Riding the Credit Boom*

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Abstract

Research on leverage and asset prices emphasizes the direct effect of bank lending enabling excessive risk-taking by financially-constrained investors. Studies implicitly focus on a special setting whereby agents are myopic and fail to anticipate higher prices resulting from credit booms. Using China's staggered liberalization of stock-margin lending from 2010-2015—which resulted in a bank/brokerage-credit-fueled stock-market bubble—we show that unconstrained investors speculated on likely-to-qualify-for-lending stocks, amplified volatility and increased constrained-household fragility. The parallel-trends criterion underlying empirics is unnecessarily restrictive in general non-myopic settings. We decompose direct versus anticipatory effects and show how policy implications differ.

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1 Introduction

An important macro-finance literature associates leverage cycles with asset price boom-bust patterns—typically using panel regressions that exploit cross-country and long time-series variation (see, e.g., Borio & Lowe, 2002; Schularick & Taylor, 2012).[1] Prominent historical examples include the rise of margin lending in the U.S. stock market preceding the Great Depression (Galbraith, 2009) and the growth of loan-to-value ratios in the U.S. housing market preceding the Great Recession of 2008. This literature has understandably attracted considerable interest from central bankers and other financial market regulators.

Theories addressing these empirical patterns emphasize a "direct" effect: an expansion of bank lending to financially constrained households generating higher asset prices and financial fragility through a variety of mechanisms. A non-exhaustive list includes (1) complacent or neglectful creditors underestimating downside or tail risk (Minsky, 1977; Kindleberger & O'Keefe, 2001; Gennaioli *et al.* , 2012); (2) reckless lending in the form of lax screening of naive investors (Dell'Ariccia & Marquez, 2006; Keys *et al.* , 2012); (3) leverage cycles (Geanakoplos, 2010; Simsek, 2013); and (4) intermediary frictions or balance sheets (Bernanke & Gertler, 1989; Kiyotaki & Moore, 1997; Adrian & Shin, 2010; Brunnermeier & Sannikov, 2014). A key element of all such narratives is the notion that new buying by previously constrained households is responsible for price movements.

Following these theories, empirical work studying the causal effect of credit supply on asset prices generally utilizes deregulatory changes that increase credit supply for some group (treatment) and not others (control). To identify a direct effect, these studies verify a parallel-trends assumption, that is, they ensure that asset prices for the treated and control groups trend similarly before the deregulatory event date (see, e.g., Favara & Imbs, 2015; Di Maggio & Kermani, 2017). Policy work, in turn, focuses on the right level of leverage regulation to attenuate these direct effects.

Our paper points out that direct effects are only half the story. Theoretically, if the direct credit-supply effect is large—as is the premise of existing theoretical and empirical work—then anticipatory speculation by agents becomes ex-ante profitable. In the context of stock markets, speculation by unconstrained and sophisticated investors such as hedge funds and mutual funds can lead to overshooting and greatly magnify asset price movements (De Long *et al.* (1990), Lakonishok *et al.* (1992), Hong & Stein (1999), Abreu & Brunnermeier (2003), Stein (2009)). Existing empirical research has at least anecdotally implicated unconstrained investors in riding booms such as the Internet Bubble of the late 1990s (Brunnermeier & Nagel, 2004; Griffin *et al.* , 2011) or the South Sea Bubble two centuries before (Temin & Voth, 2004). Similar effects

[1] An antecedent literature in emerging markets associates banking crises with currency crises and international financial market contagion (see, e.g., Kaminsky & Reinhart (1999)). Recent contributions also include Jordà *et al.* (2013), Mian *et al.* (2017), and Baron & Xiong (2017). Credit booms might also be measured using credit spreads as opposed to leverage ratios (i.e., Krishnamurthy *et al.* (2015) and López-Salido *et al.* (2017)).

may be found in real estate markets where agents, both first time home buyers and investors, are able to time their purchases in anticipation of a boom (see, e.g., Glaeser *et al.* , 2008; Haughwout *et al.* , 2011; Choi *et al.* , 2016; DeFusco *et al.* , 2017).

However, by requiring parallel trends, empirical studies implicitly focus on special settings in which agents are "myopic" and fail to anticipate higher prices resulting from credit booms. In general, this criterion is unnecessarily restrictive and will only hold in particular contexts where (1) there are no agents who can time their purchases; (2) agents are explicitly myopic; (3) there are substantial frictions to timing investments or arbitrage, or (4) the deregulatory (credit supply shift) events are entirely unpredictable. While necessary for implementing standard difference-in-difference designs, the parallel trends criterion eliminates the possibility of studying the rich and economically meaningful patterns generated by speculation. We often expect asset prices to rise in anticipation of credit supply shocks and hence should focus on identification criteria that are valid in general non-myopic settings.

This is particularly the case because extrapolating from myopic settings or studies is problematic from a policy perspective. Anticipatory speculation may generate excess price volatility and lead constrained households to buy assets at higher prices relative to a myopic world. If the dangers of credit booms stem from speculation in anticipation of credit—rather than the extension of new credit per-se—then estimates of the costs and benefits of macro-prudential regulations on leverage or credit will be biased. Furthermore, in contexts where anticipation contributes to credit-booms gone wrong, anti-speculative measures may be productively included in the macro-prudential policy toolkit.

We propose measuring three distinct empirical objects related to credit expansions: (i) anticipatory effects, or ex-ante changes in asset prices in the lead up to a credit boom, (ii) direct effects, the ex-post changes in asset prices that would occur in the absence of anticipation, and (iii) overshooting, or the degree to which ex-ante speculation causes ex-post asset prices to *exceed* the level implied by the direct effect.

We utilize the recent credit cycle in China as an example of a non-myopic setting to demonstrate the implications of this proposal for methodology and policy. From 2010 to 2015 the Chinese stock market received a large credit supply shock as a result of the government liberalization of margin lending. In contrast to margin deregulations in other countries, the Chinese government actively encouraged government-owned banks and brokerage firms to lend to households for stock purchases. As a result, there was a historically rapid expansion of margin debt, peaking at 3.5% of GDP and 4% of market capitalization (see Figure 1). At the peak, nearly 2 trillion yuan of margin loans were supplied to Chinese households. Since China has stringent short-sales constraints, speculation and credit gave rise to a bubble (Scheinkman & Xiong (2003), Geanakoplos (2010)), which subsequently gave way to a crash and government bailouts. In other words, the Chinese stock market had a credit-fueled speculative stock market bubble that—given the lack of corresponding productivity increases in China during this period—is suggestive of the "direct" effects narrative found in the literature.[2]

The Chinese experience provides a particularly clean context in which to study the impacts of credit supply because the government staggered the deregulation over a series of different *vintages*, including a new cohort of stocks in the margin lending liberalization at five distinct points between 2011 and 2014. The partial and gradual nature of the deregulation enables us to measure direct effects using a difference-in-difference approach—comparing marginable stocks to not-yet or never marginable stocks before and after deregulation.

Additionally, for the last three of these five vintages, the government committed to a formal rule for screening and ranking, whereby new stocks qualified for margin lending according to a published formula based on publicly available information on market capitalization and trading volume. This allows us to characterize the ex-ante information on the coming credit expansion that was available to sophisticated, unconstrained investors. This feature provides a unique opportunity to identify anticipation: we are able to test whether ex-ante information leads to staggered advance increases in asset prices—mirroring the staggered nature of the liberalization.[3]

We begin our analysis by presenting several pieces of evidence showing unconstrained institutional investors speculating on the timing of the margin lending roll-out across different vintages. Note that if there are no anticipation effects, we should see price and trading effects for a given stock only at the moment at which or after margin becomes available. Alternatively, if there are anticipation effects, we expect prices and trading—i.e. buying by unconstrained investors—to rise in advance of credit supply becoming available (but after the government publicly announces their intentions). Notice that these increases need not be instantaneous and should be gradual to the extent that unconstrained investors have holding costs or there is uncertainty regarding the likelihood or form of deregulation. These findings, which are summarized in Figures 4–6, show that asset prices, purchases by large investors and mutual funds, and turnover all rise for affected stocks in the months preceding the roll-out.[4]

To capture these patterns, we propose and implement a non-myopic difference-in-difference estimator to test for anticipation and appropriately measure (net) ex-post effects. Our approach allows for differential ex-ante effects amongst soon-to-be-marginable stocks, enabling us to both account for attenuation due to pre-trends and explicitly quantify the role of anticipation. Consistent with the aforementioned figures, we

[2] This narrative regarding the Chinese context is not dissimilar to arguments as in Mian & Sufi (2011) for an increase in subprime mortgage loans in the face of deteriorating income among those households.

[3] In other settings, this timing information is missing and studies typically use a short window with which to verify the parallel-trends assumption. Such an arbitrarily short window might miss anticipation effects to the extent prices of treated and control groups already converged.

[4] In all figures, vertical lines display the start date of each of the three vintages for which the criteria for a stocks inclusion was published ex-ante.

find strong evidence of anticipation: valuations among soon-to-be-marginable stocks grow differentially just before deregulation. With this strategy, we estimate an overall ex-post effect suggesting a 57-cent increase in market capitalization per dollar of margin debt. This is substantially higher than myopic estimates would predict.

In the presence of anticipation, theory suggests that a stock's valuation at the moment of deregulation will incorporate any direct effect, but will also be influenced by the extent to which anticipatory speculation may have mis-estimated this effect. In particular, where unconstrained investors over-estimate the direct effect of margin lending on stock prices will over-estimate the direct effect if we do not account for potential overshooting. As a result, our anticipation corrected estimate—that overall a dollar of margin debt leads to an increase of 57 cents in market capitalization—might actually be the sum of two distinct effects of interest: (i) the direct effect of margin debt on market cap, and (ii) overshooting due to anticipation.

To this end, we develop an empirical strategy to test for the presence of overshooting, and to separate these two effects. We utilize the fact that anticipatory increases in valuations are concentrated in those most likely to become marginable ("high-ranked" according to the publicly disclosed formula as opposed to "low-ranked" stocks that barely ended up on the marginable list). Accordingly, we hypothesize that—if anticipation led to overshooting—ex-post valuations should be highest for those highly-ranked stocks. We develop and implement an expanded, triple-difference version of our non-myopic strategy. Our conservative estimates using this approach suggest that anticipation is responsible for at least 50 percent of the ex-post effect, while the remaining 50 percent is due to the direct effect of margin debt.

We further exploit the difference between high-ranked and low-ranked stocks to examine the policy implications of anticipation effects. To do so, we use a random sample margin trading accounts from a brokerage house to develop measures of leverage at the account level.[5] We show that leverage of households owning "high-ranked" stocks are comparable to those owning "low-ranked" stocks. Given our earlier results on price overshooting for "high-ranked" stocks, this indicates an increase in financial fragility driven by anticipation: since overshooting increases volatility—but leverage is the same—the distance to default is lower for these households.

Finally, we link our results regarding anticipation of the introduction of formal margin lending to the introduction of shadow margin lending at the end of 2014. While only the period surrounding the introduction of Vintage 4 overlaps directly with shadow margin, anticipation of non-formal lending may have played a role in the peak of the bubble and crash in 2015. Estimates place shadow margin in 2015 at almost 1 trillion yuan—roughly half of the formal margin amount during at the peak of the bubble—and research

[5]Around 2 percent of our sample of brokerage accounts had margin accounts during our sample period, and they typically owned only a few stocks at any given point in time.

implicates this lending in amplifying the market crash (Bian *et al.* (2017a), Bian *et al.* (2017b)). We use data on shadow margin lending from a peer-to-peer platform that encompassed around 10% of the market during this period. We show that holdings of non-marginable stocks by unconstrained investors went up significantly going into 2015. These non-marginable stocks, which had previously under-performed, out-performed the market during this period. Unconstrained investors appear to have anticipated the rise in shadow margin lending in the Chinese stock market as well.

Our paper proceeds as follows. In Section 2, we provide the background to our empirical design and describe our data. In Section 3, we develop our methodology. In Section 4, we present our results. We conclude in Section 5.

2 Background and Data

2.1 China's staggered deregulation of margin lending

The Chinese regulatory agency began experimenting with margin lending on February 13th, 2010. As a pilot program, an initial set of 90 stocks (Vintage 0) were opened to margin lending. The stocks selected for this initial vintage were simply those included in the two major stock market indices: the Shanghai 50 Index (50 stocks) and the Shenzhen Component index (40 stocks). Investors with at least 500,000 RMB of assets in their stock brokerage account and six months or more of trading experience qualified for margin—provided by their brokerage firms—to buy these stocks.

Effective on November 25th, 2011, the Chinese government officially began the margin lending program for stock trading, extending the list of marginable stocks based on stocks' membership in two broader market indices. The extended list included 278 stocks: 180 stocks from the Shanghai 180 Index and 98 stocks from Shenzhen 100 Index. Throughout, we refer to the stocks added at this point as Vintage 1. In the official regulations released at the start of Vintage 1 the exchanges explicitly stated that they would extend the list of marginable securities in a staggered manner.[6]

For the later extensions (Vintages 2-4), the regulatory agency adopted a screening-and-ranking rule to determine which stocks would be included in each vintage. This procedure had two steps: (i) Screening out stocks that did not satisfy several criteria that ruled particularly small, volatile, illiquid, and newly listed stocks—the so called Article 24 for Shanghai and Rule 3.2 for Shenzhen;[7] (ii) Ranking the remaining stocks

[6] See Article 28 in the Rule released by the Shanghai Stock Exchanges.

[7] The criteria for both exchanges are the same: they require stocks to satisfying all of the following criteria: (1) being traded for more than three months; (2) the number of tradable shares is larger than 100 million or market value of tradable shares is larger than 500 million; (3) the number of shareholders is more than 4,000; (4) in the past three months, the following has never happened: a) daily turnover is lower than 20% of the turnover rate of market index, b) the average of absolute value of price changes is higher or lower than that of the market index by 4%, and c) volatility is higher than the market volatility for 500%; (5) has completed the share

according to the formula described in Equation (1) and selecting the top 100 as the candidates for the next vintage (with some discretion). As shown in Equation (1), the ranking is based on a value-weighted average of a stock's size and trading volume within the exchange. The ranking procedure was conducted by the Shanghai (SH) and Shenzhen (SZ) Stock Exchanges separately.

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

Vintages 2-4 were opened to margin lending on January 25th 2013 (Vintage 2), September 6th 2013 (Vintage 3), and September 12th 2014 (Vintage 4).[8] Each time, approximately 100 stocks from each of the two exchanges become newly marginable (Although there were 120 stocks from the Shanghai exchange included in Vintage 2). After all five of these vintages, there were approximately 900 stocks in total that could be bought on margin across the two exchanges.[9] Table 1 summarizes the timeline of the deregulation and the number of newly marginable stocks for each extension.

According to the regulator's public statements, the ranking procedure was based on market data over a period before the start date of each vintage. As both the data used for the screening-and-ranking procedure and the procedure itself are public, we were able to fairly successfully replicate the procedure for each extension on each exchange; we discuss this exercise below. More importantly, unconstrained investors might plausibly be able to use these same guidelines starting at the end of 2011 to forecast in the roll-out of margin lending in real time. While the stocks included in Vintages 0 and 1 were potentially difficult to forecast, Vintages 2, 3 and 4 could in principle be predicted fairly easily.

2.2 Margin lending and the bubble-crash episode of 2010-2015

Since the official announcement of margin deregulation at the end of 2011, margin lending in China expanded dramatically. In Figure 1, we plot the ratio of margin debt to market capitalization and the total market capitalization. One can see that the ratio of total margin debt to total market capitalization increased from 0.5% around the end of 2012 to 4.5% in June 2015. In yuan terms, total margin debt increased from a negligible amount at the beginning of 2012 to almost two trillion yuan in 2015.

reform; (6) not special treated stocks; and (7) other conditions. The official document from exchanges does not specify what the other conditions refer to. See rules on stock trading with margin loans on each stock exchange's website.

[8]These are the announcement dates of the marginable stock list. The corresponding implementation dates are January 31st 2013 (Vintage 2), September 16th 2013 (Vintage 3), and September 22th 2014 (Vintage 4). But for the purposes of our analysis which is at the monthly level, there is no distinction between them since they are so close to each other.

[9]The concrete number of newly marginable stocks in each extension may be slightly more than 100, as occasionally a few marginable stocks become non-marginable for not stratifying the screening rule.

Coincident with the high level and rapid growth of margin debt, the Chinese stock market experienced an enormous boom during this period. As shown in Figure 1, total market capitalization increased from 20 trillion yuan in mid-2014 to over 50 trillion at its peak in June 2015, after which the market collapsed by more than 20% within two weeks. Over the same period, the Shanghai Composite index rose from about 2000 in mid-2014 to a peak of 5166 on June 12, 2015. Subsequently, the market crashed to 3709 within three weeks.

2.3 Data and variable construction

We utilize stock price, trading, and financial information from CSMAR, excluding stocks on the Growth Enterprise Board (GEB). Formal margin debt balance is released by Shanghai and Shenzhen stock exchanges on a daily basis. Our sample is from March 2009, one year before margin lending starts, to October 2015. The pre-crash period is from March 2009 to May 2015. Our analysis is primarily at the monthly level.

The key independent variable of interest in our paper is Margin Debt$_{i,t}$, which refers to the dollar balance of margin borrowing for stock i at the end of month t. Our primary outcome variable, Market Cap$_{i,t}$ is the market value of stock i's tradable shares. We also consider Turnover$_{i,t}$, the number of shares traded over month t scaled by the number of floating shares in Shanghai or Shenzhen stock exchange. As a control, stocks are sorted into deciles based on the past year's book equity value; we denote decile dummies as $BE_{i,t}$.

A crucial piece of our analysis regarding anticipation effects revolves around trading behavior of unconstrained investors. While the margin lending deregulation was meant to help constrained households facing financial constraints, there are many institutional investors in China who do not face such constraints, such as insurance companies or mutual funds. We rely on two datasets to get at these investors' trading behavior. The first is an analog of the 13-F quarterly institutional ownership filings in US markets typical used in studies of trading by institutional investors. While data on institutional ownership in China is not quite as high quality, public companies in China do have to disclose the largest ten shareholders and their ownership in quarterly financial reports. This data comes with the names of the investors. The majority of top 10 shareholders are institutions such as insurance companies, brokerages, and occasionally mutual funds. While not a perfect measure of institutional ownership in a stock, this variable is likely to be highly correlated, and to reflect the holdings of relatively unconstrained investors with lots of capital. For our analysis, we sum total ownership across the top 10 holders of floating shares and label it as the Top 10 Investors Ownership Share.

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.

2.4 Replicating the screening-and-ranking procedure using public data

To validate the relevance of the screening and ranking procedure discussed in Section 2.1, we use public stock market data to try to predict the list of marginable stocks for Vintages 2-4. It is worth mentioning that there are a handful of limitations that may prevent us from doing so precisely. First, the exact time window used by the exchange is not clear. However, according to some industry sources, the exchanges use data on a three-month period before the formal announcement of each vintage, although we assume there must be at least some small gap for calculation between the data-collection period and the announcement. Here, we take the end of the most recent month prior to the formal announcement as the end of the three month evaluation period. Second, there is some room at the margins for discretion on the part of the exchanges, with little in the way of published detail. As such, we do not expect to be able to precisely predict inclusion.

For each vintage, we examine the set of stocks that are non-marginable before the vintage is announced. We follow the screening rule and first exclude stocks that do not meet the criteria over the three-month window. Then, we calculate the ranking indicator as specified in Equation 1 for the remaining stocks and rank them into descending order. We denote stock i's rank for Vintage k as $Rank_i^k$, where $k = \{2, 3, 4\}$.

Let τ_i^k equal one if $Rank_i^k \leq C^k$ and zero otherwise, where C^k is the number of newly marginable stocks in stock i's exchange in Vintage k. That is, τ_i^k is the predicted marginable status for stock i for Vintage k. Define the indicator of actual marginable status as D_i^k, which equals one if stock i becomes marginable for Vintage k. For the reasons listed above, τ_i^k does not perfectly predict D_i^k for all stocks. Nonetheless, as long as τ_i^k is an effective predictor of D_i^k, we can still use it to proxy the anticipation effect of credit supply. To formally test this, we follow Chang *et al.* (2014) and run the first-stage regression of a fuzzy RD. That is, for Vintage k and stock i satisfying the screening criteria,

$$D_i^k = \alpha_{0l} + \alpha_{1l}(Rank_i^k - C^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Rank_i^k - C^k)] + \epsilon_i \tag{2}$$

If τ can strongly predict D, we expect α_{0r} to be close to one and the R-squared of the regression to be high. Table 2 presents the results for each vintage. While the rankings for stocks are done by exchange, we pool together observations from both exchanges to implement their regression. In column (1), the regression is estimated using the sample of stocks for Vintage 2. The point estimate of α_{0r} is 0.78, and R^2 equals 0.84, showing that predicted inclusion (τ) can effectively forecast the announced inclusion (D). The results are similarly strong for Vintages 3 and 4. This confirms the

In our following analysis, we use the pre-ranking, i.e., $Rank_i$, to identify stocks likely-to-qualify for the margin debt program.

3 Methodology

The literature on credit booms posits a direct effect: credit supply to financially-constrained households (which in our context corresponds to margin loans or debt to retail investors) leads to higher valuations as they bid up prices. In a myopic context, identifying the direct effect using credit-supply shifts is straightforward and well understood. Studies typically examine a deregulatory change and validate the parallel trends assumption to confirm there are no compromising anticipatory pre-trends. The intuition behind this standard estimation is captured by the red line in Figure 2. The y-axis of this figure displays an asset's price, while the vertical line on the x-axis denotes the relevant event date (e.g. deregulation). In a myopic world, prices are flat before the event date and then jump discretely to a higher price after the event. The difference in the two prices represents the direct effect. In our context, this would correspond to an increase in buying by constrained households when a stock qualifies for margin lending leading to higher valuations or a larger bubble. In other words, a positive direct effect on prices. In any setting, this approach assumes that there are not large substitution effects on the part of households into other stocks as a result.[10]

However, as we have pointed out, we should not typically expect the pattern displayed by the red line—or, more flexibly, the parallel trends assumption—to hold. In general non-myopic settings such as ours it is necessary and possible to estimate not just the direct effect, but effects that arise from anticipation. If unconstrained speculators anticipate that overconfident retail investors will get access to leverage and that prices will rise as a result, it is optimal for them to buy in advance. The anticipatory effect is positive: unconstrained investors or arbitrageurs will optimally start buying stocks that they view as likely to qualify for margin lending in advance of the margin lending deregulation, rather than substituting away from the stocks when credit is rolled out. Notice that their buying and hence price adjustment will be gradual, given that there is both uncertainty regarding the policy change and a cost of holding securities.

The gray and black lines in Figure 2 represent anticipation scenarios. For the grey line, prices move in advance of the event date (so parallel trends does not hold) but undershoot the direct effect. For the black line, prices again move in advance of the event date (and again parallel trends does not hold) but overshoot the direct effect. The destabilizing effects (the black line or overshooting scenario as opposed to the grey line) of institutional investors speculating on the path of prices are well-established in the literature and can

[10]This assumption seems plausible in China, as we show below, since most retail investors hold only a few of their favorite speculative stocks. Moreover, to the extent there are indirect effects from substitution to lower price stocks, this would downward bias our measure of a direct effect.

arise in a variety of settings. To the extent there are enough institutions buying, they will have a price impact and lead to even higher prices or overshooting relative to a world with no such anticipatory speculation. This additional price effect or overshooting is particularly emphasized in De Long *et al.* (1990), Lakonishok *et al.* (1992) and Stein (2009).

In both cases, the pre-trends generated by anticipation pose two problems for conventional approaches. First, standard (e.g. difference-in-difference) techniques will underestimate the true average net effect of the event—which we refer to as the *ex-post effect*—because they compare the period after the event to an artificially inflated pre-period. Second, because the price of the asset after the event pools the effect of credit with any under- or over-estimation by speculators, even an accurate estimate of the ex-post effect will not capture the direct effect of credit supply on asset prices.

3.1 OLS

We begin by specifying the simplest approach OLS approach one might take to capture the direct effect of margin lending on market capitalization. For stock i in month t, we estimate:

$$IHS(\text{Market Cap}_{i,t}) = \alpha + \beta_0 IHS(\text{Margin Debt}_{i,t}) + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it} \tag{3}$$

where $IHS(\text{Market Cap}_{i,t})$ and $IHS(\text{Margin Debt}_{i,t})$ refer to the inverse hyperbolic sine of market cap and margin debt for stock i in month $t+1$, both in RMB.[11] Book-equity deciles refer to dummy variables for inclusion in each decile of book equity at the month level, and γ_i and δ_t are stock and month$\times$year fixed effects, respectively. β_0 is expected to be positive. We are primarily interested in the economic magnitude of this coefficient, and because IHS-IHS as roughly similar to a log-log specification, we interpret the coefficient of interest β_0 as an elasticity.

Of course, β_0 as estimated from this approach is unlikely to identify the direct effect (or even the ex-post effect) for a number of reasons. First, this specification is subject to endogeneity concerns that are present even in myopic settings. For example, within a vintage, the fastest growing stocks might attract the largest share of margin debt, even in the absence of any direct effect. Second, even absent these concerns, the OLS an approach will not be valid in non-myopic settings. Anticipation will cause $\text{Market Cap}_{i,t}$ to rise even while $\text{Margin Debt}_{i,t}$ is mechanically constrained to be 0, biasing any estimates.

[11] We use IHS rather than log as a transformation because margin debt for a stock can be zero.

3.2 First stage: Exploiting the staggered deregulation

To deal with the endogeneity concerns described above, we exploit the liberalization of stock margin lending, which allowed brokerage firms to lend large amounts to retail households. In particular, we instrument using the staggered rollout of the margin deregulation. To capture the impact of this deregulation on margin debt—effectively the first stage for the IV approaches described below—our simplest specification is:

$$IHS(\text{Margin Debt}_{i,t}) = \gamma_0 + \gamma_1 \text{Margin Trading Active}_{i,t} + \lambda_1 BE_{i,t} + \eta_i + \tau_t + v_{it}, \tag{4}$$

Here, Margin Trading Active$_{i,t}$ is a dummy variable equal to one if margin trading is active in month t for stock i, and zero otherwise. η_i and τ_t are stock and month fixed effects, respectively. We refer to this as our "collapsed specification."

We also consider a more general specification that allows for flexible effects across different vintages, which we refer to as our "full instruments specification:"

$$\begin{aligned} IHS(\text{Margin Debt}_{i,t}) = \gamma_0 + \sum_{k=0}^{4} \gamma_1^k \text{Margin Trading Active}_{i,t} \times Vintage\_k_i \\ + \lambda_1 BE_{i,t} + \eta_i + \tau_t + v_{it}. \end{aligned} \tag{5}$$

Here $Vintage\_k_i$ is an indicator equal to one if stock i is in Vintage k. Thus, Margin Trading Active$_{i,t}$ $\times$ $Vintage\_k_i = 1$ if margin lending is allowed in month t for stock i in Vintage k and zero otherwise.

Our first-stage regressions are similar to other studies that use deregulatory changes. The standard approach in myopic settings would directly utilize the specification in Equation (4) to instrument for $IHS(\text{Margin Debt}_{i,t})$ in a first stage, and estimate Equation (3) in a second stage. However, as doing so is not valid in the presence of anticipation, we suggest a non-myopic approach.

3.3 Difference-in-difference specifications in non-myopic Settings

3.3.1 Accounting for anticipation

In order to estimate anticipatory effects, and to appropriately measure the net ex-post effects of the deregulation of margin trading in China, we consider non-myopic difference-in-difference specifications following Malani and Reif (2015). The basic notion of this approach is to use 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.

This strategy can be seen most clearly in the following reduced-form specification:

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (6)$$

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. Here, 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. Here, S captures the number of periods in advance investors might feasibly speculate upon the coming introduction of margin lending. In the stylized example shown in Figure 2, $\beta_1, ..., \beta_S$ capture the upward trend preceding the event date, while β_0 captures the difference between the average in the period *after* the event date and the baseline price.[12]

Because we are explicitly interested in the impacts of margin debt, we focus primarily on an IV version of the above, rather than the reduced form itself. In the first stage, we use Margin Trading Active$_{i,t}$ as an excluded instrument for IHS(Margin Debt)$_{i,t}$ following Equation (4).[13] In the second stage, we estimate:

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{IHS(Margin Debt)}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (7)$$

While market cap is the primary variable of interest, we also consider non-myopic specifications for a variety of other outcomes to support our analysis. In particular, we estimate similar specifications using the proportion of institutional ownership of stocks and turnover of those stocks as dependent variables. In all specifications, $\beta_j > 0$ for $j > 0$ indicates the presence of anticipatory effects: the market capitalization of soon-to-be marginable stocks grows relative to a control group in the period leading up to the roll out. Appropriately accounting for anticipation, the coefficient β_0 captures the net ex-post effect of margin lending.

In this context, the standard approach in the literature—which we call our myopic estimate—is simply a special case in which we set for $\beta_j = 0$ for $j > 0$. As noted in Malani & Reif (2015), failing to account for any ex-ante changes in anticipation of the margin lending roll-out will cause a researcher to estimate the true (ex-post) effects with bias. In particular, if stock prices rise in anticipation of future margin lending, the myopic approach will *underestimate* the true effects. The intuition here is simple, the myopic difference-in-difference estimator compares a post-treatment price to an artificially high pre-treatment price—which has

[12] i.e. the x-axis or the price level before the black and gray lines diverge from the red.

[13] In more general settings, it would be necessary to modify the first stage itself to account for anticipation, i.e. to also include $\sum_{j=1}^{S} \psi_j D_{i,t+j}$ in Equation 4. However, because margin debt is mechanically constrained to be 0 before deregulation in our setting, these effects are redundant.

already risen in anticipation of treatment.

3.3.2 Accounting for price overshooting

We next develop two strategies that provide further evidence on the presence of anticipation, and allow direct tests of overshooting. In our first and preferred specification, we further interact the non-myopic specifications above with a variable intended to capture variation in investors ability to anticipate which stocks were about to become marginable: the likelihood of inclusion in the coming vintage.

The later vintages were determined on the basis of a well defined rule based on a stock's ranking in terms of volume and market value at the time of the rollout. As a result, in the months prior to the roll-out itself, there was uncertainty over which stocks would have a sufficient ranking to qualify. We exploit cross-sectional variation in this uncertainty by comparing—within the stocks that qualified for a given vintage—those with the highest rankings (above median rank) to those with the lowest rankings (below median rank).

The logic underlying this approach is that, from the perspective of a speculator ex-ante, the highest ranking stocks are almost certain to qualify: a large shift in the rankings would be necessary to disqualify such a stock. Alternatively, low-ranking stocks are relatively uncertain, as a marginal change in rank might lead to disqualification. As a result, we expect speculators to differentially focus attention on and buy the highest ranking stocks, generating larger anticipatory price effects in these stocks. However, because both high and low ranking stocks are quite similar otherwise, there is no reason to expect a differential direct effect of margin debt. Put differently: in the absence of anticipation, there is no reason a constrained household with new access to credit would prefer high- over low-ranking stocks within a vintage. Consequently, any difference in ex-post effects between high- and low-ranking stocks must be the result of overshooting due to speculation.

To be explicit, in a second stage, we estimate

$$\begin{aligned}\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{IHS(Margin Debt)}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} \\ + \eta_0 \text{IHS(Margin Debt)}_{i,t} \times \text{High Rank}_i + \sum_{j=1}^{S} \eta_j D_{i,t+j} \times \text{High Rank}_i \\ + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \qquad (8)\end{aligned}$$

Here High Rank$_i$ is an indicator equal to one if a stock is above the median rank within its vintage. In a first stage, we include Margin Trading Active$_{i,t}$ and Margin Trading Active$_{i,t}$ $\times$ High Rank$_i$ as excluded instruments for IHS(Margin Debt)$_{i,t}$ and IHS(Margin Debt)$_{i,t}$ $\times$ High Rank$_i$. These IV estimates provide coefficients that can be interpreted as elasticities. our approach provides a direct test of overshooting. However, there may, of course, be overshooting in both high and low ranking stocks. To the extent that this is the case, our strategy provides a lower bound on the magnitude of the overshooting effect.

We also recognize that researchers may not be able to utilize the above strategy in other contexts. Nonetheless, one can still decompose anticipation versus direct effects if one is willing to assume that overshooting effects are transitory and persists only for some period S after the event date, and also that direct effects are persistent. (Note that we use S here for symmetry with our pre-trend event window for notational convenience. These event windows can differ depending on the particular context.) To capture this, we develop a specification in which we separately estimate difference-in-difference coefficients for (i) the period just before the margin lending roll-out, and (ii) the entire period after the roll-out ("long-run") while (iii) allowing for differential effects in the period immediately after the rollout. (i) Captures any anticipatory effects, (ii) captures the direct effect, and (iii) separately estimates the impact of overshooting.

Specifically, we can estimate IV versions of the specifications above, in which we explicitly estimate the elasticity of market cap with respect to margin debt in the long run period (times $t > S$). In particular, in a first stage, we allow Margin Trading Active$_{i,t}$ to instrument for IHS(Margin Debt)$_{i,t}$

$$\text{IHS(Margin Debt)}_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=-(S-1)}^{S} \beta_j D_{i,t+j} + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (9)$$

For the second stage, we estimate:

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{IHS(Margin Debt)}_{i,t} + \sum_{j=-(S-1)}^{S} \beta_j D_{i,t+j} + \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (10)$$

We prefer our first strategy. While we have a sense of what the pre-trend event window S is given our institutional context, we do not have the same confidence on how long it might take the overshooting to mean revert. As such picking an S for our post-event overshooting window becomes more challenging.

4 Empirical Results

4.1 OLS Estimates

The results from our OLS specifications are shown in Table 3. The coefficients of $IHS(\text{Margin Debt}_{i,t})$, which we interpret as elasticities, are all positive and statistically significant. In column (1) where there are no controls, the coefficient of margin debt is significantly positive at 0.081. In column (2) where we add book-equity decile dummies and industry fixed effects, the coefficient is 0.038. In column (3) where we add book-equity and time effects, the coefficient is .025.

Our most conservative specification includes both time and stock fixed effects. Even for this conservative specification, there is a precisely measured positive effect. As shown in Column (4), the estimated elasticity is 0.005. Given the relative scales of market cap and margin debt in this context (margin debt is nearly two orders of magnitude smaller than market cap), even this conservative estimate is economically sizable. Evaluated at the means of both variables, this coefficient suggests that a one dollar increase in margin debt corresponds to roughly a 40 cents increase in stock valuation.[14] A useful comparison is the case of perfect pass through, in which a one dollar increase in margin debt would correspondingly increase market cap by one dollar.

4.2 First-stage estimates

Figure 3 displays the staggered rollout of stock margin lending by vintages. It is easy to see from this figure that the staggered deregulation events will be powerful instruments for margin debt. In Table 4, we display results from the corresponding first stage regressions described in Equations (4) and (5). The column labeled collapsed, corresponding to Equation (4), shows that there is a strong, positive, and statistically significant impact of the margin lending rollout on margin debt itself, with a coefficient of just over 19. The column labeled full, corresponding to Equation (5), shows that these effects are relatively constant across the five vintages, if marginally smaller in earlier vintages.

4.3 Non-myopic IV estimates

As Figure 4 makes clear, the assumption of no anticipatory effects appears to be untrue in the context of the deregulation of margin lending in China. The figure, which plots the inverse hyperbolic sine of market cap—after netting out stock, month, and book-equity decile fixed effects—displays evidence of sharp rises in market cap for Vintages 2, 3 and 4 in anticipation of the introduction of margin trading for those stocks. In this figure—and many that follow—we omit Vintages 0 and 1 in order to keep the graphs clear and avoid over-cluttering. As discussed elsewhere, these vintages were difficult to predict and hence (as expected and shown in Table 8) displayed minimal evidence of anticipation.

There are evident pre-trends for the treated groups in the pre-treatment periods. While in other settings, it might be difficult to attribute these pre-trends to anticipation—they might, for example, reflect the endogeneity of treatment itself—we believe the staggered nature of the roll-out provides strong support of

[14] Average market cap over our sample period is 80.8 times the size of average margin debt.

anticipation. Replicating the roll-out, the increases in market cap are themselves staggered, with the rises for each vintage just preceding deregulation for that vintage.

Table 5 presents both reduced form and IV evidence from non-myopic specifications intended to capture the patterns presented in Figure 4. In the reduced form specification given in the caption, the net effect of margin lending is captured by the coefficient labeled *Ex-Post Effect*. The coefficients labeled IHS(Margin Debt) in our IV specifications can be explicitly interpreted as elasticities of market capitalization with respect to margin debt. As a baseline, the fourth and sixth columns, labeled *Myopic*, report the myopic reduced form and IV specifications described in the table caption where we deliberately and erroneously assume there are no anticipation effects. The remaining columns account for anticipation: the second and fourth columns allow for six months of anticipation, while the third and sixth columns allow for six quarters of anticipation.

First, these estimates provide strong evidence of the existence of anticipatory effects, as evidenced by the positive and significant coefficients labeled as ex-ante effects in these tables. Market cap grows significantly in soon-to-be-marginable stocks in the months or quarters just prior to the margin lending roll-out, when compared with stocks that are not marginable.

Additionally, these estimates show that failing to account for anticipation substantially attenuates the net impact of margin lending on market cap. When accounting for six months of anticipatory effects, the estimated reduced form coefficient rises from 0.065 to 0.127, and further to 0.214 when accounting for six quarters of anticipation. The IV estimates, which provide more economically interpretable coefficients, suggest that the elasticity grows from 0.003 with no anticipation, to 0.007 or 0.011 with six months or six quarters. Evaluated at the averages, our six month estimates suggest that—accounting for anticipation—an additional dollar of margin debt leads to a 57 cent increase in market cap, compared to 24 cents in our myopic specification.

4.4 Further evidence from unconstrained-investor holdings and trades

To confirm that the results above are driven by anticipation, we next directly examine the behavior of two groups of investors we expect to be relatively unconstrained even prior to the introduction of margin lending. Specifically, we examine the behavior of mutual funds, and of 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. Figure 5 displays the patterns of ownership by both mutual funds and the top 10 investors over our sample period for Vintages 2, 3 and 4. We also include the shares held in never marginable stocks, as a comparison group. The two panels plot residuals of the share of ownership by mutual funds and the top 10 investors, respectively, after netting out stock, quarter, and book equity decile fixed effects. For all three vintages, there is graphical evidence that these relatively unconstrained investors contributed to the anticipatory rise in market cap. Relative to the never marginable group, the share of ownership for these unconstrained investors rose in the months prior to the roll-out date of the vintage (and much farther in advance for Vintage 2). Perhaps the most pronounced evidence comes from Vintage 4, which shows a steep increase in both groups in relatively close proximity to the roll-out.

Table 6 displays regression results corresponding to Figure 5, but, to be conservative, including stocks in all vintages. The specifications are identical to those in Equation (6), but replace the dependent variable with the share of ownership by unconstrained investors (defined as either mutual funds or the top 10 investors). They are estimated at the quarterly level, corresponding to the frequency of our data on these investors. In our non-myopic specifications we show two quarters of ex-ante effects to match the 6 months shown in our specifications that utilize monthly data.

The first two columns of this table present results for mutual funds. As a baseline, the column labeled *Myopic* shows results from a myopic difference-in-difference specification that does not account for anticipation. Ignoring anticipation, it appears that there is a marginally significant negative effect of the margin lending rollout on mutual fund holdings. However, the column labeled *Quarterly Lags* shows that this negative effect is largely an artifact of anticipation. Allowing for two quarters of anticipation, the negative effect drops and becomes insignificant.

Perhaps more importantly, there are statistically significant positive coefficients representing ex-ante effects in each of the two quarters preceding the margin lending roll-out. These coefficients suggest that by the quarter just prior to the roll-out, mutual fund holdings increased by 0.7 percentage points on average—or 41 percent—relative to never marginable stocks. The top 10 investors show a similar pattern: a negative (although insignificant) coefficient in the myopic specification, which is attenuated when allowing for anticipation. Again, there are positive and statistically significant ex-ante effects for the top 10 investors in the quarters just before the roll-out. These estimates suggest that these investors had increased their holdings in soon to be marginable stocks by 3.6 percentage points, or 7.8 percent, in last quarter before margin lending began.

We also examine whether there is visible evidence of anticipation in turnover for the stocks that qualified for margin lending. Figure 6 shows residualized turnover for Vintages 2, 3 and 4, as well as for never marginable stocks, over our sample period. Within each group, this figure plots average turnover after netting out month, stock, and book-equity decile fixed effects. The plot shows sharp increases in turnover for each of the vintages just prior to the margin lending rollout, particularly for Vintages 2 and 3. For all three vintages, these spikes recede fairly quickly following the roll-out.

The final two columns of Table 6 display regression results corresponding to these figures. We once again estimate a version of Equation 6 at the monthly level, but replace the dependent variable with our measure of turnover. There are significant increases in turnover relative to the never marginable group both ex-ante, in the two quarters preceding the roll-out, and after the rollout. The effect on turnover in the quarter before the roll-out, at 0.164, is nearly double the ex-post effect of 0.087. These anticipatory increases in turnover are directly consistent with the presence of unconstrained investors speculating in anticipation of the margin lending roll-out.

4.5 Direct effect versus price overshooting from anticipation

The elasticities estimated in Table 5 suggest that, on average, each additional dollar of margin debt leads to a 57 cent increase in market cap. However, this average potentially captures two distinct effects, the direct effect of margin debt, and the impact of overshooting due to speculation. To separate these effects, we follow the first strategy outlined in section 3.3.2, and, within each vintage, compare the stocks most likely to qualify for margin lending to those least likely to qualify.

Figure 7 displays shows the basics of this approach. Within each vintage, we plot month-stock-book equity residualized market cap, split by those above (high ranking) vs. below (low ranking) the median according to the screening and ranking formula. There are two primary takeaways from these figures. First, there is differential anticipation for high- versus low ranking stocks in the period preceding the introduction margin lending: market cap rises before the roll-out to a greater extent for high ranking stocks in all three of the predictable vintages (Vintages 2-4). This confirms our assertion that speculators are more likely to purchases stocks that are almost sure to be included in the upcoming vintage. Second, this difference does not disappear once margin lending becomes active. Despite there being little fundamental difference between high and low ranking stocks, market cap for high ranking stocks remains above that of low ranking stocks for months after the roll-out. This provides direct evidence that speculation led to overshooting.

To quantify these effects, we show the results from the non-myopic triple-difference specification described in Equation 8 (as well as a reduced form version of the same specification) in Table 7.[15] This table confirms both points described in the previous paragraph. As confirmed in columns, (1), (2), (4), and (5), anticipation caused high ranking stocks to have statistically significantly higher market cap in each of the 6 months or quarters preceding the introduction of margin lending. Furthermore, high ranking stocks saw a significantly larger effect ex-post. Summing the ex-post effect for low ranking stocks in column (4) (denoted by the coefficient on IHS(Margin Debt)) with the differential ex-post effect for high ranking stocks (denoted by the coefficient on IHS(Margin Debt) $\times$ High Rank), the overall elasticity for high ranking stocks is 0.014.

[15]This table omits vintages 1 and 2, as those vintages were not selected using the screening and ranking rule.

This suggests that, for high ranking stocks, each additional dollar of margin debt led to an additional \$1.13 of market cap. The differential between low and high ranking stocks suggests that, as a lower bound, 64 cents of this is due to overshooting—suggesting the direct effect is 49 cents. In other words, more than 50 percent of the net ex-post effect is due to overshooting for high ranking stocks.

4.6 Placebo tests

4.6.1 Heterogeneity in early versus late vintages

While the stocks included in Vintages 2, 3 and 4 were included on the basis of a well defined and publicly available rule, the early vintages were chosen in a less systematic and transparent way. As a result, we expect that the marginable stocks in these vintages were more difficult to predict in advance of the rollout. To test this prediction, we estimate a triple difference version of our non-myopic specifications, incorporating the difference between early and later vintages. In particular, we run:

$$
\begin{aligned}
\text{IHS(Market Cap)}_{i,t} = \alpha &+ \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} \\
&+ \eta_0 \text{Margin Trading Active}_{i,t} \times \text{Late Vintage}_i + \sum_{j=1}^{S} \eta_j D_{i,t+j} \times \text{Late Vintage}_i \\
&+ \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \qquad (11)
\end{aligned}
$$

$\eta_j > 0$ provides further evidence for anticipatory effects: suggesting that the later, more predictable vintages saw larger increases in market cap in the months prior to the rollout. Additionally, evaluating the hypothesis $\eta_0 > 0$ provides evidence of overshooting, suggesting that the stocks that were more predictable ex-ante also had higher valuations ex-post.

We also estimate IV versions of these specifications, where, in a second stage, we estimate:

$$
\begin{aligned}
\text{IHS(Market Cap)}_{i,t} = \alpha &+ \beta_0 \text{IHS(Margin Debt)}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} \\
&+ \eta_0 \text{IHS(Margin Debt)}_{i,t} \times \text{Late Vintage}_i + \sum_{j=1}^{S} \eta_j D_{i,t+j} \times \text{Late Vintage}_i \\
&+ \theta_1 BE_{i,t} + \gamma_i + \delta_t + \varepsilon_{it}. \qquad (12)
\end{aligned}
$$

In a first stage, we include Margin Trading $\text{Active}_{i,t}$ and Margin Trading $\text{Active}_{i,t} \times$ Late Vintage_i as excluded instruments for $\text{IHS(Margin Debt)}_{i,t}$ and $\text{IHS(Margin Debt)}_{i,t} \times$ Late Vintage_i. These IV estimates provide coefficients that can be interpreted as elasticities.

Table 8, which shows estimates from the specifications described in Equation (11), displays significant evidence of both anticipation and overshooting. In monthly and quarterly specifications, there are positive and highly significant differential ex-ante effects for late vintages, suggesting that later more predictable vintages saw significantly larger anticipatory effects. Additionally, there is substantial evidence of overshooting: in all specifications the ex-post effect is significantly larger for later vintages than for early vintages. In particular, the estimated elasticities—evaluated at the means of margin debt and market cap—suggest that, in the monthly specification, an additional dollar of margin debt is associated with a 1.28 dollar larger increase in market cap for later vintages compared with earlier vintages.

4.6.2 Randomizing event dates

One potential concern is that the particulars of the screening and ranking procedure itself might create mechanical effects in the set stocks that we study, even in the absence of any speculative or direct impact of margin debt. To rule out this possibility, we perform a series of placebo exercises. In particular, we randomly select a date and define a placebo treatment group relative to that date following the screening and ranking rule. Effectively choosing the top 100 stocks on each exchange according to the published formula. We then conduct our main specifications: both the initial non-myopic difference-in-difference specification, and the triple difference that incorporates high-vs-low ranking stocks.

Table 9 displays the results from these exercises. The first five columns present versions of the non-myopic difference-in-difference approach. To avoid contaminating our placebo treatment group with actual impacts of the margin lending roll out, we took two approaches. In the first three columns, we randomly selected a date using our entire period, but considered only stocks that never qualified for margin lending in constructing our placebo group. In the second two columns, we restricted our dates to the period preceding the start of Vintage 2, but included stocks that qualified for Vintages 2, 3 and 4 when constructing our placebo group. Note that, because of the early start date in these last two columns, we are unable to show quarterly lags. In both cases, we see no evidence of anticipation. Furthermore, there is no evidence of a positive direct effect. In fact, there appears to be a negative ex-post effect, likely caused by regression to the mean amongst high ranking stocks.

The final three columns repeat the first three columns but implement our triple difference approach. We see no evidence of anticipation, and no evidence of a difference between high and low ranking stocks before or after the placebo date. This suggests that our findings are not driven by the procedure, and that our assertion that high and low ranking stocks are similar is reasonable.

5 Policy Implications

5.1 Speculation and fragility of financially-constrained households

We now draw some implications of anticipation effects for policy. Returning to Figure 2, households end up having to buy at higher prices in the overshooting scenario represented by the black line than the other scenarios. That is, anticipation can increase the fragility of financially-constrained households assuming household leverage is not appropriately lower in this scenario compared to other scenario.

To see empirically if this is indeed the case, we extend our previous analysis on price overshooting for high-ranked versus low-ranked stocks in Vintages 2, 3, and 4. We can interpret this analysis as the causal effect of unconstrained investor speculation for price overshooting. Given that households typically own a few stocks in their portfolio, we can measure the leverage of financially-constrained households owning high-ranked versus low-ranked stocks. Constrained households are likely to have take a substantial margin, typically anywhere from 1.5 to 3 depending on our sample period. We can then compare the leverage for households owning high-ranked stocks versus the leverage of households owning low-ranked stocks. This comparison would tell us the causal effect of speculative anticipation and overshooting on financially-constrained households. In particular, if the leverage ratios were similar, this would all else equal mean that financially-constrained households end up being more fragile as a result of anticipatory overshooting since these higher priced stocks would on an ex-ante basis be more volatile. Since leverage is the same across the two scenarios, this would mean that households owning high-ranked stocks and subject to a treatment of anticipatory speculation would have a lower distance to default.

To conduct this additional analysis, we obtain account data of margin and regular trading from a nationwide discount broker in China. This brokerage house has representative geographic coverage in China. The sample we have consists of 709,813 accounts, 18,593 of which are margin accounts. The sample period, from January 2011 and December 2015, overlaps with the whole episode of deregulation in margin trading, and the first margin trade appears in June 2012. For each account, we observe the records of all trades, and for each trade there is information regarding the transaction price and number of traded shares. In addition, for margin accounts, each trade record has a label indicating whether the transaction goes through the brokerage margin system.[16]

Although the data do not provide snapshots of accounts' detailed stock holdings or cash balance, we can nonetheless calculate accounts' leverage level with a few reasonable assumptions and the following steps. We first construct each account's stock holdings at the end of each day by adding up all buys and sells of each stock (adjusted for stock splits). One issue here is that for accounts that started trading before 2011 we

[16]Unfortunately, the dataset does not have any accounts' demographic information

do not observe their initial stock positions. This issue first makes some positions appear to be negative based on our calculation. To deal with this, we assume negative positions whenever they appear be zero. Those negative positions are likely due to unobservable long positions before the sample starts given that during the sample period shorting is limited in China. Second, this issue biases down our estimate of accounts' total portfolio value, because we miss the value of long positions an account starts to hold prior to 2011. That being said, the under-estimation should not be severe given the high turnover rate of retail investors in China. Once we have each account's portfolio, we calculate the mark-to-market value of her stock holdings, labeled as $Asset$.

The second step is to track the balance of margin loans. Since we do not observe an account's cash balance or repayment to margin loan, we assume that the pecking order is cash over loan, that is, a margin account will repay the outstanding loan whenever she has cash. This is reasonable given that margin loans are more costly than cash, though we acknowledge that there may be some investors who have outstanding loan and cash at the same time and this may bias down our estimate of the true amount of margin loans. Here is our calculation: when the account places a buy order through the margin system, the value of the purchased position will be accumulated to the account's margin loan; when the account executes a sell order, no matter it is through margin or not, the proceeds from sales will be treated as repayment to her outstanding margin borrowings (if any). In this way, we obtain each margin account's daily balance of margin loans, denoted as $Loan$. The total amount of margin loans based on our data and calculation is 1.15 billion yuan at the end of June 2015, which is approximately 0.05% of the total margin debt in the market.

Then the account leverage level (Lev) is calculated as,

$$Lev = \frac{Asset}{Asset - Loan}. \tag{13}$$

Note that this ratio is mark-to-market, and in order to avoid data errors we winsorize account leverage at the 99th percentile by month. Table 10 presents the time-series average of cross-sectional statistics of margin investors' characteristics. One can find that the sample mainly consists of small, retail investors. The average portfolio size over the time is 134,421 yuan, while the median is only 46,744 yuan. Also, their portfolios are under-diversified; the average investor holds only 7.4 stocks and the median is 4.6.

Another interesting pattern is that the average leverage is small with mean of 1.09 and median of 1. Also, there is a sizeable variation in the cross section: the 95th percentile of leverage is 1.45. Our estimates are very close to that in Bian *et al.* (2017a), who use similar account data from another source. They show that the average account leverage from the formal margin channel is 1.6 (see Table 1 of their paper). The cross-sectional dispersion of leverage also increases during the booming period. For example, the 90th percentile is higher than 2 in June, while the 95th percentile reaches 3.3, at the peak. Recall that maintenance margin requirements mean that household leverage can be at maximum around 3.

We are interested in the time trend of leverage for households that own high-ranked versus low-ranked stocks. In particular, we are interested in the leverage of financially constrained households owning high-ranked versus low-ranked stocks for the different vintages. To this end, in Figure 8, we plot the 95th percentile of leverage across households that own high-ranked versus low-ranked stocks for the different vintages. The 95th percentile likely picks up these financially-constrained households as many households might have a margin account but do not really trade or need it to buy equity. Those at the extreme are our interest as those highly levered investors are the ones who are particularly fragile during the crash period. We can see from the figures that the leverage of these financially-constrained households in high-ranked stocks are very similar to those owning low-ranked stocks. As such, we would conclude that anticipatory price overshooting likely made financially-constrained households more fragile on an ex-ante basis. The counterfactual where this would not be the case is if the leverage ratios for households in high-ranked stocks were lower, presumably because banks might not lend to households in these situations or if the households themselves understood the consequences. But in practice, we can see that this is not the case.

5.2 Anticipating shadow margin the peak of the bubble in 2015

While our analysis has been constrained thus far to the official deregulation of margin lending, the notion of anticipation we describe should, in principle, apply to any foreseeable expansion of credit. To conclude our analysis, we briefly consider the expansion of what is often referred to shadow margin: the provision of margin via peer-to-peer platforms distinct from formal brokerages, allowing smaller investors to informally buy *any* stock on margin. Although the introduction of shadow margin was not as precisely delineated as lending through formal channels, the process expanded rapidly in late 2014 and early 2015. Estimates place shadow margin in 2015 at almost 1 trillion yuan, roughly half of the formal margin amount during at the peak of the bubble.

The patterns in Panel A of Figure 9 (which reproduces the later period of Figure 5) suggest that mutual funds began to increase their relative positions in non-marginable stocks after the introduction of Vintage 4, the final official set of marginable stocks. Similar patterns can be seen for the top 10 investors, to a lesser extent, in Figure 5. We now argue that this buying anticipated the rise in shadow margin lending in the Chinese stock market which was concentrated in these non-marginable stocks. The non-marginable stocks, which had previously under-performed, outperformed the market during the final period prior to the crash (Figure 4).

To show that this is indeed the case, we gather data on shadow margin lending from a peer-to-peer platform that encompassed around 10% of the market during this period. We measure the presence of shadow margin at the stock level using data from a large technology provider. This technology company routed the trades of 180 peer-to-peer platforms that provided leverage for stock purchases. Each platform had a master account which qualified for margin with the stock exchange. This master account was subdivided into smaller managed accounts for individual households that could then buy stocks on margin provided by the platform. The technology company managed the website and routing of trades. As a result, it aggregated for us all the buys and sells from the 180 peer-to-peer platforms. They calculate for us the net buys and sells each day and the cumulative net buys and sells over time for each stock, which we then use as a proxy for shadow margin. This shadow margin figure is not identical to the margin balance data from the exchanges since the net buys and sells is marked to market daily. But it does provide a measure of shadow margin activity across different stocks. In our analysis, we scale shadow margin by a factor of 10 to reflect that the peer-to-peer platform we collected data only accounts for 10% of the market.

Panel B of Figure 9 plots shadow margin debt for the different vintages, and for all stocks that were not part of any vintage, in the latter part of our sample. Unsurprisingly, shadow margin begins to expand later in the sample, around the end of 2014, suggesting that it is not a major concern for our primary analysis. Further, the majority of shadow margin debt is concentrated in non-marginable stocks, suggesting that mutual funds and unconstrained investors were at least matching—if not anticipating—the flows of shadow margin debt. These findings are reminiscent of Brunnermeier & Nagel (2004) and Griffin *et al.* (2011) who found that hedge funds rather than shorting internet stocks actually overweighted internet stocks going into the dot-com bubble. The collection of these forces surely contributed to the dramatic surge and subsequent crash in the non-marginable stocks (along with the rest of the market) in 2015.

6 Conclusion and Implications for Housing Markets

In this paper, we argue that the literature on credit booms gone wrong, which has predominantly focused on the direct effect of credit supply, should simultaneously account for anticipatory effects. Our empirical exercises consider the context of the stock market and the staggered deregulation of margin lending in China that took place between 2011 and 2014. However, our points apply generally to any asset market in which agents are non-myopic.

In non-myopic settings, parallel-trends style criterion should not, in general, be expected to hold. To identify the impact of credit expansions on asset prices in such contexts, we suggest implementing a simple non-myopic difference-in-difference estimator. In the case of the Chinese margin lending, this estimator compares stocks in each of the five vintages of the deregulation to non-marginable stocks, before and after the vintage in question become marginable. To test for anticipation—and to correct the bias in ex-post effects generated by anticipation driven pre-trends—the non-myopic estimator allows for differential effects in the treatment group (marginable stocks) in the periods just before deregulation.

Moreover, we note that the net change in asset prices immediately following a credit supply shock—which we term the *ex-post effect*—is the sum of two distinct channels: the direct effect of margin debt on asset prices, and overshooting caused by anticipatory speculation. To disentangle these two, we consider a tripe-difference generalization of our non-myopic approach. Within each vintage of marginable stocks, we used the regulators ranking criterion to compare the stocks that were ex-ante more versus less likely to become marginable. We find that there are larger speculative increases in these stocks, suggesting that investors were confident they would become marginable. Furthermore, these higher prices were sustained in the early period after the stocks actually became marginable, suggesting that speculation led to overshooting.

The anticipatory speculation we highlight has policy consequences. Demand by unconstrained investors in advance of a credit expansion may lead to larger price increases and excess volatility where overshooting occurs. This, in turn, may limit the amount constrained agents can buy when credit becomes available, increase the leverage they choose to take, and overall decrease their distance-to-default. Furthermore, even in the absence of overshooting, the fact that prices rise in anticipation of credit supply shocks has important distributional consequences. The constrained buyers that are typically the target of regulatory credit-expansions wind up facing the highest prices. Consideration of speculative anticipation may allow regulators to better manage leverage cycles.

Of course, given the importance of housing in the great recession, much of the recent work on credit cycles has focused on real-estate markets. Our points regarding the role of anticipation apply directly to this literature. For instance, Favara & Imbs (2015) use the staggered US branch banking deregulation index created by Rice & Strahan (2010) to argue for a direct effect of credit supply on home prices. In their setting pre-trends are unmodeled and parallel trends are implicitly assumed. However, their approach ultimately combines the role of anticipation and direct effects. Much of the cross-state variation in the Rice-Strahan index occurs in the 1994-1997 period, an interim between the announcement of deregulation and final implementation in 1997. As such, any differences in housing prices (and new loan originations) in this period are likely driven by speculation on the 1997 changes, rather than credit supply per-se.

This is not to say that anticipation manifests identically in stock and real estate markets. In fact, there are potentially richer intertemporal anticipation effects in housing. The closest analog of unconstrained investors in the stock market are investment home buyers, who played a significant role in the housing bubble of 2002-2008. As financing is less of an issue, these investors would optimally time their purchases in anticipation of buying by financially-constrained households. Yet in real estate markets even constrained agents—for example first-time buyers—may change their behavior in anticipation of a credit supply shock. While expected price increases might induce some to buy earlier, the anticipation of cheaper credit, combined with frictions in refinancing, might cause some to delay their purchases until credit becomes available. Ex-ante, it is not clear which effect dominates. As a result, we might actually find new loans rising or *falling* anticipatorily. Regardless, the edge case of parallel trends is unlikely to hold.

Finally, viewing the patterns of pre-2007 lending through lens of anticipation may be constructive in integrating competing narratives of the housing crisis. A dominant view of the housing crisis, following Mian & Sufi (2009), emphasizes the expansion of credit to subprime borrowers as the key driver of the boom and subsequent crash. However, recent research, (e.g. Adelino *et al.* , 2015, 2016; Albanesi *et al.* , 2017), has challenged this view. These papers highlight the role of relatively high-income/high credit score borrowers, as well as investors, in the wave of new loans that preceded the recession. Anticipatory effects can nest both views. If households are non-myopic, new credit even to some segment of the distribution will change expectations and demand for all borrowers.

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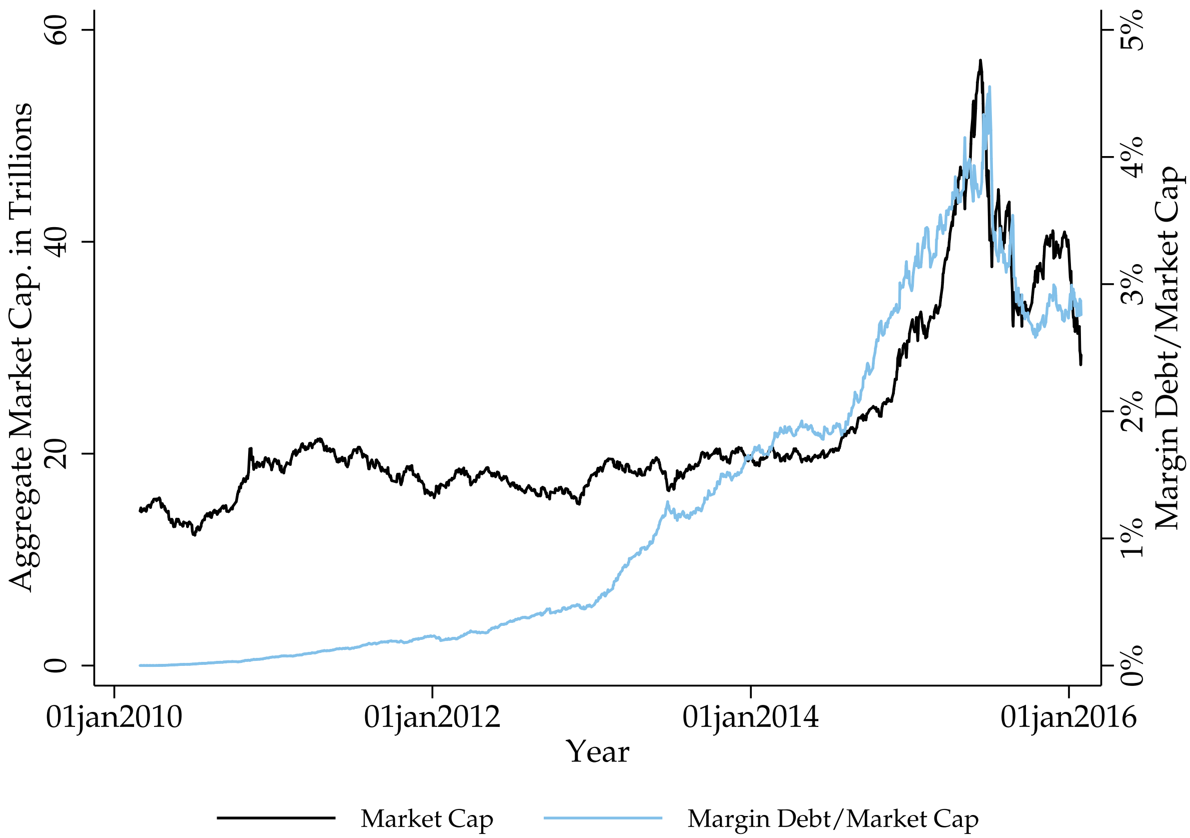
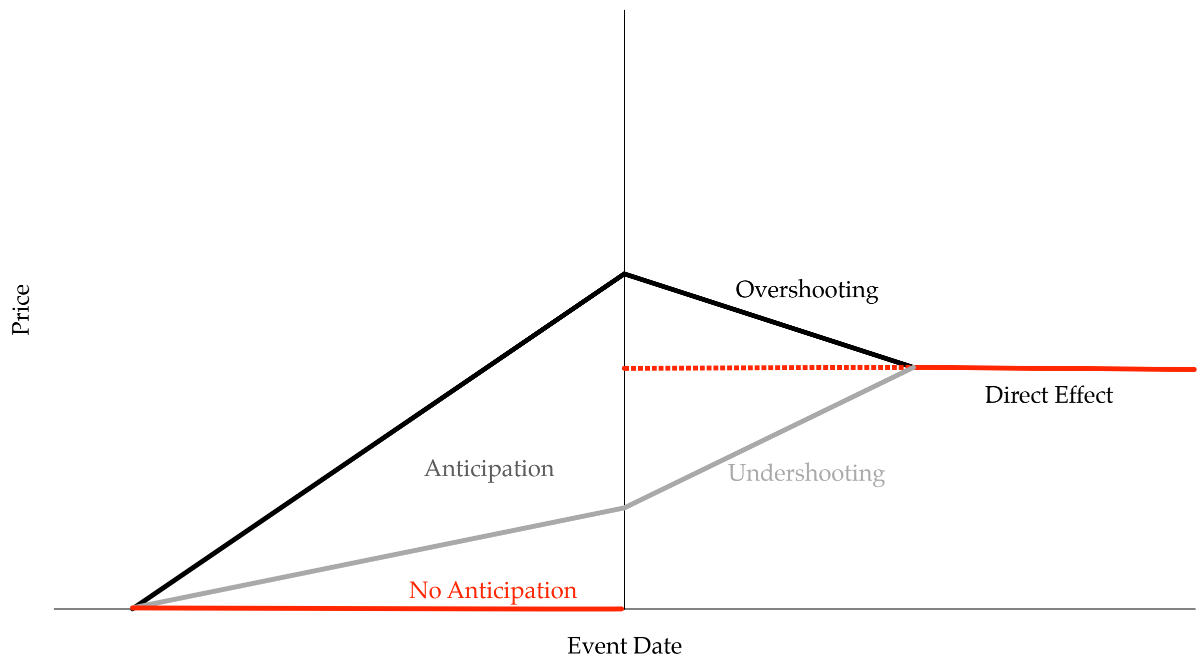
FIGURE 1: AGGREGATE MARKET CAP. AND MARGIN DEBT/MARKET CAP. OVER TIME



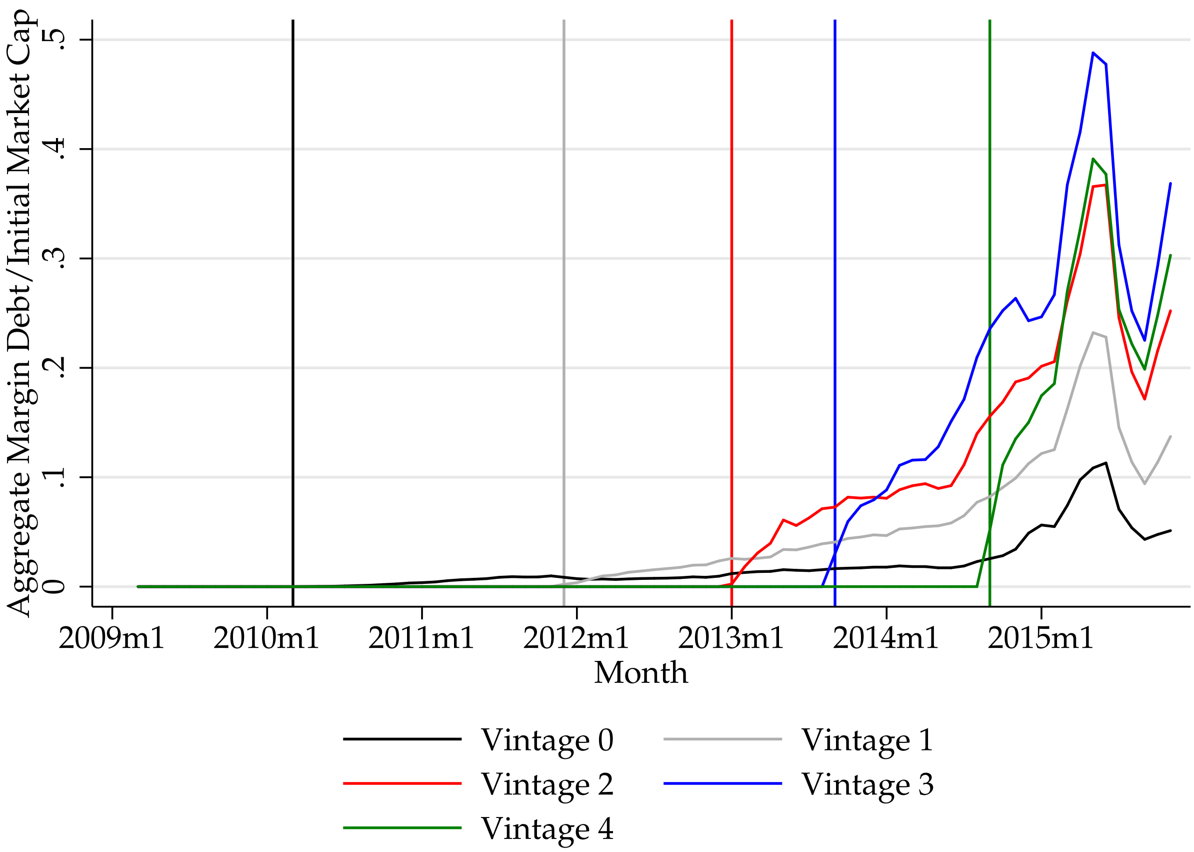
Notes: Plot shows monthly aggregate market cap (in black) and the ratio of margin debt to market cap (in blue) for all stocks in sample. Both market cap and margin debt are measured in trillions of yuan.

FIGURE 2: ANTICIPATION EFFECTS OF AN INCREASE IN CREDIT SUPPLY



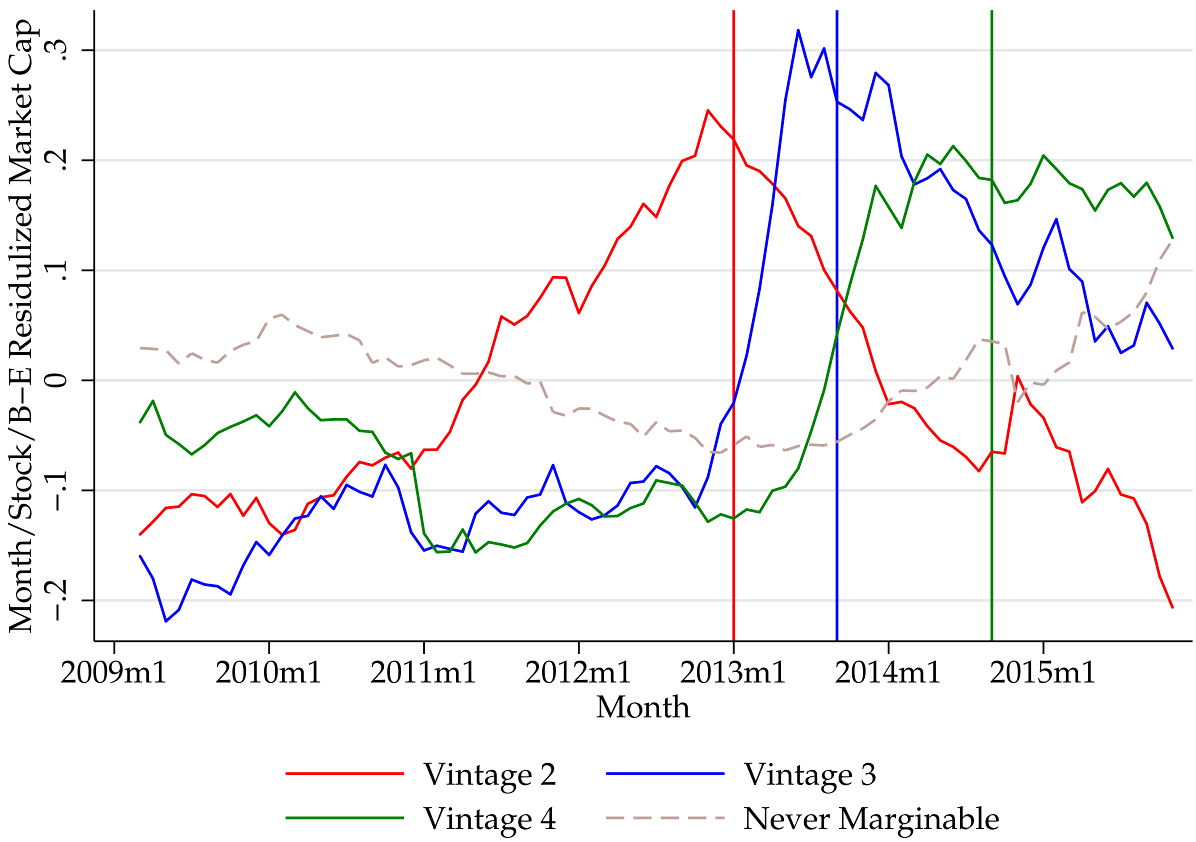
Notes: The y-axis is stock price. The x-axis is time with the vertical line representing the event date when credit supply increases. The red lines present no anticipation effects. The red line on the x-axis is stock price before the event date. The discrete jump in the red line represents the long-run direct effect of credit supply on price. The grey line represents anticipation effects that under-shoot the long-run direct effect. The black line represents anticipation effects that over-shoot the long-run direct effect

FIGURE 3: STAGGERED ROLLOUT OF STOCK MARGIN LENDING: MARGIN DEBT/MARKET CAP BY VINTAGE



Notes: Plot shows the average ratio of margin debt to initial market cap (measured as the 2009 average at the stock level) for each of the five vintages of the margin lending roll-out. Both market cap and margin debt are measured in trillions of yuan. Vertical lines show the starting date of each vintage, with black, gray, red, blue and green representing Vintages 0, 1, 2, 3 and 4, respectively.

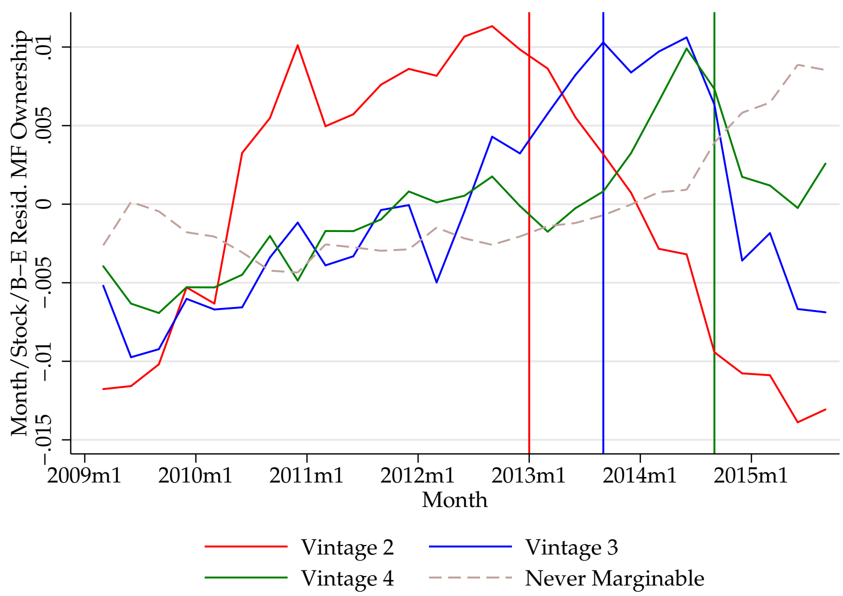
FIGURE 4: MARKET ANTICIPATION OF MARGIN LENDING ROLLOUT: RESIDUALIZED IHS(MARKET CAP)$_{t+1}$ BY VINTAGE



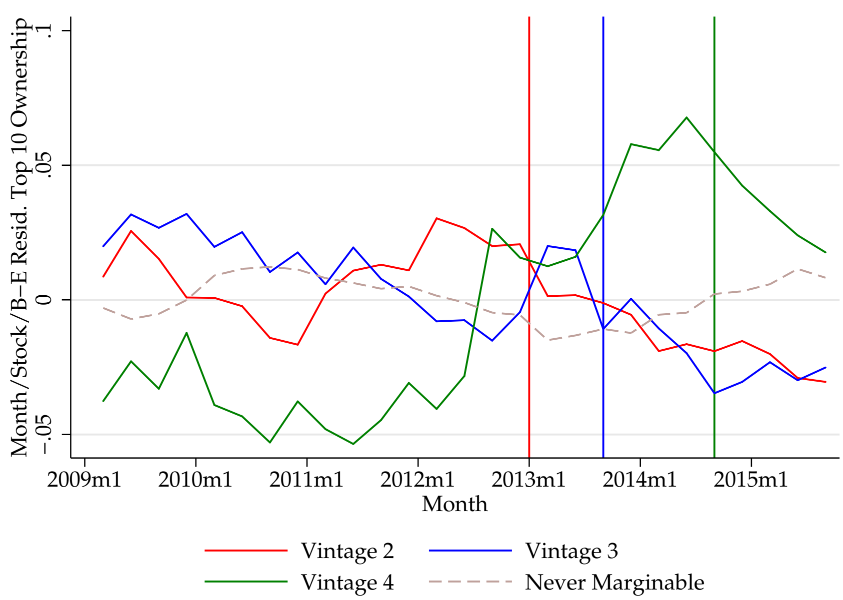
Notes: Plots show residuals from regressions of IHS(Market Cap)$_{t+1}$ at the stock-month level on stock fixed effects, month$\times$year fixed effects and dummies for membership in each decile of book equity at the month level. Market cap is measured in yuan. Residuals are calculated from a single regression with all stocks in sample, and plotted separately for Vintages 2, 3, and 4 of the margin lending roll-out and for the set of stocks that were never marginable. Vertical lines show the starting date of each vintage, with red, blue and green representing Vintages 2, 3 and 4, respectively.

FIGURE 5: UNCONSTRAINED INVESTORS' ANTICIPATION OF MARGIN LENDING ROLLOUT: RESIDUALIZED INSTITUTIONAL OWNERSHIP BY VINTAGE

Panel A: Mutual Fund Ownership Share

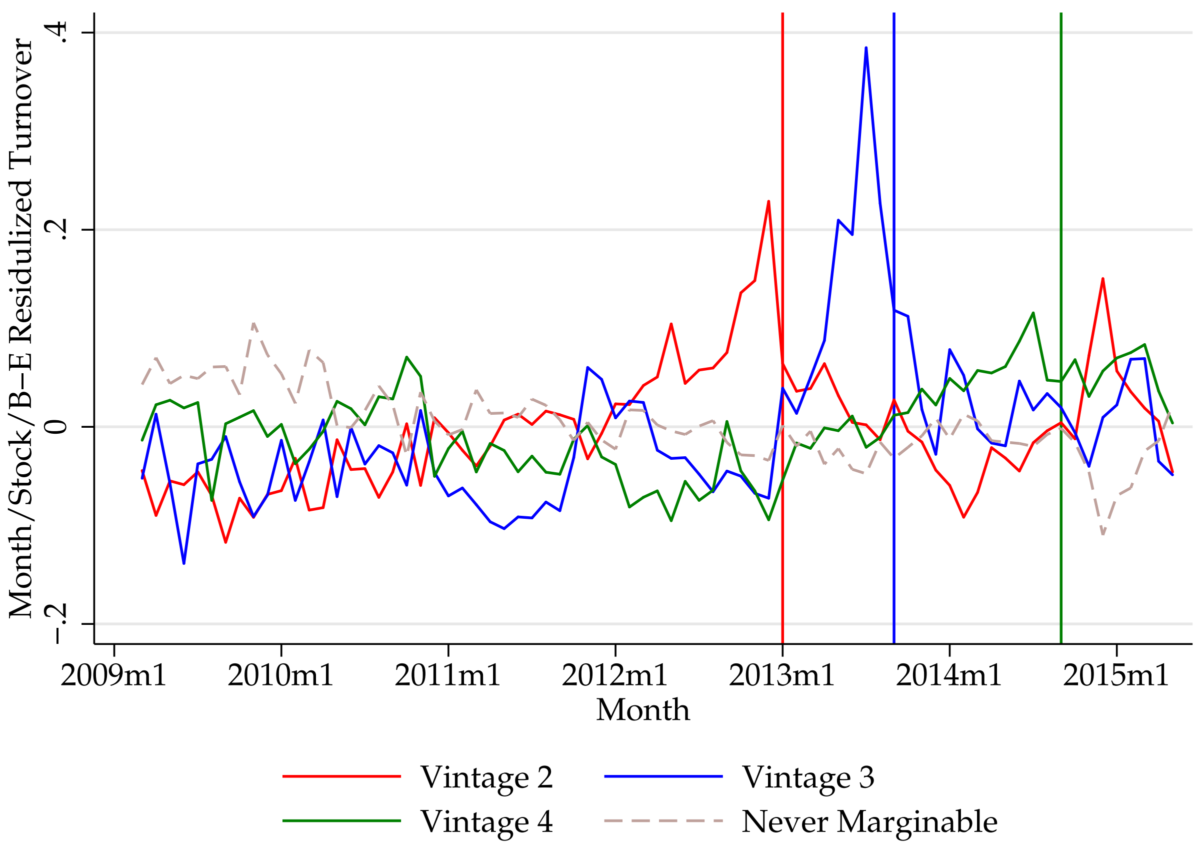


Panel B: Top 10 Investors Ownership Share



Notes: Plots show residuals from regressions of the proportion of institutional ownership at the stock-quarter level on stock fixed effects, quarter fixed effects and dummies for membership in each decile of book equity at the month level. Residuals are calculated from a single regression with all stocks in sample, and plotted separately for Vintages 2, 3, and 4 of the margin lending roll-out and for the set of stocks that were never marginable. Vertical lines show the starting date of each vintage, with red, blue and green representing Vintages 2, 3 and 4, respectively.

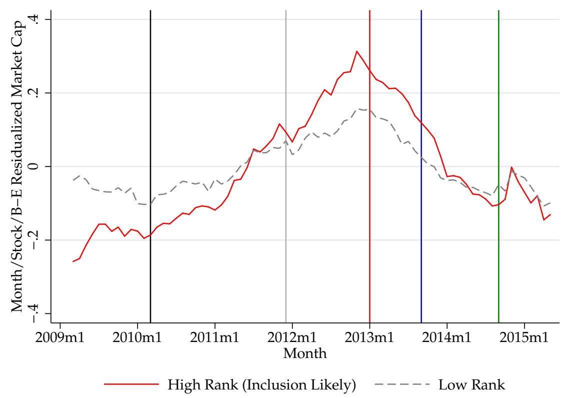
FIGURE 6: ANTICIPATORY MARKET ACTIVITY: RESIDUALIZED TURNOVER BY VINTAGE



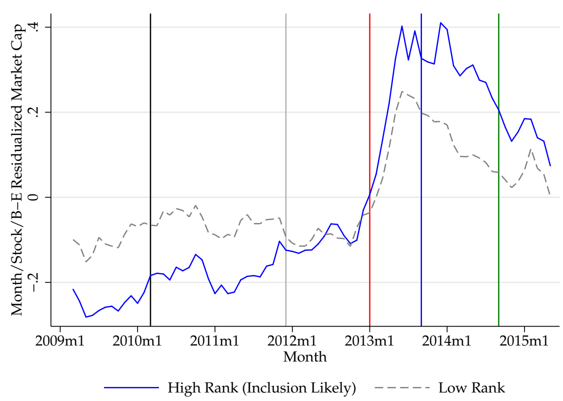
Notes: Plots show residuals from regressions of turnover at the stock-month level on stock fixed effects, month×year fixed effects and dummies for membership in each decile of book equity at the month level. Market cap is measured in yuan. Residuals are calculated from a single regression with all stocks in sample, and plotted separately for Vintages 2, 3, and 4 of the margin lending roll-out and for the set of stocks that were never marginable. Vertical lines show the starting date of each vintage, with red, blue and green representing Vintages 2, 3 and 4, respectively.

FIGURE 7: DIFFERENTIAL ANTICIPATION FOR PREDICTABLY MARGINABLE STOCKS: RESIDUALIZED IHS(MARKET CAP)$_{t+1}$ BY LIKELIHOOD OF INCLUSION

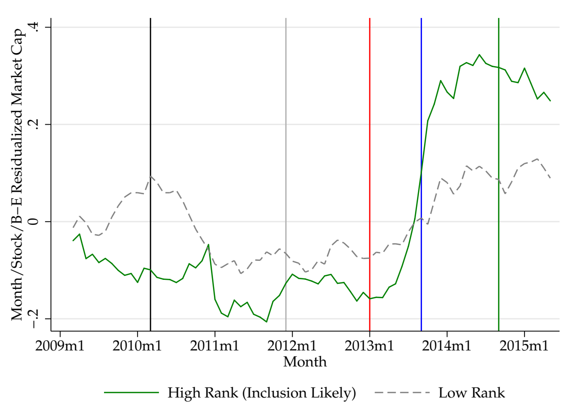
PANEL A: VINTAGE 2 STOCKS



PANEL B: VINTAGE 3 STOCKS

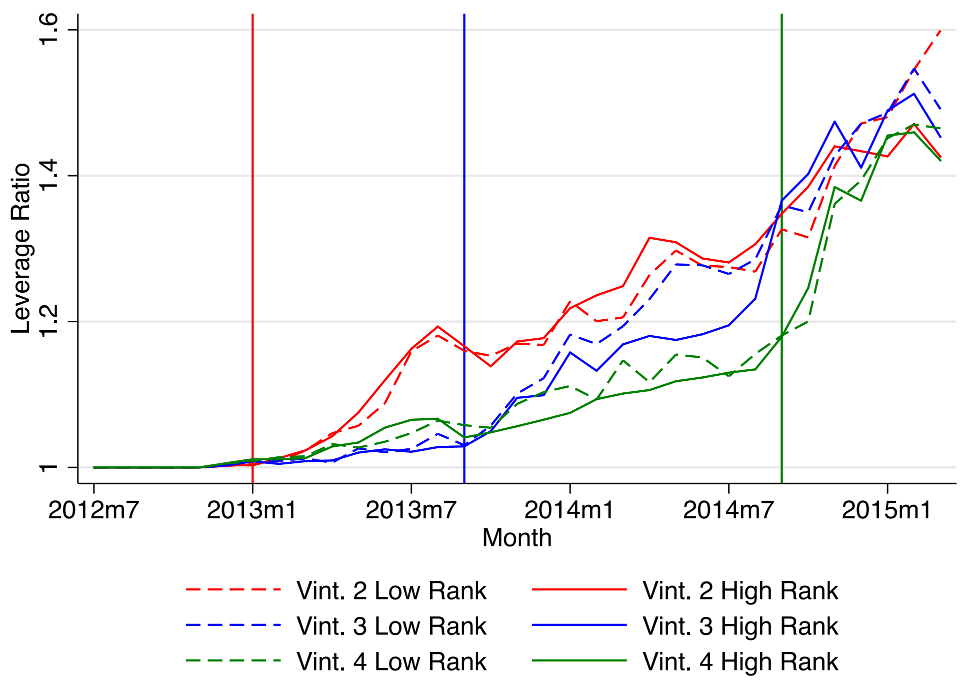


PANEL C: VINTAGE 4 STOCKS



Notes: Plots show residuals from regressions of IHS(Market Cap)$_{t+1}$ at the stock-month level on stock fixed effects, month$\times$year fixed effects and dummies for membership in each decile of book equity at the month level. Market cap is measured in yuan. Residuals are calculated from a single regression with all stocks in sample, but plotted separately—within stocks ultimately included in Vintages 2, 3 and 4—for stocks with above vs. below median rank on the index that determines inclusion in the vintage. Those with low rank were ex-ante the most likely to be included in the next vintage, whereas those with high rank were ex-ante the least likely to be included. Vertical lines show the starting date of each vintage, with black, gray, red, blue and green representing Vintages 0, 1, 2, 3 and 4, respectively.

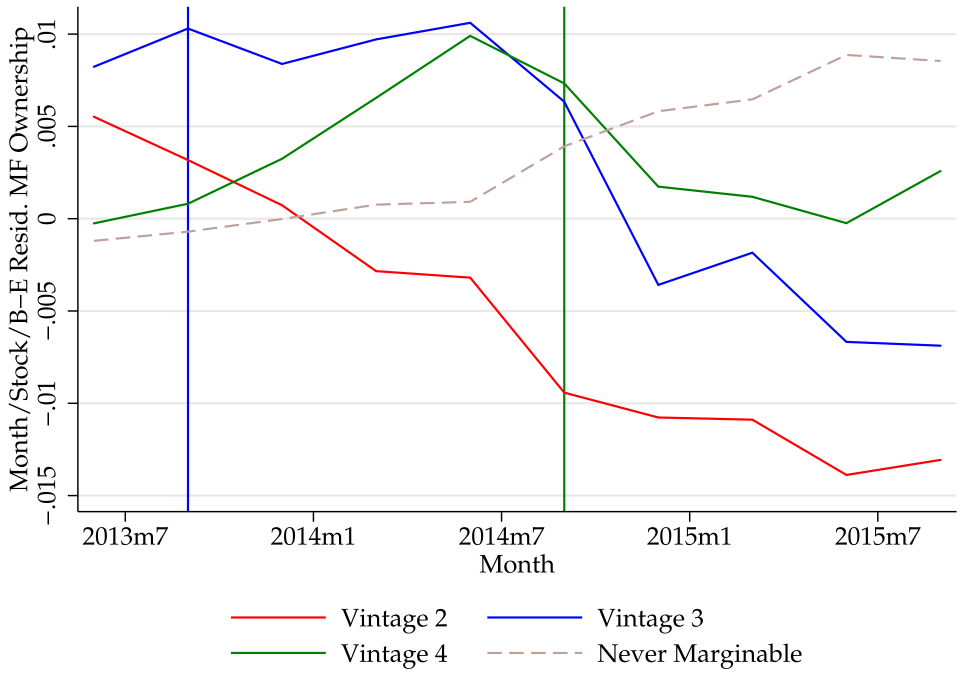
FIGURE 8: LEVERAGE BY LIKELIHOOD OF INCLUSION



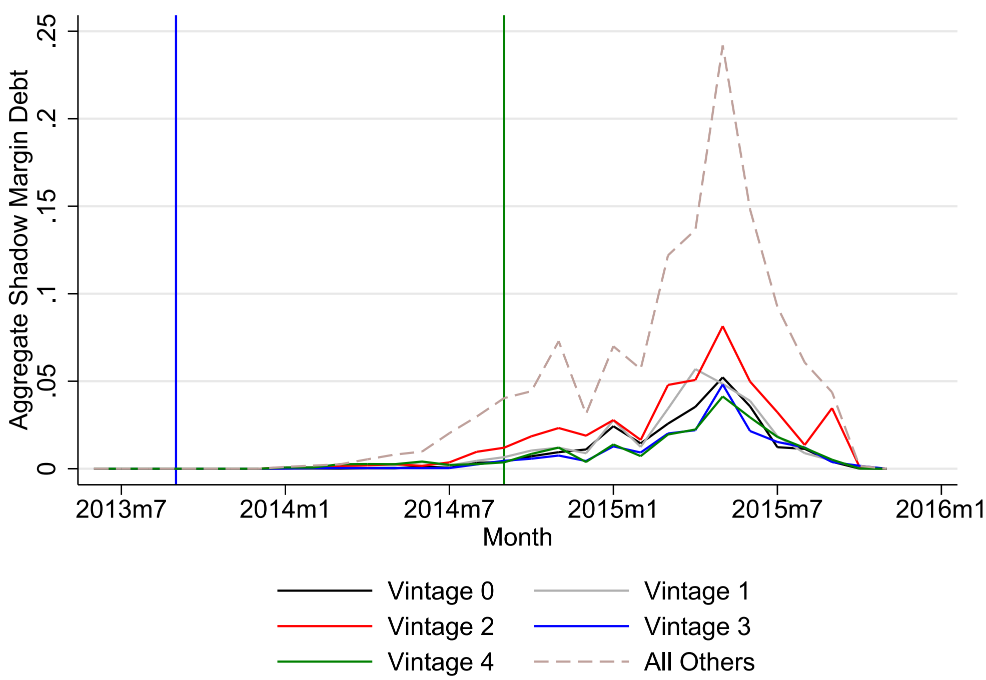
Notes: Plots show the 90th percentile of leverage across households that hold each stock, averaged, with in each vintage, over stocks with above vs. below median rank on the index that determines inclusion.

FIGURE 9: SHADOW MARGIN DEBT BY VINTAGE

Panel A: Residualized Mutual Fund Ownership From Mid-2013 On



Panel B: Aggregate Shadow Margin Debt



Notes: Panel A shows residuals from regressions of the proportion of mutual fund ownership at the stock-quarter level on stock fixed effects, quarter fixed effects and dummies for membership in each decile of book equity at the month level. Residuals are calculated from a single regression with all stocks in sample, and plotted separately for Vintages 2, 3, and 4 of the margin lending roll-out and for the set of stocks that were never marginable. Panel B shows aggregate shadow margin debt by vintage in trillions of yuan, calculated by scaling our observed shadow margin debt by a factor of 10. Vertical lines show the starting date of each of the last two vintages, with blue and green representing Vintages 3 and 4, respectively.

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
0	February 13th, 2010	50	40	51.74%
1	November 25th, 2011	131	60	66.31%
2	January 25th, 2013	163	113	75.23%
3	September 6th, 2013	104	102	77.95%
4	September 12th, 2014	104	114	78.48%

TABLE 2: PREDICTIVE REGRESSIONS OF MARGINABLE MEMBERSHIP (2ND, 3RD, AND 4TH VINTAGE)

	Vintage 2	Vintage 3	Vintage 4
Dep Var: D	(1)	(2)	(3)
τ	0.778***	0.776***	0.874***
	(0.042)	(0.051)	(0.041)
R^2	0.839	0.828	0.876
N	1,869	1,771	1,630

Coefficients from predictive regressions of marginable membership for Vintages 2–4 as,

$$D_i^k = \alpha_{0l} + \alpha_{1l}(Rank_i^k - C^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Rank_i^k - C^k)] + \epsilon_i$$

where $k = \{2, 3, 4\}$. D_i^k is the indicator, which equals one if stock i is added to the marginable list in Vintage k; $Rank_i^k$ is stock i's ranking that we produce based on exchanges' procedure. C^k is the number of stocks added to the marginable list in Vintage k. τ_i^k equals one if $Rank_i^k - C^k \leq 0$; otherwise zero (i.e., predicted marginable status based on our ranking). The sample only includes non-marginable stocks that satisfy screen criteria in the evaluation period. For each extension, we run the regression using the pooled sample of stocks in Shanghai and Shenzhen. The evaluation window is 2014/06/01-2014/08/31, 2013/06/01-2013/08/31, and 2012/10/01-2012/12/31, for the fourth, third, and second vintage, respectively. The point estimate of is reported and robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3: OLS ESTIMATES OF ASSOCIATION BETWEEN IHS(MARKET CAP) AND IHS(MARGIN DEBT)

	IHS(Market Cap)			
	(1)	(2)	(3)	(4)
IHS(Margin Debt)	0.081^{***}	0.038^{***}	0.025^{***}	0.005^{***}
	(0.003)	(0.002)	(0.003)	(0.001)
Mean of Dep. Var.	22.7	22.7	22.7	22.7
R^2	0.32	0.63	0.82	0.89
N	137698	137698	137696	137696
Book-Equity Deciles	No	Yes	Yes	Yes
Industry Fixed Effects	No	Yes	No	No
Month $\times$ Year Fixed Effects	No	No	Yes	Yes
Stock Fixed Effects	No	No	No	Yes

Coefficients from OLS regressions of the inverse hyperbolic sine (IHS) of market cap in month t on the inverse hyperbolic sine of margin debt in month t. Both market cap and margin debt are measured in RMB at the stock-month level. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Mean of dep. var refers to the mean of IHS(Market Cap)$_t$. Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4: IMPACT OF MARGIN LENDING ROLLOUT ON IHS(MARGIN DEBT)

	First Stage: IHS(Margin Debt)	
	Collapsed	Full
	(1)	(2)
Margin Trading Active	19.045^{***}	
	(0.201)	
Vintage 0 Margin Trading Active		18.650^{***}
		(0.487)
Vintage 1 Margin Trading Active		17.535^{***}
		(0.421)
Vintage 2 Margin Trading Active		19.581^{***}
		(0.191)
Vintage 3 Margin Trading Active		19.873^{***}
		(0.179)
Vintage 4 Margin Trading Active		20.081^{***}
		(0.232)
Mean of Dep. Var.	3.50	3.50
R^2	0.95	0.95
N	137696	137696
Book-Equity Deciles	Yes	Yes
Month $\times$ Year Fixed Effects	Yes	Yes
Stock Fixed Effects	Yes	Yes

Coefficients from regressions of IHS(Margin Debt) on the indicators *Margin Trading Active*. These indicators are 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. The column labeled *Collapsed* includes a single indicator for all stocks included in the rollout at any point. The column labeled *Full* includes separate *Margin Trading Active* indicators for each of the five vintages of stocks that became marginable. Margin debt is measured in RMB at the stock-month level. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Mean of dep. var refers to the mean of IHS(Margin Debt) or IHS(Market Cap). Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5: MARKET ANTICIPATION OF IMPACT OF MARGIN LENDING ROLLOUT ON IHS(MARKET CAP): NON-MYOPIC APPROACH

	IHS(Market Cap)					
	Difference-in-Difference			IV		
	Monthly Lags	Quarterly Lags	Myopic	Monthly Lags	Quarterly Lags	Myopic
	(1)	(2)	(3)	(4)	(5)	(6)
Ex-Post Effect	0.127^{***}	0.214^{***}	0.065^{***}			
	(0.025)	(0.031)	(0.023)			
IHS(Margin Debt)				0.007^{***}	0.011^{***}	0.003^{***}
				(0.001)	(0.002)	(0.001)
Ex-Ante Effect (t-1)	0.266^{***}	0.337^{***}		0.266^{***}	0.336^{***}	
	(0.051)	(0.034)		(0.052)	(0.035)	
Ex-Ante Effect (t-2)	0.272^{***}	0.281^{***}		0.272^{***}	0.280^{***}	
	(0.044)	(0.027)		(0.045)	(0.028)	
Ex-Ante Effect (t-3)	0.252^{***}	0.245^{***}		0.252^{***}	0.244^{***}	
	(0.039)	(0.029)		(0.040)	(0.029)	
Ex-Ante Effect (t-4)	0.228^{***}	0.186^{***}		0.228^{***}	0.185^{***}	
	(0.033)	(0.028)		(0.034)	(0.027)	
Ex-Ante Effect (t-5)	0.207^{***}	0.133^{***}		0.207^{***}	0.131^{***}	
	(0.027)	(0.027)		(0.028)	(0.027)	
Ex-Ante Effect (t-6)	0.199^{***}	0.086^{***}		0.198^{***}	0.084^{***}	
	(0.027)	(0.027)		(0.028)	(0.027)	
Mean of Dep. Var.	22.7	22.7	22.7	22.7	22.7	22.7
R^2	0.89	0.89	0.89	0.89	0.89	0.89
N	137696	137696	137696	137696	137696	137696
Book-Equity Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Month $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Results from non-myopic difference-in-difference and IV specifications of IHS(Market Cap)$_t$ on the margin lending roll-out. For our difference-in-difference specifications we report coefficients from the following regression

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

For the second stage of IV specifications, we replace Margin Trading Active with IHS(Margin Debt) in the above, and use Margin Trading Active as an instrument for IHS(Margin Debt) in a first stage. Market Cap and Margin Debt are measured in RMB at the stock-month level. *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 myopic approach includes no ex-ante effects, and is equivalent to the collapsed difference-in-difference approaches presented above. Non-myopic specifications include indicators aimed at capturing ex-ante effects for the six months and six quarters leading up to the roll-out for each stock. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Mean of dep. var refers to the mean of IHS(Market Cap)$_t$. Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 6: ANTICIPATION OF MARGIN LENDING ROLLOUT: INSTITUTIONAL OWNERSHIP AND TURNOVER

	Mutual Fund Share		Top 10 Share		Turnover	
	Quarterly Lags	Myopic	Quarterly Lags	Myopic	Quarterly Lags	Myopic
	(1)	(2)	(3)	(4)	(5)	(6)
Ex-Post Effect	-0.004	-0.006^{*}	-0.009	-0.016	0.087^{***}	0.055^{***}
	(0.003)	(0.003)	(0.013)	(0.011)	(0.016)	(0.016)
Ex-Ante Effect (t-1)	0.007^{***}		0.036^{**}		0.164^{***}	
	(0.002)		(0.016)		(0.034)	
Ex-Ante Effect (t-2)	0.006^{***}		0.033^{**}		0.081^{***}	
	(0.002)		(0.014)		(0.022)	
Mean of Dep. Var.	0.017	0.017	0.46	0.46	0.50	0.50
R^2	0.51	0.51	0.62	0.62	0.47	0.47
N	38819	38819	38819	38819	137696	137696
Book-Equity Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Month $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Results from non-myopic difference-in-difference specifications of either the proportion of institutional ownership or turnover on the margin lending roll-out in the vein of Malani and Reif (2015). We report coefficients from the following regression

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

Where $y_{i,t}$ represents either 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. *Margin Trading Active* is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in half-years 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. For each outcome, we include a myopic approach with no ex-ante effects and a non-myopic approach with two quarters of anticipation. Sample covers March 2009-May 2015. Mean of dep. var. refers to the mean of $y_{i,t}$ Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 7: DIFFERENCE IN IMPACT OF MARGIN LENDING ROLLOUT ON IHS(MARKET CAP): HIGH RANK (LIKELY TO BE MARGINABLE) VS. LOW RANK (LESS LIKELY TO BE MARGINABLE) STOCKS

	IHS(Market Cap)					
	Difference-in-Difference			IV		
	Monthly Lags	Quarterly Lags	Myopic	Monthly Lags	Quarterly Lags	Myopic
	(1)	(2)	(3)	(4)	(5)	(6)
Ex-Post Effect × High Rank	0.163^{***}	0.206^{***}	0.131^{***}			
	(0.049)	(0.054)	(0.045)			
Ex-Post Effect	0.114^{***}	0.162^{***}	0.067^{*}			
	(0.036)	(0.040)	(0.034)			
IHS(Margin Debt) × High Rank				0.008^{***}	0.010^{***}	0.006^{***}
				(0.002)	(0.003)	(0.002)
IHS(Margin Debt)				0.006^{***}	0.008^{***}	0.003^{**}
				(0.002)	(0.002)	(0.002)
Ex-Ante Effect (t-1) × High Rank	0.227^{***}	0.271^{***}		0.226^{***}	0.271^{***}	
	(0.044)	(0.046)		(0.043)	(0.046)	
Ex-Ante Effect (t-2) × High Rank	0.233^{***}	0.247^{***}		0.233^{***}	0.247^{***}	
	(0.038)	(0.046)		(0.035)	(0.046)	
Ex-Ante Effect (t-3) × High Rank	0.231^{***}	0.198^{***}		0.230^{***}	0.198^{***}	
	(0.039)	(0.047)		(0.037)	(0.047)	
Ex-Ante Effect (t-4) × High Rank	0.217^{***}	0.151^{***}		0.217^{***}	0.150^{***}	
	(0.038)	(0.044)		(0.035)	(0.043)	
Ex-Ante Effect (t-5) × High Rank	0.208^{***}	0.104^{**}		0.208^{***}	0.104^{**}	
	(0.039)	(0.042)		(0.037)	(0.042)	
Ex-Ante Effect (t-6) × High Rank	0.194^{***}	0.071^{*}		0.194^{***}	0.071^{*}	
	(0.039)	(0.039)		(0.037)	(0.038)	
Ex-Ante Effect (t-1)	0.252^{***}	0.281^{***}		0.252^{***}	0.282^{***}	
	(0.043)	(0.038)		(0.043)	(0.038)	
Ex-Ante Effect (t-2)	0.241^{***}	0.198^{***}		0.242^{***}	0.198^{***}	
	(0.038)	(0.032)		(0.038)	(0.032)	
Ex-Ante Effect (t-3)	0.211^{***}	0.159^{***}		0.212^{***}	0.158^{***}	
	(0.033)	(0.035)		(0.032)	(0.035)	
Ex-Ante Effect (t-4)	0.183^{***}	0.108^{***}		0.183^{***}	0.108^{***}	
	(0.024)	(0.032)		(0.021)	(0.032)	
Ex-Ante Effect (t-5)	0.144^{***}	0.083^{***}		0.144^{***}	0.082^{***}	
	(0.025)	(0.029)		(0.021)	(0.029)	
Ex-Ante Effect (t-6)	0.136^{***}	0.056^{**}		0.136^{***}	0.055^{**}	
	(0.034)	(0.027)		(0.033)	(0.027)	
Mean of Dep. Var.	22.4	22.4	22.4	22.4	22.4	22.4
R^2	0.83	0.83	0.82	0.83	0.83	0.82
N	117735	117735	117735	117735	117735	117735
Book-Equity Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Results from non-myopic triple-difference specifications of IHS(Market Cap)$_t$ on the margin lending roll-out, differentiated by high vs. low ranking stocks amongst those included in each vintage. For our triple-difference specifications we report coefficients from the following regression

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{it} + \eta_0 \text{Margin Trading Active}_{it} \times \text{High Rank}_{it}$$
$$+ \sum_{j=1}^{S} \left[\beta_j D_{i,t+j} + \eta_j D_{i,t+j} \times \text{High Rank}_{it} \right] + \gamma_i + \delta_t + \varepsilon_{it}$$

For the second stage of IV specifications, we replace Margin Trading Active with IHS(Margin Debt) everywhere in the above, and use Margin Trading Active and Margin Trading Active$_{it}$ × High Rank$_{it}$ as instruments for IHS(Margin Debt) and IHS(Margin Debt)$_{it}$ × High Rank$_{it}$ in a first stage. Market Cap and Margin Debt are measured in RMB at the stock-month level. High rank is a dummy variable that indicates stocks in each vintage that are above median rank within the vintage according to the index that determines inclusion. *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 myopic approach includes no ex-ante effects, and is equivalent to the collapsed difference-in-difference approaches presented above. Non-myopic specifications include indicators aimed at capturing ex-ante effects for the six months and six quarters leading up to the roll-out for each stock. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Vintages 0 and 1 are excluded as inclusion in those vintages was not based upon a pre-defined rule. Mean of dep. var refers to the mean of IHS(Market Cap)$_t$. Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 8: DIFFERENCE IN IMPACT OF MARGIN LENDING ROLLOUT ON IHS(MARKET CAP): EARLY (UNPREDICTABLE) VS. LATER (PREDICTABLE) VINTAGES

	IHS(Market Cap)					
	Difference-in-Difference			IV		
	Monthly Lags	Quarterly Lags	Myopic	Monthly Lags	Quarterly Lags	Myopic
	(1)	(2)	(3)	(4)	(5)	(6)
Ex-Post Effect$\times$ Late Vintage	0.311^{***}	0.263^{***}	0.272^{***}			
	(0.047)	(0.048)	(0.045)			
Ex-Post Effect	-0.087^{**}	0.016	-0.109^{***}			
	(0.040)	(0.045)	(0.036)			
IHS(Margin Debt)$\times$ Late Vintage				0.016^{***}	0.013^{***}	0.014^{***}
				(0.003)	(0.003)	(0.002)
IHS(Margin Debt)				-0.005^{**}	0.001	-0.006^{***}
				(0.002)	(0.003)	(0.002)
Ex-Ante Effect (t-1)$\times$ Late Vintage	0.422^{***}	0.364^{***}		0.423^{***}	0.364^{***}	
	(0.039)	(0.041)		(0.035)	(0.041)	
Ex-Ante Effect (t-2)$\times$ Late Vintage	0.374^{***}	0.352^{***}		0.376^{***}	0.352^{***}	
	(0.043)	(0.045)		(0.042)	(0.045)	
Ex-Ante Effect (t-3)$\times$ Late Vintage	0.344^{***}	0.323^{***}		0.345^{***}	0.323^{***}	
	(0.039)	(0.038)		(0.037)	(0.039)	
Ex-Ante Effect (t-4)$\times$ Late Vintage	0.303^{***}	0.248^{***}		0.304^{***}	0.248^{***}	
	(0.039)	(0.036)		(0.037)	(0.036)	
Ex-Ante Effect (t-5)$\times$ Late Vintage	0.230^{***}	0.203^{***}		0.231^{***}	0.202^{***}	
	(0.048)	(0.038)		(0.048)	(0.039)	
Ex-Ante Effect (t-6)$\times$ Late Vintage	0.195^{***}	0.184^{***}		0.196^{***}	0.183^{***}	
	(0.043)	(0.046)		(0.043)	(0.046)	
Ex-Ante Effect (t-1)	-0.053	0.059		-0.054^{**}	0.060	
	(0.032)	(0.037)		(0.027)	(0.038)	
Ex-Ante Effect (t-2)	-0.015	0.096^{**}		-0.016	0.096^{**}	
	(0.032)	(0.036)		(0.027)	(0.037)	
Ex-Ante Effect (t-3)	-0.014	0.205^{***}		-0.015	0.205^{***}	
	(0.033)	(0.033)		(0.030)	(0.033)	
Ex-Ante Effect (t-4)	-0.010	0.145^{***}		-0.011	0.146^{***}	
	(0.033)	(0.027)		(0.032)	(0.027)	
Ex-Ante Effect (t-5)	0.019	0.106^{***}		0.017	0.105^{***}	
	(0.042)	(0.028)		(0.041)	(0.028)	
Ex-Ante Effect (t-6)	0.034	0.058^{**}		0.033	0.058^{**}	
	(0.029)	(0.028)		(0.026)	(0.028)	
Mean of Dep. Var.	22.7	22.7	22.7	22.7	22.7	22.7
R^2	0.89	0.89	0.89	0.89	0.89	0.89
N	137696	137696	137696	137696	137696	137696
Book-Equity Deciles	Yes	Yes	Yes	Yes	Yes	Yes
Month $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Results from non-myopic triple-difference specifications of IHS(Market Cap)$_t$ on the margin lending roll-out, differentiated by early versus late vintages. For our triple-difference specifications we report coefficients from the following regression

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{it} + \eta_0 \text{Margin Trading Active}_{it} \times \text{Late Vintage}_{it}$$
$$+ \sum_{j=1}^{S} \left[\beta_j D_{i,t+j} + \eta_j D_{i,t+j} \times \text{Late Vintage}_{it} \right] + \gamma_i + \delta_t + \varepsilon_{it}$$

For the second stage of IV specifications, we replace Margin Trading Active with IHS(Margin Debt) everywhere in the above, and use Margin Trading Active and Margin Trading Active$_{it}$ $\times$ Late Vintage$_{it}$ as instruments for IHS(Margin Debt) and IHS(Margin Debt)$_{it}$ $\times$ Late Vintage$_{it}$ in a first stage. Market Cap and Margin Debt are measured in RMB at the stock-month level. Late Vintage is a dummy variable that indicates stocks in Vintages 2, 3 and 4. *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 myopic approach includes no ex-ante effects, and is equivalent to the collapsed difference-in-difference approaches presented above. Non-myopic specifications include indicators aimed at capturing ex-ante effects for the six months and six quarters leading up to the roll-out for each stock. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Mean of dep. var refers to the mean of IHS(Market Cap)$_t$. Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 9: NON-MYOPIC ESTIMATES OF MARGIN DEBT ON MARKET CAP: PLACEBO TESTS

	IHS(Market Cap)$_{t+1}$							
	Never Marginable			Pre-Period		High vs. Low Rank (Never Marginable)		
	Monthly Lags	Quarterly Lags	Myopic	Monthly Lags	Myopic	Monthly Lags	Quarterly Lags	Myopic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ex-Post Effect	-0.271^{***}	-0.268^{***}	-0.272^{***}	-0.329^{***}	-0.346^{***}	-0.273^{***}	-0.264^{***}	-0.270^{***}
	(0.034)	(0.038)	(0.032)	(0.053)	(0.045)	(0.042)	(0.047)	(0.038)
Ex-Post Effect × High Rank						0.003	-0.009	-0.003
						(0.060)	(0.067)	(0.054)
Ex-Ante Effect (t-1)	-0.039	0.002		-0.064^{*}		-0.075^{**}	-0.025	
	(0.027)	(0.036)		(0.036)		(0.033)	(0.043)	
Ex-Ante Effect (t-2)	0.008	0.013		0.007		-0.026	0.010	
	(0.027)	(0.031)		(0.035)		(0.034)	(0.040)	
Ex-Ante Effect (t-3)	0.028	0.028		0.031		0.001	0.039	
	(0.026)	(0.029)		(0.033)		(0.033)	(0.038)	
Ex-Ante Effect (t-4)	0.019	0.021		0.094^{***}		0.004	0.036	
	(0.026)	(0.027)		(0.033)		(0.032)	(0.035)	
Ex-Ante Effect (t-5)	0.005	-0.002		0.063^{**}		0.002	0.026	
	(0.024)	(0.025)		(0.026)		(0.032)	(0.033)	
Ex-Ante Effect (t-6)	0.007	-0.013		0.033		-0.002	0.012	
	(0.022)	(0.024)		(0.024)		(0.031)	(0.032)	
Ex-Ante Effect (t-1) × High Rank						0.073	0.054	
						(0.050)	(0.059)	
Ex-Ante Effect (t-2) × High Rank						0.071	0.006	
						(0.050)	(0.057)	
Ex-Ante Effect (t-3) × High Rank						0.056	-0.021	
						(0.049)	(0.052)	
Ex-Ante Effect (t-4) × High Rank						0.030	-0.032	
						(0.048)	(0.049)	
Ex-Ante Effect (t-5) × High Rank						0.006	-0.057	
						(0.044)	(0.045)	
Ex-Ante Effect (t-6) × High Rank						0.019	-0.051	
						(0.041)	(0.044)	
Mean of Dep. Var.	22.8	22.8	22.8	22.8	22.8	22.8	22.8	22.8
R^2	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89
N	149009	149009	149009	149009	149009	149009	149009	149009
Book-Equity Deciles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stock Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Results from non-myopic difference-in-difference and triple-difference specifications of IHS(Market Cap)$_t$ on indicators for a *placebo* treatment analogous to the deregulation of margin trading. To generate a placebo treatment group, a treatment start date was randomly selected, and the top 100 stocks according to the screening and ranking rule were selected in each exchange using data for the three months prior to that date. In specifications labeled "Never Marginable" the top 100 stocks were chosen after excluding any stocks that actually qualified for margin lending under the official deregulation. In specifications labeled "Pre-Period", no stocks were excluded, but placebo dates were chosen only in the period preceding the actual start date of Vintage 2. We report coefficients from the following regressions:

$$\text{IHS(Market Cap)}_{i,t} = \alpha + \beta_0 \text{Placebo Active}_{it} + \eta_0 \text{Placebo Active}_{it} \times \text{High Rank}_{it}$$
$$+ \sum_{j=1}^{S} \left[\beta_j D_{i,t+j} + \eta_j D_{i,t+j} \times \text{High Rank}_{it} \right] + \gamma_i + \delta_t + \varepsilon_{it}$$

Placebo Active$_{it}$ is equal to one for stocks selected as part of the placebo treatment group only in the period after the randomly selected treatment start date. Market Cap is measured in RMB at the stock-month level. High rank is a dummy variable that denotes the above median (top 50) stocks in the placebo treatment group for each exchange. $D_{i,t+j}$ is equal to one for placebo treatment stocks if the placebo treatment start date is exactly period $t+j$, and zero otherwise. The number of *ex-ante effect* coefficients indicates the value of S for the regression in question. The myopic approach includes no ex-ante effects. The first five columns omit any interactions with High Rank$_{it}$. Non-myopic specifications include indicators aimed at capturing ex-ante effects for the six months and six quarters leading up to the roll-out for each stock. Standard errors, clustered at the stock and month level, are included in parentheses. Sample covers March 2009-May 2015. Vintages 0 and 1 are excluded as inclusion in those vintages was not based upon a pre-defined rule. Mean of dep. var refers to the mean of IHS(Market Cap)$_t$. Book-equity deciles refer to dummy variables for each decile of book equity in the previous fiscal year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 10: CHARACTERISTICS OF MARGIN ACCOUNT

	Mean	SD	P01	P5	P10	P25	P50	P75	P90	P95	P99
N_Stock	7.4	12.1	1.0	1.0	1.3	2.5	4.6	8.5	14.7	21.4	47.7
Asset	134,421.1	536,113.7	879.4	3,325.8	6,546.7	17,742.4	46,744.7	116,690.3	272,122.0	464,103.0	1,334,955.9
Loan	14,118.3	176,718.8	0.0	0.0	0.0	0.0	0.0	3,919.9	23,374.3	53,877.6	217,202.3
Lev	1.09	0.30	1.00	1.00	1.00	1.00	1.00	1.03	1.18	1.45	3.22
# unique accounts	18,593										

This table presents the time-series average of cross-sectional summary statistics of margin accounts' characteristics. N_Stock is the number of stocks in the account's portfolio at month end. $Asset$ is the market value of the total assets in the account, and $Loan$ is the amount of outstanding margin loan (in yuan). Lev equals $Asset$ divided by $Asset$ minus $Loan$. The sample is from June 2012 to December 2015 and includes 18,593 unique margin investors.