Anticipating the Direct Effects of Credit Supply*

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Abstract

Empirical studies of the direct effects of credit supply on asset prices often use difference-in-difference or event-study designs based on deregulatory events. The assumptions underlying such strategies are violated when there is anticipation by unconstrained buyers. We develop an information-revelation based model that allows us to estimate the direct effects of credit supply in the presence of anticipatory pre-trends using a simple linear dynamic panel approach. We apply the model to China's 2010-2015 stock margin lending reform, which precipitated a credit cycle and a stock market boom and bust. We estimate that margin lending increased the prices of treated stocks by 20%. More than 60% of this effect was already impounded in prices six months before the actual deregulation.

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1 Introduction

An important macro-finance literature relates credit cycles to 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). 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.[1] At the core of many such narratives is the notion that new buying by previously constrained households is responsible for asset price inflation and excessive price volatility.

There is a well known difficulty in identifying direct effects, defined here as the asset price impact of changes in credit supply. Credit is not exogenous and may itself depend on the prices of assets used as collateral (Rajan & Ramcharan, 2015). To address this, empirical studies often rely on difference-in-difference or event study designs based on deregulatory interventions (see, e.g., Favara & Imbs, 2015; Di Maggio & Kermani, 2017) that require parallel trends style assumptions. The direct effect in such settings is simply the ex-post comparison of price changes in treated and control groups after deregulation.

However, as a matter of theory, the assumptions underlying these event-based strategies are often restrictive due to anticipation by unconstrained buyers. Unless markets are highly illiquid or a deregulatory event is unpredictable, buying by unconstrained investors will lead prices to rise in advance of new credit supply by an amount equal, in aggregate, to the direct effect. Price increases need not be instantaneous—they will be gradual to the extent information is revealed slowly or unconstrained investors face idiosyncratic risks. As such, parallel trends or equivalent assumptions do not hold except under certain circumstances.

We propose a method for measuring direct effects in the presence of anticipatory pre-trends. We introduce a new set of tools—an information-revelation based model of anticipation estimated using a dynamic panel approach—that allow researchers to recover the direct effect by incorporating and modeling these trends. Intuitively, with anticipation, the direct effects of credit supply

[1] 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 (Scheinkman & Xiong, 2003; Geanakoplos, 2010; Simsek, 2013); and (4) intermediary frictions or balance sheets (Bernanke & Gertler, 1989; Kiyotaki & Moore, 1997; Adrian & Shin, 2010).

are well-captured by asset price pre-trends but not by difference-in-difference style pre- versus post-event comparisons. The ex-post comparison no longer measures the direct effect when it has dribbled out in prices ahead of the event.

Of course, this concern is not strictly limited to credit supply. Anticipation is a potential challenge for any event based approach to determining causal effects on asset prices. However, because shocks to credit supply often involve national governments (Herrera *et al.* , 2020) and have potentially large impacts on asset markets, these events are particularly on the radar of unconstrained buyers or speculators.

We apply our method to the deregulation of stock margin lending in China between 2010 and 2015. Beginning in 2010, the Chinese government built out the market for margin loans, for example by creating the China Securities Finance Corporation (CSFC), whose mission was to make subsidized loans to brokerage houses.[2] As a result, there was a historic rise of margin debt in the Chinese stock market, eventually reaching 3.5 percent of GDP and roughly 4.5 percent of market capitalization. At the peak nearly 2 trillion yuan (or roughly 350 billion dollars) of margin loans were supplied to Chinese households. This unprecedented expansion of credit to financially constrained households coincided with a stock market boom and bust.

Beyond the historic size of the credit supply shock, there are at least two reasons why this Chinese deregulatory event is ideal for our purposes. The first is that Chinese authorities introduced margin debt gradually in a series of several *vintages*, or sets of stocks. Like many deregulatory settings used in the literature, these events were at least partially predictable in advance. As a consequence, relatively unconstrained buyers—mutual funds and other major investors—were candidates for anticipatory buying. The step-by-step roll-out of the Chinese margin deregulation allows us to (i) show how standard event study and difference-in-difference approaches are sensitive to anticipation-driven pre-trends and (ii) implement our new framework.

Second—unique to the Chinese margin deregulatory experiment—specific stocks qualified for margin lending based on a published formula using publicly available real-time data on market capitalization and trading volume. The ranking rule used to determine eligibility generated ex-

[2]The 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 the CSFC is typically 1 to 2% higher than the interbank market rate. For more information about the CSFC see: https://www.ft.com/content/c1666694-248b-11e5-9c4e-a775d2b173ca.

ante local uncertainty about the specific set of qualifying stocks in a neighborhood around the cutoff for each vintage. This enables us to recover regression discontinuity estimates of the direct effect that are not tainted by anticipation. While this institutional detail is not typical of most credit expansions, it allows us to validate the direct effect estimates inferred from pre-trends using our new framework (which can be run by researchers in a quite broad array of settings).

Our paper has three parts. In the first, we provide evidence of substantial anticipation in the period prior to the deregulation and show that this prevents standard event based approaches from recovering direct effects. Event study and expanded difference-in-difference approaches show that, across all vintages, asset prices rose consistently in the months leading up to marginability. There was little change in prices after the deregulation date. Anticipation was more concentrated in high-ranked stocks within each vintage—stocks that investors could confidently predict would qualify for margin lending.

In other words, investors preempted the expansion of margin debt, exactly as theory would predict in a world where changes in credit are not entirely unexpected. This anticipation creates a generic pre-trend which prevents straightforward application of difference-in-difference or event study designs. Notably, the trends are gradual instead of a sharp step, as would be expected in a world where investors are not learning from a single announcement, but rather gradually resolving uncertainty about the existence, size, targeting, and extent of the deregulation.

One concern is the possibility that our estimated anticipatory 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 the presence of anticipation is not mechanical.

In the second part of the paper we introduce a framework for recovering the direct effect of credit supply (or another anticipated event) from pre-trends. Our framework is based on a simple information revelation model of anticipation in the presence of a coming credit expansion to constrained investors. In contrast to more ad-hoc strategies—such as estimating abnormal returns in some arbitrary window prior to an event—our methodology allows the researcher to remain agnostic about the precise time at which information first became available.

Specifically, we allow unconstrained investors to receive messages about the size of the expansion into the infinite past. We then show that with a reasonable parametric assumption on the nature of these messages—that the variance of each message is exponentially increasing as the event draws close—the framework can be estimated with a simple linear dynamic panel model. We address a generic endogeneity concern with a forward looking instrumental variables strategy following Malani & Reif (2015) and Arellano & Bond (1991).

Our approach allows us to recover both the direct effect of the expansion and the exponential rate at which information is revealed. We estimate that the margin lending deregulation led to a roughly 20 percent increase in prices for treated stocks. These direct effects were anticipated gradually as information became available. Even six months before the actual event, more than 60 percent of direct effect had already been impounded into stock prices.

In the third part of our paper, we validate the direct effect estimates from our model by comparing them to the results of a regression discontinuity approach that compares stocks just below vs. just above the cut-offs used to determine marginability. The key assumption is that there is no differential anticipation in a neighborhood of the cut-off, which allows us to set aside anticipation concerns. This assumption is based upon the fact that high frequency variation in the inputs to formula determining marginabililty generated local ex-ante uncertainty over the specific set of qualifying stocks for each vintage. Because slight movements in market capitalization or turnover might cause one stock to qualify or another to be disqualified, investors could not perfectly predict the set of stocks in each vintage prior to the introduction of margin debt.[3]

We find that stocks just above the cut-off saw a sharp influx of margin debt in the year following its introduction. This, in turn, led to a non-trivial direct effect—an increase in asset prices. Our estimates suggest that marginability led to one year cumulative abnormal returns of around 20 percent for impacted stocks, on par with the results from our model.

Our paper makes three contributions to the literature. The first contribution comes in developing an easy-to-estimate model of anticipation that captures direct effects. The challenges of treatment anticipation for difference-in-difference style designs have been noted at least since Ashenfelter (1978), and more recent work (e.g. Malani & Reif, 2015; Freyaldenhoven *et al.* , 2019)

[3]Or, more precisely, until the exact set of stocks in each vintages was announced by regulators. This coincided quite closely with the introduction of margin debt. In our primary specifications, we focus on the announcement date, rather than the implementation date of deregulation.

has developed procedures that are robust to or incorporate pre-trends in estimation. We build on past work by building and estimating an information revelation based asset pricing model of anticipation that integrates pre-trends.

Our approach can be applied to asset markets beyond the stock market, including housing and bond markets. In housing markets, investment home buyers play the analog of relatively unconstrained buyers in our model and have been implicated in the US housing bubble (Glaeser *et al.* , 2008; Haughwout *et al.* , 2011; DeFusco *et al.* , 2017; Nathanson & Zwick, 2018). In bond markets, trading by large investment firms are thought to play a role in the anticipation of asset purchase programs by central banks following the financial crisis, such as European Central Bank's 2015 Public Sector Purchase Programme.[4]

The second is showing the importance of anticipation in the context of credit supply expansions. In stylized models with a binding constraint for all investors, prices may indeed not change until credit becomes available. This is not the case in an economy with unconstrained agents. Our finding that the direct effect is anticipated in asset markets a number of months in advance indicates that anticipation has the potential to alter macroeconomic cycles. It complements recent research (e.g. Adelino *et al.* , 2016; Albanesi *et al.* , 2017; Kaplan *et al.* , 2020) that highlights the importance of beliefs and relatively unconstrained agents—as opposed to a narrow focus on binding constraints—in the US housing boom and bust of the 2000s. Our findings indicate that anticipation of credit supply shocks ought to be incorporated into theoretical models of credit cycles (e.g. those following Kiyotaki & Moore, 1997; Geanakoplos, 2010). and into empirical work on credit booms gone wrong (e.g. Jordà *et al.* , 2013; Krishnamurthy *et al.* , 2015; Mian *et al.* , 2017; Baron & Xiong, 2017; Muir, 2017; López-Salido *et al.* , 2017).

The third is in providing straightforward regression discontinuity based estimates of the direct effects of a credit supply shock on asset prices. This complements a series of other well identified empirical papers that have directly analyzed stock margin lending using regression discontinuity approaches (Kahraman & Tookes, 2017) or other credible designs (Foucault *et al.* , 2011). However, this research has not focused on direct impacts on the level of asset prices, instead addressing effects on other features of the trading environment including liquidity, volatility, and stock co-

[4]See `https://bankunderground.co.uk/2015/08/14/very-much-anticipated-ecb-qe-had-a-big-impact-on-asset-prices-even-before-it-was-officially-announced.`

movement. The primary reason we are able to measure level effects on asset prices is the fact that the scale of margin debt in our context dwarfs that studied in previous work.

Our paper proceeds as follows. In Section 2 we provide background on the Chinese deregulation of margin lending and describe our data. In Section 3 we provide stylized facts regarding anticipation and the impact of the deregulation on asset prices using standard event study and difference-in-difference approaches. In Section 4 we introduce and estimate our information revelation based model of anticipation. We contrast our model's estimates with the results from an RD strategy in Section 5. We conclude in Section 6.

2 Background and Data

2.1 China's staggered deregulation of margin lending

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

Throughout both phases, retail investors with at least 500,000 RMB of assets in their brokerage account and six months or more of trading experience qualified for margin—provided by their brokerage firms—with an initial margin requirement of 50 percent. Interest rates on margin loans from brokerage firms were generally around 8 to 9 percent annualized, significantly lower than the rates on shadow margin loans through informal channels (which typically ranged from 11 to 14 percent).[5]

The pilot itself was implemented in two stages. On February 13th, 2010, the 90 stocks included in the two major stock indexes— the Shanghai 50 Index (50 stocks) and the Shenzhen Component index (40 stocks)—were opened to margin lending. We refer to this as *Pilot A*. On November 25th, 2011, the Chinese government extended the list of marginable stocks based on membership in two

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

broader market indices. The extended list included 278 stocks: 180 from the Shanghai 180 Index and 98 from the Shenzhen 100 Index. We refer to this as *Pilot B*.

The second phase, the focus of our analysis, was announced in late 2011. Official regulations were released explicitly stating that the list of marginable stocks would be extended in a staggered manner in a series of waves, which we call *Vintages*.[6] To determine the set of qualifying stocks for each vintage, the regulatory agency published a screening-and-ranking rule. This procedure had three steps: (i) screening out stocks that did not satisfy a set criteria intended to disqualify particularly small, volatile, illiquid, and newly listed stocks—the so called Article 24 for Shanghai and Rule 3.2 for Shenzhen;[7] (ii) ranking the remaining stocks according to the formula shown in Equation 1 below and (iii) selecting the top candidates in each exchange (with some discretion).[8]

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

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

Table 1 summarizes the timeline of deregulation and the number of newly marginable stocks for each extension. The set of stocks included in *Vintage 1* was announced January 25th, 2013, and margin lending for these stocks was implemented on January 31st, 2013. Similarly, *Vintage 2* was announced on September 6th, 2013 and implemented on September 16th. *Vintage 3* was announced on September 12th, 2014, and implemented on September 22nd, 2014. For the purposes

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

[7] The criteria for both exchanges are the same: they require that stocks: (1) have been traded for more than three months; (2) have either more than 100 million tradable shares or a market value of tradable shares over 500 million; (3) have more than 4,000 shareholders; (4) have not experienced any of the following in the previous three months: (a) daily turnover less than 20 percent of the turnover rate of the market index; (b) the average of the absolute value price changes more than 4 percent off of the market index; (c) market volatility higher than the market volatility by 500 percent; (5) have completed the share reform; (6) are not specially treated stocks; and (7) other conditions. The official documentation does not specify what these other conditions refer to. See rules on stock trading with margin loans on each stock exchange's website.

[8] 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.

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.

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

Following the deregulation, margin lending in China expanded dramatically. In Figure 1 we plot the ratio of margin debt to market capitalization alongside total market capitalization. The ratio, shown in blue, increased from a negligible amount at the beginning of 2012 to roughly 4.5 percent in June of 2015. At the peak, the level of margin debt reached roughly roughly two trillion yuan.

Coincident with the high level and rapid growth of margin debt, the Chinese stock market experienced an enormous boom. The black line in Figure 1 shows that total market capitalization increased from 20 trillion yuan in mid-2014 to over 50 trillion at its peak in June 2015. This peak was followed by a substantial crash: total market capitalization collapsed by more than 20 percent within two weeks. This pattern was mirrored in the major indexes. The Shanghai Composite index rose from about 2000 in mid-2014 to a peak of 5166 on June 12, 2015 before crashing 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). Stock level margin debt outstanding is available at a daily frequency from the Shanghai and Shenzhen stock exchanges. We focus primarily on the period between March 2009 (roughly a year before Pilot A) and May 2015 (just before the crash). The majority of our analysis is conducted at the monthly level.

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

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

3 Stylized Facts on Stock Price Pre-Trends and Anticipation

We begin our analysis by providing evidence of considerable anticipatory buying in advance of the deregulation. While the introduction of large quantities of margin debt should be expected to inflate asset prices under even conservative assumptions, we find no discernible increase in prices at or after the introduction of margin lending using standard event-study or difference-in-difference approaches. Instead, we find substantial returns, on average, for soon-to-be marginable stocks in the months prior to the introduction. In other words, strong pre-trends capture the impact of margin debt on stock prices and invalidate the use of standard event-based approaches to recovering the direct effect.

3.1 The Quantity and Impact of Margin Debt

The large quantities of margin debt shown in Figure 1 suggest a non-trivial impact on stock prices. This is reinforced by Figure 2, which plots the rise of margin debt relative to market capitalization separately for each of the three vintages, with event dates denoted by red vertical lines. For each vintage, the quantity of margin debt reached 3-5 percent of the each vintage's market capitalization within a few months, ultimately peaking between 8 and 10 percent.

One way to think about the impact of margin debt on stock prices is using a noise trader model (De Long *et al.* , 1990a). Assuming retail investors using margin buying are noise traders—i.e. that they have price-inelastic demand as was reported by the media at the time[9]—then the price impact

[9]Media coverage of the market surge noted the outsized role of retail investors (`https://dealbook.nytimes.com/2014/12/29/seeking-to-ride-on-chinas-stock-market-highs/` with relatively low education levels (e.g. `https://qz.com/371412/two-thirds-of-new-investors-in-chinas-stock-market-mega-rally-didnt-finish-high-school/`) while highlighting their inexperience and the potential for "panic buying" (`https://www.ft.com/content/66342736-a3f0-11e5-873f-68411a84f346`).

of margin debt is given by

$$Return = -\frac{1}{\epsilon}\%\Delta Demand, \tag{2}$$

where $Return$ is the return associated with a credit supply event (i.e. the direct effect), $\%\Delta Demand$ is the increase in margin debt relative to market cap driven by buying by previously constrained retail investors when margin debt becomes available and ϵ is the elasticity of demand of the unconstrained buyers or arbitrageurs in the economy. A reasonable estimate of ϵ for small to mid-capitalization US stocks is -1.[10]

Extrapolating this estimate to the Chinese stock market, Equation 2 suggests that a 5 percent ratio of margin debt to market capitalization would generate at least a 5 percent increase in prices for treated stocks, and that the 10 percent peak would generate a price increase of 10 percent. Much larger impacts are possible under reasonable assumptions. In other words, we should expect the expansion of margin debt to have a meaningful impact on asset prices.

3.2 Event study comparing marginable to non-marginable stocks

Given the sizable direct effects implied by the inflow of margin debt, we begin our empirical analysis with a standard event-study approach to try to recover the impact on prices. We construct our estimates as follows. For each of Vintages 1, 2 and 3, we consider the cross section of all stocks that are either (i) included in the corresponding vintage or (ii) not marginable at the time margin debt was introduced for that vintage. We pool these together and consider cumulative returns in the period immediately following the official announcement/implementation of margin debt. Specifically, we consider regressions of the form:

$$Ret_i^k = \beta_0 \text{Marginable}_i^k + \theta_k + \varepsilon_i^k. \tag{3}$$

Here Ret_i^k is the cumulative DGTW adjusted return in the 1, 3 or 12 month window following the announcement/implementation month for vintage k.[11] Marginable_i^k is an indicator equal to one

[10]Estimates of ϵ are based on exogenous changes in passive indexing demand are summarized in Wurgler & Zhuravskaya (2002). The most recent and comprehensive study based on the US Russell 2000 stock index from Chang *et al.* (2014) estimates ϵ at around -1. Stocks in Vintages 1-3 were mid-cap stocks encompassing roughly 12 percent of total market capitalization—comparable to Russell 2000 stocks.

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

if stock i becomes marginable in vintage k. θ_k is an indicator equal to one if the observation is included in the cross-section corresponding to vintage k, and captures the average return for non-marginable stocks in the relevant window. Our coefficient of interest is then β_0 which captures the deviation in cumulative returns from the average for other non-marginable stocks. We cluster our standard errors at the stock level.

The first three columns of Table 2 show that the relative returns for newly marginable stocks were, if anything, slightly negative in the period immediately following marginability. We see no significant differential return in the first month following marginability, a marginally significant negative return of roughly 1.5 percent in the first three months, and a negative return of roughly 3 percent in the first 12 months.

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

Indeed, Columns 4-6 in Table 2 suggest the former: the direct effect was anticipated and priced in by the time of the official announcement. In these specifications, we repeat the analysis shown in Equation 3 but consider cumulative DGTW returns in the 1, 3 or 12 months prior to the announcement/implementation. We see strong evidence of *positive* returns in the period preceding marginability. We estimate significant differential DGTW returns of roughly 1.5 percent in the month just prior to implementation, of 4.5 percent in the 3 months preceding implementation, and of over 20 percent in the year before implementation. Furthermore, these returns did not dissipate following implementation. Column 7 shows that cumulative returns from 12 months prior to 12 months after the announcement/implementation month were approximately 20 percent. In other words, the returns on soon-to-be marginable stocks were positive on average in the year leading up to marginability, suggesting that unconstrained investors differentially purchased these stocks in anticipation of the introduction of margin debt.

The results in Table 2 can be visualized vintage by vintage in Figure 3. Panel (a) plots the log of monthly market cap—after netting out stock, month, and book-equity decile fixed effects—and displays evidence of sharp rises in market cap for Vintages 1, 2 and 3 in anticipation of the

serve as the benchmark for that stock's adjustment.

introduction of margin debt. Panel (b) plots cumulative DGTW returns from March 2011 onwards, and displays virtually the same pattern.

One potential concern is that our results showing negative or flat returns in the period following marginability might be mechanically driven by anticipation in the set of not-yet-marginable stocks. Specifically, our effective control group for vintage k includes stocks in vintage $k+1$. Therefore, what we interpret as a negative or flat relative return for stocks in vintage k might simply be an artifact of a *positive* relative return for stocks in vintage $k+1$ due to anticipation. To rule out this concern, Appendix Table A.I repeats our analysis, but includes for each vintage k only (i) the set of stocks in vintage k and (ii) the set of stocks that never become marginable. As a result of this sample restriction, the control group does not include soon-to-become marginable stocks. The results for both returns prior to and following marginability align with the estimates in our main specifications (although there is no evidence of a significant negative return in the period following marginablity).

3.3 Difference-in-difference approach

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

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

$$Return_{i,t} = \alpha + \beta_0 \text{Margin Trading Active}_{i,t} + \sum_{j=1}^{S} \beta_j D_{i,t+j} + \gamma_i + \delta_t + \varepsilon_{it}. \tag{4}$$

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

As a dependent variable, we use monthly returns. As such, $\beta_1, ..., \beta_S$, which we refer to as *Ex-Ante Effects*, capture the average monthly return on soon-to-be-marginable stocks in the period leading up to marginability.[12] Similarly, β_0 captures the average differential return in the period *after* margin debt is available, which we refer to as the *Ex-Post Effect*. We consider both raw and DGTW returns.

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

The remaining columns account for anticipation: the second and fifth columns allow for three months of anticipation while the third and sixth columns allow for three quarters of anticipation.[13] In these specifications we again find no evidence of positive ex-post effects. Our estimated coefficients range from -0.7 percent to -2 percent per month.

Our estimated ex-ante effects indicate the presence of anticipation. Whether considering returns in the three months or three quarters leading up to marginability, we see consistently positive differential returns for soon-to-be marginable stocks. We estimate differential monthly returns as large as 2.0 percent (using raw returns) or 2.5 percent (using DGTW returns). One notable exception is that we see no 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 reaffirm the evidence for anticipatory pre-trends, i.e. that prices were driven up in advance of the

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

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

introduction of margin debt by unconstrained investors.

3.4 Placebos

One potential concern is that the anticipation 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 3 and compare our placebo coefficients to those generated using the actual treatment group. Because our sample period is contaminated by the deregulation itself, we implement this approach using an alternative window that matches the broad stock market dynamics of our primary sample. In particular, we consider the previous Chinese stock market bubble, which occurred following share reforms in China and during the lead-up to the Beijing Olympics. We include data from July 2001 to September 2007, the same number of months as included in our primary sample period.

For each of our placebo regressions, we randomly select three event dates. At each of these dates, we calculate the inclusion index for each stock according to Equation 1.[14] For the earliest date, we define the top 100 stocks in each exchange as Placebo Vintage 1. At the next date, we exclude stocks in Placebo Vintage 1, and define the top 100 remaining stocks in each exchange as Placebo Vintage 2. At the final date, we exclude stocks in either of the first two placebo vintages, and define the top 100 remaining stocks in each exchange as Placebo Vintage 3. We do not apply the screening procedure as the relevant criteria are not available in this earlier sample.

With these vintages and our randomly selected event dates in hand, we re-run the regressions in Table 3 and store the estimated coefficients. We then repeat this process 10,000 times, each time randomly drawing the three event dates (with replacement). To test whether our results in Table 3 are mechanical, we compare the true coefficients to the distribution of placebo coefficients. As a summary, we show one sided placebo p-values in square brackets in Table 3. These p-values show

[14]As in Section 5 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 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.

3.5 Further Evidence of Anticipation

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

Larger stock price pre-trends for higher ranked stocks

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

To capture this, we conduct a triple-difference version of Equation 4, further interacting Margin Trading Active$_{i,t}$ and all $D_{i,t+j}$ with an indicator equal to one if stock i is highly ranked within its vintage. We define highly ranked stocks to be those with above median rank among the set that ultimately qualified within each vintage and exchange. The results, presented in Table 4, show significant positive coefficients on the interaction between ex-ante effects and our dummy variable for highly ranked stocks. In other words, high ranking stocks saw significantly greater pre-trends in the months prior to marginability. This suggests that unconstrained investors were indeed anticipating the introduction of margin lending (and that they were better able to anticipate the inclusion of high-ranking stocks).

Unconstrained-investor holdings and trades

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

There is strong evidence that these unconstrained investors increased their holdings in anticipation of the roll-out of margin lending. In Table 5 we display regression results following Equation 4, but replace the dependent variable with the share of ownership by unconstrained investors (defined as either mutual funds or the top 10 investors). The regressions are estimated at the quarterly level, corresponding to the frequency of our data on these investors. In Columns 1 and 3 we show traditional difference-in-difference specifications with no ex-ante effects. In Columns 2 and 4 we allow for three quarters of ex-ante effects. We find positive ex-ante effects in each of these three quarters, suggesting that mutual funds differentially increased their holdings in soon-to-be marginable stocks by 0.5-0.7 percentage points per quarter in the period leading up to deregulation. Similarly, the top 10 ownership share differentially increased by 3.7-4.3 percentage points per quarter. Furthermore, we find negative effects in the quarters after deregulation for both outcomes—on the order of 0.4 percentage points per quarter—suggesting that unconstrained investors sold-out once margin debt was available.

In the final two columns of Table 5 we repeat the exercise but include monthly stock level turnover as our dependent variable. We find elevated levels of trading in the period preceding marginability for soon-to-be marginable stocks, consistent with elevated trading levels due to anticipatory buying. In contrast to our results on unconstrained investors, we find positive and significant ex-post effects, suggesting that there was also differentially high turnover once margin debt became available. This too is consistent with formerly constrained investors buying from ex-ante unconstrained investors.

4 Retrieving Direct Effects from Stock Price Pre-Trends

The previous section provides evidence of significant anticipatory pre-trends and shows that this invalidates typical event based strategies used for recovering the impact of credit supply on asset prices. Consequently, further structure is necessary to recover this direct effect. While ad-hoc adjustments to standard approaches are possible—for example, by accounting for differential effects in some fixed pre-event window as in our expanded difference-in-difference approach (Equation 4)—these adjustments typically require the econometrician to take an a priori stand on the precise time at which information first became available.

In this section we develop a framework that allows us to recover the direct effect without taking such a stand. Specifically, we introduce a parsimonious competitive stock pricing model and show that, under a set of reasonable assumptions, this model translates into an easy-to-estimate linear dynamic panel model. The model has the added benefit of providing a simple parameterization of the rate of anticipation. We take this to our data and estimate the direct effect.

4.1 Information Revelation Model of Anticipatory Pre-Trends

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

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

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

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

4.1.1 An Unexpected Shock

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

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

for all $t \geq 0$.

In this context, it is natural to view $m = \Delta\gamma\sigma_\pi^2$ as the direct effect of the credit expansion. The unanticipated demand shock of Δ leaves effectively $(Q - \Delta)$ shares for the risk-averse unconstrained investors to own, leading to a lower required rate of return or higher prices. The black line in Figure 4 shows a stylized example of the price path following this sort of surprise. The x-axis represents time, with the vertical line at 0 corresponding to the date of credit supply expansion. The y-axis represents the price of an asset which can be purchased with credit after the event date. In a world with no anticipation, we would expect the path of prices to follow the black line: flat before the event date, with a sharp jump to a higher price immediately or shortly after the deregulatory event. The difference in the two prices represents the direct effect—$\gamma\sigma_\pi\Delta$ in our model—which is the degree to which prices jump after the event date. If the shock is totally unanticipated, a simple difference-in-difference or event study design can pick up the direct effect.

4.1.2 An Anticipated Shock

Suppose instead that unconstrained investors begin to receive signals about the inelastic demand shock Δ in period $t = -n < 0$. Specifically, we assume that each period they receive signals m_t about $m = \gamma\sigma_\pi^2\Delta$, which we model as[15]

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

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

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

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

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

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

In our setting, we are interested in studying the prices of stocks that receive a *positive* credit supply shock at time t=0. In our model, this translates to stocks with $m > 0$.[16] We refer to these throughout as *treated* stocks. For such treated stocks, the expected price at any time t is given by:

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

Notice that this equation shows two distinct sources of pre-trends in prices for treated stocks. First, to the extent there is anticipation, we expect prices to begin to rise in advance beginning at $t = -n$. This is captured by the expectation on the righthand side of Equation 7. Based on our assumptions, this expectation term is positive and grows as t increases towards 0. This captures the gradual introduction of information about the shock.

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

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

4.2 Exponential Decay Information Structure

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

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

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

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

The red and blue lines to the left of the event date in Figure 4 show the price path—including anticipation on the part of unconstrained investors—given this framework. We show for two values of θ (holding the size of the direct effect fixed). the blue line shows a relatively high value of θ—effectively a very high rate of decay prior to the event. In this case, anticipation only begins to meaningfully impact prices in the last few periods prior to the event. The red line shows a lower value of θ. In this case, prices begin to rise noticeably much earlier.

Note that the unconditional variance of m can be written as

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

The price at time $t < 0$ (normalizing $Q = 1$ for simplicity) is then [17]

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

Now let us again consider anticipation and the price path for a set of treated stocks: those that

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

ex-post were found to have received a positive margin lending shock. To model this, we again consider a stock to be treated with a positive supply shock if $m > 0$.

Recalling the normality and independence of the signals m_t the expected price for treated stocks at any point t is:

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

If we define $\tilde{p} = p^* - \frac{\gamma\beta}{1-\frac{1}{\theta}}$, we may rewrite this as[18]

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

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

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

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

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

4.3 Panel Estimation Strategy

We now show that Equation 8 translates naturally into a simple panel regression model that we can use to estimate key quantities of interest. To see this, first consider the price realization for a given "treated" stock i at time t. Recall that we define a stock to be treated if it receives a positive

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

shock ($m^i = \sum_{j=-\infty}^{0} m_j^i > 0$). Given Equation 8, we have:

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

where ε_{it} is mean 0 and uncorrelated across stocks. Taken literally, this error term represents the difference between the realized stream of messages for stock i and the conditional expectation for the treated group:

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

Of course, in practice ε_{it} will also include any unmodeled stock and time specific factors not captured by the expression in Equation 8. Iterating Equation 9 forward one period and rearranging, we may rewrite this as:[19]

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

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

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

[19]This follows because we may write:

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

Furthermore,

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

Therefore:

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

Substituting this in the original expression gives the result.

credit supply shock.

With such a control group, a natural generalization is a difference-in-difference version of Equation 10 using a broader sample that includes both treated and control stocks:[20]

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

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

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

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

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

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

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

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

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

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

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

4.4 Instruments

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

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

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

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

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

Note that this restriction implies that e_{it} will be mean independent of D_{it} and its leads. In other words, in any given periods, the stock specific error term must not correlate with future treatment status.[21] If Equation 9 is taken literally (i.e. signals m_{it} are the only source of idiosyncratic price fluctuations) then this restriction is be satisfied given the rational expectations assumptions of our model. More generally, this restriction is analogous to the assumptions in a standard difference-in-difference: that the control allows us to construct a reasonable counterfactual for the price of the treatment group in the absence of any credit supply shock. This would be violated if, for example, the prices of stocks in the treated group were trending differently for reasons unrelated to the shock (e.g. because of differential exposure to some underlying factor), or if some unrelated shock hit the treated group during the sample period.

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

Of course, our strategy also assumes we have specified the appropriate model of price formation, i.e. that the path of prices reflects investors' rational expectations about the coming shock. There are naturally other models that might generate trends in anticipation of a coming shock. For example, De Long *et al.* (1990b) style models in which noise trading increases with higher prices due to a positive feedback mechanism. In such models, prices will typically overshoot fundamental values due to feedback trading. This sort of phenomenon would pose a problem for the restriction in Equation 12 and our estimation, leading us to overestimate both the rate at which information is incorporated, and the direct effect. However, our results from Section 3 suggest that there is not a significant amount of overshooting.

4.5 Implementation and Results

We implement our estimation strategy using our sample of stocks in vintages 1-3 and those that were never marginable. The stocks in vintages 1-3 here serve as the treated stocks in our model. The set of never marginable stocks—largely composed of stocks very far from the threshold for inclusion according to the screening-and-ranking rule (and therefore with little ex-ante probability of becoming marginable)—serve as control stocks. We consider monthly data covering March 2009-October 2015. To account for scale effects, we normalize p_{it}, the price of stock i in month t by the price of that stock in March 2009. Here $D_{it} = 1$ if margin lending is available for stock i in month t and 0 otherwise. We consider OLS estimates and several versions of our IV specifications, including standard two-stage least squares and system GMM style approaches.

The first column of Table 6 shows an OLS version of Equation 11.[22] The OLS estimate of δ_1, which we expect to be biased due to the endogeneity concern described above, is $\hat{\delta}_2^{OLS} = 0.883$. Taken literally, this would suggest that the direct effect was anticipated gradually, with the equivalent of a 12 percent monthly discount rate. We also have $\delta_1^{OLS} = 0.013$, which translates to a direct effect ($\frac{\delta_1}{1-\delta_2}$) of roughly 0.11, i.e. that treated stocks cumulatively experienced a differential increase of 11 percent of the March 2009 price once margin lending was fully rolled out.

The second column of Table 6 implements our instrument based estimation strategy using a standard two-stage-least squares approach. In the first stage, we instrument for p_{it+1} using leads

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

of D_{it}. Specifically, we use leads 2 through 4 and estimate first stage:

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

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

The final two columns of Table 6 show the results of implementing our strategy using an Arellano and Bond style one-stage GMM approach with leads of D_{it} as instruments. In column 3 we use 2-4 leads for comparability with our two stage least squares approach. In column 4 we use a much broader set of instruments, employing 2-10 leads. We follow Malani & Reif (2015) and transform the data using forward orthogonal deviations instead of first differences.[23]

Our results are consistent with the two stage least squares approach. $\hat{\delta}_2^{AB}$ ranges from 0.92 to 0.94, suggesting that the ultimate price effects of margin lending were anticipated gradually—with an effective discount rate of 6-8 percent—as information slowly became available. This rate of anticipation suggests that more than 60 percent of the direct effect of credit supply was already priced in even 6 months prior to deregulation.

The direct effect implied by these estimates ranges from 0.19-0.24. In other words, treated stocks cumulatively experienced a differential increase of 19-24 percent due to the introduction of margin lending. This is in line with estimates of just over 20 percent found using using an RD approach, which we show as corroborating evidence in the next section. Note that our estimates also align with the magnitudes from back-of-the-envelope event-study specifications examining cumulative returns in the year prior to marginability (see the last column of Table 2).

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

5 Comparing Our Model-Based Estimates to Regression Discontinuity Estimates

In this section we validate the direct effect estimates from our model with a regression discontinuity approach. This RD approach avoids bias due to anticipation or other pre-trends by focusing on cross-sectional comparisons in a small neighborhood around the threshold for marginability in each vintage. Distinguishing the likelihood of inclusion ex-ante within this neighborhood was effectively impossible, as slight movements in market capitalization or turnover might cause one stock to qualify or another to be disqualified. We find estimates of the direct effect of just over 20 percent, on par with the results from our model.

5.1 Defining the inclusion index and marginability threshold

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 the introduction of margin debt to qualifying stocks can be plausibly viewed as an unexpected credit expansion in a small neighborhood around the threshold.

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

The second step is to identify the relevant discontinuity in the inclusion index. In theory, this discontinuity should be sharp and exactly equal to the value of the index for the lowest-ranking included stock (for each vintage and exchange). In practice the discontinuity is slightly less sharp for two reasons. First, there is some uncertainty over our ability to precisely replicate the procedure used by regulators, 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. Second, and more importantly, there was some room at the margin for discretion on the part of the exchanges, with little in the way of published detail. This meant that stocks ranking well above the lowest included stock were occasionally excluded, and, similarly, stocks ranked low enough to be disqualified were occasionally included.

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

There is little evidence that investors or insiders were able to manipulate the rankings of particular stocks locally around the threshold C_E^k. While the basic inputs into the index could certainly have been influenced to some extent, uncertainty over the exact number of stocks included in each vintage made precise control around the threshold effectively impossible. In Figure 5, we plot histograms of the inclusion index around C_E^k, which we normalize to 0 for both exchanges and all vintages. In Panel (a) we include the closest 100 stocks on each side of the threshold for each vintage and exchange and in Panel (b) we restrict the sample to values of the index within 1 of the threshold. Both panels show that the distribution of stocks is relatively smooth across the threshold. McCrary (2008) tests fail to reject the null hypothesis of no bunching around the threshold.

5.2 First stage: a discontinuity in marginability and margin debt at the threshold

We now turn to showing that the threshold C_E^k is 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 6. In this figure, we once again include data from all vintages and exchanges with an index value within 1 of the threshold, normalizing C_E^k to 0. The x-axis represents the inclusion index, our running variable. On the y-axis, we display the probability that a stock becomes marginable. The scatter plot shows averages within bins of width 0.05 in the index. Lines shows local linear fits with 95% confidence intervals on either side of the threshold. A sharp jump in the probability of marginability is evident at the threshold.

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

$$D_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + \varepsilon_i^k. \tag{13}$$

Here τ_i^k indicates that stock i is above the marginability threshold, that is, it is equal one if $Index_i^k \geq C_E^k$ and 0 otherwise. θ_k represents a vintage fixed effect. Our coefficient of interest is α_{0r}, representing the discrete change in the probability of marginability at the threshold. In our baseline specification, shown above, we include separate linear slopes on each side of the threshold (local linear regressions with a rectangular kernel). We also show results from local linear regressions with a triangular kernel throughout. We include all not-yet-marginable stocks that satisfy the screening rule, and use the covariate adjusted MSE optimal bandwidths described in Calonico *et al.* (2018).[24] We include standard errors based upon the three nearest neighbor variance estimators described in Calonico *et al.* (2014) (CCT).

In line with the pattern shown in Panel (a) of Figure 6, our regression results show a large and significant jump at the threshold. In column (1) of Table 7, $\hat{\alpha}_{0r}$ 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

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

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

To formalize these figures, columns (2) and (3) of Table 7 estimate the specification shown in Equation 13, but replace the dependent variable with stock level margin debt (in (2)) and the ratio of margin debt to market capitalization (in (3)). Column (2) shows that crossing the marginability threshold generates a discrete jump of roughly 13 million yuan of margin debt, on average. Similarly, column (3) suggests that this jump is equivalent to about 1.7 percent of market capitalization, on average. Columns (5) and (6) repeat this exercise with a triangular kernel, showing similar results. All estimates are statistically significant, and together show that our threshold indeed corresponds to a credit supply shock. There was a discontinuous increase in the probability of marginability and the use of margin debt for stocks just above the threshold.

5.3 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 credit shock 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 7 shows plots similar to those in Figure 6. The inclusion index is displayed on the x-axis (normalized to set the threshold to 0). Cumulative raw returns are shown the y-axis. These plots introduce the basic results we flesh out more formally below. In the first month, returns for stocks just above the threshold are only slightly higher than returns for those below the threshold.

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 13:

$$Ret_i^k = \alpha_{0l} + \alpha_{1l}(Index_i^k - C_E^k) + \tau_i^k[\alpha_{0r} + \alpha_{1r}(Index_i^k - C_E^k)] + \theta_k + e_i^k. \tag{14}$$

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 DGTW adjusted returns. For our baseline specifications, we choose bandwidths and estimate standard errors exactly as in Table 7. In the Appendix we show a series of robustness exercises with varying bandwidths.

Our results, presented in Table 8, align with the plots shown in Figure 7. 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 14. 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.III and A.IV 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.

5.4 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 that accounts for the fact that the threshold does not perfectly predict marginability. We report two-stage least squares estimate, where the first stage is given by Equation 13 and the second stage is given by:

$$Ret_i^k = \gamma_{0l} + \gamma_{1l}(Index_i^k - C_E^k) + \gamma_{0r}D_i^k + \gamma_{1r}[\tau_i^k \times (Index_i^k - C_E^k)] + \theta_k + v_i^k. \tag{15}$$

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

Our results are reported in Table 9. Unsurprisingly, the qualitative patterns are similar to those presented in Table 8, with smaller returns at 1 month and sizeable and significant returns at 3 and 12 months. We see DGTW adjusted returns of 12-13 percent at three months, and 21-23 percent at 12 months. Similarly, we see raw cumulative returns of 17-18 percent at 3 months and 25-28 percent at 12 months. In summary, we estimate that marginability generated 12 month cumulative returns (a direct effect) of more than 20 percent. These numbers are on par with our model derived estimates of the direct effect.

6 Conclusion

In this paper, we analyze the use of event based strategies for recovering the direct effects of credit supply on asset prices. We show that, as a matter of theory, the parallel-trends style assumptions underlying such strategies are overly restrictive due to anticipatory buying by unconstrained buyers. Unless an event is entirely unexpected, we should expect asset prices of impacted stocks to rise in advance. We propose an information-revelation based model of anticipatory buying that can be used to recover direct effects from asset price trends before a deregulatory event. The model can be estimated using a simple linear dynamic panel approach.

We implement our model in the context of China's 2010-2015 stock margin lending deregulation. We show the presence of significant pre-trends in prices for stocks that were soon to become eligible for margin lending. Because of these pre-trends, standard event-study or difference-in-difference approaches fail to detect a meaningful direct effect of this credit-supply shock on asset prices. In contrast, our model-based strategy estimates a sizable and significant impact, albeit one that was largely priced in prior to the deregulation date. We validate our estimates using a regression-discontinuity (RD) approach—enabled by the unique features of the Chinese deregulatory episode—that is robust to anticipatory concerns. Estimates from the RD line up with those provided by our model. While clean RD-style variation is not often present, our approach is implementable anytime an event based strategy is feasible and hence may be useful for researchers in a broad set of contexts.

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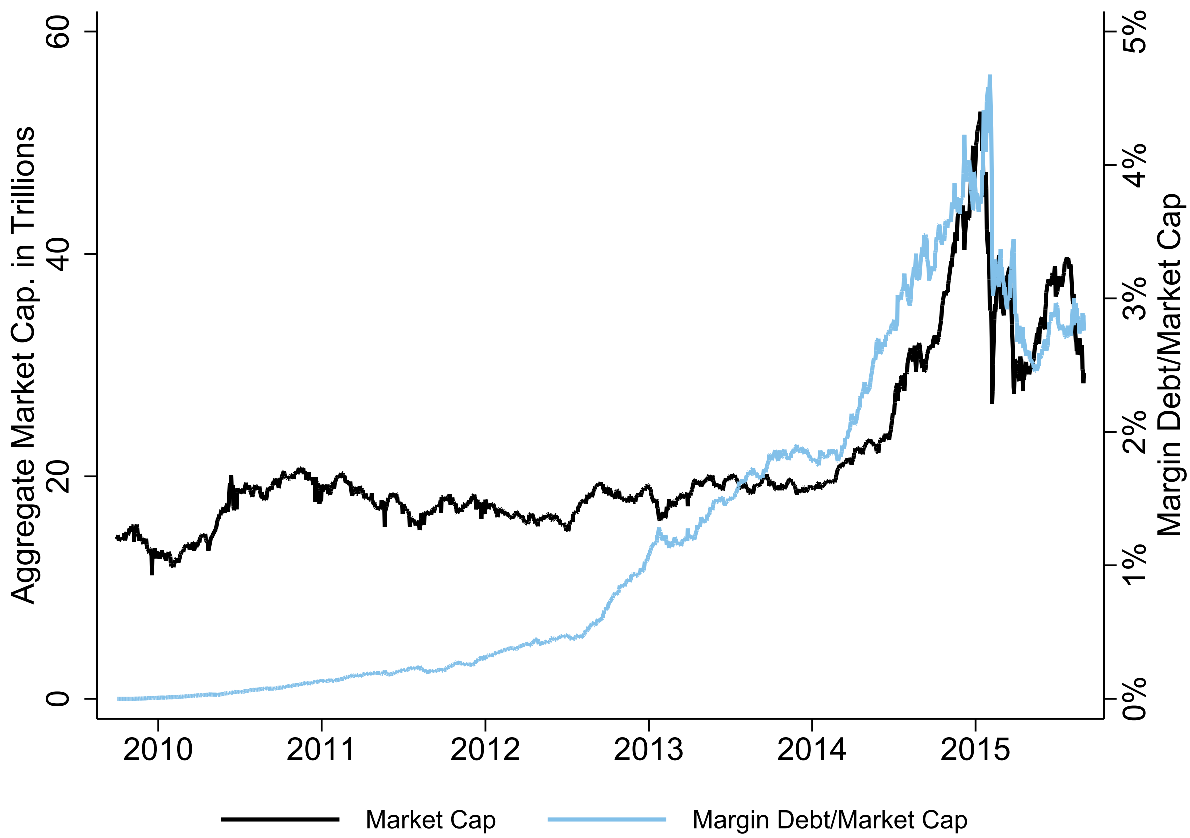
FIGURE 1: AGGREGATE MARKET CAP. AND MARGIN DEBT/MARKET CAP. OVER TIME



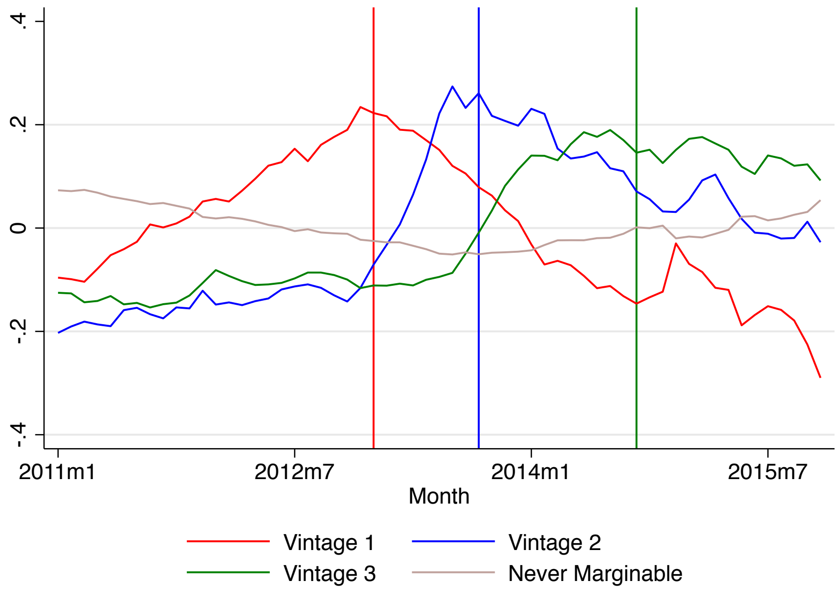
Notes: Plot shows daily aggregate market cap (in black) and the ratio of margin debt to market cap (in blue) for all stocks in sample. Both market cap and margin debt are measured in trillions of yuan.

FIGURE 2: MARGIN DEBT/MARKET CAP. BY VINTAGE

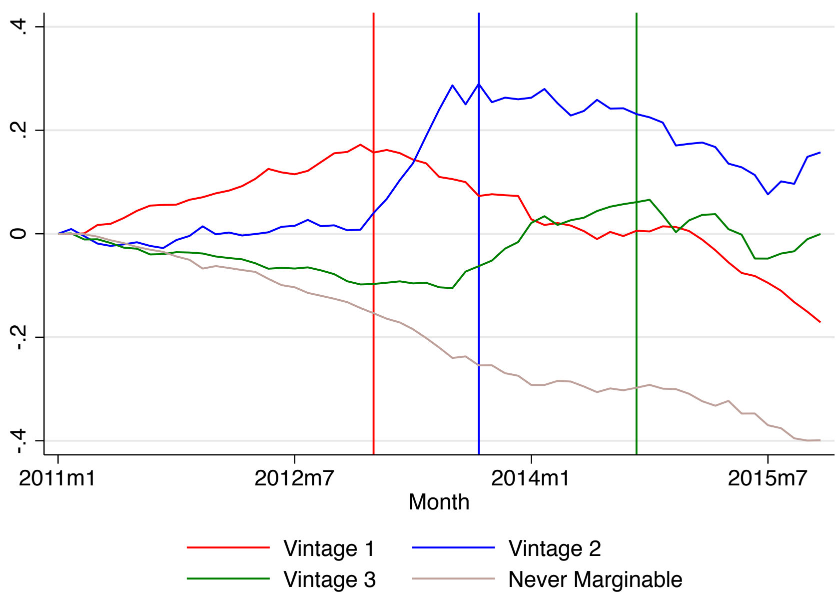
Notes: Plot shows the daily ratio of total margin debt to total market cap for each of the three vintages we study. Vertical lines denote starting dates of each vintage.

FIGURE 3: MARKET ANTICIPATION OF MARGIN LENDING ROLLOUT

(a) Residualized Log(Market Cap)



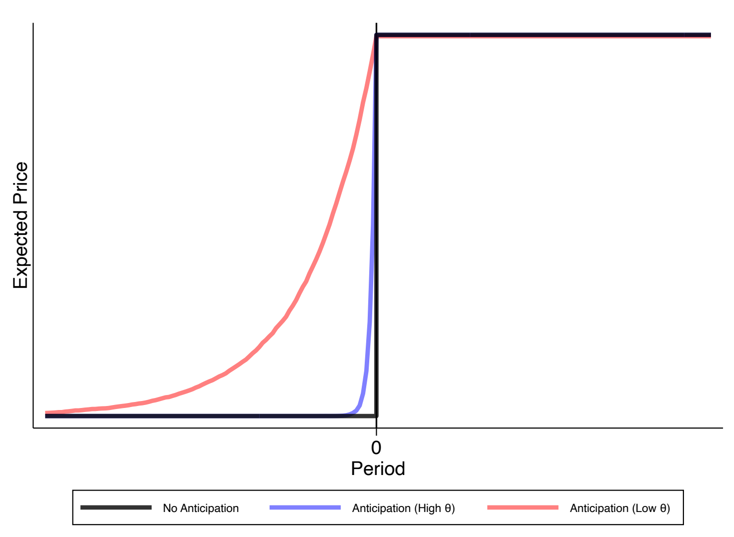
(b) Cumulative DGTW Returns



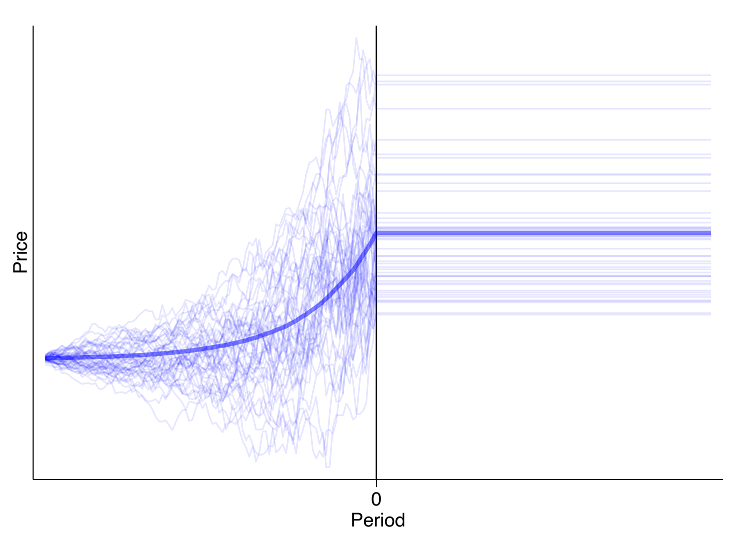
Notes: Panel (a) shows the vintage averages of residuals from a regression of Log(Market Cap) at the stock-month level on stock and month×year fixed effect using the period January 2011-September 2015. Panel (b) shows vintage average cumulative DGTW returns from January 2011 onwards. Vertical lines show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

FIGURE 4: ANTICIPATION EFFECTS OF AN INCREASE IN CREDIT SUPPLY

(a) Varying the Rate of Anticipation



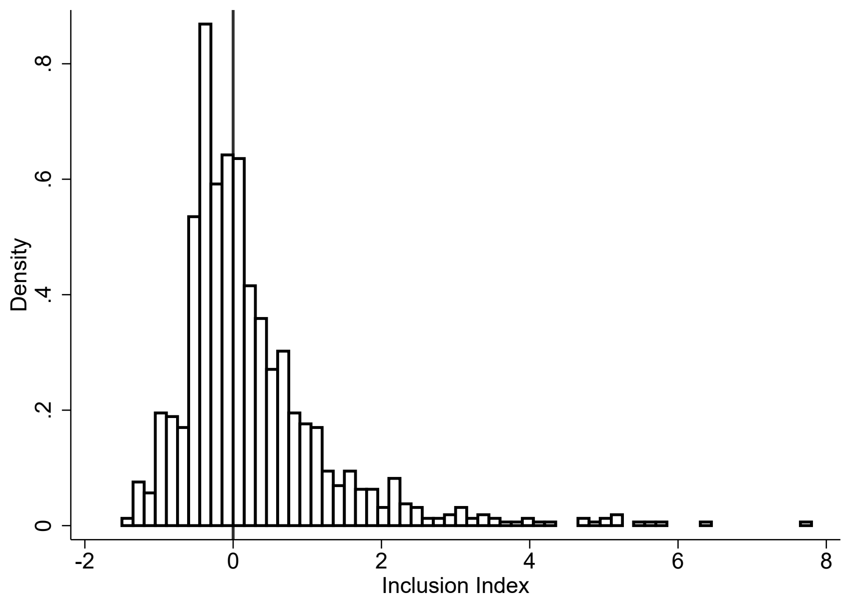
(b) Simulated Price Paths for Treated Stocks



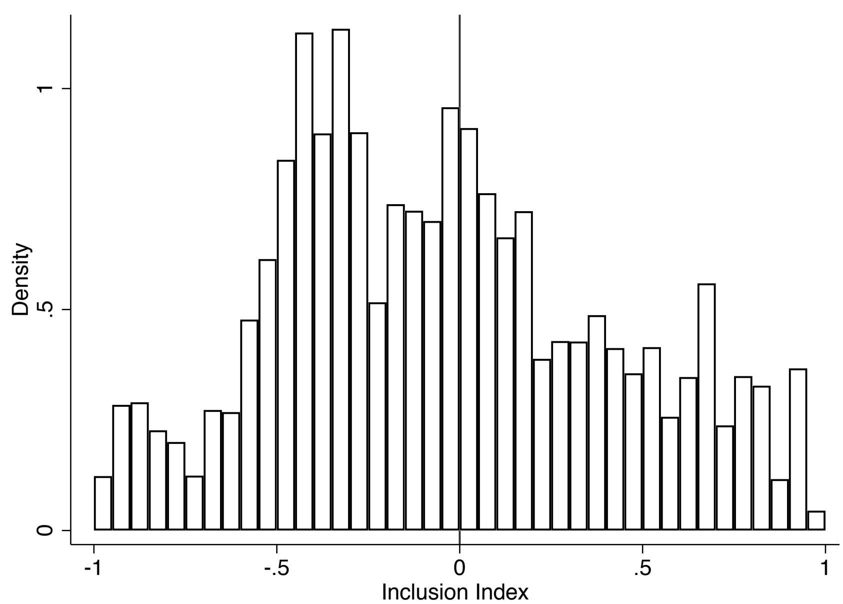
Notes: In both figures the y-axis displays price, the x-axis displays time, and the vertical line indicates the event date of a credit supply shock. Panel (a) shows the expected price path for stocks receiving a credit supply shock at time 0 under three regimes (holding the total price effect constant).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

FIGURE 5: NO EVIDENCE OF BUNCHING AT THRESHOLD

(a) Full Distribution



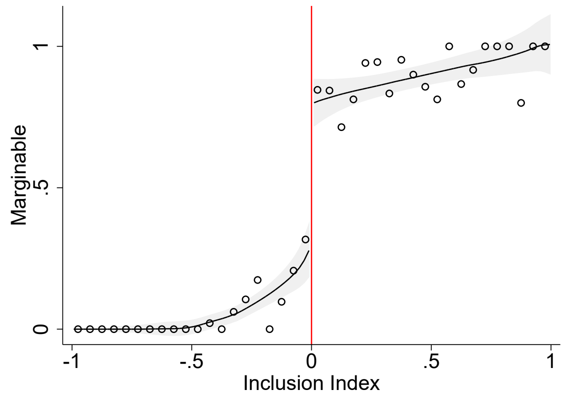
(b) Close to Marginability Threshold



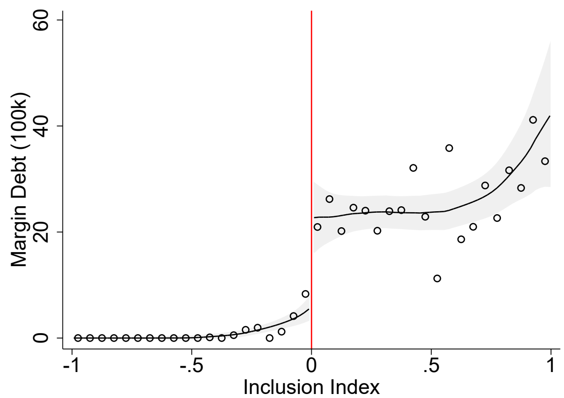
Notes: Both panels show histograms of the value of the inclusion index, normalized to the vintage specific threshold. In Panel (a), we include, for each vintage and exchange, the closest 100 stocks below the threshold and the 100 closest stocks above the threshold. In Panel (b), we further restrict the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

FIGURE 6: INCLUSION INDEX DETERMINES MARGINABILITY

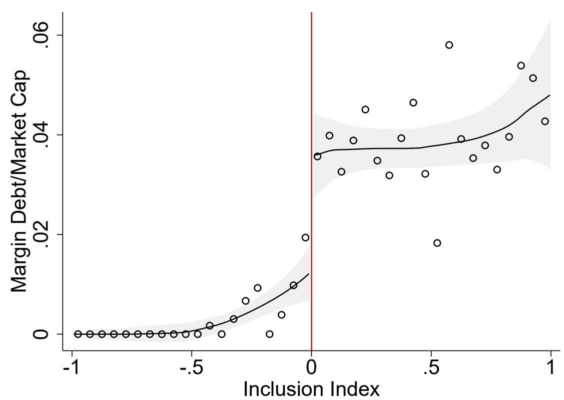
(a) Marginability



(b) Margin Debt



(c) Margin Debt/Market Cap

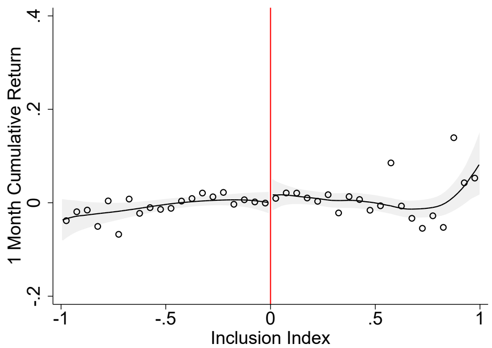


Notes: Indicator for marginability (Panel (a)), stock level margin debt (Panel (b)), and stock level ratio of margin debt to market cap (Panel (c)) plotted against inclusion index. Inclusion index normalized to set vintage specific threshold equal to 0. For each vintage, all not-yet marginable stocks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

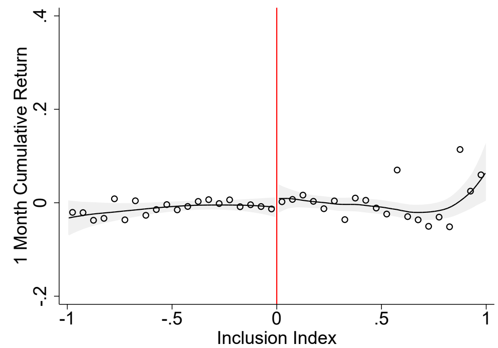
Figure 7: Positive Returns to Crossing Marginability Threshold

Panel A: 1 Month

Cumulative Returns

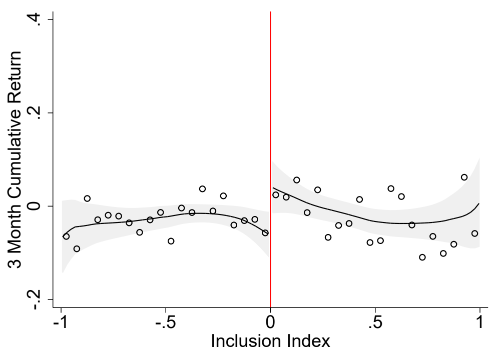


DGTW Adjusted Cumulative Returns

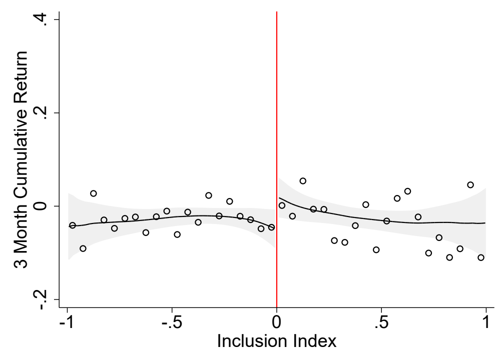


Panel B: 3 Months

Cumulative Returns

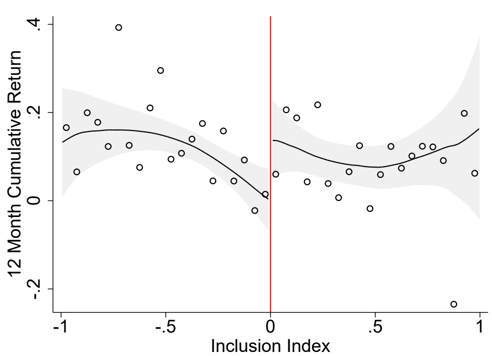


DGTW Adjusted Cumulative Returns

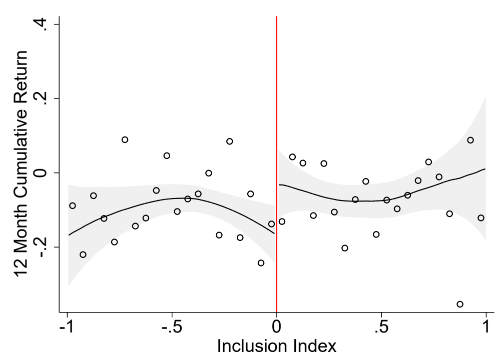


Panel C: 12 Months

Cumulative Returns



DGTW Adjusted Cumulative Returns



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

TABLE 1: NUMBER OF MARGINABLE STOCKS BY VINTAGE

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

TABLE 2: EVENT STUDY OF MARGINABILITY

	Cumulative DGTW Returns						
	Following Marginability			Preceding Marginability			Before vs. After
	0 to 1	0 to 3	0 to 12	-1 to 0	-3 to 0	-12 to 0	-12 to 12
Marginable	-0.005	-0.014^{*}	-0.030^{**}	0.014^{***}	0.045^{***}	0.230^{***}	0.198^{***}
	(0.004)	(0.008)	(0.015)	(0.005)	(0.008)	(0.014)	(0.020)
N	4513	4388	4151	4422	4338	4255	4015

First three columns show results from regressions of cumulative DGTW returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the announcement/introduction of margin debt on an indicator for newly marginable stocks. Columns 4-6 show results from regressions of cumulative returns at the stock level from 1, 3, and 12 months preceding the annoucement/introduction to the month of the introduction itself. Column 7 shows cumulative returns from 12 months before to 12 months after introduction. For each of the three vintages determined by the screening and ranking rule, we compute cumulative DGTW returns adjusted for splits and dividends for the newly marginable stocks in that vintage as well as the set of contemporanously non-marginable stocks. All specifications include dummy variables for vintage as a control. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3: MARKET ANTICIPATION OF MARGINABILITY

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

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

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

Margin Trading Active is equal to one only (i) for stocks that are included in the margin trading roll-out, and (ii) in months after margin trading is active in those stocks. $D_{i,t+j}$ is equal to one if margin trading initially becomes active for stock i in period $t+j$, and zero otherwise. The number of *ex-ante effect* coefficients indicates the value of S for the regression in question. The first and fourth columns includes no ex-ante effects, and is equivalent to a collapsed difference-in-difference approach. Other specifications include indicators aimed at capturing ex-ante effects for the three months or three quarters leading up to the roll-out for each stock. Sample covers March 2009-May 2015. The left three columns show cumulative log returns adjusted for splits and dividends but otherwise unadjusted. The right three columns show DGTW adjusted returns. Standard errors, clustered at the stock 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 4: MORE ANTICIPATION FOR HIGH RANKED STOCKS

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

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

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

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

TABLE 5: INSTITUTIONAL OWNERSHIP SURGES BEFORE MARGINABILITY

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

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

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

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

TABLE 6: INFORMATION REVELATION MODEL OF ANTICIPATION

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

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

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

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

TABLE 7: 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 8: 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 9: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Internet Appendix: For Online Publication

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

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

First three columns show results from regressions of cumulative DGTW returns at the stock level from 1, 3, and 12 months preceding the introduction to the month of the introduction itself on an indicator for newly marginable stocks. Columns 4-6 show results from regressions of cumulative returns at the stock level from the month of marginability to 1 month, 3 months, and 12 months following the introduction of margin debt. Column 7 shows cumulative returns from 12 months before to 12 months after introduction. For each of the three vintages we consider only the newly marginable stocks in that vintage as well as the set never marginable stocks. We compute cumulative DGTW returns adjusted for splits and dividends. All specifications include dummy variables for vintage as controls. Standard errors, clustered at the stock level, are included in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A.II: 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.III: 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.IV: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.