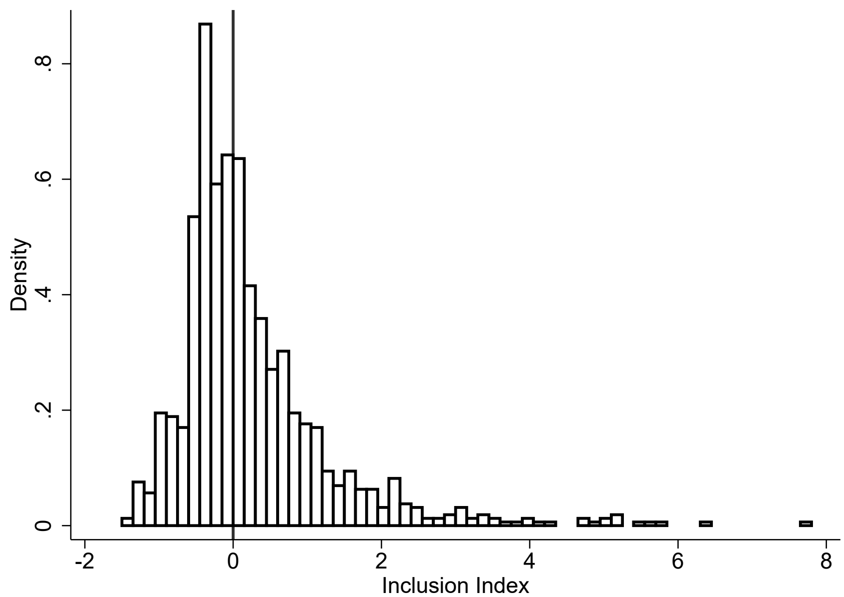
(b) Cumulative DGTW Returns



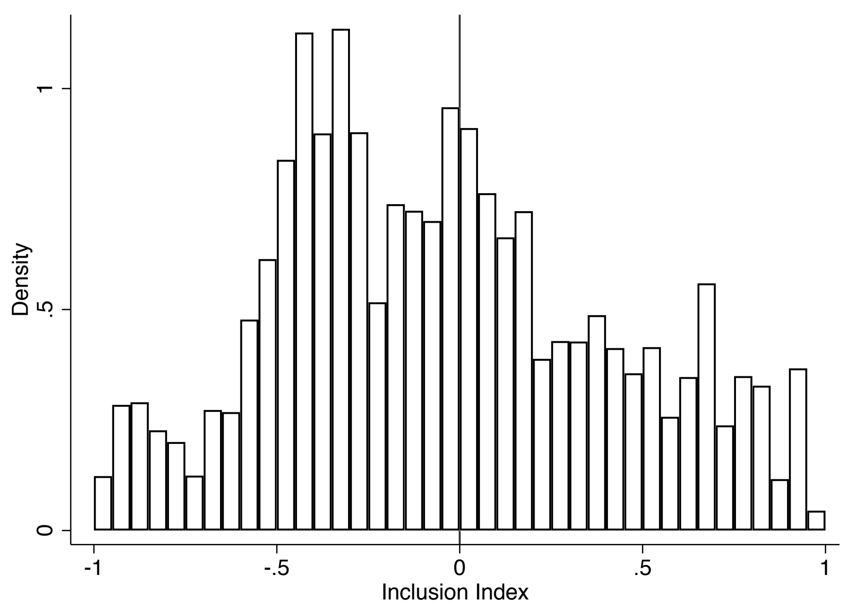
Notes: Panel (a) shows the vintage averages of residuals from a regression of Log(Market Cap) at the stock-month level on stock and month×year fixed effect using the period January 2011-September 2015. Panel (b) shows vintage average cumulative DGTW returns from January 2011 onwards. Vertical lines show the starting date of each vintage, with red, blue and green representing Vintages 1, 2 and 3, respectively. Brown lines represent never marginable stocks.

FIGURE 4: NO EVIDENCE OF BUNCHING AT THRESHOLD

(a) Full Distribution



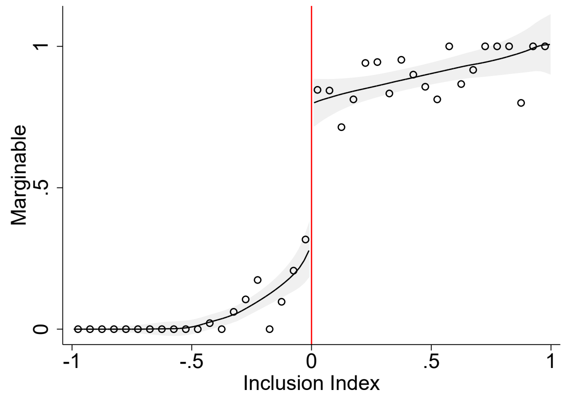
(b) Close to Marginability Threshold



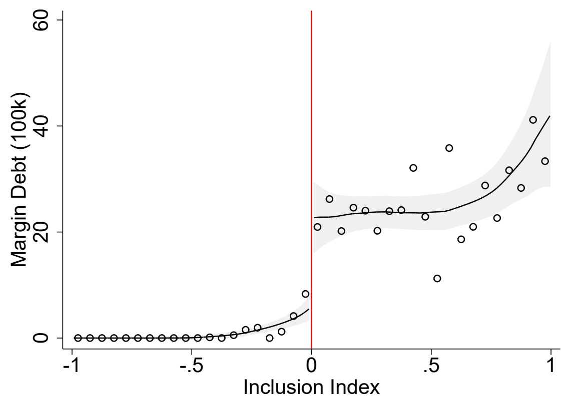
Notes: Both panels show histograms of the value of the inclusion index, normalized to the vintage specific threshold. In Panel (a), we include, for each vintage and exchange, the closest 100 stocks below the threshold and the 100 closest stocks above the threshold. In Panel (b), we further restrict the sample to show only the stocks in Panel (a) that additionally have a value of the inclusion index less than one in magnitude. McCrary tests give a t-statistic of -0.99.

FIGURE 5: INCLUSION INDEX DETERMINES MARGINABILITY

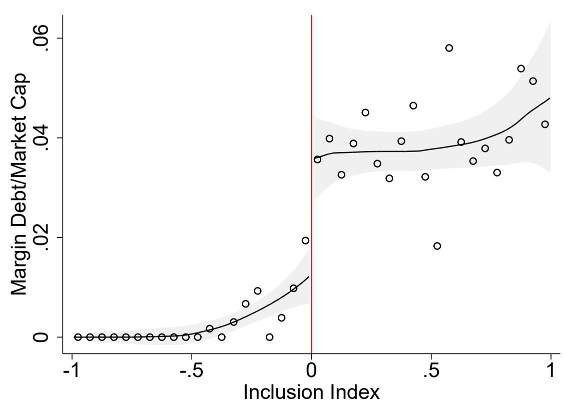
(a) Marginability



(b) Margin Debt



(c) Margin Debt/Market Cap

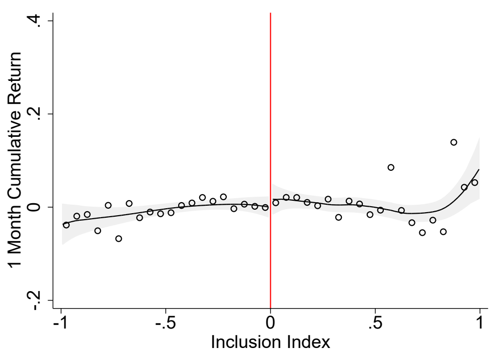


Notes: Indicator for marginability (Panel (a)), stock level margin debt (Panel (b)), and stock level ratio of margin debt to market cap (Panel (c)) plotted against inclusion index. Inclusion index normalized to set vintage specific threshold equal to 0. For each vintage, all not-yet marginable stocks 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.

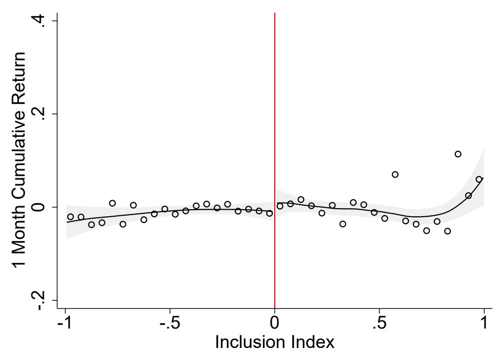
FIGURE 6: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD

PANEL A: 1 MONTH

CUMULATIVE RETURNS

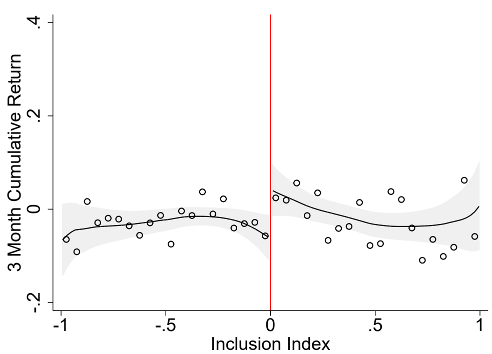


DGTW ADJUSTED CUMULATIVE RETURNS

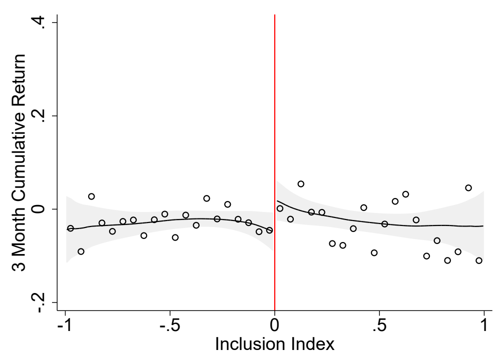


PANEL B: 3 MONTHS

CUMULATIVE RETURNS

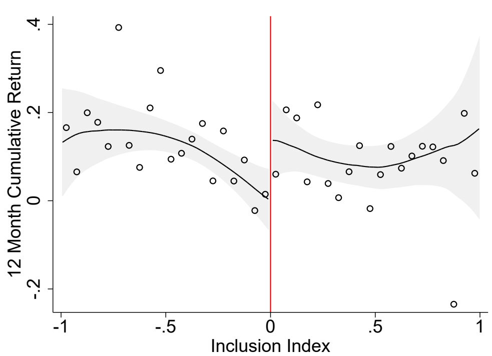


DGTW ADJUSTED CUMULATIVE RETURNS

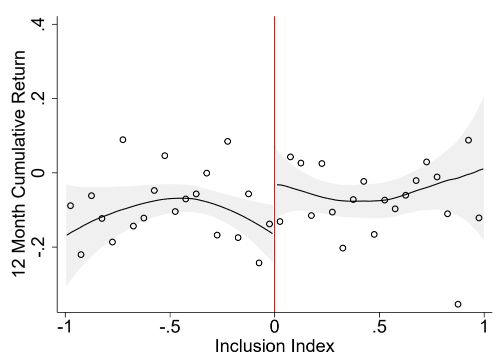


PANEL C: 12 MONTHS

CUMULATIVE RETURNS



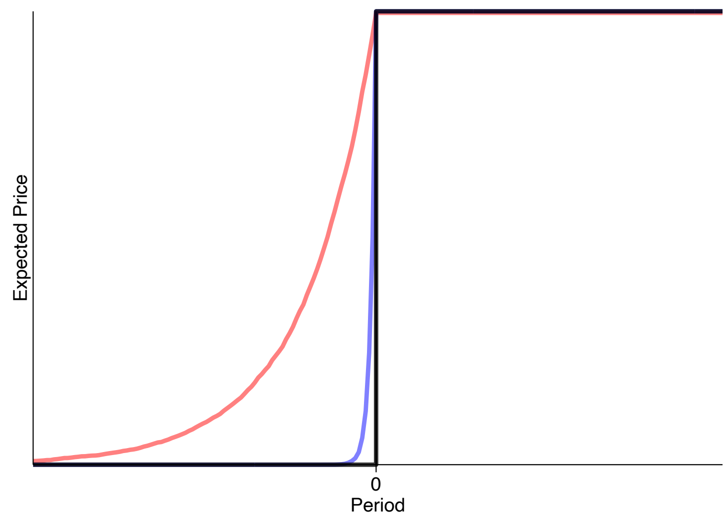
DGTW ADJUSTED CUMULATIVE RETURNS



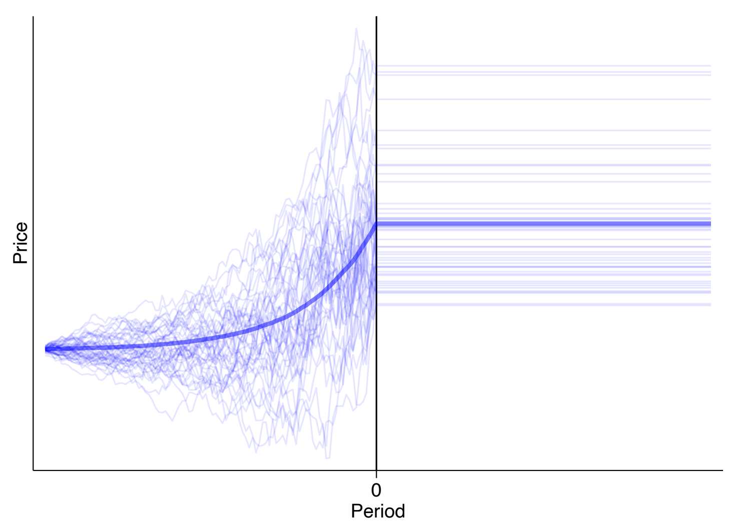
Notes: Cumulative and DGTW adjusted cumulative returns from marginability to 1, 3, or 12 months post-marginability. Inclusion index normalized to set vintage specific threshold equal to 0. Returns are adjusted for splits and dividends. For each vintage, all not-yet marginable stocks with inclusion index within 1 of the threshold at the time marginability was determined are included. 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 7: ANTICIPATION EFFECTS OF AN INCREASE IN CREDIT SUPPLY

(a) Varying the Rate of Anticipation



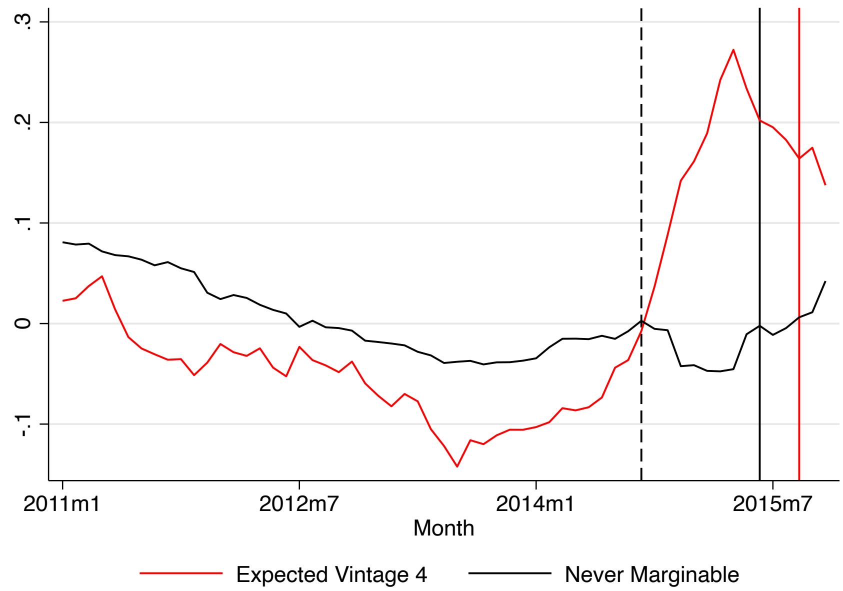
(b) Simulated Price Paths for Treated Stocks



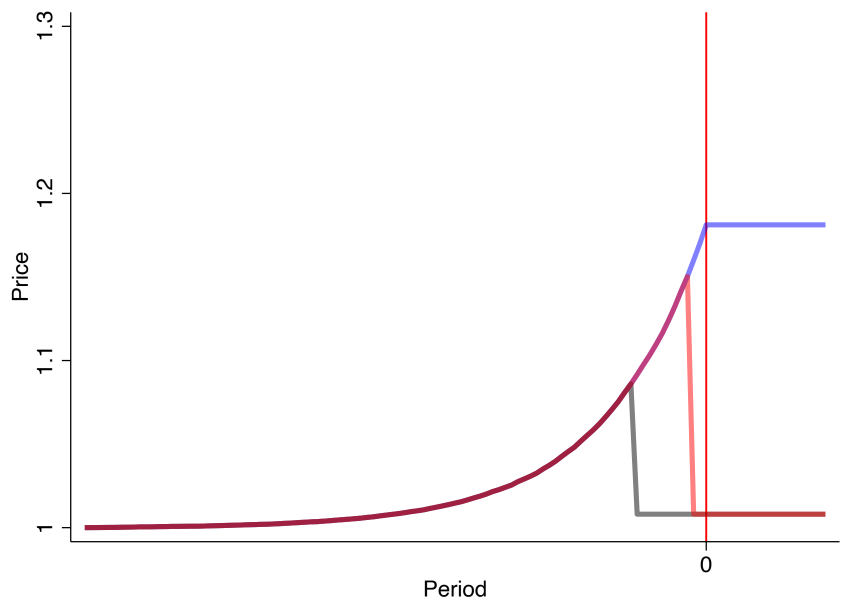
Notes: In both figures the y-axis displays price, the x-axis displays time, and the vertical line indicates the event date of a credit supply shock. Panel (a) shows the expected price path for stocks receiving a credit supply shock at time 0 under three regimes (holding the total price effect constant). 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 $\gamma = 0.2$, $\theta = 1.05$, $\beta = 1.43$.

FIGURE 8: THE TIMING OF A CRACKDOWN ON MARGIN LENDING

(a) Realized Price Path for Predicted Vintage 4



(b) Model Implied Price Path Under Three Scenarios



Notes: Panel A plots the stock and month residualized log market cap of stocks predicted to be in vintage 4 (in red) and all never marginable stocks (in black). Dashed vertical black represents the starting date for vintage 3, solid vertical black line represents the date of the margin lending crackdown, and the vertical red line represents our predicted starting date for vintage 4. Panel B shows the model implied price paths under three scenarios: (i) the expansion continuing as predicted (in blue), (ii) an unexpected reversal 3 months prior to the predicted starting date (in red), and (iii) an unexpected reversal 1 year prior to the predicted starting date (in black).

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

	Cumulative DGTW 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	-0.005	-0.014^{*}	-0.030^{**}	0.014^{***}	0.045^{***}	0.230^{***}	0.198^{***}
	(0.004)	(0.008)	(0.015)	(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 DGTW 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 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 returns from 12 months before to 12 months after introduction. For each of the three vintages determined by the screening and ranking rule, we compute cumulative DGTW returns adjusted for splits and dividends for the newly marginable stocks in that vintage as well as the set of contemporanously non-marginable stocks. All specifications include dummy variables for vintage as a control. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3: MARKET ANTICIPATION OF MARGINABILITY

	Unadjusted Returns			DGTW Returns		
		Monthly Lags	Quarterly Lags		Monthly Lags	Quarterly Lags
Ex-Post Effect	-0.021^{***}	-0.020^{***}	-0.018^{***}	-0.011^{***}	-0.010^{***}	-0.007^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.001]	*[0.001]*	*[0.006]*	*[0.000]*	*[0.000]*	*[0.020]*
Ex-Ante Effect (t-1)		-0.006	0.012^{***}		-0.002	0.016^{***}
		(0.005)	(0.003)		(0.004)	(0.003)
		[0.703]	*[0.043]*		*[0.671]*	*[0.000]*
Ex-Ante Effect (t-2)		0.020^{***}	0.011^{***}		0.017^{***}	0.012^{***}
		(0.005)	(0.003)		(0.005)	(0.002)
		[0.097]	*[0.042]*		*[0.009]*	*[0.001]*
Ex-Ante Effect (t-3)		0.017^{***}	0.013^{***}		0.025^{***}	0.015^{***}
		(0.005)	(0.003)		(0.005)	(0.003)
		[0.075]	*[0.059]*		*[0.000]*	*[0.001]*
Mean of Dep. Var.	0.0144	0.0144	0.0144	-0.00614	-0.00614	-0.00614
N	126131	126131	126131	126131	126131	126131

Results from difference-in-difference regressions of stock level log monthly returns on marginability. For our difference-in-difference specifications we report coefficients from the following regression

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

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. The number of *ex-ante effect* coefficients indicates the value of S for the regression in question. The first and fourth columns includes 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 months or three quarters leading up to the roll-out for each stock. Sample covers March 2009-May 2015. The left three columns show cumulative log returns adjusted for splits and dividends but otherwise unadjusted. The right three columns show DGTW adjusted returns. 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 4: 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 5: 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	0.263	0.274	0.326	0.294	0.315
N	350	323	329	400	351	383

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 third 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. 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 6: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: REDUCED FORM

	Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.027*	0.103***	0.095*	0.020	0.085**	0.126**
	(0.015)	(0.036)	(0.056)	(0.014)	(0.036)	(0.049)
P-Value	0.064	0.004	0.088	0.149	0.017	0.010
CCT Robust P-Value	0.076	0.003	0.151	0.155	0.019	0.015
Bandwidth	0.360	0.312	0.292	0.476	0.394	0.458
N	438	378	323	590	472	516
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.022	0.073**	0.147**	0.020	0.060*	0.122**
	(0.014)	(0.032)	(0.059)	(0.013)	(0.031)	(0.053)
P-Value	0.125	0.024	0.013	0.126	0.056	0.022
CCT Robust P-Value	0.167	0.018	0.027	0.142	0.052	0.035
Bandwidth	0.387	0.313	0.305	0.484	0.434	0.442
N	466	382	342	593	524	497

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 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 include 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. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 7: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: FUZZY RD

	Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.034	0.179^{**}	0.280^{***}	0.039	0.168^{**}	0.246^{**}
	(0.030)	(0.071)	(0.109)	(0.027)	(0.073)	(0.105)
P-Value	0.261	0.011	0.010	0.145	0.021	0.019
CCT Robust P-Value	0.197	0.010	0.007	0.141	0.022	0.017
Bandwidth	0.294	0.324	0.361	0.435	0.408	0.486
N	350	394	403	532	495	546
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.043	0.132^{**}	0.209^{**}	0.038	0.119^{*}	0.233^{**}
	(0.027)	(0.063)	(0.103)	(0.025)	(0.065)	(0.109)
P-Value	0.118	0.037	0.043	0.134	0.066	0.033
CCT Robust P-Value	0.088	0.029	0.021	0.132	0.054	0.022
Bandwidth	0.328	0.326	0.455	0.482	0.422	0.519
N	401	394	513	593	507	572

Fuzzy 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 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 include 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. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: 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

TABLE A.I: EVENT STUDY OF MARGINABILITY: CUMULATIVE DGTW RETURNS

	Comparing Each Vintage to Never Marginable Stocks						
	Preceding Marginability			Following Marginability			Before vs. After
	-1 to 0	-3 to 0	-12 to 0	0 to 1	0 to 3	0 to 12	-12 to 12
Marginable	0.018^{***}	0.045^{***}	0.243^{***}	-0.003	-0.005	0.007	0.249^{***}
	(0.005)	(0.008)	(0.014)	(0.004)	(0.008)	(0.016)	(0.022)
Mean of Dep. Var.	-0.0110	-0.0184	-0.0638	-0.00547	-0.0263	-0.102	-0.154
N	3944	4338	3784	4026	3906	3677	3554

First three columns show results from regressions of cumulative DGTW 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. Columns 4-6 show results from regressions of cumulative returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the introduction of margin debt. 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 never marginable stocks. We compute cumulative DGTW returns adjusted for splits and dividends. All specifications include dummy variables for vintage as controls. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.II: MORE ANTICIPATION FOR HIGH RANKED STOCKS

	Unadjusted Returns		DGTW Returns:	
		Quarterly Lags		Quarterly Lags
Ex-Post Effect	-0.018^{***}	-0.015^{***}	-0.008^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Ex-Post Effect × High Rank	-0.007^{***}	-0.005^{***}	-0.005^{**}	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Ex-Ante Effect (t-1)		0.013^{***}		0.016^{***}
		(0.003)		(0.003)
Ex-Ante Effect (t-1) × High Rank		-0.002		0.000
		(0.005)		(0.005)
Ex-Ante Effect (t-2)		0.005		0.005^{*}
		(0.003)		(0.003)
Ex-Ante Effect (t-2) × High Rank		0.013^{**}		0.014^{***}
		(0.005)		(0.005)
Ex-Ante Effect (t-3)		0.008^{**}		0.011^{***}
		(0.004)		(0.004)
Ex-Ante Effect (t-3) × High Rank		0.010^{*}		0.010^{*}
		(0.005)		(0.005)
Mean of Dep. Var.	0.0144	0.0144	-0.00614	-0.00614
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 DGTW adjusted returns. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.III: CROSSING MARGINABILITY THRESHOLD PREDICTS MARGIN DEBT: DIFFERENT BANDWIDTHS

	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
	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

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. Top panel employs Imbens and Kalyanaraman bandwidth, while bottom panel sets 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.IV: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: IK BANDWIDTH

	Cumulative Returns					
	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
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.013	0.050^{*}	0.086^{*}	0.018	0.060^{*}	0.111^{**}
	(0.012)	(0.028)	(0.045)	(0.013)	(0.031)	(0.046)
P-Value	0.302	0.074	0.057	0.142	0.055	0.016
CCT Robust P-Value	0.094	0.084	0.015	0.171	0.341	0.047
Bandwidth	0.589	0.446	0.689	0.589	0.446	0.689
N	679	538	671	679	538	671

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 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 include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use the Imbens and Kalyanaraman bandwidth. 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. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.V: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: BANDWIDTH=0.5

	Cumulative Returns					
	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
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.047*	0.111**	0.020	0.057*	0.119**
	(0.013)	(0.027)	(0.049)	(0.013)	(0.029)	(0.051)
P-Value	0.226	0.083	0.023	0.129	0.053	0.019
CCT Robust P-Value	0.126	0.070	0.056	0.228	0.229	0.167
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 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 include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use a bandwidth of 0.5. 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. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 3 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.[27] 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.

With these vintages and our randomly selected event dates in hand, we re-run the regressions in Table 3 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 3 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 3. 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.

[27] As in Section 4 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-post effects range from 0.000 to 0.020, suggesting that the treatment effect was more negative than at least 98 percent of the placebo estimates. For our quarterly lag specifications, the p-values hover near 0.05 for unadjusted returns, and near 0.001 for DGTW returns. The latter means that our estimated quarterly ex-ante effects using DGTW returns are larger than 99.9 percent of our placebo coefficients.