Riding the Credit Boom*

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August 9, 2019

Abstract

From 2010-2015, China liberalized margin lending, resulting in an unprecedented expansion of margin loans to financially constrained households. We implement a regression discontinuity design based on the ranking procedure used during the deregulation and estimate a large impact of this credit boom on asset prices. However, this direct effect—the focus of most academic and policy work—is only half the story. As theory predicts, we find that unconstrained speculators front-ran predictably marginable stocks, generating price-overshooting. Among mid-cap stocks favored by households, the direct effect accounts for 20% of their 2015 bubble, while front-running led households to pay 8.5% higher prices.

*We thank Jeremy Stein, Jiang Wang, Jiangze Bian, Zhi Da, Dong Lou, Emi Nakamura, Haizhou Huang, Matthieu Gomez, Yu Qin, Baolian Wang, and seminar participants at Seoul National University, CICF, Cavalcade Asia-Pacific meeting, Norges Bank, Aalto, INSEAD, the Summer Institute for Finance at SAIF, CIFFP at UIBES, the NBER Conference on the Chinese Economy at Tsinghua University and Columbia University for helpful comments, and Jingxuan Chen for excellent research assistance. Email inquiries to c.hansman@imperial.ac.uk.

1 Introduction

An important macro-finance literature associates credit cycles with asset price boom-bust patterns—typically using panel regressions that exploit cross-country (e.g. Borio & Lowe, 2002; Schularick & Taylor, 2012) or cross-county variation (e.g. Mian & Sufi, 2009).[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 regulators.

Theories addressing these empirical patterns emphasize a "direct" effect: an expansion of bank lending to financially constrained households leading to asset price bubbles and financial fragility via a variety of mechanisms. A non-exhaustive list includes (1) complacent or neglectful creditors underestimating downside or tail risk (Minsky, 1977; 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) optimism and leverage constraints (Geanakoplos, 2010); and (4) intermediary frictions or balance sheets (Bernanke & Gertler, 1989; Kiyotaki & Moore, 1997; Adrian & Shin, 2010). At the core of many such narratives is the notion that new buying by previously constrained households—who are implicitly price-inelastic—is responsible for deviations from fundamental values and excessive price volatility.

In this paper we utilize the 2010-2015 deregulation of margin lending in the Chinese stock market, which coincided with a large bubble and crash, to estimate the direct effect of credit-supply on asset prices. We then show that these direct effects are only half of the story. Theoretically, if the impact of of credit-supply is large, anticipatory speculation becomes ex-ante profitable. Rather than waiting for constrained agents to receive credit, unconstrained investors have strong incentives to buy and drive prices up in advance. This front running may, in turn, further destabilize markets and lead to overshooting.

The scale of China's margin deregulation make it an ideal context in which to study credit booms. In contrast to liberalizations of margin lending in other countries, which we discuss further

[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), Baron & Xiong (2017), and Muir (2017). Credit booms might also be measured using credit spreads as opposed to bank debt to GDP ratios (i.e. Krishnamurthy *et al.* , 2015; López-Salido *et al.* , 2017).

below, there was a *dramatic* expansion of margin loans immediately following the deregulatory event in China. Beginning in 2010, there was a rapid rise of margin debt, eventually reaching 3.5 percent of GDP and roughly 4.5 percent of market capitalization (see Figure 1). At the peak, nearly 2 trillion yuan (or roughly 350 billion dollars) of margin loans were supplied to Chinese households. A key driver of this aggressive growth, relative to other historical experiences, was the concerted effort of the Chinese government to build out the market for margin loans. Specifically, the government created the China Securities Finance Corporation (CSFC), whose mission was to make subsidized loans to brokerage houses.[2]

In other words, the Chinese liberalization event generated a large credit supply shock, providing a window into the impact of credit booms on asset prices. Since China has stringent short-sales constraints, speculation gave rise to a bubble (Scheinkman & Xiong, 2003; Geanakoplos, 2010), which subsequently gave way to a crash and government bailouts. That is, the Chinese stock market had a credit-fueled speculative stock market bubble which—given the lack of corresponding productivity increases in China during this period—is suggestive of the "direct effect" narrative found in the literature.

China's unique implementation of the deregulation allows us to estimate both the direct effect and the role of anticipation by unconstrained speculators. China did not open margin lending to all stocks at once. Instead, the authorities introduced margin debt gradually in a series of several *vintages*, or sets of stocks. To determine the specific stocks that would be included in each vintage, the authorities committed to a formal screening and ranking rule. This meant that stocks qualified for margin lending according to a published formula based on publicly available real-time data on market capitalization and trading volume. High-frequency variation in the inputs to this formula generated ex-ante *local uncertainty* over the specific set of qualifying stocks for each vintage.

This local uncertainty is the key to our first empirical exercise: estimating the direct effect of margin debt on stock prices. Because slight movements in market capitalization or turnover might cause one stock to qualify or another to be disqualified, investors could not perfectly predict the set of stocks in each vintage prior to the introduction of margin debt.[3] As a result, we are able

[2]CSFC, established in 2011, is wholly state owned and describes itself as "the only institution that provides margin financing loan services to qualified securities companies in China's capital markets." The interest rate of the loans from CSFC is typically 1 to 2% higher than the interbank market rate. For more information about CSFC, see https://www.ft.com/content/c1666694-248b-11e5-9c4e-a775d2b173ca.

[3]Or, more precisely, until the exact set of stocks in each vintages was announced by regulators. This coincided quite

to isolate the direct effect using a regression discontinuity approach that compares the stocks just below versus just above the cut-off ultimately used to determine marginability.

We find that stocks just above the cut-off saw a sharp influx of margin debt in the year following its introduction—our approach suggests that marginability generated an increase in margin debt, which, in turn, led to a non-trivial increase in asset prices. Our estimates suggest that marginability generated an increase in one year cumulative returns of more than 20 percent. Alternatively, they indicate that an influx of margin debt on the order of 1 percent of market cap generates a 6 percent return.

The stocks at the center of our regression-discontinuity analysis are in the middle of market capitalization distribution (mid-cap). They are typically favored by households seeking speculative profits, relative to stocks further up the size distribution. Our numbers imply an inelastic demand curve that is in line with estimates for indexing effects found for small to mid-capitalization stocks in the US stock market (Chang *et al.* , 2014). Given that total margin debt reached 4.5 percent of aggregate market capitalization at the 2015, our estimates suggest that this new source of credit accounts for a nearly 30 percent increase in valuations among these mid-cap stocks. As we elaborate on below, this represents almost 20 percent of the total run up in the market capitalization of these stocks during the boom.

Existing empirical work on the causal effect of credit supply on asset prices typically utilizes deregulatory or supply side shocks that increase credit supply for some group (treatment) and not others (control) (see, e.g., Favara & Imbs, 2015; Di Maggio & Kermani, 2017). To identify a direct effect, these studies generally rely on a parallel-trends assumption, that is, that ex-ante differences between treatment and control groups enable the researcher to construct a valid counterfactual for the treatment group. However, as we discuss below, such an assumption is theoretically puzzling in the context of credit supply shocks. Unless a credit expansion is entirely unexpected, or all investors are constrained, anticipatory speculation will generate compromising pre-trends.

Isolating the direct effect as distinct from anticipation therefore requires a credit supply shock that is not predictable ex-ante. This makes rule-based regression discontinuity approaches—as used here and in earlier work by Kahraman & Tookes (2017)—ideal, so long as there is ex-ante un-

closely with the introduction of margin debt. In our primary specifications, we focus on the announcement date, rather than the implementation date of deregulation.

certainty regarding the precise set of qualifying assets. The primary difference between that work and our paper is the setting and scale. Using data from India, Kahraman & Tookes (2017) find that relatively small quantities of margin debt allowed market makers (i.e. margin traders) to stabilize markets during normal periods, i.e. borrow to buy stocks with liquidity shocks, and destabilize markets in downturns. However, given that the average quantity of daily margin outstanding to these traders was only around 88 million dollars, they find limited impacts on prices.[4] This stands in sharp contrast to our setting, in which roughly 350 billion dollars (2 trillion yuan) of margin debt was extended to financially constrained Chinese households, resulting in large price effects.

After estimating direct effects locally around the marginability cut-off, we turn to investigating the role of anticipation. If the direct effect of credit supply is large—as we document—then anticipatory speculation becomes ex-ante profitable for stocks far above the cut-off. Because these stocks are almost certain to qualify for margin, unconstrained and sophisticated investors such as hedge funds and mutual funds face strong incentives to buy in advance and drive up prices. Once margin debt is available, they may then sell their shares gradually to price-inelastic retail investors, who use new credit to buy at inflated prices. (De Long *et al.* , 1990; Lakonishok *et al.* , 1992; Brunnermeier & Pedersen, 2005). In other words, when credit supply shocks are predictable, they should be expected to lead to front-running, predatory trading, or more general riding the bubble strategies (Abreu & Brunnermeier, 2003; Hong & Stein, 1999; Stein, 2009) which may, in turn, lead to price overshooting.[5]

We test several predictions of this front running hypothesis by comparing the full set of soon-to-be-marginable stocks (largely comprising those far above the cutoff, which were almost sure to qualify) to all non-marginable stocks (largely comprising those far below the cutoff, which were almost sure *not* to qualify). In the absence of front-running, we would expect price and trading effects to begin only when or after margin becomes available. Alternatively, with front-running effects, we expect prices and trading—i.e. buying by unconstrained investors—to rise in advance of new credit supply. These increases need not be instantaneous and should be gradual to the extent that unconstrained investors have holding costs or uncertainty regarding the likelihood

[4]Earlier papers on margin lending deregulation in other markets, such as Hong Kong, find similarly muted price effects using different identification strategies (e.g. Cheng *et al.* , 2007).

[5]Existing empirical research has also 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).

or form of deregulation. Moreover, once the roll-out of margin lending begins, the returns of marginable stocks may actually fall due to overshooting as unconstrained investors begin to sell their positions.

To capture these patterns, we propose and implement both event-studies and a non-myopic difference-in-difference estimator (Malani & Reif, 2015) to test for front-running and appropriately measure (net) ex-post effects. Our approach allows for differential ex-ante effects amongst soon-to-be-marginable stocks, enabling us to account for attenuation due to pre-trends and explicitly quantify the role of front-running. We find strong evidence of anticipatory speculation. Using our event study approach, we estimate cumulative returns for soon-to-be-marginable stocks to be between 16 and 23 percent in the year leading up to the roll-out, depending on the adjustments we use for returns. Furthermore, this front-running appears to have generated overshooting: we estimate a reversal of returns of at least 8.5 percent after the roll-out of margin lending using our difference-in-difference approach. In other words, front-running led households to pay as much at least 8.5 percent more than they would have absent unconstrained speculators.

A major concern is the possibility that our estimated front-running effects are mechanically driven by the screening and ranking rule, which selects large high-turnover stocks. The rule might, for example, make stocks with abnormally positive returns prior to deregulation more likely to be treated. To address this concern, we implement a series of placebo tests—using data from the Chinese stock market bubble that took place between 2001 and 2007—and show that our estimated effects are not mechanical.

A series of complementary specifications further support the hypothesis that unconstrained investors speculated on the roll-out of margin debt. Both mutual funds and other large investors rapidly increased their holdings in soon-to-be-marginable stocks in the months leading up to each wave of deregulation. Turnover in these stocks surged in the same period. Furthermore, we see stronger price effects in the stocks furthest above the cut-off ex-ante—those with the highest likelihood of marginability.

To conclude our analysis, we use brokerage house data on the portfolios of retail investors to verify that retail margin buyers are highly price-inelastic, which suggests that front-running may indeed be destabilizing.[6] Finally, we provide additional evidence of front-running prior to the

[6]In this analysis, we also provide a discussion of indirect effects — spillovers of the introduction of margin lending

introduction of shadow margin lending at the end of 2014, which is thought to have played a key role in the subsequent crash of the Chinese stock market (Bian *et al.* , 2017a,b).

Beyond our empirical results, we see three further takeaways from our analysis. First, using difference-in-difference or event study based designs to measure the direct effect of credit supply on asset prices is challenging. Unless all investors are surprised, myopic or constrained we should expect anticipatory investment in advance of credit supply shocks. This means that either pre-trends will exist, in which case estimates of direct effects will be biased downwards, or that researchers must restrict themselves to special—and often theoretically puzzling—cases in which anticipation is not present.

Second, and relatedly, pre-trends created by speculation and front-running are not simply a confound to estimating a relevant effect, but are economically meaningful and policy relevant in their own regard. Front-running may generate excess volatility and lead constrained households to buy assets at higher prices than they would in a myopic world, with both welfare and distributional consequences. If the dangers of credit booms stem in part from speculation in anticipation of credit—rather than the extension of credit per-se—then anti-speculative measures may be productively included in the macroprudential policy toolkit.

Finally, speculation may play an underappreciated role in debates over the importance of credit-expansion to subprime borrowers during the financial crisis emphasized by Mian & Sufi (2009). Recent research (e.g. Adelino *et al.* , 2016; Albanesi *et al.* , 2017) has challenged the focus on subprime credit, highlighting the role of investors and relatively high-income/high credit score borrowers—in other words, unconstrained agents—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.

Our paper proceeds as follows. In Section 2, we provide background on the Chinese deregulation of margin lending and describe our data. In Section 3, we develop our regression discontinuity estimates for the direct effect. In Section 4, we present evidence of the importance of front-running in credit booms. We work out the implications of our analysis in Section 5. We conclude in Section 6.

onto never marginable stocks — which would if anything downward bias our estimates.

2 Background and Data

2.1 China's staggered deregulation of margin lending

Between early 2010 and late 2014, Chinese regulators gradually begin to allow margin lending on certain stocks listed on the Shanghai and Shenzhen exchanges. The deregulation occurred in two overall phases. In the first phase, which we refer to as the pilot, regulators allowed the stocks belonging to major stock market indexes to be purchased on margin. In the second phase, regulators progressively expanded margin lending, including specific stocks on the basis of a published formula that incorporated market capitalization and share turnover. Because our identification strategy is centered around this formula, we focus our analysis on the second phase. Throughout, retail investors with at least 500,000 RMB of assets in their brokerage account and six months or more of trading experience qualified for margin—provided by their brokerage firms—with an initial margin requirement of 50 percent. Interest rates on margin loans from brokerage firms were generally around 8 to 9 percent annualized, significantly lower than the rates on shadow margin loans through informal channels (which typically ranged from 11 to 14 percent).[7]

The pilot itself was implemented in two stages. On February 13th, 2010 90 stocks were opened to margin lending (*Pilot A*). The stocks selected were simply those included in the two major stock market indices: the Shanghai 50 Index (50 stocks) and the Shenzhen Component index (40 stocks). On November 25th, 2011, the Chinese government extended the list of marginable stocks based on membership in two broader market indices (*Pilot B*). The extended list included 278 stocks: 180 from the Shanghai 180 Index and 98 from the Shenzhen 100 Index.

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

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

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

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

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

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

This ranking procedure, effectively a value weighted average of a stock's size and trading volume, was conducted separately by the Shanghai (SH) and Shenzhen (SZ) Stock Exchanges.

Margin lending was ultimately expanded to three vintages using this procedure. The set of stocks included in *Vintage 1* was announced January 25th, 2013, and margin lending for these stocks was implemented on January 31st, 2013. Similarly, *Vintage 2* was announced on September 6th, 2013 and implemented on September 16th and *Vintage 3* was announced on September 12th, 2014, and implemented on September 22nd, 2014. For the purposes of our analysis, which is at the monthly level, there is no distinction between announcement and implementation. By the time Vintage 3 was implemented, roughly 900 stocks in total could be bought on margin across the two exchanges.

Table 1 summarizes the timeline of deregulation and the number of newly marginable stocks for each extension. The stocks in Vintages 1-3 are mid-cap stocks that encompass roughly 12% of total market capitalization. Figure 2 plots the rise of margin debt relative to market capitalization separately for each of the three vintages. At the peak of the Chinese stock market bubble in 2015, the ratio of margin debt to market capitalization in these stocks was nearly 10 percent.

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

Following 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

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

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

and total market capitalization. This ratio increased from 0.5 percent around the end of 2012 to 4.5 percent in June 2015. In yuan terms, total margin debt increased from a negligible amount at the beginning of 2012 to roughly two trillion yuan in 2015.

Coincident with the high level and rapid growth of margin debt, the Chinese stock market experienced an enormous boom. 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. It then 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

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

A crucial piece of our analysis regarding front-running effects concerns the trading behavior of unconstrained investors. While the margin lending deregulation targeted households facing financial constraints, there are many institutional investors in China who do not face such constraints. These include insurance companies and 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 used in studies of trading by institutional investors. While data on institutional ownership in China is not quite as high quality as in the US, public companies in China do have to disclose the largest ten shareholders and their ownership in quarterly financial reports. The majority of top 10 shareholders are institutions such as insurance companies, brokerages, and occasionally mutual funds. While not a perfect measure, this variable is likely to be highly correlated with institutional ownership in a stock, reflecting 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.

3 Regression Discontinuity Estimates of the Direct Effect

3.1 Defining the Inclusion Index and Marginability Threshold

In this section we use a regression discontinuity approach to estimate the direct effect of margin lending on asset prices. We focus on a discontinuity in the formula used to determine marginability (we refer to this formula, which is shown in Equation 1, as the inclusion index). Because only a fixed number of stocks could be included in each vintage, a sharp cut-off exists at the value of the inclusion index held by the lowest ranking stock. In principle, stocks to the right of this value qualified for margin debt, and stocks to the left did not. Furthermore, because both the date at which the stocks in each vintage were to be chosen and the precise number of stocks included in each vintage was unknown ex-ante, investors could not perfectly predict the set of stocks included in each vintage. As a result—in a small neighborhood around the threshold—the introduction of margin debt to qualifying stocks can be plausibly viewed as a unexpected credit expansion.

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

The second step is to define the relevant discontinuity in the inclusion index. In theory, this discontinuity should be sharp and exactly equal to the value of the index for the lowest ranking included stock (for each vintage and exchange). In practice, however, the discontinuity is slightly less sharp for two reasons. Firstly, there is some uncertainty over our ability to precisely replicate the procedure used by regulators, either because the window we use to collect data on market capitalization may not be perfectly aligned, or because of minor ambiguities in the screening procedures used to rule out certain stocks. Secondly, and more importantly, there was some room at the margin for discretion on the part of the exchanges, with little in the way of published detail. This meant that stocks ranking well above the lowest included stock were occasionally excluded, and, similarly, stocks ranked low enough to be disqualified were occasionally included.

To prevent this discretion from contaminating our discontinuity, we define our threshold as follows: (i) for each exchange and each vintage, we rank the full set of not-yet-marginable stocks; (ii) we then take the realized number of stocks actually included and set the threshold to be the index value of the stock with a ranking equal to that number. For example, if 100 stocks were included, the threshold is defined to be the index value for the 100th ranked stock, whether or not it was actually the lowest ranking stock included. We define C_E^k to be the threshold for vintage k in exchange E.

There is little evidence that investors or insiders were able to manipulate the rankings of particular stocks locally around the threshold C_E^k. While the basic inputs into the index could certainly have been influenced to some extent, uncertainty over the exact number of stocks included in each vintage made precise control around the threshold effectively impossible. In Figure 3, we plot histograms of the inclusion index around C_E^k, which we normalize to 0 for both exchanges and all vintages. In Panel (a) we include the closest 100 stocks on each side of the threshold for each vintage and exchange and in Panel (b) we restrict the sample to values of the index within 1 of the threshold. Both panels show that the distribution of stocks is relatively smooth across the threshold. Indeed, McCrary (2008) tests fail to reject the null hypothesis of no bunching around the threshold.

3.2 First Stage: A Discontinuity in Marginability and Margin Debt at the Threshold

We now turn to showing that the threshold C_E^k is indeed associated with a discontinuity in the probability that margin lending is introduced for a given stock. This is displayed most clearly in Panel (a) of Figure 4. In this figure, we once again include data from all vintages and exchanges, normalizing C_E^k to 0. The x-axis represents the inclusion index, our running variable. On the y-axis, we display the probability that a stock becomes marginable. The scatter plot shows averages within bins of width 0.05 in the index. Lines shows local linear fits with 95% confidence intervals on either side of the threshold. A sharp jump in the probability of marginability is evident at the threshold.

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

$$D_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + \varepsilon_i^k. \tag{2}$$

Here τ_i^k indicates that stock i is above the marginability threshold, that is, it is equal one if $Index_i^k \geq C_E^k$ and 0 otherwise. θ_k represents a vintage fixed effect. Our coefficient of interest is α_{0r}, representing the discrete change in the probability of marginability at the threshold. In our baseline specification, shown above, we include separate linear slopes on each side of the threshold (local linear regressions with a rectangular kernel), although we also display local linear regressions with a triangular kernel. We include all not-yet-marginable stocks that satisfy the screening rule, and use the covariate adjusted MSE optimal bandwidths described in Calonico *et al.* (2018)[11] and include standard errors based upon the three nearest neighbor variance estimators described in Calonico *et al.* (2014) (CCT).

In line with the pattern shown in Panel (a) of Figure 4, our regression results show a large and significant jump at the threshold. In column (1) of Table 2, $\hat{\alpha}_{0r}$ is 0.509 and significant at the 1% level, suggesting that being just to the right of the threshold is associated with a roughly 50 percentage point jump in the probability of marginability. Our estimates using a triangular kernel are nearly identical, at 0.496. The fact that these coefficients are smaller than 1 indicates the importance of discretion in the deregulation process.

Corresponding to the sharp increase in marginability, Panel (b) of Figure 4 shows that there is also a sharp increase in the total quantity of margin debt used to purchase stocks just above the

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

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

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

3.3 Reduced-form: Price Effects at the Threshold

As our threshold corresponds to a sharp increase in marginability and margin debt, we next turn to our central question: what is the impact of this shock to credit on asset prices? To begin, we take a reduced-form approach and estimate the impact of being just above versus just below the threshold on stock returns. Specifically, we examine whether stocks just above the threshold saw higher cumulative returns in the month, 3 months or 12 months following the announcement and implementation of each vintage.

Figure 5 shows plots similar to those in Figure 4. The inclusion index is displayed on the x-axis (normalized to set the threshold to 0), with cumulative raw returns on the y-axis. These plots show the basic pattern we flesh out more formally below. In the first month, returns for stocks just above the threshold are only slightly higher than returns for those below the threshold. This suggests that the immediate influx and price impact of margin debt was not huge. This result acts as a sort of sanity check: there were not massive differences in returns above vs. below the threshold at baseline. However, a large and statistically significant difference is evident for 3 month returns and persists through 12 month returns.

We next estimate these effects using a regression discontinuity approach analogous to the one outlined in Equation 2:

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

Here, Ret_i^k refers to the cumulative return for stock i in the 1, 3 or 12 months following the announcement of Vintage k. We consider both raw cumulative returns, and size and book-to-market adjusted returns. For the latter, we follow a Daniel *et al.* (1997) (DGTW) style adjustment using independent sorts of quintiles of size and book-to-market to get 25 portfolios.[12] Each stock is assigned to one of these 25 bins. The equal-weighted returns in each bin then serve as the benchmark for that stock's adjustment. For our baseline specifications, we choose bandwidths and estimate standard errors exactly as in Table 2. In the Appendix we show a series of robustness exercises with varying bandwidths.

Our results, presented in Table 3, align with the plots shown in Figure 5. In the first column of the top panel, we see a small and only marginally significant impact of 2.7 percent on one month raw cumulative returns. However, by 3 months, we see highly statistically significant returns of just over 10 percent, suggesting a large price impact for stocks just above the threshold. These effects appear to persist through a year, as we see an impact on 12 month returns of 9.5 percent.

The remaining columns of the top panel show that these results are not dependent on the linear spline specification shown in Equation 3. We see statistically indistinguishable results allowing for local linear regressions with a triangular kernel on either side of the threshold, although the point estimates are marginally smaller (8.5 percent) at three months, and marginally larger (14.7 percent) at 12 months. We see similar results when using DGTW adjusted returns rather than raw returns, as shown in the bottom panel. In Appendix Tables A.II and A.III we show that these results are also not sensitive to the choice of bandwidth. We see similar results when using either the bandwidth selection procedure suggested by Imbens & Kalyanaraman (2012) or setting the bandwidth to 0.5 to allow comparability across specifications.

[12] Since there is no momentum in the Chinese stock market, we do not condition on past price changes when calculating DGTW benchmark portfolios.

3.4 Quantifying the Direct Effect

While the reduced form effects documented above indicate that the introduction of margin debt generated economically significant returns for stocks just above versus just below the threshold, the fuzzy nature of the discontinuity makes understanding these results difficult. In this subsection we provide more directly interpretable estimates and contextualize our results.

To begin, we quantify the direct effect of becoming marginable using a fuzzy regression discontinuity approach, which accounts for the fact that the threshold does not perfectly predict marginability. To do so, we report two-stage least squares estimates, where the first stage is given by Equation 2, and the second stage is given by:

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

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

Our results are reported in Table 4. Unsurprisingly, the qualitative patterns are similar to those presented in Table 3, with smaller returns at 1 month and larger and sizeable and significant returns at 3 and 12 months. Our results suggest raw cumulative returns of 17-18 percent at 3 months and 25-28 percent at 12 months. Similarly, we see DGTW adjusted returns of 12-13 percent at three months, and 21-23 percent at 12 months. In summary, we estimate that marginability generated 12 month cumulative returns on the order of 21-28 percent. While large, these estimates should be taken in context of the broader boom in China during our sample, during which aggregate market capitalization grew substantially.

Of course, beyond the average impact of marginability in this sample, the impact relative to the *quantity* of margin debt is also relevant. A simple way to quantify this relationship is by considering the ratio of the impact on returns of being above the threshold, as shown in Table 3 to the impact on margin debt shown in Table 2. For example, Column 3 of Table 2 suggests that by three months after the implementation there was a sharp difference in the quantity of margin debt used to purchase stocks just above the threshold—equivalent to 1.7 percent of market cap, on average. Meanwhile, Column 2 of Table 3 indicates that by the same point, these stocks had also experienced just over 10 percent higher returns. Together, these two estimates suggest that an influx of margin debt equal to 1 percent of market cap generates a return of 6 percent.

Note that, at the peak of the margin lending boom, outstanding margin debt through formal margin lending was equal to approximately 4.5 percent of aggregate market capitalization.[13] If each percent of market cap indeed generates a 6 percent return, this means that the peak of margin debt generated a 27 percent return on average for our set of mid-cap stocks. Given that total market capitalization of these stocks went up by roughly 150 percent during the bubble period, our calculations suggests that conservatively around 20 percent of the total increase in their market cap is directly attributable to margin debt.

Our results suggest that both previously constrained and unconstrained investors (e.g. those using margin debt and not) had relatively inelastic demand for marginable stocks. To help contextualize these magnitudes, our final step is to relate our findings to the literature on the stock price impact of indexing. A useful metric used in that literature to quantify the elasticity of non-indexed market participants is:

$$Elasticity = -\frac{1}{Return/\%\Delta Demand}, \tag{5}$$

where $Return$ is the return associated with an indexing event and $\%\Delta Demand$ is the amount of money indexed to the stock as a fraction of the market capitalization of the stock (Wurgler & Zhuravskaya, 2002). Of course indexing is a special event, as it entails guaranteed buying regardless of price. Consequently, this formula is derived under the assumption that the source of $\Delta Demand$ is perfectly inelastic. In our context, we take $\Delta Demand$ to be the increase in margin debt relative to market cap. Naturally, the source of this shift in demand may not be as inelastic as that in an indexing event. However, we believe treating it as such is a useful exercise to assist in understanding our findings, and not unreasonable given the relative inelasticity of the retail investors taking advantage of the deregulation of margin debt.

To generate an estimate of the elasticity, we again take the three month return of 10.3 percent from Column 2 of Table 3 and take as our estimate of $\Delta Demand$ the 1.7 percent estimated in Column 3 of Table 3. Using the formula provided in Equation 5, we estimate an elasticity of -0.17.

[13] 4.5 percent is a conservative estimate as some mid-cap stocks had margin to market capitalization of closer to 6% and it neglects informal margin debt coming through alternative channels. We discuss the role of this “shadow” margin debt below.

While this suggests that unconstrained investors were relatively inelastic, it is not entirely out of line with the literature on indexing. For example, Chang *et al.* (2014) find an elasticity for Russell 2000 stocks that is anywhere from -1.46 to -0.39, depending on assumptions about $\Delta Demand$.

Earlier studies from other markets cited above suggest that demand curves for large stocks, those in Pilots A and B, are more elastic than mid-cap stocks but are still significantly downward-sloping (see, e.g., Shleifer (1986)). To the extent we are willing to make this extrapolation, and given how large our direct effects for mid-cap stocks are to begin with, we can conclude from our analysis that margin debt is also likely to matter significantly for large stocks, though to a lesser degree.

4 Estimates of Front-Running Effect

Our estimates above focus on estimating the direct effect of margin debt in a neighborhood around the marginability threshold. Our strategy relies on the assumption of local unpredictability, that is, that it would have been very difficult for an investor to predict which stocks would qualify very close to the threshold. However, most stocks were not particularly close. In fact, predicting marginability ex-ante was likely relatively easy for the majority of stocks: either a stock was well above the threshold and almost sure to qualify, or well below and almost sure not to qualify.

We expect to see very different dynamics for the set of stocks that investors were confident would qualify. If margin debt indeed has a direct effect on asset prices—as the previous section shows—then unconstrained investors have an incentive to purchase sure-to-qualify stocks in advance.[14] As such, we would expect these investors to anticipate the rollout of margin debt and drive prices up in advance, with little or no sharp increase in prices the moment margin debt becomes active.

Furthermore, if the new demand enabled by margin debt is highly inelastic—as the magnitudes in Section 3 suggest—then we may expect to see more than just anticipation. Where this is the case, theory (especially De Long *et al.* , 1990; Brunnermeier & Pedersen, 2005) predicts that unconstrained investors will optimally front-run stocks that are sure to become marginable. That

[14] Of course, they also may have incentive to front-run any stock with some positive probability of becoming marginable. In fact, it is possible, and perhaps even likely, that there was some anticipatory purchasing of stocks near the threshold. The key to our regression discontinuity strategy is simply that there be no sharp difference in anticipation right at the threshold.

is, they will buy in large quantities in advance to drive prices above fundamental values and, once margin debt is available, sell out slowly to new inelastic investors, gradually allowing prices to fall. The destabilizing effects of institutional investors speculating on the path of prices are well-established theoretically in the literature and can arise in a variety of settings (See, e.g. Lakonishok *et al.* , 1992, for a discussion).

In other words, the key predictions of anticipation or front-running of soon to be marginable stocks (relative to those well below the cutoff) are twofold: (i) positive expected returns before the marginability date accompanied by buying from unconstrained agents (e.g. institutional investors), and, if margin debt enabled demand is relatively inelastic, (ii) negative expected returns after the marginability date accompanied by selling by unconstrained agents. Both of these predictions suggest distinct economic phenomena from a simple direct effect, with potential distributional and welfare implications. We label (i) an anticipation effect and label (ii) an overshooting effect.

Figure 6 provides further background on the distinctions between these phenomena. The x-axis represents time, with the vertical black line corresponding to the date of some credit supply expansion. The y-axis represents the price of some asset which can be purchased with this credit. In a world with no anticipation or front-running, we would expect the path of prices to follow the red lines: flat before the event date, with a sharp jump to a higher price immediately or shortly the event. The difference in the two prices represents the direct effect.

In our context, this pattern corresponds to an increase in price after a stock qualifies for margin lending, driven by buying by previously constrained households. This is what is being captured by our regression discontinuity estimates. The gray line to the left of the event date represent two forms of anticipation on the part of unconstrained investors, both featuring a run-up in prices in advance of the event driven by unconstrained investors. Note that this run-up is gradual to the extent that unconstrained investors face carrying costs or a reduction in uncertainty as the date approaches. The gray line captures the case of perfect foresight, in which investors push price to a long run level commensurate with the direct effect, and do not attempt to further profit on the expansion of margin debt. The black line captures optimal-front-running driven overshooting. In what follows, we provide empirical evidence of both anticipation and overshooting surrounding the introduction of margin debt.

4.1 Event study comparing marginable to non-marginable stocks

To test for both anticipation and overshooting, we begin with a simple event-study style approach in Table 5. Using this strategy, we find strong evidence that unconstrained investors drove the prices of soon-to-be marginable stocks up in advance, leading to price reversion after margin debt was introduced.

We construct our estimates as follows. For each of Vintages 1, 2 and 3, we consider the cross section of all stocks that are either (i) included in the corresponding vintage or (ii) not marginable at the time margin debt was introduced for that vintage. We pool these together and consider cumulative returns in the periods just before and just after the announcement/implementation of margin debt. Specifically, we consider regressions of the form:

$$Ret_i^k = \beta_0 \text{Marginable}_i^k + \theta_k + \varepsilon_i^k \tag{6}$$

Here Ret_i^k is again the cumulative return measured as either the raw return or the DGTW adjusted return, considered in 1, 3 and 12 month windows leading up to and following the announcement/implementation month. Marginable$_i^k$ is an indicator equal to one if stock i becomes marginable in vintage k. θ_k is an indicator equal to one if the observation is included in the cross-section corresponding to vintage k, and captures the average return for non-marginable stocks in the relevant window. Our coefficient of interest is then β_0 which captures the deviation in cumulative returns from the average for other non-marginable stocks. We cluster our standard errors at the stock level.

The top panel of Table 5 shows that the relative returns for newly marginable stocks were, on average, negative in the period immediately following marginability. In this panel we use cumulative returns in the month, 3 months, or 12 months following marginability as the dependent variable. In terms of raw cumulative returns newly marginable stocks underperform non-marginable stocks by by 15.4 percent in the 12 month window. Using DGTW returns, they underperform by 3 percent. These negative returns are relevant for two reasons. First, they are highly inconsistent with an *unexpected* direct effect caused by margin debt, which would produce positive returns. In other words, the fact that the coefficient is non-positive suggests that, for most stocks, the direct effect was priced in at the time of marginability. Furthermore, the fact that these estimates are on average negative is consistent with front-running driven overshooting.

The bottom panel of figure Table 5 more directly captures the presence of anticipation itself. In this panel we consider cumulative returns from 1 month, 3 months or 12 months before marginability up until the month of announcement/implementation itself. We see strong evidence of *positive* returns in the period preceding marginability. Raw cumulative returns were 2.4 percent higher in the 3 months preceding marginability and 16 percent higher in the 12 months preceding marginability. Similarly, DGTW returns were 23 percent higher in the 12 months preceding marginability. In other words, the returns on soon-to-be marginable stocks were positive on average in the year leading up to marginability, suggesting that unconstrained investors differentially purchased these stocks in anticipation of the introduction of margin debt.

The results in Table 5 can be visualized vintage by vintage in Figure 7. Panel (a) plots the log of monthly market cap—after netting out stock, month, and book-equity decile fixed effects—and displays evidence of sharp rises in market cap for Vintages 1, 2 and 3 in anticipation of the introduction of margin debt. Furthermore, we see negative trends following the introduction itself, consistent with overshooting. Panel (b) plots cumulative DGTW returns from March 2011 onwards, and displays virtually the same pattern.

4.2 Difference-in-difference approach

In order to more precisely capture both front-running and overshooting we next consider quasi-myopic difference-in-difference specifications following Malani & 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 (ex-post effects).

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

$$Return_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (7)$$

Here Margin Trading Active$_{i,t}$ is an indicator equal to one if stock i is eligible for margin trading in month t. γ_i and δ_t represent stock i and month t fixed effects, respectively. The key to this approach is the inclusion of a series of dummies to allow differential effects for treated stocks in the period just before deregulation. These are captured by the indicators $D_{i,t+j}$, which are equal to one if margin trading initially becomes active for stock i in period $t + j$, and zero otherwise. Put more simply, $D_{i,t+j}$ is variable that, for a specific stock i, indicates that margin lending is about to roll-out. S captures the number of periods in advance investors might feasibly speculate upon the coming introduction of margin lending. In the stylized example shown in Figure 6, $\beta_1, ..., \beta_S$ would capture the upward trend preceding the event date, while β_0 captures the difference between the average and the baseline price in the period *after* the event date.[15] The standard difference-in-difference approach in the literature is simply a special case in which we set for $\beta_j = 0$ for $j > 0$.

As a dependent variable, we use monthly returns. As such, $\beta_1, ..., \beta_S$, which we refer to as *Ex-Ante Effects* capture the average monthly return on soon-to-be-marginable stocks in the period leading up to marginability.[16] Similarly, β_0 captures the average effect once margin debt is available, which we refer to as the *Ex-Post Effect*. As above, we consider both raw and DGTW returns.

The results, shown in Table 6 are largely consistent with the evidence shown in our event studies and in Figure 7. As a baseline, the first and fourth columns report the ex-post effects where we deliberately assume there are no anticipation effects. In both columns we see evidence that returns were negative on average in the months following the introduction of margin debt, with estimated effect sizes of 1-2 percent per month. These results again highlight the existence of front-running driven overshooting surrounding the introduction of margin debt. The remaining columns account for anticipation: the second and fifth columns allow for three months of anticipation while the third and sixth columns allow for three quarters of anticipation. In these specifications our estimated ex-post effects remain similar, ranging from 0.7 percent to 2 percent per month. Taking the most conservative estimate, this suggests a reversion of 0.7 percent$\times$ 12 or approximately 8.5 percent over the course of a year. In other words, there was substantial volatility driven by overshooting and, when margin debt was introduced, previously financial constrained agents paid *at least* 8.5 percent more than they would have absent front-running by unconstrained speculators.

Furthermore, our estimated ex-ante effects indicate the presence of anticipation. Our estimates

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

[16] More formally, this is the average after differencing out their own average return in the period well before deregulation, and the average return of non-marginable stocks in the same period

suggest that soon-to-be marginable stocks saw consistently positive differential returns in the 9 months leading up to marginability. We estimate differential monthly returns as large as 1.7 percent (using raw returns) or 2.5 percent (using our DGTW measure). One notable exception is that we see no effect in the month just prior to the introduction of margin debt, suggesting that the direct effect had already been priced in by this point. In general, the patterns here suggest that prices were driven up in advance of the introduction of margin debt by unconstrained investors.

4.3 Placebos

One potential concern is that the front-running and overshooting effects we estimate might be in part mechanical, driven by the ranking procedure used to select marginable stocks. In this subsection, we use the same ranking procedure to construct and implement a series of placebo regressions and confirm that this is not the case.

Our basic approach is to randomly select placebo event dates and use the ranking formula outlined in Equation 1 to define a set of treated stocks at those dates. We then repeat the regressions shown in Table 6 and compare our placebo coefficients to those generated using the actual treatment group. Because our sample period is contaminated by the deregulation itself, we implement this approach using an alternative window that matches the broad stock market dynamics of our primary sample. In particular, we consider the previous Chinese stock market bubble. It occurred following share reforms in China and during the lead-up to the Beijing Olympics. There was during this earlier episode a widespread belief in government support of the market. This belief is not dissimilar to the belief during our margin liberalization episode. We include data from July 2001 to September 2007, the same number of months as included in our primary sample period.

For each of our placebo regressions, we randomly select three event dates. At each of these dates, we calculate the inclusion index for each stock according to Equation 1.[17] For the earliest date, we define the top 100 stocks in each exchange as Placebo Vintage 1. At the next date, we exclude stocks in Placebo Vintage 1, and define the top 100 remaining stocks in each exchange as Placebo Vintage 2. At the final date, we exclude stocks in either of the first two placebo vintages, and define the top 100 remaining stocks in each exchange as Placebo Vintage 3. We do not apply

[17] As in Section 3 we use data for the three months prior to the event date. We also exclude all stocks in the indices used to form Pilots A and B.

the screening procedure as the data for some of the criteria are not available in this earlier sample.

With these vintages and our randomly selected event dates in hand, we re-run the regression in Table 6 and store the estimated coefficients. We then repeat this process 10,000 times, each time randomly drawing the three event dates (with replacement). To test whether our results in Table 6 are mechanical, we compare the true coefficients to the distribution of placebo coefficients. As a summary, we show one sided placebo p-values in square brackets in Table 6. These p-values show the fraction of our placebo coefficients that are smaller than our ex-post effects or larger than our ex-ante effects.

Our results suggest that neither our ex-ante nor or ex-post effects are mechanically driven. The placebo p-values for our ex-post effects range from 0.000 to 0.020, suggesting that the treatment effect was more negative than at least 98 percent of the placebo estimates. For our quarterly lag specifications, the p-values hover near 0.05 for unadjusted returns, and near 0.001 for DGTW returns. The latter means that our estimated quarterly ex-ante effects using DGTW returns are larger than 99.9 percent of our placebo coefficients.

4.4 Unconstrained-investor holdings and trades

To confirm that the results in Table 6 are indeed driven by front-running, we next ask whether unconstrained investors indeed purchased soon-to-be marginable stocks in advance. We focus on the behavior of two groups of investors that we expect to be relatively unconstrained even prior to the introduction of margin lending: (i) mutual funds, and (ii) the largest holders of each stock—defined as the top ten investors by quantity of shares at the stock-quarter level.

There is strong evidence that these unconstrained investors increased their holdings in anticipation of the roll-out of margin lending. In Table 7 we display regression results following Equation 7, but replace the dependent variable with the share of ownership by unconstrained investors (defined as either mutual funds or the top 10 investors). The regressions are estimated at the quarterly level, corresponding to the frequency of our data on these investors. In columns 1 and 3, by showing traditional difference-in-difference specifications with no ex-ante effects. We then allow for three quarters of ex-ante effects in columns 2 and 4. We find positive ex-ante effects in each of these three quarters, suggesting that mutual funds differentially increased their holdings in soon-to-be marginable stocks by 0.5-0.7 percentage points per quarter in the months leading up marginability. Similarly, the top 10 ownership share differentially increased by 3.7-4.3 percentage points per quarter. Furthermore, we find negative effects in the quarters after the roll-out took effect for both outcomes—on the order of 0.4 percentage points per quarter—suggesting that these unconstrained investors sold-out once margin debt was available.

In the final two columns of Table 7 we repeat the exercise but include monthly stock level turnover as our dependent variable. We find elevated levels of trading in the period preceding marginability for soon-to-be marginable stocks, consistent with elevated trading levels due to front running. In contrast to our results on unconstrained investors, we find positive and significant ex-post effects, suggesting that there was also differentially high turnover once margin debt became available. This too is consistent with formerly constrained investors—with access to previously unavailable margin debt—buying from unconstrained investors.

One interesting observation from these findings is that front-running by institutions begin well in advance of the official marginability roll-out. Gradual front-running is consistent with earlier findings in the literature on front-running by institutions during the Dot-com Bubble (Brunnermeier & Nagel, 2004; Griffin *et al.* , 2011). This suggests that it may have been difficult to quickly establish large positions in the Chinese stock market, which squares with our earlier findings on the inelasticity of investors in the period. This also may explain why we do not see unconstrained institutions trying to establish positions after the margin roll-out even as it took a couple of months for margin debt to accumulate in stocks just above the threshold.

4.5 Larger front-running effects for higher ranked stocks

As our final test of the role of front-running by unconstrained investors, we further exploit heterogeneity in the particular stocks that became eligible for margin debt. While our RD approach exploits a local region around the threshold (in which marginability was plausibly unpredictable) and our difference-in-difference approaches exploit the full set of stocks (in which marginability was largely predictable), there is no clear delineation after which a stock becomes perfectly predictable. Instead, we should observe a relatively continuous scale. Among the set of stocks that ultimately become marginable, high ranking stocks should be *relatively* more predictable, and low ranking stocks should be *relatively* less predictable. If the patterns we observe are indeed driven by anticipatory speculation, we should therefore expect greater anticipation ex-ante (and hence greater overshooting driven reversion ex-post) among higher ranking stocks.

To capture this, we conduct a triple-difference version of our approach, further interacting Margin Trading Active$_{i,t}$ and all $D_{i,t+j}$ with an indicator equal to one if stock i is highly ranked within its vintage. We define highly ranked stocks to be those with above median rank among the set that ultimately qualified within each vintage and exchange. Our results, displayed in Table 8, provide further confirmation that differential ex-ante returns for soon-to-be marginable stocks were driven by front-running. The results in the second and fourth columns of Table 8 show that high ranking stocks saw differentially larger ex-ante effects in the months prior to marginability. Note that these effects are largest, between 1.0-1.4 percent per month, in the period 3-9 months prior to marginability versus the quarter just before. This is what would be expected if, for example, the relative uncertainty surrounding the marginability of high versus low ranking stocks began to be resolved as the date approached. Additionally, we see stronger reversion for high ranking stocks, on the order of 0.5 percent per month. This suggests that increased front-running on highly ranked stocks generated greater overshooting.

5 Further Analyses and Implications

5.1 Fragility of financially-constrained households

From a regulatory perspective, overshooting generated by front-running—and the associated increase in volatility—is a primary concern. While generally problematic, speculation driven volatility is likely to be particularly dangerous when it coincides with the expansion of credit to retail investors. Put simply, it is not ideal for asset prices to become excessively volatile just as cash constrained investors are sharply increasing leverage. Recall, for example, the overshooting scenario presented in Figure 6. Note that prices have already risen by the time new credit becomes available. Consequently, to participate in the market, constrained households must buy on margin at higher prices, and face any potential corrections that follow. If the households interested in such stocks do not correspondingly adjust their leverage, they will face an increase in financial fragility.

To see if this mechanism is empirically relevant, we briefly extend our previous analysis in Table 8 and further exploit the distinction between high- and low-ranking stocks within each vintage. Because we see greater overshooting in high-ranked stocks, investors holding high ranking stocks are likely to face greater volatility. Given this, we compare leverage for retail investors holding high-ranked versus low-ranked stocks.[18] The mechanism we describe above is likely to be relevant to policymakers *unless* we see significantly lower levels of leverage amongst constrained households holding highly-ranked stocks.

To conduct our 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, and the sample we have consists of 709,813 accounts, 18,593 of which are margin accounts. The sample period, January 2011 to December 2015, overlaps with the whole episode of deregulation in margin trading. 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 went through the brokerage margin system.[19] Although the data do not provide snapshots of accounts' stock holdings or cash balance, we can nonetheless calculate accounts' leverage level with a few reasonable assumptions, which we describe in Appendix A.

Our 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. The portfolios are generally under-diversified; the median investor holds only 4.6 stocks. There is a sizable variation in the cross sectional leverage ratio: the 95th percentile of leverage is 1.45 and the mean is 1.09. Overall, our estimates are similar 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 should be at most 3.

To test whether financial constrained households holding higher ranked stocks also have less leverage, we consider the time series of leverage for households holding high-ranked stocks and households holding low ranked stocks. To this end, we plot the 95th percentile of each of these groups in Figure 8.[20] We can see directly from the figures that the leverage of these financially-

[18]Because the households in our sample typically own only a few stocks in their portfolios, this exercise gives us a view into the impact of front-running driven overshooting on leverage itself.

[19]Unfortunately, the dataset does not have any demographic information on the accounts.

[20]We choose the 95th percentile to focus on financially-constrained households. (Many less constrained households may have a margin account that they do not use or need to buy equities). Those at the extreme are our interest as those

constrained households in high-ranked stocks is very similar to those owning low-ranked stocks. As such, we conclude that front-running induced price overshooting likely made financially-constrained households more fragile on an ex-ante basis. This would not be the case if the leverage ratios for households in high-ranked stocks were lower—which might occur if banks were unwilling to lend to households holding these stocks or if the households themselves understood the dynamics at play and de-levered.[21]

5.2 Anticipating Shadow Margin

While our focus is on formal margin lending, anticipation of non-formal "shadow" lending—which began only around the time of our Vintage 3—may also 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 implicates this lending in amplifying the market crash. 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, outperformed the market during this period. We conclude that unconstrained investors appear to have anticipated the rise in shadow margin lending analogously to their anticipation of formal margin lending. Details on these exercises are included in Appendix B.

5.3 Connections to Housing Markets

Given the importance of housing in the Great Recession of 2008, much of the recent work on credit cycles has focused on real estate markets. While our primary analysis focuses on the Chinese stock market, it is valuable to consider the relevance of anticipation in housing. In general, we expect anticipation in housing markets to be as salient as in stock markets. Mortgage credit expansions, whether driven by regulation or technology, are unlikely to come as a complete surprise.

highly levered investors are the ones who are particularly fragile during the crash period.

[21] We can use the same household level portfolio data to examine whether there are indirect effects associated with the onset of marginability — whereby households use margin on one set of stocks to free up equity to buy another set of stocks that are non-marginable. Such indirect effects would only downward bias our already significant findings. To bias the regression-discontinuity results, households would have to rebalance among stocks just around the discontinuity. We leave indirect effects for future research.

The simplest implication of our findings is that the pattern shown in Figure 6 is likely to be relevant in housing markets. For example, we should expect investors, home-builders, and other less credit-dependent agents to optimally time their purchases and investments in anticipation of buying by more financially constrained households. Indeed, investment home buyers are thought to have played a major role in the housing bubble (see, e.g., Glaeser *et al.* , 2008; Haughwout *et al.* , 2011; Choi *et al.* , 2016; DeFusco *et al.* , 2017). Furthermore, there is some evidence of speculative patterns in the data, for example, DeFusco *et al.* (2017) find that investment home volumes led the peak of the housing bubble.

More broadly, 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, 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.* , 2016; Albanesi *et al.* , 2017), has challenged this wisdom. 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, demand, and investment for all borrowers.

5.4 Parallel-trends criterion

Finally, our approach highlights the difficulties of studying credit supply shocks in standard event-study or difference-in-difference frameworks (see e.g. Favara & Imbs, 2015). Such approaches typically require some sort of parallel trends criteria. However, if agents are able to foresee credit supply shocks—as is often the case—they have explicit incentives to begin buying and driving up asset prices in advances. In other words, economic logic suggests that parallel trends will not hold except in special cases where (1) no agents are able to time their purchases; (2) agents are explicitly myopic; (3) there are substantial frictions to arbitrage, or (4) deregulatory events (credit supply shifts) are entirely unpredictable. Furthermore, by requiring parallel trends, and hence focusing on these special cases, we are likely to miss and hence downplay the role of front-running and speculation, which may be a first order concern for policy. Ultimately, ideal approaches should allow us to both estimate the direct effects of credit supply shocks on asset prices and understand the dynamics surrounding it.

6 Conclusion

In this paper, we make two contributions to the literature on credit booms gone wrong, focusing on the staggered deregulation of margin lending in China that took place between 2011 and 2015. First, we identify a large direct effect of margin lending to financially constrained households on stock prices using a regression discontinuity design. We find large causal estimates which suggest inelastic demand on the part of financially constrained households.

Second, we show that the direct effect of credit supply is only half the story. We find evidence that unconstrained investors anticipated and speculated on the roll out of margin debt, and that this speculation led to overshooting. This anticipation has implications for policymakers, and should be expected to occur in any market where agents are non-myopic, including housing. Empirical studies of credit booms can and should consider both the direct effect of credit supply, and the speculation driven dynamics that surround credit-expansions.

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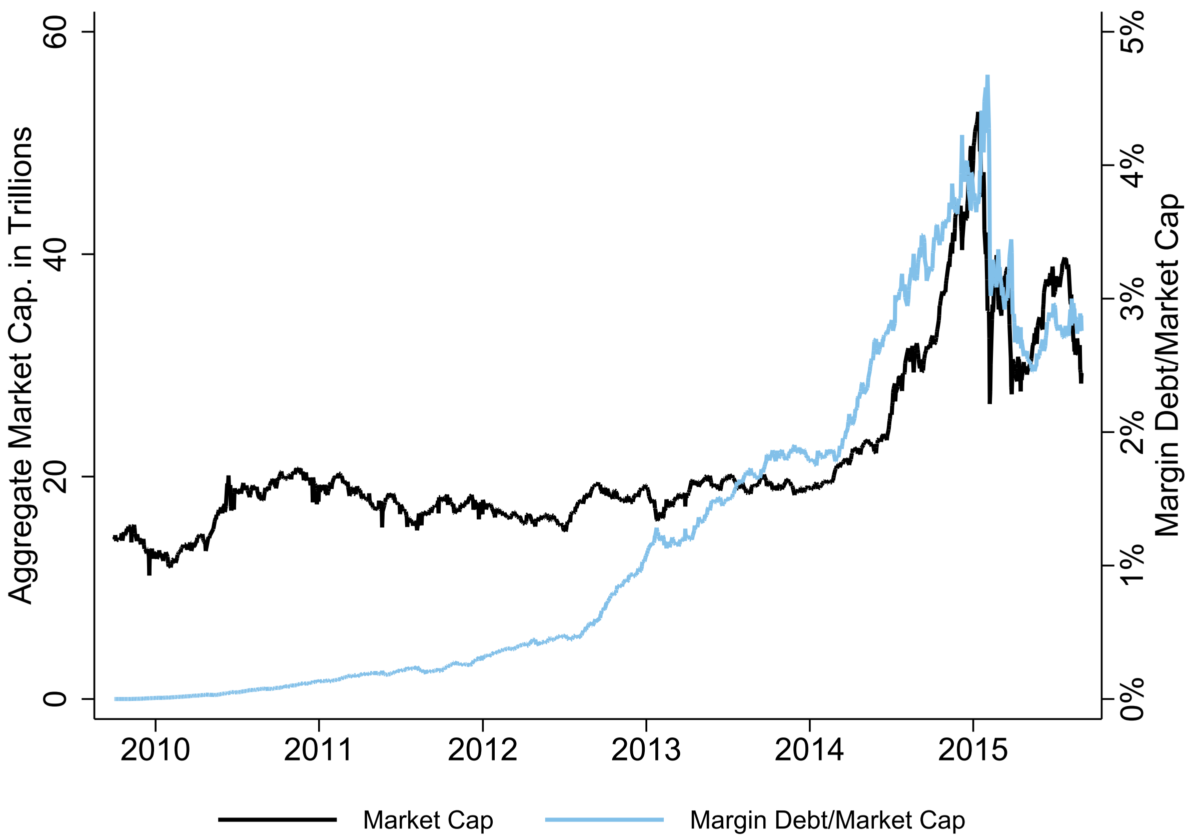
Figure 1: Aggregate Market Cap. and Margin Debt/Market Cap. Over Time



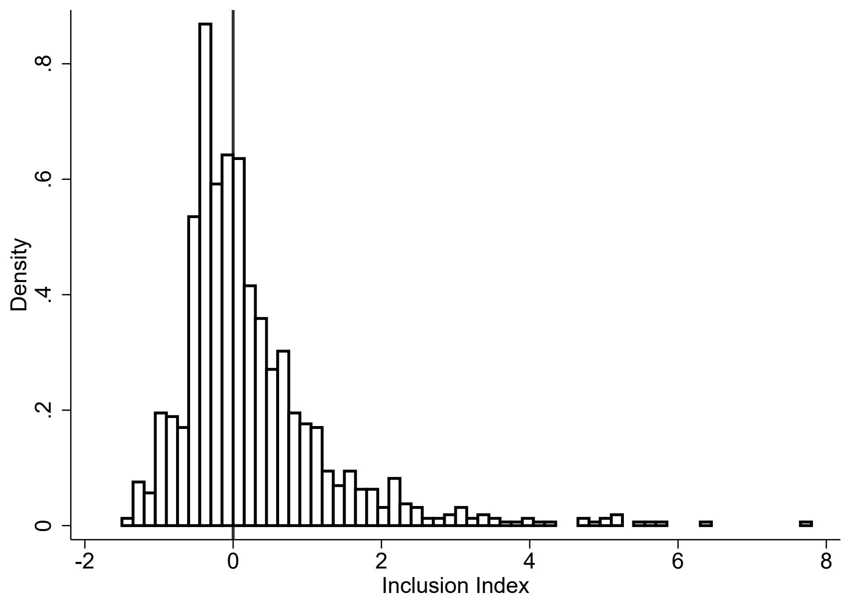
Notes: Plot shows daily aggregate market cap (in black) and the ratio of margin debt to market cap (in blue) for all stocks in sample. Both market cap and margin debt are measured in trillions of yuan.

FIGURE 2: MARGIN DEBT/MARKET CAP. BY VINTAGE

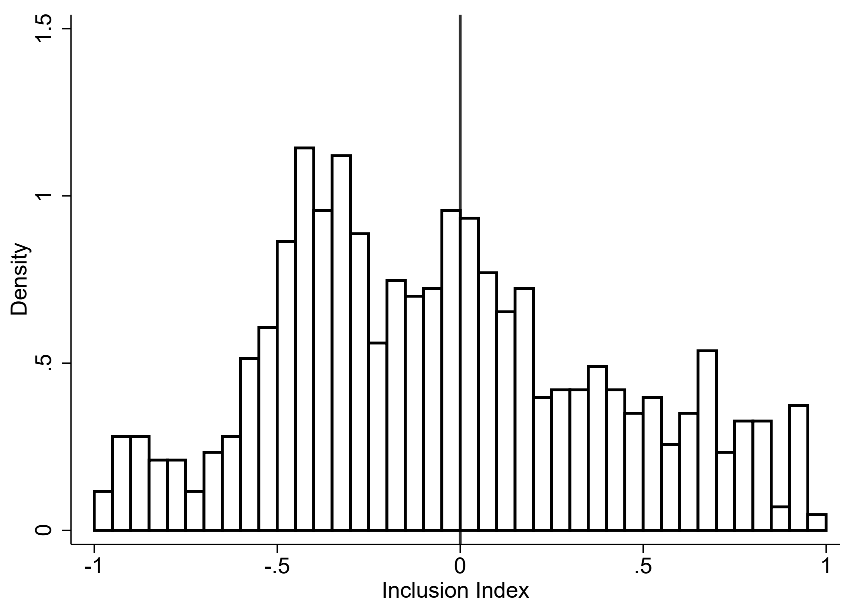
Notes: Plot shows the daily ratio of total margin debt to total market cap for each of the three vintages we study. Vertical lines denote starting dates of each vintage.

FIGURE 3: NO EVIDENCE OF BUNCHING AT THRESHOLD

(a) Full Distribution



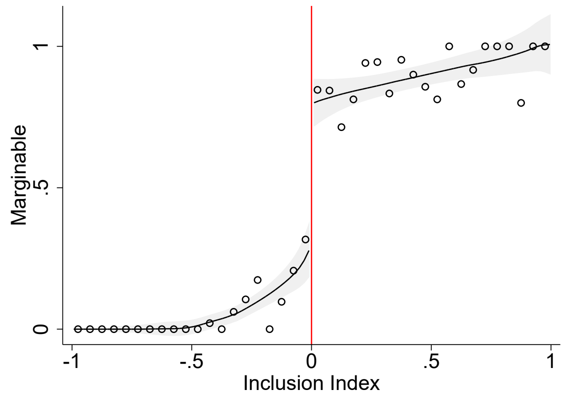
(b) Close to Marginability Threshold



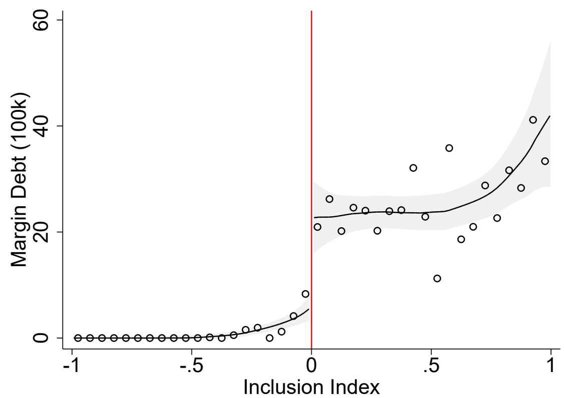
Notes: Both panels show histograms of the value of the inclusion index, normalized to the vintage specific threshold. In Panel (a), we include, for each vinatage and exchange, the closest 100 stocks below the threshold and the 100 closest stocks above the threshold. In Panel (b), we further restrict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

FIGURE 4: INCLUSION INDEX DETERMINES MARGINABILITY

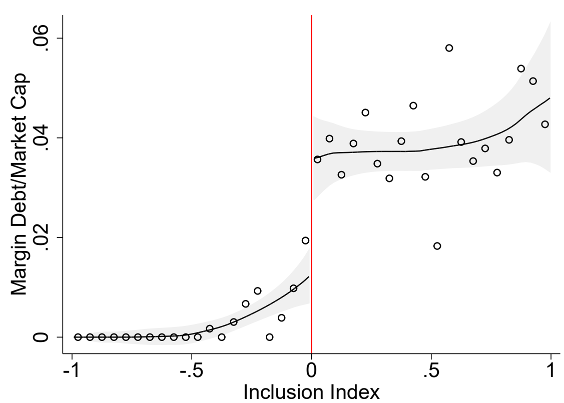
(a) Marginability



(b) Margin Debt



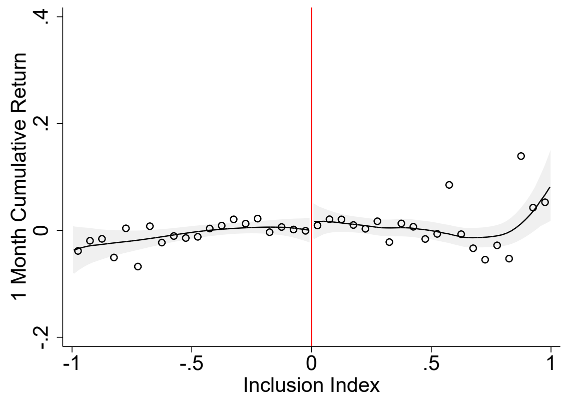
(c) Margin Debt/Market Cap



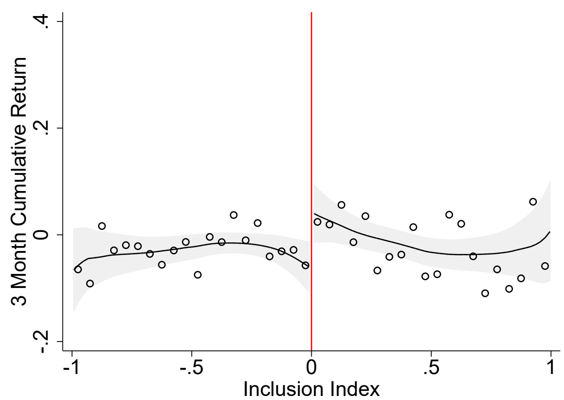
Notes: Indicator for marginability (Panel (a)), stock level margin debt (Panel (b)), and stock level ratio of margin debt to market cap (Panel (c)) plotted against inclusion index. Inclusion index normalized to set vintage specific threshold equal to 0. For each vintage, all not-yet marginable stocks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

Figure 5: Positive Returns to Crossing Marginability Threshold

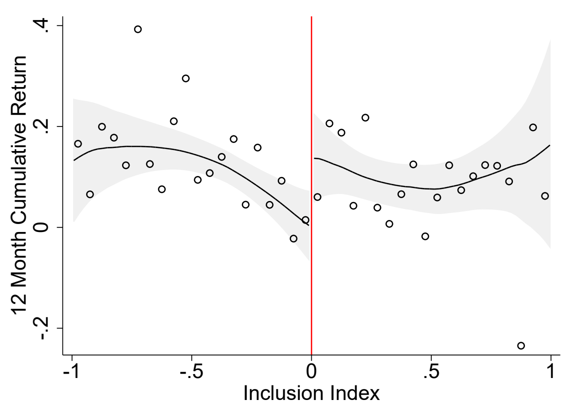
(a) 1 Month



(b) 3 Months

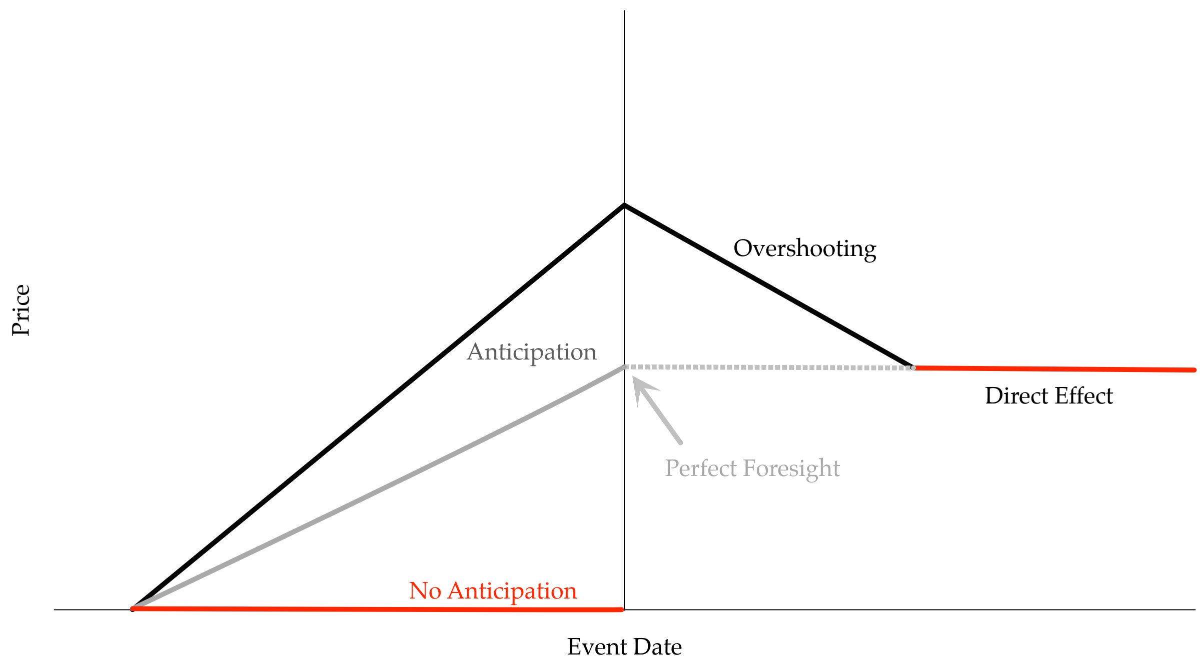


(c) 12 Months



Notes: Cumulative returns from marginability to 1, 3, or 12 months post-marginability. Inclusion index normalized to set vintage specific threshold equal to 0. Returns are adjusted for splits and dividends. For each vintage, all not-yet marginable stocks with inclusion index within 1 of the threshold at the time marginability was determined are included. Points show averages within bins of width 0.05 in the index. Lines shows local linear fits with 95% confidence intervals on either side of the threshold.

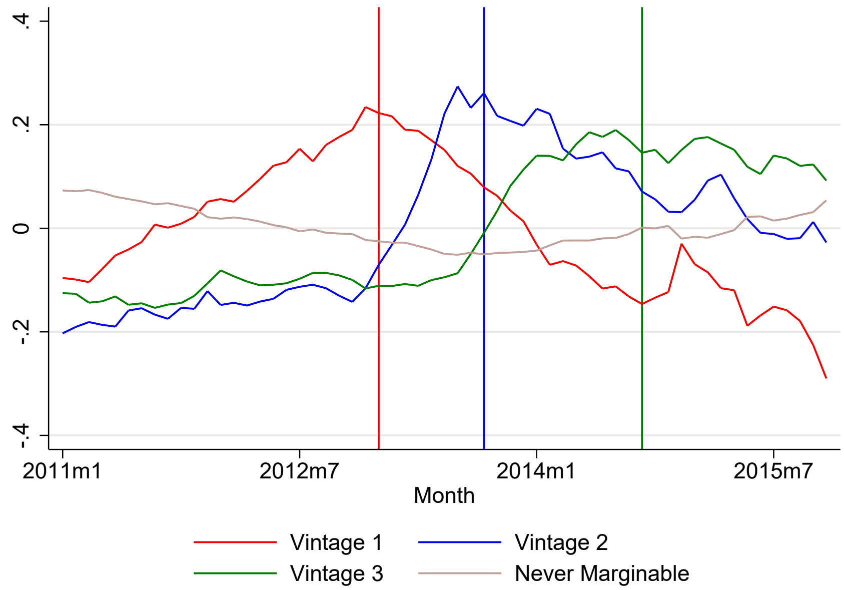
FIGURE 6: ANTICIPATION EFFECTS OF AN INCREASE IN CREDIT SUPPLY



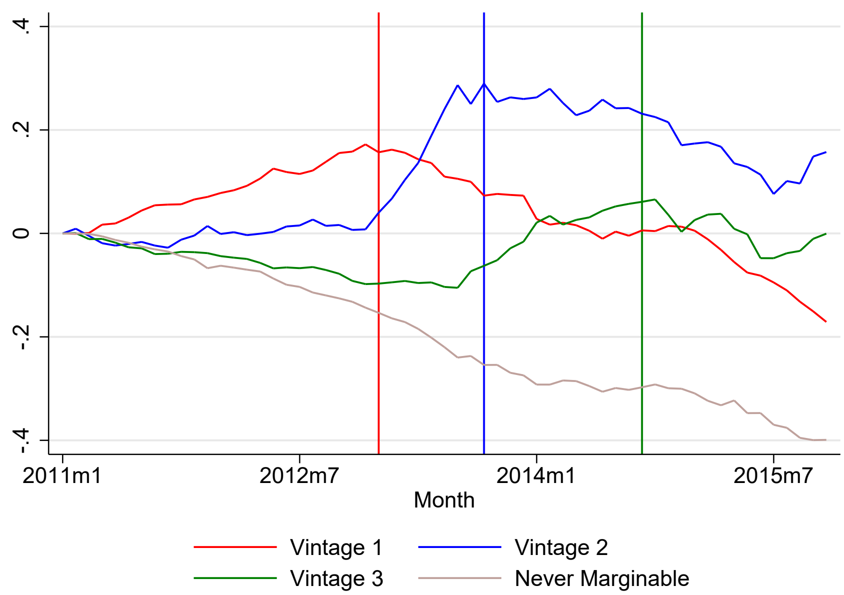
Notes: In this figure the y-axis displays price, the x-axis displays time, and the vertical line indicates the event date of a credit supply shock. The red lines represent the case of no anticipation effects. The red line on the x-axis is price before the event date. The discrete jump in the red line represents the long-run direct effect of credit supply on price. Gray and black lines portrays two additional cases of anticipation with limited frictions to arbitrage. The gray line represents the case of perfect foresight, in which agents are able to exactly predict the long-run direct effect. The black line represents a case of overshooting, in which agents overshoot the long-run direct effect.

FIGURE 7: MARKET ANTICIPATION OF MARGIN LENDING ROLLOUT

(a) Residualized Log(Market Cap)

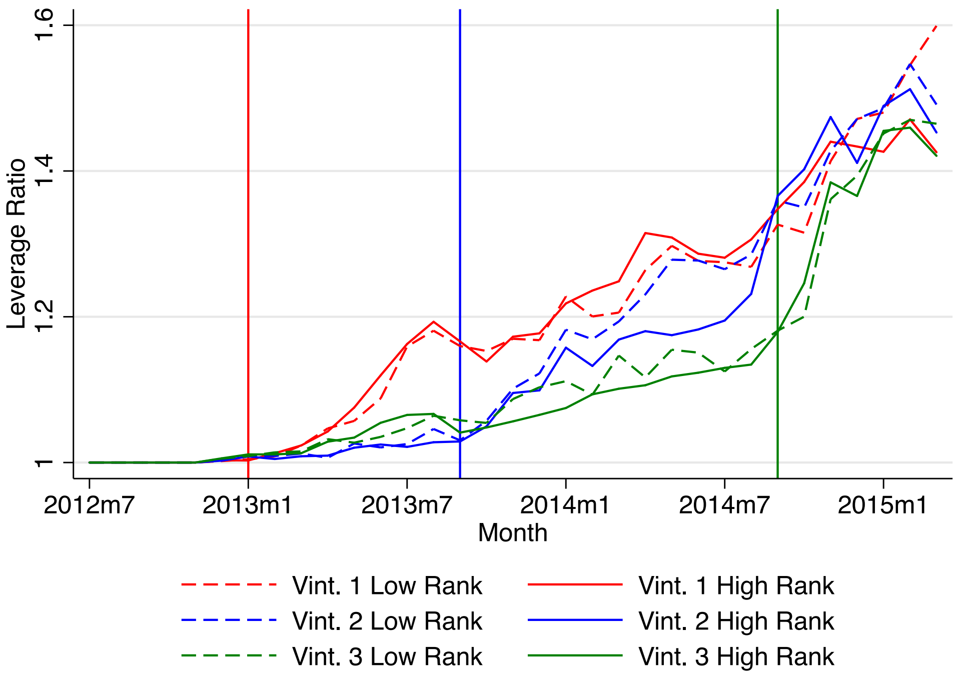


(b) Cumulative DGTW Returns



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

FIGURE 8: LEVERAGE BY LIKELIHOOD OF INCLUSION



Notes: Plots show the 95th percentile of leverage across households that hold each stock, averaged, within each vintage, over stocks with above vs. below median rank on the index that determines inclusion.

TABLE 1: NUMBER OF MARGINABLE STOCKS BY VINTAGE

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

TABLE 2: CROSSING MARGINABILITY THRESHOLD PREDICTS MARGIN DEBT

	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.509^{***}	13.129^{***}	0.017^{**}	0.496^{***}	11.242^{***}	0.016^{**}
	(0.077)	(3.462)	(0.007)	(0.080)	(3.808)	(0.007)
P-Value	0.000	0.000	0.011	0.000	0.003	0.024
CCT Robust P-Value	0.000	0.002	0.053	0.000	0.022	0.093
Bandwidth	0.289	0.263	0.274	0.326	0.294	0.315
N	350	323	329	400	351	383

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

TABLE 3: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: REDUCED FORM

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

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with index value within the specified bandwidth of the threshold at the time marginability was determined. We consider cumulative returns 1,3, and 12 months after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use the covariate adjusted MSE optimal bandwidths described in Calonico, Cattaneo, Farrell, and Titiunik (2017). Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 4: POSITIVE RETURNS TO CROSSING MARGINABILITY THRESHOLD: FUZZY RD

	Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.034	0.179**	0.280***	0.039	0.168**	0.246**
	(0.030)	(0.071)	(0.109)	(0.027)	(0.073)	(0.105)
P-Value	0.261	0.011	0.010	0.145	0.021	0.019
CCT Robust P-Value	0.197	0.010	0.007	0.141	0.022	0.017
Bandwidth	0.294	0.324	0.361	0.435	0.408	0.486
N	350	394	403	532	495	546

	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.043	0.132**	0.209**	0.038	0.119*	0.233**
	(0.027)	(0.063)	(0.103)	(0.025)	(0.065)	(0.109)
P-Value	0.118	0.037	0.043	0.134	0.066	0.033
CCT Robust P-Value	0.088	0.029	0.021	0.132	0.054	0.022
Bandwidth	0.328	0.326	0.455	0.482	0.422	0.519
N	401	394	513	593	507	572

Fuzzy regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with index value within the specified bandwidth of the threshold at the time marginability was determined. We consider cumulative returns 1,3, and 12 months after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use the covariate adjusted MSE optimal bandwidths described in Calonico, Cattaneo, Farrell, and Titiunik (2017). Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 5: EVENT STUDY OF MARGINABILITY

	Negative Relative Returns Immediately Following Marginability					
	Cumulative Returns			Cumulative DGTW Returns		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Marginable	-0.012^{***}	-0.009	-0.154^{***}	-0.005	-0.014^{*}	-0.030^{**}
	(0.004)	(0.009)	(0.015)	(0.004)	(0.008)	(0.015)
Mean of Dep. Var.	0.00812	-0.0183	0.198	-0.00352	-0.0187	-0.0731
N	4513	4388	4151	4513	4388	4151
	Positive Returns In Year Preceding Marginability					
	Cumulative Returns			Cumulative DGTW Returns:		
	-1 to 0	-3 to 0	-12 to 0	-1 to 0	-3 to 0	-12 to 0
Marginable	-0.002	0.024^{***}	0.162^{***}	0.014^{***}	0.045^{***}	0.230^{***}
	(0.005)	(0.008)	(0.015)	(0.005)	(0.008)	(0.014)
Mean of Dep. Var.	0.0779	0.173	0.194	-0.00851	-0.0184	-0.0576
N	4422	4338	4255	4422	4338	4255

Top panel shows results from regressions of cumulative returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the introduction of margin debt on an indicator for newly marginable. Bottom panel shows results from regressions of cumulative returns at the stock level from 1, 3, and 12 months preceding the introduction to the month of the introduction itself. For each of the three vintages determined by the screening and ranking rule, we compute cumulative returns for the newly marginable stocks in that vintage as well as the set of currently non-marginable stocks. The left three columns show cumulative log returns adjusted for splits and dividends but otherwise unadjusted. The right three columns show DGTW adjusted returns. All specifications include dummy variables for vintage as controls. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 6: MARKET ANTICIPATION OF MARGINABILITY

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

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

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

Margin Trading Active is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in months after margin trading is active in those stocks. $D_{i,t+j}$ is equal to one if margin trading initially becomes active for stock i in period $t+j$, and zero otherwise. The number of *ex-ante effect* coefficients indicates the value of S for the regression in question. The first and fourth columns includes no ex-ante effects, and is equivalent to a collapsed difference-in-difference approach. Other specifications include indicators aimed at capturing ex-ante effects for the three months or three quarters leading up to the roll-out for each stock. Sample covers March 2009-May 2015. The left three columns show cumulative log returns adjusted for splits and dividends but otherwise unadjusted. The right three columns show DGTW adjusted returns. Standard errors, clustered at the stock and month level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. One sided p-values from placebo exercise shown in square brackets based on 10000 recreations of each regression using the period of July, 2001 to September 2007. P-values represent the fraction of placebo regressions with larger (for ex-ante effects) or smaller (for ex-post effects) values of the relevant coefficient.

TABLE 7: INSTITUTIONAL OWNERSHIP SURGES BEFORE MARGINABILITY

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

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

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

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

TABLE 8: MORE ANTICIPATION AND OVERSHOOTING FOR HIGH RANKED STOCKS

	Unadjusted Returns		DGTW Returns:	
		Quarterly Lags		Quarterly Lags
Ex-Post Effect	-0.018^{***} (0.001)	-0.015^{***} (0.001)	-0.008^{***} (0.001)	-0.005^{***} (0.001)
Ex-Post Effect × High Rank	-0.007^{***} (0.002)	-0.005^{***} (0.002)	-0.005^{**} (0.002)	-0.003 (0.002)
Ex-Ante Effect (t-1)		0.013^{***} (0.003)		0.016^{***} (0.003)
Ex-Ante Effect (t-2)		0.005 (0.003)		0.005^{*} (0.003)
Ex-Ante Effect (t-3)		0.008^{**} (0.004)		0.011^{***} (0.004)
Ex-Ante Effect (t-1) × High Rank		-0.002 (0.005)		0.000 (0.005)
Ex-Ante Effect (t-2) × High Rank		0.013^{**} (0.005)		0.014^{***} (0.005)
Ex-Ante Effect (t-3) × High Rank		0.010^{*} (0.005)		0.010^{*} (0.005)
Mean of Dep. Var.	0.0144	0.0144	-0.00614	-0.00614
N	126131	126131	126131	126131

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

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

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

Internet Appendix: For Online Publication

A Calculating Leverage from Household Portfolio Data

To do so, we take 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 do not observe their initial stock positions. As a result, some positions appear to be negative based on our calculation. To deal with this, we set negative positions, whenever they appear, to be zero. Given that shorting is limited in China during our sample period, those negative positions are likely due to unobservable long positions that predate our sample. As a result, this correction will only bias down our our estimate of each account's total portfolio value. 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 stock holdings, which we refer to 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 of margin loans, we assume a pecking order of cash over loans. In other words, 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 loans and cash at the same time. This may bias down our estimate of the true value of margin loans. Our calculation is as follows: when an 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, whether 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 quantity of margin loans based on our data and this 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. We calculate account leverage level (Lev) as:

$$Lev = \frac{Asset}{Asset - Loan}. \tag{8}$$

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.

B Anticipating shadow margin: The peak of the bubble in 2015

While our analysis is constrained 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 A.I suggest that mutual funds began to increase their relative positions in non-marginable stocks after the introduction of Vintage 3, the final official set of marginable stocks. Similar patterns can be seen for the top 10 investors, to a lesser extent. 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.

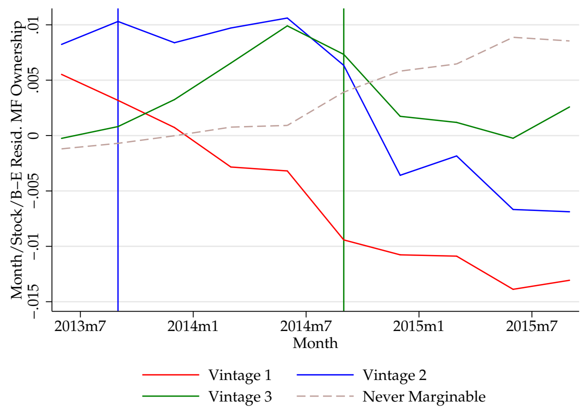
To show that this is indeed the case, we gather data on shadow margin lending from a peer-to-peer platform that encompassed around 10% of the market during this period. We measure the presence of shadow margin at the stock level using data from a large technology provider. This technology company routed 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 sub-divided 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 A.I 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.

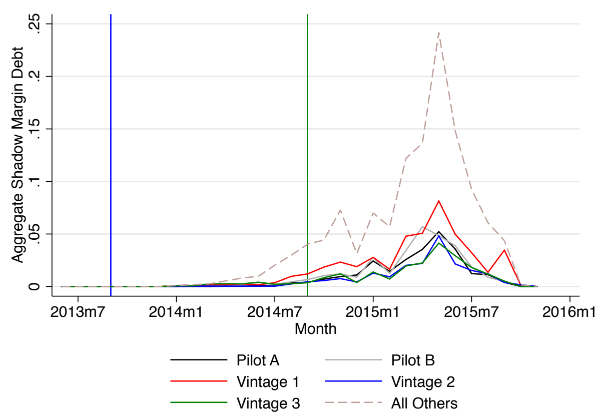
C Appendix Figures

FIGURE A.I: 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.

D Appendix Tables

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

	IK Bandwidth					
	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.586^{***}	11.212^{***}	0.017^{***}	0.549^{***}	9.544^{***}	0.015^{***}
	(0.054)	(1.498)	(0.003)	(0.061)	(1.666)	(0.003)
P-Value	0.000	0.000	0.000	0.000	0.000	0.000
CCT Robust P-Value	0.000	0.001	0.002	0.000	0.002	0.007
Bandwidth	0.564	0.559	0.718	0.564	0.559	0.718
N	664	662	749	664	662	749
	Bandwidth=0.5					
	Linear Splines			Local Linear (Triangular Kernel)		
	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$	Marginable	Margin	$\frac{\text{Margin}}{\text{Market Cap}}$
Above Marginable Threshold	0.567^{***}	10.025^{***}	0.015^{***}	0.538^{***}	9.143^{***}	0.014^{***}
	(0.057)	(1.594)	(0.003)	(0.064)	(1.740)	(0.004)
P-Value	0.000	0.000	0.000	0.000	0.000	0.000
CCT Robust P-Value	0.000	0.001	0.012	0.000	0.003	0.036
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500
N	610	610	607	610	610	607

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

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

	Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.058^{**}	0.105^{**}	0.019	0.075^{**}	0.121^{***}
	(0.013)	(0.029)	(0.042)	(0.013)	(0.030)	(0.045)
P-Value	0.237	0.044	0.014	0.157	0.014	0.006
CCT Robust P-Value	0.148	0.011	0.015	0.245	0.037	0.063
Bandwidth	0.530	0.597	0.590	0.530	0.597	0.590
N	627	665	623	627	665	623
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.013	0.050^{*}	0.086^{*}	0.018	0.060^{*}	0.111^{**}
	(0.012)	(0.028)	(0.045)	(0.013)	(0.031)	(0.046)
P-Value	0.302	0.074	0.057	0.142	0.055	0.016
CCT Robust P-Value	0.094	0.084	0.015	0.171	0.341	0.047
Bandwidth	0.589	0.446	0.689	0.589	0.446	0.689
N	679	538	671	679	538	671

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with index value within the specified bandwidth of the threshold at the time marginability was determined. We consider cumulative returns 1,3, and 12 months after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use the Imbens and Kalyanaraman bandwidth. Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.III: Positive Returns to Crossing Marginability Threshold: Bandwidth=0.5

	Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.068^{**}	0.117^{***}	0.019	0.082^{**}	0.125^{***}
	(0.013)	(0.030)	(0.045)	(0.013)	(0.032)	(0.047)
P-Value	0.241	0.025	0.009	0.152	0.011	0.008
CCT Robust P-Value	0.157	0.016	0.036	0.273	0.088	0.128
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500
N	607	595	557	607	595	557
	DGTW Adjusted Cumulative Returns					
	Linear Splines			Local Linear (Triangular Kernel)		
	1 Month	3 Months	12 Months	1 Month	3 Months	12 Months
Above Marginable Threshold	0.016	0.047^{*}	0.111^{**}	0.020	0.057^{*}	0.119^{**}
	(0.013)	(0.027)	(0.049)	(0.013)	(0.029)	(0.051)
P-Value	0.226	0.083	0.023	0.129	0.053	0.019
CCT Robust P-Value	0.126	0.070	0.056	0.228	0.229	0.167
Bandwidth	0.500	0.500	0.500	0.500	0.500	0.500
N	607	595	557	607	595	557

Regression discontinuity estimates based upon crossing the vintage-specific threshold of the index used to determine marginability. For each vintage we include all not previously marginable stocks in our primary sample with index value within the specified bandwidth of the threshold at the time marginability was determined. We consider cumulative returns 1,3, and 12 months after marginability. The first three columns allow for separate linear slopes in the running variable on either side of the threshold (local linear regressions with a rectangular kernel), while the final three columns include local linear regressions with a triangular kernel on either side of the threshold. Indicators for vintage are included as covariates. All specifications use a bandwidth of 0.5. Standard errors based upon three nearest neighbor variance estimators described in Calonico, Cattaneo, and Titiunik (2014) are included in parentheses. CCT robust P-Value is based upon robust bias correction described in Calonico, Cattaneo, and Titiunik (2014). N refers to the effective number of observations within the relevant bandwidth of the threshold. Cumulative returns refer to raw cumulative returns (adjusted for splits and dividends) vs. DGTW adjusted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.