ESSAYS ON FIRMS, INVESTORS, AND STOCK MARKETS

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My dissertation uses international evidence to provide fresh perspectives on the interaction of firms and the secondary market. Chapter 1 is an empirical study of the causal effect of an initial public offering (IPO) on a firm's investments. The study focuses on a historical event: the 2012 Chinese IPO moratorium. This event addresses the endogeneity problem associated with IPOs. It independently sorted into two groups the Chinese applicant firms of 2012—those that went public as scheduled and those that were delayed for at least a year. I find that firms that went public on schedule made significantly more investment in fixed assets after their IPO compared to similar firms that faced a one-year delay. The increase in investment is on average 36.4% of the firms' pre-IPO level—a pronounced amount that generates policy implications. Chapter 2 uses a unique dataset from 1999–2015 to show that the number of trading halts is a significant source of illiquidity in the market for Chinese A shares. The risk of trading halts is not predicted by the bid-ask spread but is predicted by end-of-day transactions cost measures, suggesting that the measures of transactions costs have more information than the bid-ask spread. The discretionary trading halts are associated with a robust "run up and reverse" pattern of cumulative abnormal return. On average they earn negative returns for investors who purchase the stocks right before the trading halts, but earn positive returns for existing investors who already hold the stocks for a longer period. Financially constrained firms and firms engaging in earnings manipulation create more trading halts and their trading halts have a larger impact on the price. Trading halts generally

decrease for firms that split stock supporting the “catering” motive for halts. Chapter 3 examines whether sensation-seeking is an important determinant of trading volume by looking at cross-country evidence. This paper validates that sensation-seeking plays a significant role in explaining cross-country variation in excess trading volume. Internationally, market efficiency is not strong enough to drive sensation-seekers out of the market. Overconfidence and risk appetite do not take away the explanatory power of sensation-seeking.

BIOGRAPHICAL SKETCH

Yuzheng Sun was born in Yantai, China. He graduated from Lake Forest College in 2011, where he received his B.A. in economics and mathematics. Upon graduation, he joined the doctoral program in economics at Cornell University. In June 2017, he defended his doctoral dissertation. His first position will be as an economist at Amazon in Seattle.

To mom and dad

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Chapter 1

The Causal Effect of IPO on Firms' Investment

1. Introduction

The Initial Public Offering (IPO) is one of the most significant events in the life of a firm. An IPO drastically changes two aspects of it: financial constraints and ownership. On the one hand, an IPO provides the issuing firm with a large amount of cash, allows it to access the equity market for the first time, and mitigates the firm's financial constraints. On the other hand, an IPO changes the firm's status from private to public, forces the firm to disclose its quarterly performance, and can exacerbate the firms' agency problem through such issues as managerial short-termism.[1]

The mitigation of financial constraints and the increase in agency problems can have profound but opposite impacts on a firm's investment behavior. Financial constraints theory suggests that firms under financial constraints forego valuable investment opportunities due to financial distress risk and the higher cost of capital (Almeida, Campello, & Weisbach, 2004; Fazzari, Hubbard, Petersen, Blinder, & Poterba, 1988). Getting listed reduces firms' financial constraints and enables them to increase their investment to achieve more optimal levels (Brav, 2009; Maksimovic, Phillips, & Yang, 2013; Saunders & Steffen, 2011). Nevertheless, research into agency problems suggests that managerial short-termism and myopic behavior can lead public firms to under-invest because some long-term investment can hurt short-term earnings through depreciation and other start-up charges (Asker, Farre-Mensa, & Ljungqvist, 2015; Graham, Harvey,

[1] Ritter and Welch (2002) survey discusses the various motives for IPO.

& Rajgopal, 2005; Jensen & Meckling, 1976; Stein, 2003). The effect of an IPO on investment remains an empirical question.

The purpose of this study is to examine how an IPO changes a firm's investment behavior. The answer is not obvious. The results of prior empirical studies are often mixed. For example, Pagano, Panetta, and Zingales (1998) find that Italian firms decrease investment after their IPOs. Chemmanur, He, and Nandy (2010) find that U.S. firms, in contrast, increase investment after their IPO. Asker et al. (2015) document that public firms invest substantially less than similar private firms and are less sensitive to investment opportunities; while Gilje and Taillard (2016) argue that public firms in the natural gas industry are more sensitive to investment opportunities than private firms. These seemingly contradictory results can coexist because attempts to identify empirically the treatment effect of IPOs on investment are complicated: firms endogenously choose whether to conduct an IPO (selection) and when to do so (timing). Selection bias, such as the inherent difference between firms who undertake IPOs and those firms that do not, can bias cross-sectional estimations. Meanwhile, the timing of the IPO decision can bias time-series estimations. Most IPO researchers thus limit their goal to studying the within-firm dynamic around an IPO or to the comparison of public and private firms without drawing causal inferences.

To address this endogeneity problem, an ideal setting would be a randomized controlled experiment involving a set of identical firms: Randomly assign IPO permissions to some of the firms while rejecting the others. In this paper, I investigate a quasi-experimental event that is similar to this ideal setting. The event—the 2012 Chinese IPO moratorium—independently sorted more than 600 firms that had applied for an IPO into two groups: The first group could conduct their IPO on schedule; the other group had to wait at least a year. I take advantage of this real,

historical event to study the causal effect of the IPO, by itself, on firms' subsequent investment behavior.

The moratorium was exogenous to firms' characteristics and their decisions to make stock offerings. In November 2012, to keep the stock market stable, the Chinese stock market supervising authority, the Chinese Securities Regulatory Commission (CSRC), initiated an unannounced IPO moratorium and it froze IPO reviews. The moratorium was unprecedented in duration and impact. It affected more than 600 firms for more than a year, 332 of which were eventually listed. In the 14 months between November 2012 and January 2014, there was no IPO activity in the entire Chinese stock market (A shares market), which had been the largest IPO market in the world before the moratorium and remained so afterwards. The moratorium denied applicant firms' access to both the stock market and the bond market.[2] In Section 2, I provide details of the moratorium, including its exogenous nature. One of the key mechanisms is the merit-based regulation system of stringent requirements that the CSRC enacted to make applicant firms relatively homogeneous. The regulation also resulted in a long preparation stage for Chinese IPO applications that commonly took three years. In this study, I define the *treatment* as the event of listing, the *treatment group* as the 154 firms that were listed in 2012 right before the moratorium, and the *control group* as the 332 moratorium-affected firms that experienced at least a one-year delay in their IPO process. The merit-based regulation system ensures that the treated group and the control group are similar in these characteristics: they are both IPO applicants and eventually got listed. The long preparation stage also ensures that the firms were unlikely to have anticipated the moratorium when they made their decision to go IPO three years before. Therefore, the

[2] The Chinese corporate bond market is hardly available to private firms.

moratorium is exogenous to the firms' characteristics and their decision to go public, and some checks of robustness further validate this claim.

Utilizing a difference-in-difference (DID) strategy, I find that going public generates a positive effect on firms' investments in fixed assets. The results are both statistically and economically significant. On average, the investment rate, as measured by a firm's investment in fixed assets over its 2011 level for an average firm that conducts an IPO on schedule, is 36.4% more than that of a similar moratorium-affected firm. As the moratorium is exogenous, the effect can be neatly attributed to the difference between the two groups in receiving IPO treatment or not. My results are also robust to different definitions of the treatment and different definitions of investment. Matched public firms are also used as an alternative control group; the results from both these two tests remain qualitatively similar. A placebo test is conducted to compare 2014-listed firms and 2015-listed firms within the control group. No significant difference exists between the 2014-listed firms and the 2015-listed firms, suggesting that time-of-application does not drive my results. This positive effect of the IPO event on firms' investment has two implications. First, it suggests that the financial-constraints effect prevails over the impact arising from agency problems in determining post-IPO firms' investment behaviors. Second, it suggests that firms conduct an IPO to increase their potential productivity rather than to monetize high valuations after their productivity peak. I test these two implications.

To investigate the financial-constraints channel, I first look at whether the treated firms' post-IPO characteristics are consistent with lessened financial constraints. I find that an IPO increases the treated firms' cash balance and reduces cash spending on financing activities. These two results that arise subsequent to the IPO demonstrate that going public can reduce firms' distress risk and financial cost. Although post-IPO firms initially decrease their liabilities in the first post-

IPO year, they increase their liabilities in subsequent years. The increase in liabilities suggests that post-IPO firms appear to consider the amount of cash raised through an IPO insufficient since they sought additional funding, which had a higher cost of capital. This behavior supports the notion that financial constraints were binding for the pre-IPO firms. Finally, I find that the treatment group experienced a larger asset growth than the control group, even after the entire control group went public. These results suggest that going public alleviated a firm's financial constraints. Not only did the IPO provide capital; it also enabled firms to raise more debt financing. This allowed the firms with their IPO on schedule to grow larger than the moratorium-affected firms. The finding further strengthens the argument that pre-IPO firms are under financial constraints and have not reached their optimal investment level; going public increases investment through lessening financial constraints.

After establishing the benefit of increasing investment after IPOs, I next investigate whether the agency problems effect is binding, especially that of managerial short-termism. Agency theory predicts that increasing investments come at the expense of short-term-earnings loss as the main motivation for public firms' underinvestment. I investigate this channel by looking at the earnings of post-IPO firms. The post-IPO decrease in firms' net profit is well documented in the empirical literature. Researchers typically use the life cycle theory of firms to explain this stylized fact. In particular, firms conduct an IPO after their productivity has peaked to monetize higher firm value; firms are unable to maintain the high operating performance after their IPOs. To test this explanation directly, I compare firms that are at a similar stage in their life cycles but received a different IPO treatment. I find that the decrease in net profit only happens to firms that conduct an IPO, not to similar firms whose IPO was delayed. This evidence suggests that the IPO causes a decrease in earnings rather than signaling that productivity has peaked. The result supports

the claim that the agency-problems effect is binding. The combined results—that both financial-constraints effect and agency-problems effect are binding—are consistent with firms choosing to increase their investment despite a negative-earning effect and a loosening of financial constraints outweighing managerial short-termism.

Overall, the study makes three distinct contributions to the existing literature. First, it contributes to the IPO literature by using a quasi-experimental setting to address the endogeneity problem associated with IPO: It quantifies the causal effect of IPOs on firms' investment. The magnitude of my result—the IPO causes a firm to increase its investment on fixed assets by 36.4%——suggests that an IPO can profoundly increase firms' investments. In analyzing the trade-off between lessened financial constraints and increased agency problems after IPOs, firms choose to increase long-term investment despite the short-term negative-earning effect. The study also provides causal evidence for the relationship of the IPO to firms' productivity as implied by the life cycle theory of firms. Going public helps a firm increase its productivity rather than signaling that its productivity has peaked.

Second, the results corroborate findings in recent papers that compare the difference in investment behavior between public and private firms (Asker et al., 2015; Gilje & Taillard, 2016). By studying a particular group of firms—IPO applicants—I provide a dynamic perspective on their findings. In determining their post-IPO investment behaviors, financial constraints have a much larger effect than agency problems for firms that are large, mature and profitable. This result stands in contrast to much of the previous literature, which assumes that financial constraints are less—or even not—binding for large firms. In addition, my results speak to the recent literature about the real effect of the stock market (Florysiak & Goyal, 2016; Levine, Lin, & Xie, 2016; Zingales, 2015). My results establish a causal channel, which suggests that gaining access to the

stock market can increase firms' long-term investment, thus increasing total societal output, given that corporate investment is one of the key drivers of economic growth.

Finally, this paper has potential policy implications given the argument of Shan & Zhu (2015) that problematic IPOs are one of the main contributors to China's poor stock market performance. To the best of my knowledge, my study is the first attempt to use quasi-experimental methods to quantify the real effect of the 2012 Chinese IPO moratorium. Since the CSRC's mission is to stabilize the market, the performance of firms who apply for an IPO is a secondary issue. My study quantifies the real effect of going public; it suggests that the IPO moratorium resulted in a large potential loss in investment opportunities as well as a loss in economic growth arising from the moratorium. This study has relevance to the recent historically high number of IPO applicant firms. It also increases our understanding of the Chinese IPO market—which since 2000 has often been the world's largest IPO market (Doidge, Karolyi, & Stulz, 2013). My paper quantifies the real effect of IPOs on corporate investment and urges the CSRC to reconsider its role to better promote the welfare of IPO applicant firms.

The rest of this paper is organized as follows: Section 2 provides relevant institutional details about the Chinese market, details about the moratorium, and a discussion of why the Chinese IPO moratorium was an exogenous shock to the IPO market. Section 3 introduces the details of my methodology, describes the data, and provides summary statistics. Section 4 provides results and analysis. Section 5 concludes and provides policy implications.

2. Institutional Background

2.1 The Chinese IPO Market

According to Doidge et al. (2013), China has become the world's fastest growing IPO market. Total Chinese IPO proceeds have cumulatively exceeded those of the U.S. since 2000. The Chinese Securities Regulatory Commission (CSRC) supervises the Chinese IPO market through merit-based regulation. Under this regulation system, a firm must satisfy certain size and profitability thresholds to qualify for an IPO application. The details are reported in Appendix A. This regulation ensures that only mature, profitable and large private companies that are comparable to existing public companies can apply for an IPO in China. The purpose of these regulations is to protect retail investors who have an information disadvantage. The IPO application preparation stage typically takes about three years depending on the firm's prior conditions. After completing preparations, the firm submits its IPO application, and the Public Offering Review Committee (the PORC) holds a review meeting to decide whether or not the company will be approved for listing. The PORC consists 25 members, mostly lawyers, auditors, with some government officials and scholars. The approval decision is primarily based on the truthfulness of the reports and the PORC's judgement of the applicant's future earning potential.

Applications are reviewed in the order in which they were received. In 2012, the approval rate of the review meetings was over 80%. Prior to the 2012 moratorium, the wait time for the review meeting was usually three to six months and started to increase due to the increasing number of applicants. The wait time drastically increased following the moratorium. Although this unique regulatory procedure of the Chinese market limits the diversification of public companies, it generates a homogenous sample where all applicant firms are similar in size, profit and age. The applicant firms are also at a similar stage in their lifecycles. Therefore, the institutional structure

of the Chinese IPO market ensures that the applicant firms within a short period, such as a year, are *qualitatively comparable* both in the cross section and in the time series. Meanwhile, private companies that filed their IPO application are highly unlikely to have anticipated the moratorium given the lengthy preparation stage and review process, when they first decided to conduct their IPOs three or four years prior to the moratorium. I provide evidence for this claim in the discussion as well. Therefore, the merit based regulation system ensures that the moratorium is independent of firms' characteristics, as well as their decision of IPO.

On February 1, 2012, the PORC started to disclose the list of applicant firms. There are four dates for each firm in the disclosed list: *IPO application feedback date*, *IPO application pre-disclose date*, *IPO initial review date*, and *the PORC review meeting date*. Only the record of *the PROC review meeting date* is complete for each company; the records of the other three dates are often incomplete. In most cases, there is only one date out of the three that is recorded. The poor bookkeeping of these records also affects the accuracy of reported dates: the PORC started to disclose the applicant list on February 1, 2012, and if they did not document the data before the disclosure, they recorded the feedback date as February 1, 2012. This results in a cluster of records for February 1, 2012 in my sample. To clearly identify firms that applied for an IPO prior to the moratorium as the control group, I use the method that is the most unfavorable to my hypothesis. I construct a variable, *Review Date*, which is equal to the earliest of the four dates for each applicant firm. The *Review Date* thus represents the first time that the company appears in the PROC's disclosed applicant list. Although the date may be later than the firms' actual application, a firm with a *Review Date* earlier than October 19, 2012, the date of the last reviewing meeting before the moratorium, undoubtedly applied to have an IPO prior to the moratorium. The treatment group is defined as firms that went public in 2012 and submitted their IPO application no earlier than

2011. The control group contains firms whose review date is in 2012 but before October 19, 2012. This definition allows me to overcome the bookkeeping errors and have a clean identification of the treatment group and the control group.

Figure 1 plots the *Review Date* and the *Listing Date* for both the treatment group and the control group. The majority of the treated firms and the control firms started their application within a year with many of them overlapping with each other. Given the long preparation process associated with Chinese IPO applications, the treatment group and the control group are at a similar stage of their lifecycles.

Figure 1-1

2.2 The 2012 IPO Moratorium

Prior to 2016 there have been eight IPO moratoriums in China: four in mid-2005, one half-year moratorium from December 6, 2008 to June 29, 2009, one brief moratorium in mid-2012, one from November 3, 2012 to December 30, 2013, which was the longest of the moratoriums, and finally the most recent one from June to November 2015. In my paper, I use the term 2012 IPO moratorium to refer only to the moratorium that occurred from November 3, 2012 to December 30, 2013.

Over 600 firms submitted their applications and were waiting for the review meeting in November 2012[3]. In comparison, previous IPO moratoriums only affected dozens of firms. There was no official announcement of either the start or the end of the 2012 IPO moratorium. In 2012, the PROC met weekly to review IPO applications and announced to the public the result of the

[3] Based on the IPO applicant list published by the PROC

review meeting: which firms they reviewed, whose applications they approved and dismissed, and the reasons for the dismissals. The last review meeting in 2012 was held on October 19, and, without any official announcement, the PROC halted review meetings. The last firms who received approval at the October 19 meeting went public on November 3, 2012. The market quickly interpreted the halt of review meetings as an IPO moratorium but the majority of market participants were expecting IPO activities to resume soon, as the previous IPO moratoriums did not last very long and did not impact as many firms. Surprisingly, from November 3, 2012 to December 19, 2013, there were no review meetings, and no IPO in the A shares market.

A host of causes were responsible for the prolonged duration of the 2012 IPO moratorium, including the weak market performance and sensitive political environment throughout 2013. Appendix B discusses these reasons in detail. The motivation of the CSRC for first initiating the moratorium was to stabilize the stock market and to protect stock investors. It considered the supply of funds in the Chinese stock market to be relatively fixed and believed that IPOs would drive funds away from current stocks and exert downward pressure on current stock prices (Sun & Tong, 2003). Therefore, the CSRC halted IPOs to stabilize the price of existing stocks. Based on a time-constrained keyword search on Google, the possibility of using the moratorium to stabilize the market was not suggested until August 2012. Around October 2012 the media and retail investors regularly requested the moratorium.

Whether an IPO moratorium can help to stabilize the price of current stocks remains an open question (Packer & Spiegel, 2016); it is not the focus of this paper. This paper concentrates instead on the fact that the CSRC's primary concern is keeping the current stock market stable, while the welfare of pre-IPO firms is only a secondary consideration. This CSRC's strengthens the likelihood that the moratorium was an exogenous event to the IPO applicant firms. Neither the

firms' characteristics nor their potential lobbying efforts likely had much impact on the moratorium decision.

Apart from its unprecedented and surprisingly long duration and the large number of firms affected by it, the 2012 IPO moratorium also coincides with two regulatory changes that makes it possible to identify moratorium-affected firms and obtain their pre-application data. The first regulatory event, which was previously discussed, is the PROC's disclosure of the list of applicant firms. From this list, one can identify which firms that were allowed to proceed with their IPO and which firms' IPOs were delayed as a result of the moratorium. The second regulatory event occurred at the start of 2014. When the CSRC started to resume IPOs, it required all applicant firms to pre-disclose their financial statements from 2011 to 2014. Before the regulatory change, Chinese IPO firms were required to publicly pre-disclose financial statements for the three years prior to their IPO application, *after* they passed the PROC's review meeting. The regulatory change requires all IPO applicants to pre-disclose their financial statements even *before* the PROC's review meeting. As many moratorium-affected firms were not reviewed until 2015, this regulatory change provided the 2011 financial statements for all moratorium-affected firms which is essential for ensuring that the treatment group and the control group are similar.

3. Data and Methodology

3.1 Data Source and Sampling

My analysis is based on the universe of firms listed on the A shares stock market and specifically firms that were listed between 2012 and 2015. The financial statement data, firms' profiles and their IPO information are obtained from information that the Wind Financial Terminal

(WFT) reports[4]. WFT is the Chinese equivalent of Bloomberg, Datastream and Compustat. More than 90% of financial enterprises in the Chinese market and over 75% of Qualified Foreign Institutional Investors use WFT[5]. WFT has information on public firms' daily security information and their financial statement items. WFT's news channel also collects articles covering the moratorium.

In May 2016, the A shares market contained 2,898 listed firms; 596 of them were listed in and after 2012. The treatment group contains firms that were listed in 2012 before the moratorium but submitted their IPO applications no earlier than 2011. There are 154 firms in the treatment group. The control group includes firms that applied to IPO before October 19, 2012, were affected by the moratorium, and eventually got listed. There are 332 firms in the control group. Therefore, my sample contains the 486 firms that were listed after 2012 and applied to IPO between January 1, 2011 and October 19, 2012.

The firms' pre-disclosed financial statements prior to their IPO are combined with their disclosed financial statements after their IPO. A sample of financial statement variables from 2009 to 2015 is utilized. The sample is an unbalanced panel data set. The treatment group, with firms listed in 2012, have data from 2009 to 2015, with the data from 2009 to 2011 drawn from the treated firms' pre-disclosure. The control group, with moratorium-affected firms listed in 2014 and in 2015, have data from 2011 to 2015, with the data from 2011 to 2013 drawn from the firms' pre-disclosure. The variables are chosen from the firms' annual reports to measuring their end-of-year performance.

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[4] I thank The School of Hotel Administration at Cornell University for the purchase of this data.

[5] http://www.wind.com.cn/En/aboutus.html.

3.2 Variable Construction

The variable of interest is the change in investment. Following the convention of the corporate finance literature adapted to the Chinese accounting practices, I construct a variable, *InvestmentRate*, to proxy for firms' growth of investment spending. The *InvestmentRate* is equal to the ratio of net cash outflow for fixed assets, intangible assets, and other long-term assets scaled by the firm's fixed assets in 2011. Specifically, I obtain "cash outflow for purchasing fixed assets, intangible assets and other long-term assets" and "cash inflow from processing fixed assets, intangible assets and other long-term assets," deducting the latter from the former and constructing a variable to proxy for firms' capital expenditure, the *FixedInvestment*. The *FixedInvestment* measures the net cash outflow for fixed, intangible and long-term assets. This metric is used as the analog of capital expenditure that is commonly used in the corporate literature to measure firms' investment. The *InvestmentRate* is the *FixedInvestment* of a firm scaled by the firm's pre-IPO level. As such, the variable measures the change of capital expenditure within a firm. Namely, for firm i in year t:

$$InvestmentRate_{it} = \frac{FixedInvestment_{it}}{FixedInvestment_{i2011}} \tag{1.1}$$

There are several advantages to using the net cash outflow of fixed assets, intangible assets and other long-term assets. First, the accounting literature recognizes that firms' cash flow is less subject to manipulation (Dechow, Ge, & Schrand, 2010). Under the strict audit of IPO applicants' financial statements, the numbers from their cash flow statements are relatively more credible than from their income statements. Second, compared with investment in research and development and investment in advertisement, investment in fixed assets is more rigid and stable: the price of fixed assets, such as plants, land, and equipment, is more transparent and usually involves outside

transactions, whereas the price of paying scientists, acquiring patents, and paying agencies is less transparent and fluctuates more easily, especially in a short sample period like mine. Third, investment in fixed assets often has a larger and more direct impact on the real economy, such as employment, asset prices and productivity. Consequently, studying the cash spent on fixed assets has more straightforward implications for the real impact of an IPO.

Although *InvestmentRate* measures growth of investment and provides a straightforward interpretation of the result, it may introduce extra volatility, as firms' investment on fixed assets can swing from year to year. An alternative to using the *InvestmentRate* is to use the *InvestmentOverAssets*, which is the firms' fixed investment over its total assets in 2011, namely:

$$InvestmentOverAssets_{it} = \frac{FixedInvestment_{it}}{TotalAssets_{i2011}} \tag{1.2}$$

I use this variable in my main specification as well as a robustness check.

To further ensure that the treatment group and the control group are comparable, a large number of covariates are used to control for potential investment opportunities, firm characteristics, and a firm's equity-dependent status. Specifically, the control variables include a firm's age, total assets, cash inflow from sales to represent sales, fixed assets over number of employees to represent productivity, cash flow over total assets, leverage, operating revenue over assets, intangible assets over equity to represent firms' asset tangibility, net profit, and an industry Herfindahl index in terms of sales to represent industry competition. Given that the IPO drastically changes firms' characteristics, only firms' characteristics in 2011 are used as control variables to ensure that the firms are comparable prior to the IPO. Table 1 lists and defines all of the variables.

Table 1-1

In choosing the control variables I refer to two types of literature. The first set of articles study the real impact of financial constraints (see, for example, Hadlock and Pierce (2010)). The second set of research of which Campello et al. (2014) is representative studies the Chinese stock market. Since Chinese accounting practices differ from those in the United States, the variables chosen measure the elements that interest scholars studying the real impact of financial constraints and reflect the most stable and accurate information available through Chinese accounting practices. I also control for the listing conditions that the CSRC requires.

3.3 Descriptive Statistics

In this subsection, I supply two panels of descriptive statistics. Panel A of Table 2 compares the treatment group and the control group for 2011, 2013, and 2015. Since neither group was listed in 2011, only the treatment group was listed in 2013, while both groups were listed in 2015, the unconditional comparison provides us with an overview of the consequences of the moratorium. Panel B of Table 2 demonstrates the descriptive statistics for the average firm in the sample for the entire period. To put these numbers into perspective, I compare them with the average listed firm in the Chinese stock market. I winsorize the data to address outlier issues (Barnett & Lewis, 1994) and display the winsorized results only. Appendix C presents the un-winsorized results. Most of the key results do not change.

Table 1-2

Panel A shows that the gap between the treatment and control groups is much smaller than that between the sample firms and the public ones. The gap is especially small in 2011, when both groups were at the IPO preparation or application stages. For example, in 2011, the average size of the control group is at 90% of that of the treatment group and the other variables are of similar

scales. A simple t-test fails to reject the hypothesis that the treatment group and the control group are no different. These differences demonstrate that, quantitatively, the treatment group and the control group were similar prior to the treatment. The increased difference between the two groups in 2015, after the treatment and after both groups of firms went public, suggests that a differential IPO treatment causes the two groups to diverge. It is worth noting that even in 2013, the difference between the groups, although large, are mostly insignificant. The primary reason is the large standard deviation, a pattern of difference emerges when I compare the growth, rather than level, in my empirical tests.

From Panel B, we learn that, when compared to public firms, firms in my sample are, on average, younger, smaller, have less revenue, hire less people, and makes less profit. The difference between public firms and the firms in my sample is large. For example, in 2011, the average size of the sample firms is only 27% of that of the public firms. However, the ratio grows to 36% in 2015 after the sample firms are all listed. Meanwhile, the standard deviations of the statistics are quite large. They are usually larger than the means reflecting the diversity of the firms.

3.4 Identification Strategy

Previous studies attempting to link IPOs and investment usually do not try to infer causality due to the endogenous nature of the firms' IPO decisions. For example, Pagano et al. (1998) use firms ex-ante characteristics to predict their IPO decision but find that the explanatory power of such decision-to-go-public models is too limited to use in a two-stage procedure that corrects the selection problem. The exception is Bernstein (2015). The author uses two months of NASDAQ return fluctuation that ranges between 3% and negative 6% during IPO applicant's book building phase as the instrument variable to mitigate the selection problem. Since short-run market fluctuation is more likely to affect marginal firms, the scope of the study is limited to firms that

are prone to market timing, which may be independent of innovation studied by Bernstein, but may not be independent of investment. In contrast, the current study furthers our understanding of IPO firms by investigating a local and precipitous shock that had a large impact on a broader group of firms. The firms in my sample were determined to conduct IPOs despite the declining stock market performance[6] and the uncertain future posed by the moratorium, and they have no freedom to choose their treatment status.

The 2012 Chinese IPO moratorium is an exogenous shock on the applicant firms' permission for listing. The moratorium independently bifurcated IPO permissions into two otherwise similar (qualitatively and quantitatively) groups of firms. One group successfully went public on schedule prior to the moratorium. The moratorium delayed the other group's IPOs for over a year. Since the objective of this paper is to study the treatment effect of IPOs, the groups that were listed prior to the moratorium are defined as the treatment group with the moratorium-affected firms defined as the control group.

As shown in Angrist and Krueger (1999), a difference-in-difference (DID) methodology is well suited to identify the effects of a sharp change, like the one in my setting. The DID approach controls for potential unobserved differences between the treatment group and the control group, while exploiting the sudden shock of the moratorium to firms' IPO processes.

I estimate two slightly different DID equations. These estimations differ in the definition of treatment time and sample period. Equation (1.3) uses 2012 as the treatment time:

$$InvestmentRate_{it} = \alpha + \beta After_t + wTreatment_i + \theta After_t Treatment + Z_{it}\delta + \varepsilon_{it} \quad (1.3)$$

[6] For example, in the middle of 2013, the Shanghai Stock Exchange Composite Index, the major stock index for Chinese markets, hit a historic low of 1849.65.

Here, $InvestmentRate_{it}$ is the variable of interest defined in equation (1.1); $Treatment_i$ is an indicator variable that equals one if firms belong to the treatment group and zero otherwise; $After_t$ is an event dummy that equals one at and after 2012 and zero before. Z_{it} is a vector of control variables. θ gives the DID estimate of the effect of an IPO on y_{it}. The sample period for equation (1.3) is 2011-2013. Equation (1.3) operates on a straightforward intuition. Namely, in 2011, both the treatment group and the control group apply to IPO, and these two groups are identical. In 2012, the treatment group receives the treatment of IPO, but the control group does not; the sorting of IPO permissions is independent. In 2013, the treated firms are listed, while the control group is waiting so the different investment behaviors between the two groups corresponds fully to the differing status of their IPO. The treatment here is IPO versus non-IPO, or more specifically, on-schedule with IPO versus one year delay in IPO process. However, for this specification to measure the causal effect of IPOs, there are two prerequisites: the treatment group and the control group need to be identical and the sorting of treatment status needs to be independent. I provide evidence to argue that these two prerequisites are likely to hold through institutional details and through the comparison of the treatment group and the control group in Table 2a, and further discuss it in the next subsections.

Equation (1.4) uses event time – the year of the IPO – as the treatment time:

$$InvestmentRate_{it} = \alpha + \beta After_{it} + wTreatment_i + \theta After_{it}Treatment_i + Z_{it}\delta + \varepsilon_{it} \quad (1.4)$$

Here, y_{it} is the variable of interest *InvestmentRate*; $Treatment_i$ is an indicator variable that equals one if firms belong to the treatment group and zero otherwise; $After_{it}$ is an event dummy that equals one for the year of and the years after firm *i* goes public and zero before. Z_{it} is a vector of control variables. θ gives the DID estimate of the effect of an IPO on y_{it}. The sample period is

2009-2015, with the treatment group's coverage ranging from 2009 to 2015 and the control group's coverage ranging from 2011 to 2015. This unbalanced panel and the individual treatment effect equation estimates the treatment effect on a firm's investment of being able to conduct an IPO on schedule versus having to wait at least a one-year in its IPO process. The difference between the treatment group and the control group is the differential sorting into receiving the IPO moratorium. Yet, eventually, all firms in the sample are listed. Thus, the treatment is not IPO versus non-IPO. Rather, the treatment involves whether the IPO was on schedule or delayed. The shortcoming of equation (1.3) is that it cannot disentangle the differential effect of an IPO over time apart from time effect. As d_{it} coincides with year, including year fixed effects can absorb the treatment effect. I include this specification as a robustness test.

Following Bertrand et al. (2004) and Petersen (2009), I estimate robust standard errors clustered by each stock ticker. Four empirical specifications are used in estimating each equation. The first specification includes only T_i, d_{it} and $d_{it}T_i$. It serves as the benchmark. The second specification includes the control variables. It serves as the main specification because the θ coefficient represents the treatment effect on two firms from two different groups that are similar in terms of the control variables. I report the economic significance based on the results of this specification. The third specification includes firm fixed effects but excludes the control variables and T_i as they are absorbed by firm fixed effects. This specification serves as a robustness test to check whether the effect can be attributed to firm-specific effects. The fourth specification includes both firm fixed effects and year fixed effects. Including year fixed effects can absorb the treatment effect of equation (1.3) but should not affect the treatment effect of equation (1.4) as the sorting of treatment status happens to the treatment group and the control group at the same time.

The null hypothesis is $\theta = 0$. If the effect of financial constraints prevails over the effect of agency problem, then firms should increase their investment on fixed assets; and we expect $\theta > 0$. Otherwise, if the effect of agency problems is more important then we should observe $\theta < 0$.

4. Empirical Results

4.1 The Effect of IPO on Investment

Figure 2 previews the results intuitively by plotting the unconditional mean of *InvestmentRate* over 2011-2015, by the treatment group and the control group. Focusing on year 2011-2013, we observe that in 2011, the three groups all have the same *InvestmentRate* at one. This is a mechanical result because *InvestmentRate* is constructed as a firm's investment on fixed assets over its 2011 level. In 2012, when the treatment group went public, that group had a slightly higher *InvestmentRate*. However, in 2013, the *InvestmentRate* by the treatment group diverged significantly from that of the control group. The treatment group, which benefited from IPOs, increased investment drastically while the control group, which was affected by the moratorium, remain relatively less invested. This difference marks the results of equation (3). When we look at 2014 and 2015, we observe that the treatment of an IPO contributes to an increase of investment relative to the control group in both cases, marking the results of equation (4).

Figure 1-2

Tables 3a and 3b report the DID results of equation (3) and equation (4) respectively. Using *InvestmentRate* as the dependent variable, I find that an IPO results in a statistically significant increase in the firms' investment behavior in subsequent years. The results are robust to the inclusion of firm controls as well as firm fixed effects. The result is robust to the inclusion of year fixed effects in equation (3), but, in equation (4), year fixed effects absorb the treatment effect as

discussed earlier. Similarly, since equation (3) features a balanced panel, firms either belong to the treatment group or the control group, and there is no switching in between them, equation (3) does not include firm fixed effects. Equation (4) is robust to the inclusion of firm fixed effects. Finally, Table 3c reports the DID result of the specification in equation (3) but the variable of *InvestmentOverAssets* defined in equation (2).

Table 1-3

The results are economically meaningful. Interpreting the coefficients from the specification with control variables, Table 3a suggests that in 2012 and in 2013, a firm that could conduct an IPO on schedule invested more in fixed assets, intangible assets, and other long-term assets. The increased amount is equal to 36.4% of its 2011 level compared to a similar frim that was affected by the moratorium and had to wait. Or, simply put, that the causal effect of IPO on firms' investment on fixed assets is 36.4%.

Similarly, Table 3b suggests that an average firm that is able to conduct an IPO on schedule, compared to receiving a one-year delay in the IPO process, will invest more in fixed assets, intangible assets, and other long-term assets, and the increased amount is equal to 34.1% of its 2011 level. The significant coefficient in Table 3c suggests that the result is not driven by variation in a firm's 2011 investment but by the firm's change of investment from 2011 to 2013. Again, the conclusion of this paper is drawn based on the results of Table 3a. Table 3b and Table 3c only serve the purpose of robustness checks.

4.2 Validity of the Experiment

For the experiment to be valid, the treatment need to be exogenous. In Section 2, I introduced institutional features, especially the merit based regulation system, to qualitatively

argue that the treatment is exogenous. In this subsection, I provide quantitative evidence for the validity of the experiment – that the determinants of Y(0) are independent of the treatment and those of the treatment are independent of Y(0).

The determinants of Y(0) are mostly firm characteristics. To prove that the determinants of Y(0) are independent of the treatment, I demonstrate that the treatment group and the control group are similar firms before the moratorium. In Table 2, the summary statistics, I have already shown that the treatment group and the control group are similar in their means. Figure 3 visually demonstrates that these two groups are similar in terms of industry composition and distribution.

Figure 1-3

The determinants of the treatment are mostly firms' time of application. To prove that the determinants of the treatment are independent of Y(0), I demonstrate that the time of application does not drive my result. In other words, I demonstrate the validity of my "RDD". Drawing an analog of the treatment group and the control group with the firms listed in 2014 and the firms listed in 2015, I conduct a placebo test. If the pre-IPO trend is dependent on the time of the IPO application, then we should observe different pre-IPO trends for the 2014-listed firms and the 2015-listed firms in 2011, 2012 and 2013 prior to their IPO, as these two groups applied to have an IPO at different times. Table 4 conducts a placebo test using the 2014-listed firms as the placebo treatment group and the 2015-listed firms as the control group. If applying to have an IPO at different times results in different post-IPO investment behaviors, then we should observe significant results from this estimation. I find no significant difference in the *InvestmentRate* between the 2014-listed firms and the 2015-listed firms. The result strengthens the validity of equation (2) by showing that the timing of an IPO application does not significantly impact post-

IPO investment behavior. The sorting of IPO treatment that arises as a result of the moratorium is independent of the outcome variable.

Table 1-4

4.3 Validity of the Coefficient

After establishing the validity of the experiment, I also need to establish the validity of the coefficient. That is, does the coefficient measure what I claim to measure – the causal effect of IPO – rather than the causal effect of something else?

There are two alternative hypotheses: First, public firms and private firms face different investment opportunities, and the result may reflect the difference in investment opportunities. Second, the control group firms are affected by the moratorium. They reduce their investment as a response, and the result may reflect the negative effect of the moratorium rather than the positive effect of IPOs.

To test my hypothesis against these two alternatives, I compare the investment of the treatment group with that of an alternative control group – firms listed prior to2012. Specifically, I use a propensity score matching method to find public firms that are similar to the treatment group and compare their InvestmentRate with that of the treatment group in 2013. I find that the increased *InvestmentRate* of the treatment group is 53.4% more than that of the matched public firms. I also conduct a diff-in-diff estimation as in equation (2), using public firms as the alternative control group. Results are reported in Table 5. The coefficients of the difference-in-difference estimation are like that of the matching estimation. For example, the coefficient estimated with control variables is 51.6%. Since the treatment group's fixed investment also increased relative to that of the public firms, the two alternative hypotheses are rejected. Stated differently, this

significant result confirms that the increase in investment is due to the change brought by IPOs rather than to any public versus private difference or the negative effect of the moratorium. This makes the interpretation of the results in Table 3 more robust to being the causal effect of IPO.

Table 1-5

4.4 Theoretical Implications

After establishing that the result represents the causal effect of IPO on firms' investment, I further test the implication of this result for corporate finance theories. The theoretical tension is that an IPO changes financial constraint, which leads to an increase in investment; an IPO changes agency problems, especially managerial short-termism, which lead to a decrease in investment. Since the causal effect of IPO on investment is positive, it seems that the financial constraints effect prevails over the managerial short-termism effect and that managers choose long-term investment over short-term earnings. However, there is one additional step necessary before endorsing such a conclusion. That is, we need make sure that both effects are binding to firms. The next section evaluates these two competing channels.

4.4.1 Financial Constraints

To test whether an IPO lessens financial constraints, I examine the consequences of an IPO that are associated with lessening financial constraints. In Figure 4, going public significantly increased the treated firms' cash holdings. The mechanical result of an IPO is consistent with the financial constraints literature and indicates that IPOs reduce firms' financial constraints.

Figure 1-4

Are financial constraints binding for IPO applicants? To test this question, I examine the liabilities ratio of IPO applicants. An IPO not only raises a large amount of capital for the issuer

but can also increase the issuer's access to debt financing. According to the pecking order theory, firms prefer to use internal capital rather than debt to fund new projects. Thus, after raising significant cash through an IPO, the issuers would increase liabilities only when their financial constraints are binding. Table 6 finds that IPOs increase firms' liabilities significantly. The fact that post-IPO firms actively seek new funding in addition to the capital raised through IPOs is strong evidence that they were financially constrained at the time of their IPOs and that an IPO lessened their financial constraints.

Table 1-6

4.4.2 Agency Problems and Post-IPO Earning Underperformance

Having established that IPOs reduced firms' previously binding financial constraints, I next test whether increasing investment led firms to suffer the consequences predicted by the agency theory. The agency literature demonstrates that public firms under-invest due to managerial short-termism and their focus on earnings. The argument advanced is that although some investment may have positive net present value, firms may take a few years to realize this benefit and at the same time increase depreciation, hurting earnings. Due to agency problems, managers may forgo such investment opportunities to boost short-term earnings. Using my unique setting, I verify, in Figure 5, that an IPO does cause net profit to decrease while increasing investment. My results contribute to the agency literature by establishing a more exogenous link between investment and net profit. Moreover, my results demonstrate that firms do suffer a decrease in net profit as agency theory suggests. However, firms made a choice to increase investment despite the short-term negative effects, suggesting that the financial constraints effect plays a more important role in determining the investment behaviors of post-IPO firms.

Figure 1-5

In addition to confirming that agency problems effects are binding, this result also disentangles the two competing hypotheses for the wildly-debated empirical finding: post-IPO earning underperformance. Most papers, including mine, find that after an IPO, firms are documented to have a worse operating performance, sometimes measured by profit over assets, than their pre-IPO levels. Controversy exists in the literature as to whether firms conduct an IPO to realize potential productivity or do so after their productivity peaks to take advantage of investors' asymmetric information and to monetarize higher firm value. In other words, the association between an IPO and earning decrease is either a "signal" or a "causation". The lifecycle theory of firms supports the "signal" argument, which generally sees the motivation for conducting an IPO as the result of firms' reaching a certain stage in their lifecycles, usually around their productivity peaks. The short-term pressure view supports the "causation" argument, which generally sees increased investment after an IPO coming at the expense of decreased earnings. The current study is able to disentangle these two alternatives by comparing post-IPO firms with similar moratorium-affected firms. If the post-IPO operating underperformance is due to firms reaching their productivity peak prior to going IPO, we should expect the moratorium-affected firms to exhibit similar behaviors as the post-IPO firms, as they are at a similar stage. However, the operating underperformance, measured by net profit, only occurs for the post-IPO firms. This result suggests that an IPO serves as the cause of operating underperformance through increased investment. This evidence is consistent with firms conducting an IPO to raise funds to increase productivity rather than monetizing high firm valuations after their productivity peaks.

4.5 Limitations

4.5.1 Selection Problem:

Selection problem is a potential bias for my experiment. Specifically, moratorium-affected firms could choose to drop out of the pipeline and, in fact, many of them did. When the 2012 IPO moratorium was initiated, there were over 600 firms in the queue. The control group in my sample, the firms that eventually went public in the A shares market, contains only 332 firms. The companies that needed the money the most might not have waited and may have modified their plans instead. For example, they could choose to go public in foreign exchanges,[7] to buy shell companies, or to be acquired by other firms.[8] Further, the firms that received severe negative real shocks in their product markets would have to withdraw their IPO applications because they were no longer qualified to list. Although staying in the IPO pipeline is a choice, it biases against my result. In particular, since my study aims to test the causal effect of an IPO and the firms who dropped out of the pipeline – whether they found alternative financing or were disqualified – are the firms that needed to IPO the most, a stronger effect should exist among them. The bottom line is that, since the selection problem resulted in a more selective control group, excluding the drop-out firms from the sample implies that my findings are understated.

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[7] If a company wants to go public oversees, CSRC used to require it have at least 400 million in net assets and at least 60 million in profit the previous year, but in 12.20.2012 this requirement was dropped: http://finance.caixin.com/2012-12-20/100474975.html.

[8] See, for example, Templin (2011), about Chinese reverse mergers.

4.5.2 China Specific:

My study uses a unique quasi-experimental event that happened in the Chinese stock market. This may raise the question of the experiment's external validity. That is, how much does the result have broader implications?

The neat setting of the event and the tests for the two frictions – financial constraints and managerial short-termism – enable this paper to go beyond a Chinese-specific setting. The result represents a scenario, the Chinese stock market, where a firm faces high investment opportunities, high financial constraints, and high agency problem concerns. The statistically and economically significant results can provide better understanding of managers' choices in scenarios where there are potential investment opportunities but financial constraints are also binding. These can occur beyond the Chinese setting, in emerging markets, in specific industries – especially high-tech ones – and in certain firms, especially new ones, in the United States(Robb & Robinson, 2012). Meanwhile, since the Chinese IPO market has become the largest in the world, learning its IPO dynamic is also worthwhile.

5. Conclusions and Policy Discussions

The 2012 Chinese IPO moratorium was a precipitous policy shock with an unprecedented impact. It exogenously sorted IPO permissions to 486 otherwise similar IPO applicant firms, allowing 154 of them to conduct their IPO on schedule and delaying the IPO process for the remaining 332 firms for over a year. This paper uses this episode to study and quantify the causal effect of IPO on firms' investment behavior, shedding a unique light on the competing influence of financial constraints and agency problems associated with going public.

By employing a difference-in-difference approach, I find that an IPO has a statistically significant and economically meaningful effect on firms' investment behavior: For a typical Chinese IPO applicant firm in 2012, IPO causes its investment on fixed assets to increase by 36.4%. This is consistent with the notion that the effect of mitigating financial constraints outweigh the effect of agency problems in determining the firms' post-IPO investment behavior.

I also provide suggestive evidence to the horserace between the two proposed channels, financial constraints and agency problems. I demonstrate that financial constraints were binding for pre-IPO firms; going public lessened the firms' financial constraints. Meanwhile, an IPO causes net profit to decrease, which suggests that managers' concern of the negative earning impact arising as the result of increasing investment is valid. In choosing to increase investment, firms reveal that the financial constraints effect outweighs the agency problems effect in determining their post-IPO investment behaviors. Lastly, I also find evidence that supports the notion that an IPO causes firms' operating underperformance through increased investment, while rejecting that an IPO serves as a signal of a productivity peak. This finding provides concrete empirical evidence to support agency problems as the reason for the wildly-debated post-IPO earning underperformance phenomenon, while rejecting the lifecycle theory explanation.

My study also generates policy implications in addition to empirical contributions. First, firms in my sample are large, mature and profitable. Many previous studies on financial constraints assume that such firms are immune to financial constraints. Using a local shock, I find that these firms are also subject to financial constraints since an IPO significantly increased their subsequent investment. Consequently, that access to equity markets has a huge potential to improve the firms' investment and productivity and has a profound impact on the real economy. My finding suggests that regulators should consider reducing the barriers to public listing in a more active manner.

To the best of my knowledge, my study is the first to quantify the real impact of the 2012 Chinese IPO moratorium. A large IPO treatment effect exists with respect to the treated firms. The IPO moratorium exerted a pronounce opportunity cost on the control group firms. Although the welfare of the IPO applicant firms is only a secondary consideration of the CSRC, the CSRC should be cognizant of the impact of its administrative measures. My study provides a reference to help them make a more informed policy decision.

Figures

Figure 1-1: Sample Firms' Application Date and Listing Date

Figure 1 depicts the application dates and listing dates for the treatment group and the control group. The treatment group contains firms that were listed in 2012 before the moratorium. The control group contains firms that applied to IPO before the moratorium and went public in 2014 and 2015. X axis is *Review Date*, which is the first date that the firm appears on the Public Offering Review Committee's disclosed list. *Listing Date* is the date that the firm went public. Red triangles are control firms and grey triangles are treated firms. Hollow triangles are uncertain dates. The area between the red lines represents the moratorium.

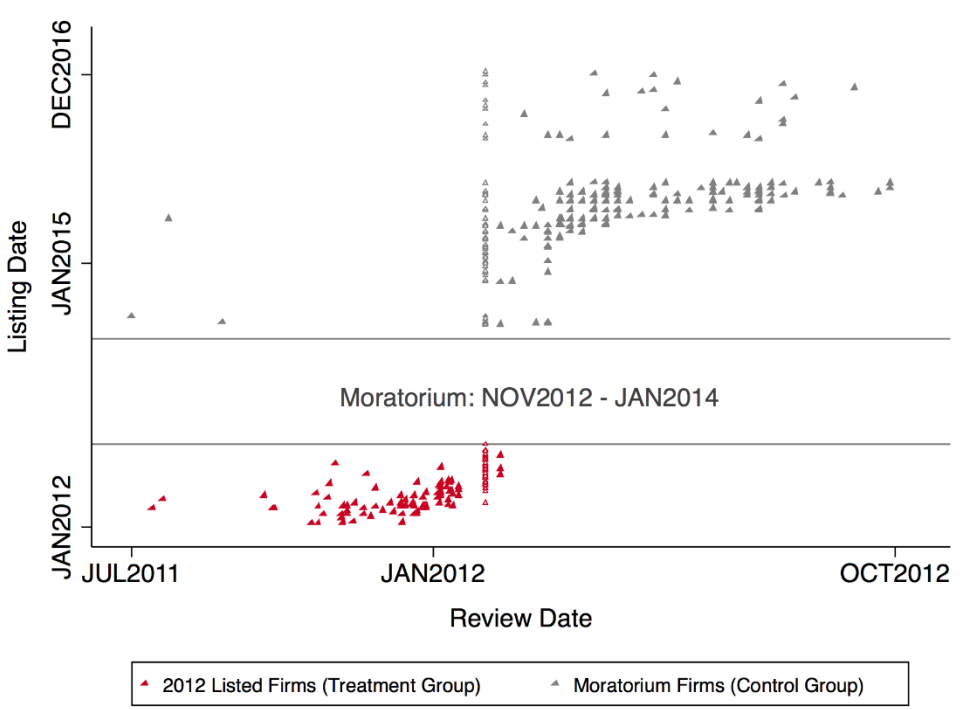


Figure 1-2: InvestmentRate around IPO

Figure 2 plots the average *InvestmentRate* along with 95% confidence intervals for the treatment group firms, and for the control group firms.

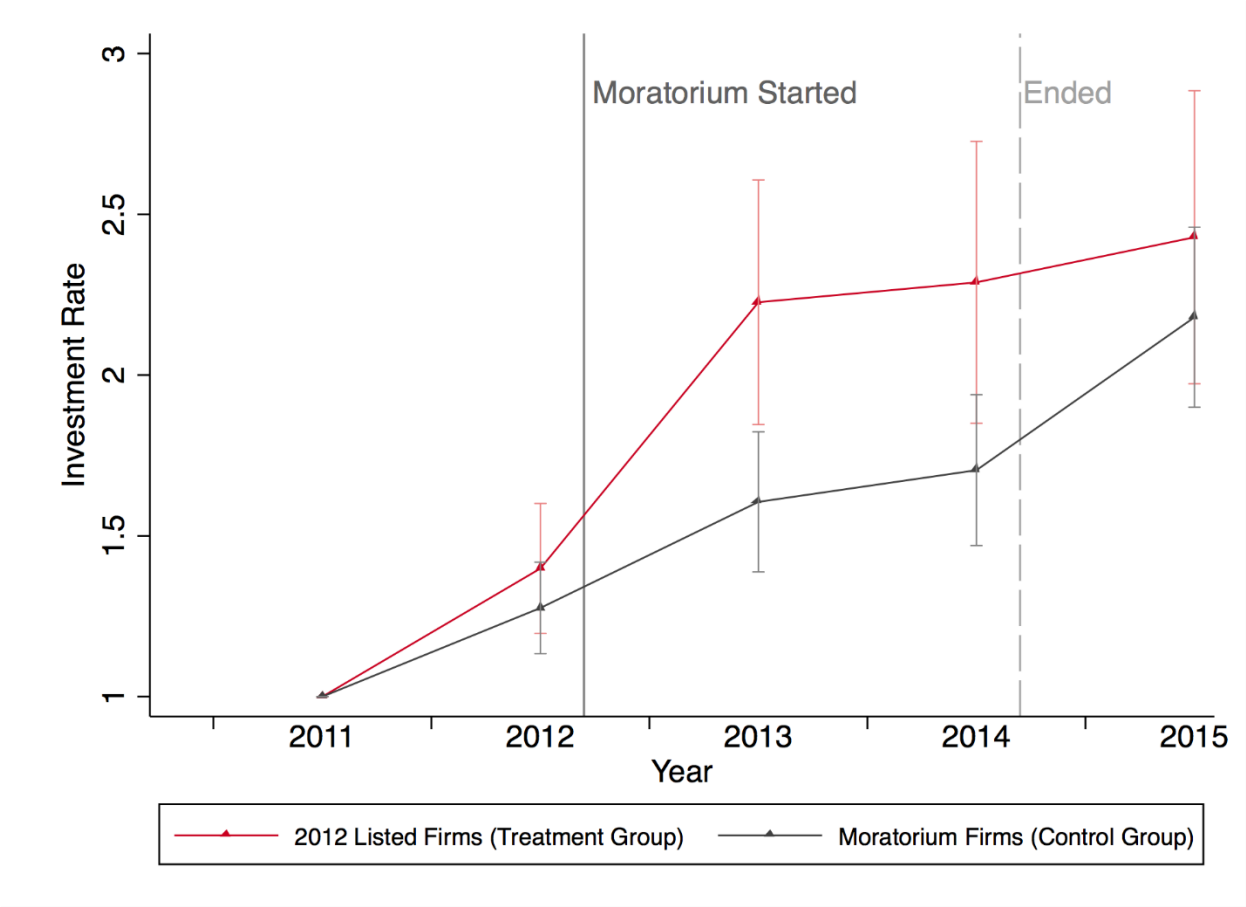


Figure 1-3: Treatment group and control group comparison

Figure 1-3a

Industry Composition

Figure 1-3a plots the industry composition for the treatment group firms, and for the control group firms.

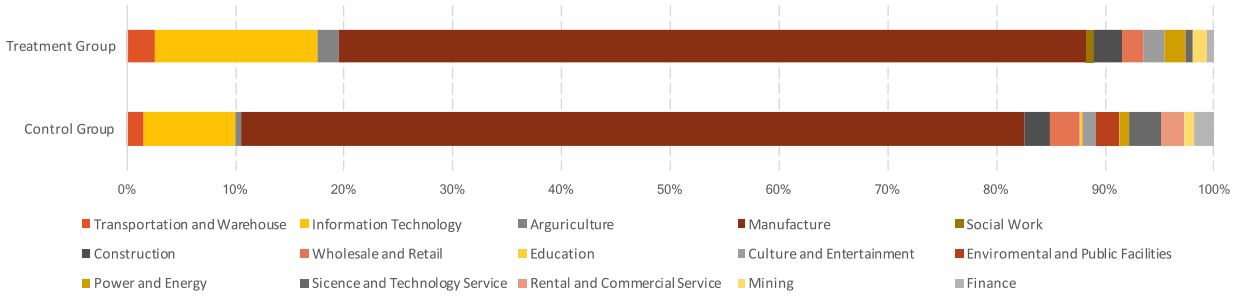


Figure 1-3b

Pre-IPO Leverage Distribution

Figure 1-3b plots the histogram of leverage at 2011 for the treatment group and for the control group.

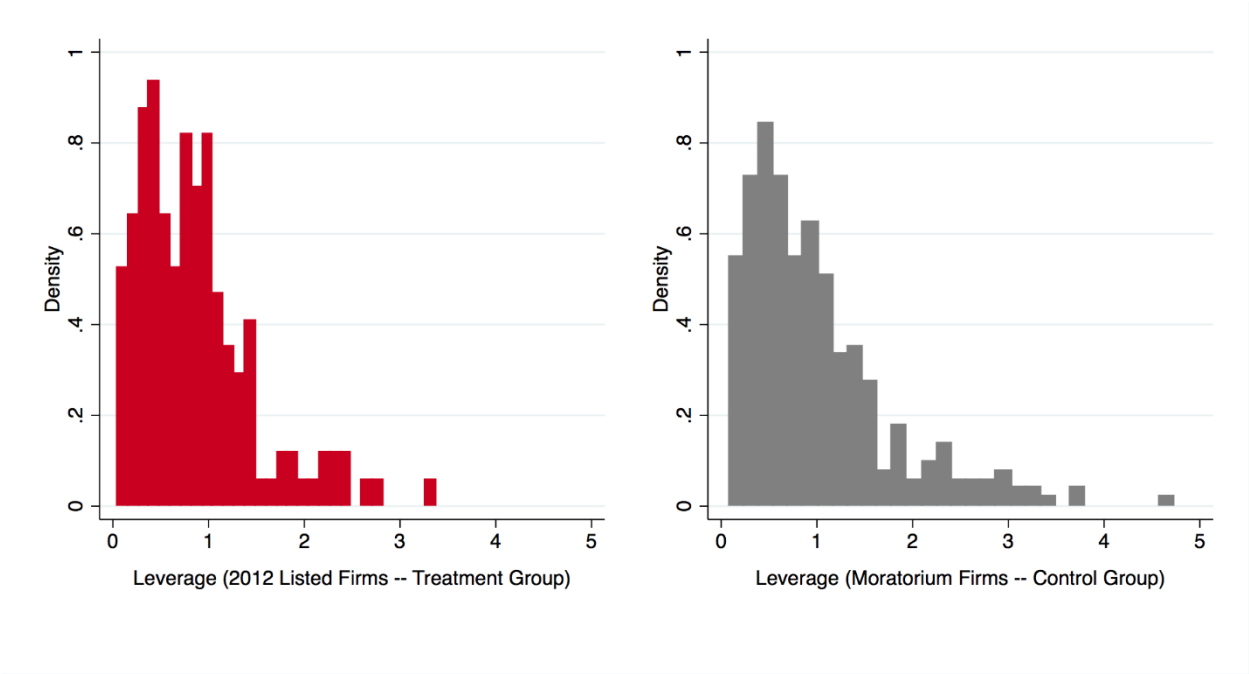


Figure 1-4: CashOverAssets around IPO

Figure 1-4 plots the average *CashOverAssets* along with 95% confidence intervals for the treatment group firms, and for the control group firms

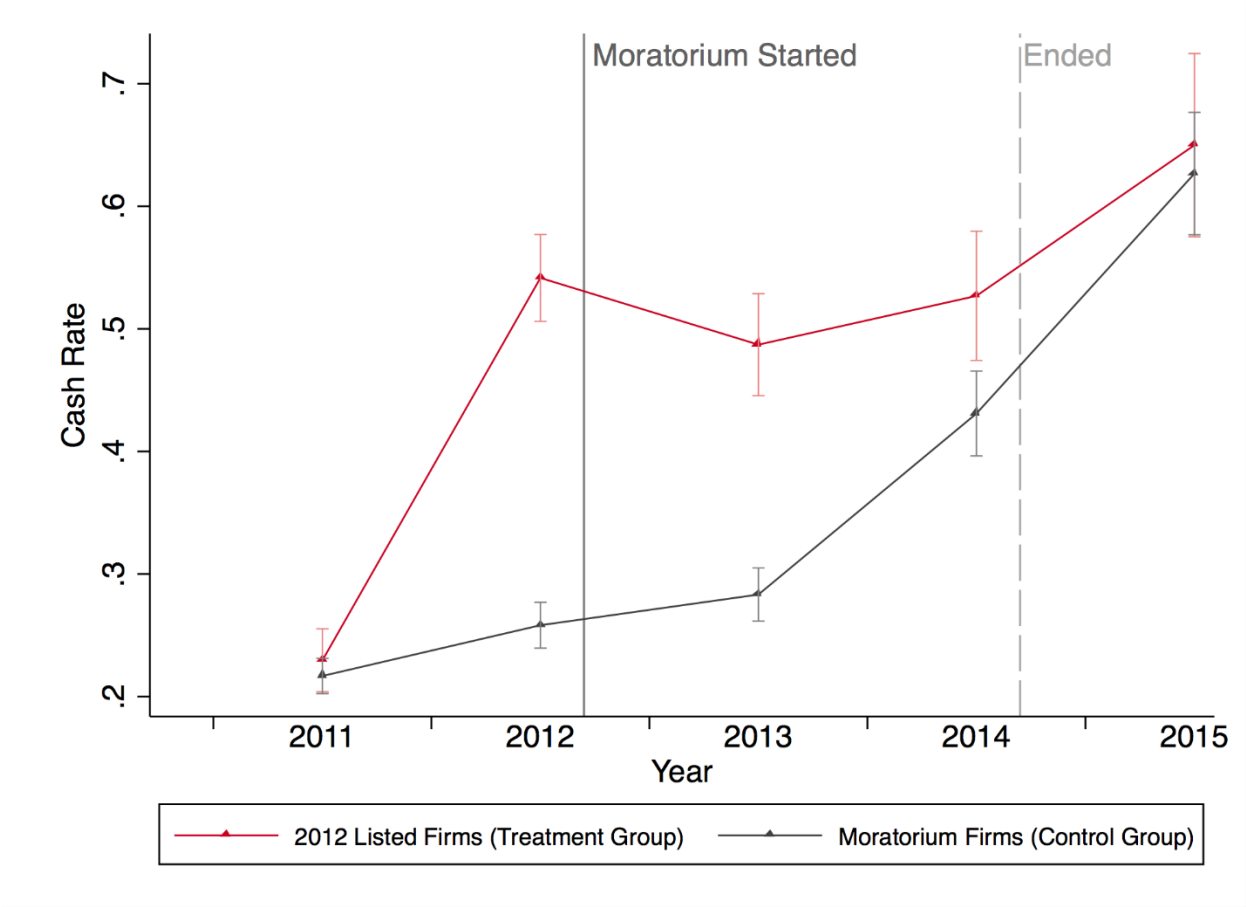


Figure 1-5: Profit around IPO

Figure 1-5a

NetProfitRate around IPO

Figure 1-5a plots the average *NetProfitRate* along with 95% confidence intervals for the treatment group firms, and for the control group firms.

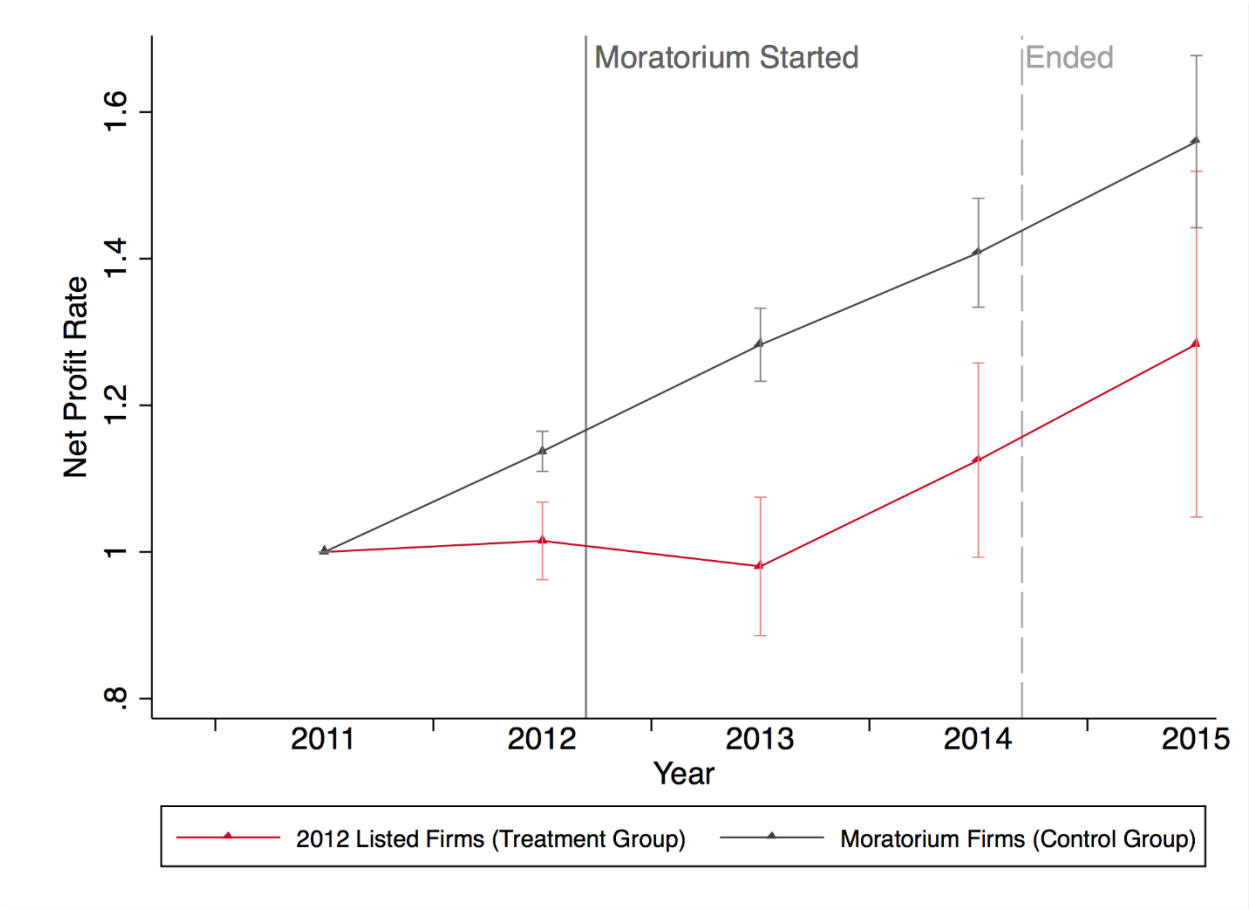
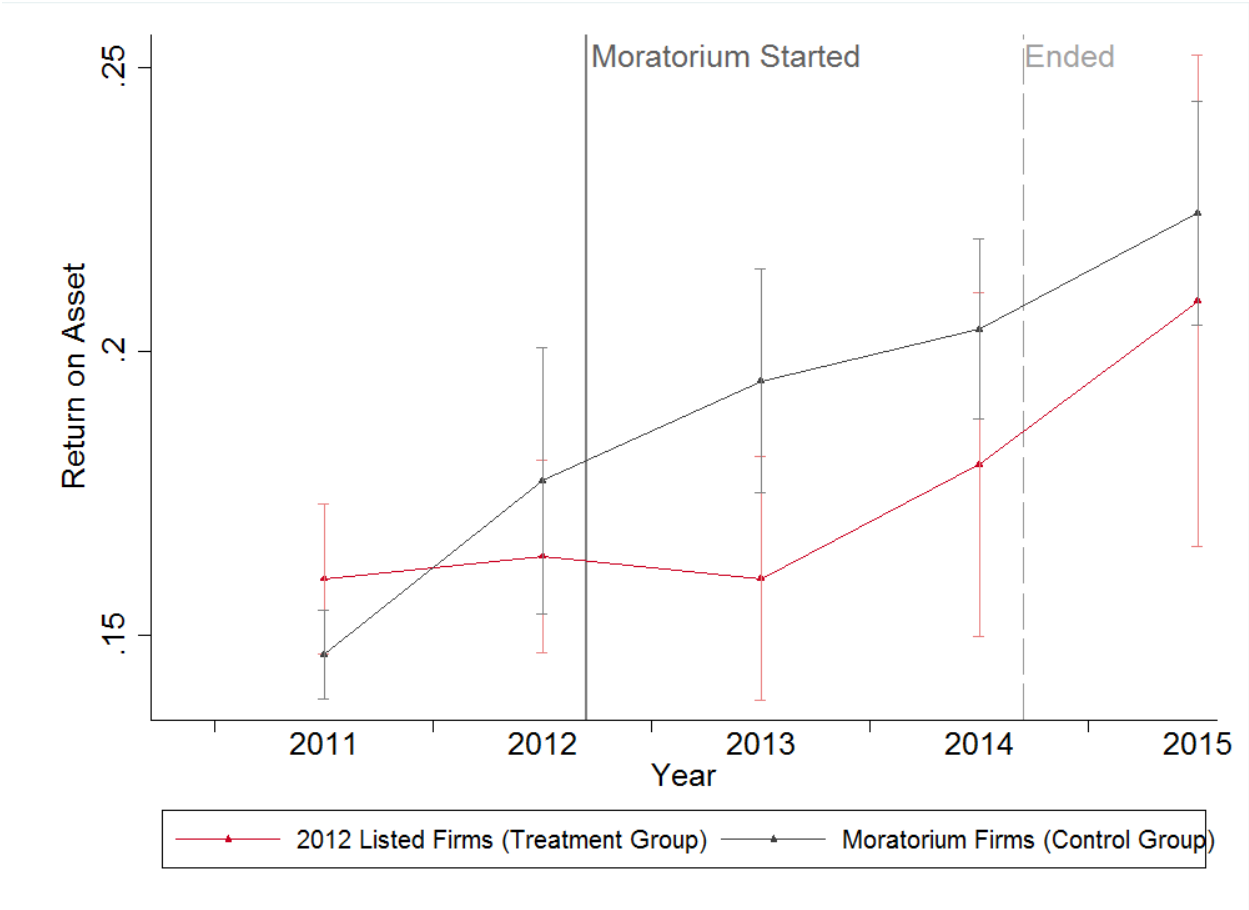


Figure 1-5b

Return on Assets around IPO

Figure 1-5b plots the average *ROA* along with 95% confidence intervals for the treatment group firms, and for the control group firms.



Tables

Table 1-1: List of Variables and Definition

Variable	Description
Variables of Interest	
FixedInvestment	*Cash outflow for fixed, intangible and other long term assets - cash inflow for fixed, intangible and other long term assets*
InvestmentRate	*FixedInvestment* / $FixedInvestment_{2011}$
Fixed Investment over Total Assets	*FixedInvestment* / $Total\ Assets_{2011}$
CashOverAssets	*Ending balance of cash* / $Total\ Assets_{2011}$
LiabilitiesRate	*Liabilities* / $Total\ Assets_{2011}$
NetProfitRate	*Net profit* / $Net\ Profit_{2011}$
Return on Assets	*Net profit* / $Total\ Assets_{2011}$
Control Variables	
Age	Firm's age in years
Total Assets	Total assets
Sales	Total cash inflow from sales
Productivity	Fixed assets over number of employees
Cash Flow	Cash flow from operations over total assets
Leverage	Total liability over total assets
Revenue	Operating revenue over total assets
IA/E	Intangible assets over equity
Industry HHI	Industry Herfindale index by assets
Net Profit	Net profit

Table 1-2: Descriptive Statistics

The table lists descriptive information for all A-share stocks from 2011 to 2015. Panel A compares the treatment group, firms listed in 2012 before the moratorium, and the control group, moratorium-affected firms that are eventually listed, in 2011 and 2013, the year before the moratorium and the year after. The table also provides t-score of the hypothesis that *Treatment – Control = 0*. Panel B provides the descriptive statistics of the sample firms, which include both the treatment group and the control group, and that of the public firms. Variables are in million RMB with the exception of Staff Headcount and Age. Standard deviations are in parentheses.

Table 1-2a

	2011				2013			
	Treatment Group	Control Group	Treatment -Control	T score	Treatment Group	Control Group	Treatment -Control	T score
Total Assets	1599.12	1462.32	136.8	-0.34	2605.26	2076.05	529.2	0.97
	(4076.8)	(4130.3)	(401.0)		(5460.3)	(5657.7)	(545.6)	
Fixed Assets	326.86	283.07	43.78	0.56	492.32	416.72	75.60	0.69
	(828.8)	(786.4)	(78.00)		(1194.4)	(1095.8)	(110.0)	
Long Term Investment	268.75	159.54	109.2	0.96	340.44	140.98	199.5	1.45
	(1473.4)	(634.9)	(113.6)		(2128.5)	(466.7)	(137.3)	
Liabilities	807.76	828.39	-20.64	-0.08	1033.72	1201.45	-167.7	-0.47
	(2252.4)	(2625.5)	(245.1)		(3240.5)	(3826.2)	(356.0)	
Net Profit	152.79	121.48	31.32	1.51	139.98	146.14	-6.159	-0.28
	(230.0)	(203.3)	(20.68)		(215.9)	(228.9)	(21.92)	
Cash End	306.10	247.66	58.44	0.86	542.72	299.85	242.9**	3.19
	(730.3)	(679.9)	(67.91)		(856.2)	(743.1)	(76.11)	
Age	10.87	10.26	0.608	1.25	12.87	12.26	0.608	1.25
	(4.376)	(5.251)	(0.487)		(4.376)	(5.251)	(0.487)	
Observations	154	332	486		154	332	486	

Table 1-2b

	All A Shares 2011-2015	Public Firms 2011	Sample Firms 2011	Public Firms 2015	Sample Firms 2015
Total Assets	6934.93	5652.70	1505.67	10508.47	3742.36
	(11657.6)	(8369.7)	(4109.7)	(16055.9)	(8836.1)
Fixed Assets	1396.18	1165.42	296.94	1991.63	619.76
	(2561.2)	(1962.8)	(799.4)	(3298.2)	(1404.9)
Liabilities	3994.97	3170.04	821.85	6153.92	1843.26
	(7774.2)	(5374.1)	(2511.0)	(10890.6)	(5847.7)
Total Revenue	3888.95	3648.55	1131.77	5101.72	1805.15
	(6445.9)	(5810.9)	(2195.4)	(7721.8)	(3561.9)
Total Operating Cost	3601.01	3345.57	979.57	4763.14	1551.00
	(5964.5)	(5372.9)	(1987.6)	(7104.5)	(2942.2)
Cash In Sales	3518.84	3278.01	1049.57	4583.96	1591.34
	(5638.4)	(4973.3)	(1926.6)	(6664.2)	(2836.7)
Net Profit	242.12	248.91	131.40	300.20	173.04
	(417.1)	(387.2)	(212.4)	(556.9)	(323.5)
Cash Out Fixed Assets	295.03	336.71	113.87	328.18	161.71
	(492.9)	(549.8)	(252.0)	(519.9)	(298.4)
Cash Out Financing	1689.34	1147.67	306.47	2881.76	813.73
	(3265.0)	(1808.8)	(751.3)	(5087.8)	(2459.4)
Cash Flow	69.54	72.38	38.76	218.50	116.62
	(389.6)	(378.3)	(166.3)	(579.4)	(373.5)
Cash	890.23	871.11	266.22	1265.87	595.99
	(1412.0)	(1198.1)	(696.1)	(1947.5)	(1188.8)
Staff Headcount	3488.64	3252.71	1483.96	4295.80	2177.30
	(4633.7)	(4131.2)	(2343.6)	(5297.3)	(3163.6)
Age	15.63	14.28	10.45	18.28	14.45
	(5.422)	(5.047)	(4.994)	(5.047)	(4.994)
Observations	14490	2412	486	2412	486

Table 1-3: Difference-in-differences (DID) Regressions

Table 1-3a

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *after* is an indicator variable that equals one if the observation is after 2012. *treatment* is an indicator variable that is one if the observation is part of the treatment group, i.e. firms that went public before the moratorium. *treatment* is equal to zero if the firm is in the control group, i.e., IPO applicant firms that applied to IPO before the moratorium whose IPO processes were delayed by the moratorium. *after*treatment* is the DID estimate. The variable of interest is *InvestmentRate*. All variables are defined in Table 1. The sample period covers 2011-2013. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	InvestmentRate		
VARIABLES	OLS Base Model	OLS with Controls	Time FE with Controls
after*treatment	0.374**	0.364**	0.366**
	(0.155)	(0.158)	(0.156)
after	0.446***	0.446***	
	(0.0833)	(0.0867)	
treatment	-0	0.0314	
	(3.13e-09)	(0.0429)	
Constant	1***	0.860***	1.008***
	(5.43e-09)	(0.204)	-0.0458
Observations	948	861	861
R-squared	0.065	0.133	0.159
Controls	No	Yes	Yes
Firm FE	No	No	No
Year FE	No	No	Yes

Table 1-3b

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *after* is an indicator variable that equals one if the firm is listed in that year. *treatment* is an indicator variable that is one if the observation is part of the treatment group, i.e. firms that went public before the moratorium. *treatment* is equal to zero if the firm is in the control group, i.e., IPO applicant firms that applied to IPO before the moratorium whose IPO processes were delayed by the moratorium. *after*treatment* is the DID estimate. The variable of interest is *InvestmentRate*. All variables are defined in Table 1. The sample period covers 2009-2015. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	InvestmentRate		
VARIABLES	OLS Base Model	OLS with Controls	Time FE with Controls
after*treatment	0.401**	0.341*	0.354**
	(0.189)	(0.191)	(0.179)
after	0.734***	0.754***	
	(0.126)	(0.131)	
treatment	-0.394***	-0.345***	
	(0.0771)	(0.0834)	
Constant	1.356***	1.109***	0.788***
	(0.0653)	(0.286)	(0.110)
Observations	1,733	1,581	1,581
R-squared	0.073	0.121	0.175
Controls	No	Yes	Yes
Firm FE	No	No	No
Year FE	No	No	Yes

Table 1-3c

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *after* is an indicator variable that equals one if the observation is after 2012. *treatment* is an indicator variable that is one if the observation is part of the treatment group, i.e. firms that went public before the moratorium. *treatment* is equal to zero if the firm is in the control group, i.e., IPO applicant firms that applied to IPO before the moratorium whose IPO processes were delayed by the moratorium. *after*treatment*, and is the DID estimate. The variable of interest is *Fixed Investment over Total Assets*. All variables are defined in Table 1. The sample period covers 2011-2013. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	Fixed Investment Over Total Assets		
VARIABLES	OLS Base Model	OLS with Controls	Time FE with Controls
after*treatment	0.0480***	0.0500***	0.0417***
	(0.0115)	(0.0118)	(0.0123)
after	0.00363	0.00406	
	(0.00584)	(0.00626)	
treatment	-0.00935	-0.0116	
	(0.00964)	(0.00923)	
Constant	0.106***	0.163***	0.105***
	(0.00534)	(0.0149)	(0.00386)
Observations	1,102	1,004	1,004
R-squared	0.027	0.091	0.065
Controls	No	Yes	Yes
Firm FE	No	No	No
Year FE	No	No	Yes

Table 1-4: Placebo Test Using 2014-listed Firms as the Treatment Group

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *time* is an indicator variable that equals one if the observation is after 2014. *treatment* is an indicator variable that is one if the observation is listed during 2014. *treatment* is equal to zero if the firm is listed during 2015. *DID* is equal to *time*treatment*, and is the DID estimate. The variable of interest is *InvestmentRate*. All variables are defined in Table 1. The sample period covers 2009-2015. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	InvestmentRate			
VARIABLES	OLS Base Model	OLS with Controls	Firm FE	Firm & Year FE
DID	0.180	0.201	0.100	0.205
	(0.236)	(0.246)	(0.221)	(0.261)
time	0.687***	0.666***	0.782***	0.211
	(0.172)	(0.177)	(0.156)	(0.218)
treatment	-0.210*	-0.0539		
	(0.117)	(0.123)		
Constant	1.419***	0.651*	1.328***	1.001***
	(0.0870)	(0.354)	(0.0291)	(0.0705)
Observations	1,105	972	1,105	1,105
R-squared	0.046	0.118	0.099	0.144
Control Variables	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 1-5: Compare Public Firms with the Control Group

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *time* is an indicator variable that equals one if the observation is after 2012. *treatment* is an indicator variable that is one if the observation is part of the treatment group, i.e. firms that went public before the moratorium. *treatment* is equal to zero if the firm is in the control group, i.e. public companies listed before 2012. *DID* is equal to *time*treatment*, and is the DID estimate. The variable of interest is *InvestmentRate*. All variables are defined in Table 1. The sample period covers 2011-2013. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	InvestmentRate			
VARIABLES	OLS Base Model	OLS with Controls	Firm FE	Firm & Year FE
DID	0.520***	0.516***	0.516***	0.514***
	(0.133)	(0.134)	(0.134)	(0.134)
time	0.300***	0.291***	0.305***	
	(0.0256)	(0.0260)	(0.0259)	
treatment	0	-0.0502**		
	(5.85e-09)	(0.0219)		
Constant	1***	0.952***	0.997***	0.997***
	(1.13e-09)	(0.0542)	(0.0165)	(0.0165)
Observations	6,243	5,975	6,243	6,243
R-squared	0.027	0.046	0.045	0.058
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 1-6: DID Regressions on Firms' Liabilities

This table contains the results of estimating difference-in-differences regressions to investigate the effect of IPO on firms' subsequent investment. *time* is an indicator variable that equals one if the firm is listed in that year. *treatment* is an indicator variable that is one if the observation is part of the treatment group, i.e. firms that went public before the moratorium. *treatment* is equal to zero if the firm is in the control group, i.e., IPO applicant firms that applied to IPO before the moratorium whose IPO processes were delayed by the moratorium. *DID* is equal to *time*treatment*, and is the DID estimate. The variable of interest is *LiabilitiesRate*. All variables are defined in Table 1. The sample period covers 2009-2015. The table reports robust standard errors clustered at the firm level in parentheses. ***, ** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

	LiabilitiesRate			
VARIABLES	OLS Base Model	OLS with Controls	Firm FE	Firm & Year FE
DID	0.105**	0.118**	0.0860*	0.227***
	(0.0488)	(0.0489)	(0.0469)	(0.0628)
time	0.307***	0.288***	0.325***	-0.237***
	(0.0338)	(0.0323)	(0.0310)	(0.0422)
treatment	-0.238***	-0.185***		
	(0.0194)	(0.0141)		
Constant	0.592***	0.331***	0.496***	0.309***
	(0.0153)	(0.0416)	(0.00932)	(0.0159)
Observations	2,738	2,474	2,738	2,738
R-squared	0.113	0.219	0.173	0.368
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Appendix

Appendix A

As of Dec 12th 2012, an application has to meet the following conditions for a firm to be listed on the Main Board or Small & Medium Size Enterprise Board (SME): (1) the positive net profits and the cumulative net profits for the last three fiscal years exceed RMB 30 million; (2) the cumulative net cash flows for the last three fiscal years, as derived from the firm's business operation, exceed RMB 50 million or the cumulative business revenues for the last three fiscal years exceed RMB 300 million; (3) the total value of stocks before the offering is no less than RMB 30 million; (4) the proportion of intangible assets (deducting land use rights, water-surface farming rights, mining rights and other rights) in its net assets at the end of the most recent fiscal period does not exceed 20%; and (5) no unrecovered losses existed at the end of the firm's most recent fiscal period.

For listing on the ChiNext Board, a firm that applies has to meet the following conditions: it must either (1) have generated profits for the last two consecutive years of a cumulative amount of no less than RMB10 million or (2) have generated net profits in the previous year of no less than RMB 5 million, have an operating income for the previous year of no less than RMB 50 million and have an annual growth rate for the last two years of no less than 30%. Moreover, the ending net profit for the latest fiscal period must be no less than RMB 20 million, the firm must have no unrecovered losses, and the total value of its stocks after the offering must be no less than RMB 30 million.

In practice, the CSRC often adopts tougher criteria than what is posted. For example, in 2012, the cut-off for net profit for listing on ChiNext Board was RMB 30 million rather than 5

million. The firms in my sample are aware of these unannounced criteria. The approval rate of the review meetings is high – the approval rate of IPOs between Jan. 1st 2012 and Nov. 3rd 2012 was 83.45% – so the applicant firms have a high expectation of being listed.

Appendix B

The CSRC initially imposed the moratorium to protect retail investors by limiting the supply of new stocks. During the moratorium, near the end of 2013, the CSRC also toughened the screening process. A large proportion of the firms that stopped their IPO application during the moratorium did so because they could not fulfill the toughened requirements. This change in regulation may result in the control group being different than the treatment group. But as discussed in Section 2.3, the difference does not bias the decision to list *ex ante* and the possible *ex post* difference only biases against my result.

Based on my conversations with officials in the CSRC, it seems that even the CSRC did not intend to have such a long moratorium, although there are no official documents that claim this. News articles indicate that throughout the first quarter of 2013 Chinese market participants expected that the IPO would resume soon. But the weak stock market performance concerned the CSRC. For example, in the middle of 2013, the Shanghai Stock Exchange Composite Index, the major stock index for Chinese markets, hit a historic low of 1849.65. 2013 also marked a dramatic change in the power structure within the Chinese Communist Party. The Twelfth National People's Congress and Chinese People's Political Consultative Conference took place, in which the congresses formally elected the new leadership of China. Later, in Nov. 2013, in the Third Plenary Session of the 18th CPC Central Committee, the new leadership announced their comprehensive reform plans for the economy, including plans for stock market reforms. The weak stock market performance, the unprecedented number of IPO applicant firms, and the sensitive political environment all contributed to the CSRC's caution in resuming IPOs. After the Third Plenary Session of the 18th CPC Central Committee, the CSRC quietly resumed the review meetings and

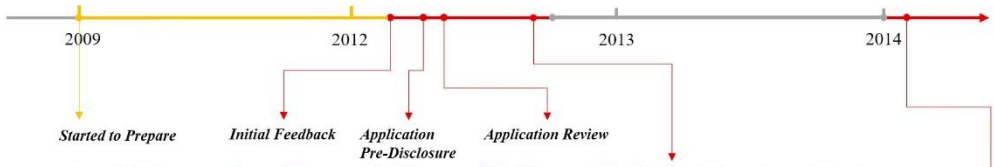
the first group of approved firms started to be listed on Dec. 30th 2013, signaling the end of the moratorium.

Appendix C

Zhejiang Shibao (002703) and NRB Corporation (002708) both belong to the auto parts industry. Their total assets in 2010 were similar -- Zhejiang Shibao had 811 million RMB in total assets and 282 million RMB in fixed assets. NRB Corporation had 602 billion RMB in total assets, and 155 billion RMB in fixed assets. These two companies also had similar history -- Zhejiang Shibao incepted on 6/2/1993, and NBR Corporation incepted on 1/8/1994. According to my collection of CSRC's review meetings, they both received initial feedback of their IPO application on 2/1/2012, suggesting that they applied to IPO around the same time. However, Zhejiang Shibao were listed on 11/2/2012, right before the moratorium, while NBR Corporation were affected by the moratorium, and did get to list until 1/21/2014. This result in a drastic difference of total assets and fixed assets between the two firms after the moratorium. In 2013, the year that Zhejiang Shibao got listed but NRB Corporation did not, Zhejiang Shibao had 1,314 million RMB in total assets and 436 million RMB in fixed assets. NRB Corporation had 796 billion RMB in total assets, and 205 billion RMB in fixed assets. In other words, IPO enabled Zhejiang Shibao to increase its fixed assets by over 50% while NRB Corporation experienced almost no growth.

Ticker	Name	Listed Date		2011	2012	2013	2014	2015
002703	Zhejiang Shibao	02NOV 2012	Total Assets	1,051	1,073	1,314	2,101	1,886
			Fixed Assets	290	299	436	449	452
			FA_Growth	1.00	1.03	**1.50**	1.55	1.56
002708	NRB Corporation	21JAN 2014	Total Assets	658	722	796	1,129	1,116
			Fixed Assets	196	202	205	273	305
			FA_Growth	1.00	1.03	**1.05**	1.39	1.56

1. FA_Growth = Fixed Assets of the year divided by Fixed Assets of 2011
2. Unit in Millions of RMB
3. Red Line = IPO



	Started to Prepare	Initial Feedback	Application Pre-Disclosure	Application Review		
ZS	Around 2009	***Around Jan***	***Apr06***	***May11***	***Nov02:*** passed the review and listed	
NRB	Around 2009	***Around Jan***	***Apr12***	***May18***	Was still waiting and hoping to get listed soon	***Jan21***: Listed

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Chapter 2

Discretionary Trading Halts, Liquidity and Catering

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"Chinese stock market is a roach motel for capital."

– *Anne Yang, NY Times, September 9, 2015*

1. Introduction

The Coasian definition of a financial exchange, such as a stock market, is the result of a firm creating a market in financial instruments where the firm's products are prices of the instruments traded on the exchange. Accurate prices and liquidity are an outcome of the rules of trade, terms of the contracts, and the market's mode of operation. In markets around the world, these are determined by the financial exchanges to maximize the accuracy and liquidity of prices. (see Mulherin, Netter, and Overdahl, (1991)) However, in China the financial exchanges have been given the right to determine the rules of trading for the firms that create the securities traded. Specifically, firms that issue common stock can choose when and for how long a stock is suspended from trading in practice. The central question of this paper is what happens to the financial exchange when the suppliers to the financial exchange, that is, issuers of securities, have the right to change the rules of trading.

If corporate managers take the market as informationally efficient, then trading suspensions are a tool for managing liquidity. However, recent research has suggested that arbitrage in securities markets is imperfect, and as a result, security prices can be too high or too low. The "market timing and catering" literature summarized by Baker and Wurgler (2013) studies how a

rational manager exploits the mispricing. The critical assumption is that the manager engages in activities that maximize the value of long-term shareholders. Arbitrage in the Chinese stock market is limited by widespread short-selling restrictions. This suggests that negative information will not have much impact on the stock price at least in the short run. If firms have managers who "cater" by maximizing the value of long-term shares, it is in the interest of firms to minimize the impact of short-term optimism. Baker, Greenwood, and Wurgler (2009) advance a catering theory of nominal share price where stock splits are a response to investor demand for securities in a price range. We suggest that stock suspensions may be a response to the same demand.

The site of the most obvious effect of allowing firms to suspend trading is liquidity. Research in market microstructure has generally assumed that firms should minimize illiquidity; they have generally recognized that trading halts are a negligible fraction of trading. Transactions costs are routinely computed from actual transaction data. However, when the financial exchange gives firms the right to start and end trading halts at their own discretion, trading halts are at least a potentially important source of illiquidity especially if they are frequent, unpredictable and vary cross-sectionally. Several papers have concluded that the market for Ashares has a high degree of liquidity but these papers do not consider trading halts. (see Campello, Ribas, and Wang (2014); Fong, Holden, and Trzcinka (2017)).

The site of the second effect of allowing firms to suspend stock is likely to be prices. First, the timing of trading halts may affect investor returns since investors should understand that the firm can suspend trading at any time. Those investors who need to trade may price securities lower. However, catering suggests that the prices of stocks that are suspended will move higher since firms are reducing the effect of trading on sentiment. Clearly, trading halts may add to the factors

that Allen, Qian, Shan, and Zhu (2015) argue change the return in the Chinese market relative to fundamentals.

As a practical matter, trading halts in the Chinese stock market have long been a concern of foreign investors. For example, the ability of firms to impose trading halts has been and continues to be a major factor for Modern Index Strategy Indexes (MSCI) in its decision to exclude A shares from their emerging market portfolio indices. In response, the Chinese Securities Regulatory Commission tightened the regulation of trading halts to reduce firms' discretion in starting and ending trading halts, in an attempt to make A shares more acceptable to foreign investors in June 2016. Remarkably, this "tightening" limited discretionary trading halts to three months[9].

Using a unique data set of all trading halts in Chinese A shares from June 1999 to December 2015, we investigate three basic questions of the unique market structure in China. First, are trading halts a significant source of illiquidity? Recognizing that in exchanges around the globe trading halts are rare and short, financial economists have generally not incorporated the probability of trading halts into transactions level measures of liquidity and use functions of bid-and-asked quotes as direct measures of the cost of trading. For example, in a study of 41 financial exchanges around the world from 1997 to 2007, Fong, Holden, and Trzcinka (2017) find that the global average of quoted spread over price is about 2.2% and trading suspensions are ignored. In their data and ours, the quoted spread over price for Chinese stocks is 0.15% of the price and is stable both cross-sectionally and over time. To estimate the liquidity risk arising from trading halts, we construct a ratio of trading halt days over all trading days—representing the percentage of market activity that

[9] http://www.bloomberg.com/news/articles/2016-05-27/china-to-restrict-trading-halts-in-boost-to-msci-inclusion-odds

is halted—to compare with bid-ask spread over price. This ratio is 5% across our sample period and 9% in the most recent three years. It is clear that the transaction costs measures used in other studies give an inaccurate picture of a highly liquid Chinese stock market by not accounting for trading halts. The bid-ask spread is negligible compared to trading halts when measuring liquidity and transaction cost in the Chinese stock market.

The second research question of this study is whether prices and widely available data contain information about the probability of trading halts. We use the bid-ask spread along with end-of-day price measures, such as the Amihud measure, the FHT measure of Fong, et al. (2017) and the high-low spread metric of Corwin-Schultz (2012) to determine whether there are investors who can predict trading halts. We find that in a hazard function, the lagged Corwin-Shultz measure, the FHT measure, and the Amihud measure are all statistically significant predictors of a future trading halt. Surprisingly, the bid-ask spread is not significant. The measures of transaction cost based on prices are clearly capturing more than the bid-ask spread. When we add accounting variables to the hazard function the liquidity measures still predict future trading halts, but the bid-ask spread is significantly negative. The liquidity measures are clearly capturing information about future transactions costs—trading halts—that is completely missed by bid-asked spreads. This suggests that the bid-ask spread is set by poorly informed traders while prices contain some information about future trading halts. The accounting variables suggest that less profitable smaller firms with lower cash and current ratios that borrow more are more likely to halt trading.

The third research question of this study is whether trading halts are harmful or beneficial for investor wealth. We compute the cumulative abnormal return (CAR) from five days before a trading halt until five days after a trading halt. The 1999–2012 period shows a "run-up and reversal" pattern: the CAR steadily increases before trading halts, and it reverses after trading halts. We

attribute this pattern to a number of factors: as the market overreacts to news, firms use trading halts to reduce asymmetric information, cool down the market, and dampen price volatility. However, the 2013–2015 period shows that trading halts increase returns after the halt. Both periods clearly show that firms use trading halts to trade off liquidity for better information quality and lower volatility for longer-term investors.

Since firms are the decision-makers for trading halts, we study the interaction of firm characteristics and trading halts with the Baker, Greenwood, and Wurgler (2009) catering theory as a guide. We first examine which characteristics affect the firms' likelihood of receiving trading halts. In particular, we analyze the role of financial constraints as a potential catalyst in trading halts. We find that firms that are more likely to be financially constrained, namely, firms with less assets, less profits-to-assets ratio, less-cash-to-assets ratio, less cash flow-to-assets ratio, more leverage, and more earning volatility-to-cash flow volatility ratio have a higher propensity to implement trading halts. These characteristics are also significant predictors in hazard estimation, where a greater likelihood of financial constraints is associated with higher risk of implementing trading halts. This result provides evidence that is consistent with our hypothesis that *ex ante,* the higher propensity of a trading halt is linked to the more financial constraints of the firm.

Using the same characteristics, we sort firms into five quintiles. We compute the cumulative abnormal returns (CAR) around trading halts for different quintiles. We confirm that the "run-up and reversal" pattern holds at all quintiles. The quintile study also finds that more financially constrained firms have steeper reversals after trading halts. Specifically, firms with less profit, less cash flow, less cash, and less assets have a steeper decline in CAR after trading halts. Following our previous interpretation of the "run-up and reverse" pattern as firms trading liquidity

for better information and less volatility, we conclude that more financially constrained firms' trading halts have a larger impact in reducing volatility and asymmetric information.

The uniqueness of our data and the scope of our analyses enable us to make several contributions to the literature. First, we provide the first complete statistical description of trading halts in the Chinese stock market. We show that trading halts play a far more important role than the bid-ask spread in measuring liquidity. A second contribution is that bid-ask spread estimators based on functions of the end of day prices provide more information than in the bid-ask spread. Our result suggests that future studies should reconsider the information difference between price impact measure and quote measures, especially when trading halts exist as a significant form of liquidity risk. Third, we find that when firms have the discretion to reduce liquidity, they do so extensively. By decreasing liquidity via trading halts, firms can improve information and reduce volatility. By letting the firms decide on when to halt trading, the Chinese financial markets have effectively decided that accurate prices are more important than liquidity.

Our result sheds light on the recent debate over the effectiveness of the stock market (Bond, Edmans, & Goldstein, 2012; Zingales, 2015). Finally, finding that financially constrained firms use trading halts differently, our paper provides additional insights into how corporate finance decisions can affect investors.

The remainder of the paper proceeds as follows. In Section 2 we discuss the relevant institutional details of trading halts in China and describe our data. We also report summary statistics to compare trading halts with the bid-ask spread. In Section 3 we outline the hazard model we use to study the determinants of trading halts, and we display the results of the model. Section 4 explores the effect of trading halts on CAR while Section 5 focuses on the association of firm characteristics, in particular financial constraint proxies, to the likelihood of trading halts and CAR

around trading halts. Recognizing that this analysis mostly documents correlations, we discuss the opportunities and challenges that remain for future research and conclude in Section 6.

2. Data and Institutional Details

Our data include information on stock trades, firm characteristics, and trading halts. Both the source of data and relevant institutional details of trading halts are provided below as well as relevant details of the Chinese stock market. This section also includes a brief comparison between bid-ask spread and trading halts to demonstrate that, in the Chinese stock market, trading halts is the more important source of illiquidity than the bid-ask spread.

2.1 Trading Information and Firm Characteristics

Most of our information on trades is from GTA Information Technology's China Stock Market and Accounting Research (CSMAR) database except for daily bid price and daily ask price, which is obtained from Thomson Reuter's Datastream. Both data from CSMAR and data from Datastream are at daily frequency.

Firm characteristics are also obtained from CSMAR at quarterly frequency, and we take the yearly average.

2.2 Trading Halts

Our data contains the historical record of Chinese trading halts provided by Wind Financial Terminal (WFT).[10] The data from WFT in China is analogous to the data available from Bloomberg, Datastream and Compustat. It serves more than 90% of financial enterprises in the Chinese market

[10] We thank School of Hotel Administration at Cornell University for the purchase of this data.

and over 75% of Qualified Foreign Institutional Investors.[11] The historical record of trading halts from the Wind Financial Terminal's data center was obtained via a special request. The Wind data is a survivor-bias-free database that covers both active and inactive stocks. Our sample covers all trading halts that are greater than or equal to one day in length, from June 1999 to December 2015, for both the Shanghai and Shenzhen stock exchanges. We exclude intraday trading halts from our sample, most of which are automatic halts due to abnormal price movements. [12]

We hand-collect trading halts data from the Shanghai Stock Exchange (SSE) website for the period from June 2009 to December 2013 to cross check the accuracy of the WFT dataset.[13] Out of the 24,878 suspension records from SSE data, 83.58% or 20,794, are included in the WFT sample. We randomly select a few trading halts that are not covered in the WFT sample. Either the trading halts turned out to be intraday halts or alternately, there was no halt on that particular day. A random check of trading halts in the WFT sample matches the SSE website. This exercise suggests that the WFT sample is a reliable source for trading halts data.

According to "Trading Rules of Shanghai Stock Exchange" and "Trading Rules of Shenzhen Stock Exchange", the stock exchanges initiate trading halts. However, firms can apply for trading halts and the requests are automatically granted. On the other hand, when firms do not apply for trading halts, the exchange seldom initiates them. Furthermore, the exchanges do not set

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[11] http://www.wind.com.cn/En/aboutus.html.

[12] For example, if a stock has hit the return ceiling or floor (10% of ordinary stocks, 5% for special treatment (ST) stocks) for three consecutive days, the exchange will halt the stock's trading at the beginning of the next trading day for an hour.

[13] The SSE website suspension record is only available since June 2009.

deadlines for ending trading halts. Thus, in most cases, the timing and length of the trading halt (start and end of trading halts) is at the discretion of the individual firms. The exceptions are part of code 7 trading halts, which are trading halts related to risk warnings; and part of code 4 trading halts, which are transaction related trading halts. They represent a very small portion of the total number of halted stock-days: there are 2,040 code 7 halted stock-days and 1,793 code 4 halted stock-days, out of 189,072 halted stock-days in total, a 2.03%.

In May 2016, the CSRC published new regulations which capped halts at three months for major asset restructuring and one month during private placements. The bourses will have the right to reject trading-halt applications under extreme market circumstances[14]. Although the news is outside of our sample period, it provides an anecdotal evidence about how arbitrary trading halts in the Chinese stock market were, and the degree of freedom that firms have to conduct trading halts. The fact that firms have discretion to initiate trading halts lead to two implications: First, when liquidity is a choice, firms do voluntarily choose to worsen their own liquidity. Second, since trading halts are not forced upon on firms by the regulatory agencies, the frequent trading halts are likely to be an outcome of market equilibrium.

When a trading halt occurs, the firm is required to announce the reason for the trading halt. Although the CSRC specifies the conditions under which trading halts can or need to happen, there is no strict format for firms to follow. We examined the guidelines from the CSRC's regulation as well as read the reasons given for the trading halts to categorize the types of trading halts. We classify the reasons for trading halts into nine categories based on keywords contained in the reports. Table 1 lists the categories as well as the Chinese keywords used.

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[14] http://www.bloomberg.com/news/articles/2016-05-27/china-to-restrict-trading-halts-in-boost-to-msci-inclusion-odds

Table 2-1

Of the nine reasons given, we need to differentiate code 1, code 4 and code 5. Code 1 represents trading halts due to shareholder meetings and code 4 represents trading halts due to transaction related reasons, which are usually scheduled or predictable. Code 5 represents trading halts due to split share reform that happened around 2005 and 2006. Campello, Ribas, and Wang (2014) provide a detailed discussion of the split share reform. It is a one-time, special event with different motivations relative to other trading halts. Therefore, we provide two sets of analysis, one including all types of trading halts, and one excluding trading halts with code 1, code 4 and code 5.

In Figure 1, we give an overview of trading halts decomposed by year, month, code and length. We separately represent code 1 (shareholder meeting) trading halts, code 5 (2005-2006 recapitalization) trading halts and all other trading halts by using different colors. There are a few obvious patterns. First, the number of trading halts grows dramatically at the end of the sample. Although year 2005 to 2008 saw a sharp increase of trading halts, the increase is largely associated with code 5 (split share) trading halts. (see Campello et al. (2014),). The trading halts in 2013 were doubled in 2014 and doubled again in 2015, almost all due to code 2 (important matters) trading halts. Figure 1b shows that the summer months have a disproportionate share of the halts but there are still many in distributed throughout the year.

Figure 2-1

Figure 1c show the trading halt by codes. The CSRC revised its rules in July 2012 canceling code 1 trading halts due to shareholder meetings[15] and code 4 trading halts, halts that are transaction related.

Trading halts due to "important matters" (code 2), has the most total length of trading halts. The language of "important matters" is intentionally vague enabling firms to provide an arbitrary reason(s) to initiate trading halts. For example, during the stock market meltdown in mid-2015, almost half of stocks in A shares were halted, which attracted global attention[16]. In that episode, "important matters" accounted for the majority of the disclosed reasons are. Thus, the frequency and total length of code 2 trading halts provide another view of how much discretion firms have in terms of conducting trading halts.

Figure 1d shows trading halts by length. The most frequent is one day but there are many over eleven days. We examine the length more carefully in the next section.

2.3 Trading Halts vs. Bid-Ask Spread

To compare the liquidity impact of trading halts relative to the bid-ask spread, we construct a ratio of halted stock-day observations over all trading stock-day observations. This ratio is 5% over our sample period. We view this ratio as the percentage of market activity that is halted. One can compare this ratio with the bid-ask spread over the daily closing price, which equals 0.15%. Table 2 provides the distribution of these variables, with the addition of other variables such as HaltLength – the total number of halted activity trading days for each trading halt. It is worth

[15] http://www.china.com.cn/policy/txt/2012-07/09/content_25852871.htm

[16] http://money.cnn.com/2015/07/08/investing/china-stocks-suspended/

nothing that the average HaltLength is 5.26 days, a sharp contrast to the predominately-intraday halts in the US market.

Table 2-2

Panel B shows the trading halts from 1999 to 2012. In 2012 the Chinese regulators ended the practice of requiring trading halts for shareholder meetings so that Code 1 trading halts disappeared. The mean level of *suspension* which is the halted stock days over all stock trading days is 3.5% with a mean level of HaltLength of 3.1 days, with trading halts of 4 days or longer accounting for 10% of the sample. In contrast, the last three years of the sample have many more halts and for longer periods. The number of stock days halted is 8.58% with the average HaltLenght of 20.6 days. Trading halts of 66 days or longer account for 10% of the sample. Table 2 clearly shows that the past three years look very different than the previous fourteen and we will examine them separately for much of this paper.

2.4 Liquidity Proxies, Trading Variables and Accounting Variables

Table 3 shows the summary statistics of the variables we use to examine our research questions. The liquidity measures from the literature are the bid-asked spread measures of Corwin-Shultz high-low measure and FHT; the Amihud price impact measure; trading variables price and volume. For our firm-level tests we use accounting variables at an annual frequency: total assets, net profit, cash flow, cash balance and leverage. Table 3 displays all the variables and their definitions;

Table 2-3

Most price impact measures and quote measures are designed to proxy for liquidity. Therefore, it is important to ensure that each variable brings new information, rather than creating

redundant information under different names. Panel A of Table 3 presents the correlation of all liquidity proxies and trading variables used in this paper. In the correlation matrix, the highest correlation is only 0.35, between daily price and daily volume. All the liquidity proxies have very low correlation among themselves, especially the bid-ask spread, where the highest absolute correlation with all other variables is only 0.05. The low correlations further support the view that the bid-ask spread is an insufficient metric to capture all the liquidity risk in the Chinese stock market.

Comparing all the liquidity measures, the bid-ask spread stands out for its low variation. The standard deviation of the bid-ask spread is 0.021, but it is mostly driven by a few outliers. When we trim the data at 1% and 99%, the standard deviation of the bid-ask spread shrinks to 0.0102, with a mean of 0.0103. The majority observations of the bid-ask spread stays constant at 0.01, which is the tick size for the Chinese stock market.

In our sample, the average firm's leverage, defined as total debt to total book asset, is 0.589 , which is drastically above the United States average of 0.29 from 1950 to 2003 reported by Frank and Goyal (2009).

3. The Hazard Estimation

The second research question of this study is whether a trading halt can be predicted from variables available to investors. If information about trading halts is leaked to the market before the trading occurs, the prices, spread and volume should reflect it. We will test the hypothesis of information leakage by estimating a hazard model.

3.1 The Hazard Model

We use a Cox proportional hazard model to estimate the propensity of a stock to receive a trading halt. While a traditional hazard model is used to analyze survivorship when only one failure is observed, in our setting, the trading of one stock can be halted multiple times. Thus, failure times, or trading halts, are correlated within a stock, violating the independence of failure times assumption in traditional survival analysis. To address this issue, we use a "variance-corrected" model that adjusts the covariance matrix to account for the additional correlation. More specifically, let X_{ki} and C_{ki} be the failure and censoring time of the *k*th trading halts (*k=1,....,K*) in the *i*th stock (*i=1,...,m*), and let Z_{ki} be a p-vector of possible time-dependent covariates, for *i*th stock with respect to the *k*th trading halt. Assume that X_{ki} and C_{ki} are independent, conditional on the covariates vector (Z_{ki}). Define $T_{ki} = \min(X_{ki}; C_{ki})$ and $\delta_{ki} = I(X_{ki} \leq C_{ki})$, and let β be a p-vector of unknown regression coefficients. Under the Cox proportional hazard assumption, the hazard function of the *i*th stock for the *k*th trading halt is

$$\lambda_k(t; Z_{ki}) = \lambda_0(t) e^{Z_{ki}\beta} \tag{1}$$

The Cox's partial likelihood function, $L(\beta)$, obtains maximum likelihood estimates of β. D. Lin (1994) showed that the estimator $\hat{\beta}$ is a consistent estimator for β and is asymptotically normal assuming independence of failure times. The covariance matrix is:

$$I^{-1} = -\partial^2 \frac{logL(\beta)}{\partial\beta\partial\beta'} \tag{2}$$

However, it does not take into account the additional correlation in the data. D. Y. Lin and Wei (1989) proposed a modification to estimate a robust variance-covariance matrix:

$$V = I^{-1}U'UI^{-1} \tag{3}$$

where U is a $m \times p$ matrix of group efficient score residuals, assuming the observations are divided into m independent groups, which, in our case, are individual stocks.

Table 2-4

With the corrected standard error that accounts for the serial correlation within each individual stock, Table 4 Panel A displays the result of the hazard estimation using the liquidity variables. In the estimation, we use lagged price impact proxies as Z_{ki}, with the bid-ask spread and trading variables used as control variables. The coefficient for all liquidity measures – Corwin's high-low spread, FHT and Amihud – are all significantly positive. This result suggests that the price impact measures can predict trading halts in the Chinese stock market even after controlling for the bid-ask spread and trading variables. What is surprising is that the bid-ask spread itself has no predictive power. FHT (2016) show that the end of day bid-ask spread that we are using is the best possible estimator for transactions-level bid asked spreads. The liquidity measures which were developed to estimate the transactions level bid-ask spread are clearly capturing more information about future trading halts than the spread itself. This suggests that the transactions-level spread has less information than each of these liquidity measures which are different functions of prices and volume. Our conclusion is that there is information in the marketplace about future trading halts but these informed investors are not setting the spread.

The significance and even the size of the variable in Table 4 Panel A does not change in Panel B which estimates the hazard function using accounting variables. The one exception to this rule is that the bid-ask spread is now significant but with a negative sign—larger spreads predict less trading halts when higher levels of all liquidity measures—indicating more transactions costs—predict more trading halts.

The accounting variables suggest that less profitable smaller firms with lower cash and current ratios who borrow more are more likely to halt trading. We explore what this means for investor returns in the next section of the paper.

4. Trading Halt and Return

In this section, we focus on understanding the reasoning behind firms' decision to reduce the liquidity of their stocks. We investigate the impact of trading halts on investors, by using an event study approach to calculate the Cumulative Abnormal Return (CAR) around trading halts. We employ the following procedure: In each active trading day t, we calculate the return for stock i, R_{it}, and obtain market return MR_t from CSMAR. The abnormal return for stock i on day t, AR_{it}, then follows:

$$AR_{it} = R_{it} - MR_t \tag{4}$$

It is worth nothing that when stock i is halted on day t, its return R_{it} becomes zero. We still calculate its abnormal return by negating the market return. This measures the opportunity cost of not being able to trade. If the market is positive the abnormal return will be the inability to obtain at least market returns, if the market return is negative, the abnormal return will reflect avoiding a falling market.

We start CAR_{it} as 1 from 5 days before a trading halt, then CAR_{it} accumulates the abnormal returns:

$$CAR_{it} = CAR_{it-1} * (1 + AR_{it}) \tag{5}$$

Again, since we calculate AR_{it} even when the stock's trading is halted, Equation (5) means that, CAR will keep accumulating even if the trading halt last for days. Thus, if firms on average choose

to strategically halt their trading when the market performs well or when the market performs poorly, the CAR will reflect that trend.

Some special cases exist of consecutive trading halts that are less than 10 days apart from each other for the same stock. In those situations, we assign a higher priority for closer dates. For example, if a trading day is 3 days after a trading halt, A, and 2 days before the next trading halt, B, it will be considered as -2 day to trading halt B. We also mark the code and length for each trading halt, and this trading day will be considered as belonging to trading halts with the same type as B.

Figure 2 displays the CAR around trading halts. In Figure 2, the pattern of the CAR around a trading halt is very distinct. We name this pattern "run-up and reversal". The CAR experiences a run-up before a trading halt, and then reverts back to the original value after the trading halt. Our quintile study in Section 5 confirms that this pattern persists in almost all quintiles. When we exclude code 1 trading halts and code 5 trading halts, the pattern persists. However, the CAR is higher on average. Also, on the 5th day after trading halts, the average CAR is 1.017, representing a 1.7% return over the original value. Therefore, an average investor can benefit from buying before trading halts and selling around trading halts. She can also lose from buying around trading halts and selling after trading halts. Meanwhile, a long-term investor may not care about trading halts in general since it has little long-term effect.

Figure 2-2

In general, the previous literature argues that trading halts reduce the asymmetric information in the market by giving investors more time to digest news (Chen, Chen, & Valerio, 2003; Corwin & Lipson, 2000; Greenwald & Stein, 1991; Kodres & O'Brien, 1994). Our result

provides fresh evidence supporting this view. The run-up of the CAR before trading halts suggests that the market either receives or anticipates news, or rumors, before trading halts. However, after the trading halts, the stock return almost always decreases, which suggests that after trading halts, the market is less optimistic about the news or completely discounts it and therefore the return falls back to its original level. It seems that trading halts, by reducing asymmetric information and allowing investors to have more time to think, serves intuitively as a thermostat cooling down the market and thus reducing volatility, while still maintaining a positive return. In a market where retail investors predominate (see appendix), reducing asymmetric information and price volatility have a significant economic and social impact.

The result that trading halts are associated with positive return and lower volatility also strengthens our argument that the amount of trading halts in the Chinese stock market is an equilibrium outcome rather than a regulatory artifact. In Section 2, we argue this point based on institutional details. In this section, we provide evidence that there exist opposite marketing forces which make trading halts attractive to some while harmful to others. Trading halts, by their nature, reduce the liquidity of stocks and are harmful to investors who would have traded during the halt. Therefore, in order for trading halts to be an equilibrium outcome, they should compensate investors elsewhere for investors' loss of liquidity. Our results show that investors who stay in the market from five days before the halt until five days after benefit from lower volatility and positive return. However, the benefit is not universal. If an investor purchases a stock right before a trading halt, she will be harmed both by reduced liquidity and by negative return. Furthermore, because firms can choose when to halt trading, there can be differential effects among different firms arising from differences in the market timing and duration of trading halts. We provide further analysis in Section 5.

5. Trading Halts and Financial Constraints

In this section, we first study what firm characteristics are associated with a higher likelihood of implementing trading halts. We next extend the analysis in Section 4 by studying the differential effects of trading halts on CAR in each quintile groups to further our understanding of why firms voluntarily choose to reduce the liquidity of their stocks.

We study the interaction of financial constraints with respect to trading halts, since financial constraints are one of the central topics of finance, and it links the capital market to the real economy (Campbell, Dhaliwal, & Schwartz, 2012). Our results suggest that the discretionary trading halts are the firms' reaction to asymmetric information in the market. They represent the firms' trade-off between liquidity and volatility. Financially constrained firms are under more pressure to do so, as their investors are more likely to be sensitive to news.

The corporate finance literature offers many indexes of financial constraints, such as the investment sensitivity of cash-flow (FHP measure) from (Fazzari, Hubbard, Petersen, Blinder, & Poterba, 1988), the KZ index of developed by Lamont, Polk, and Saaá-Requejo (2001) based on the text-based results from Kaplan-Zingales (1997), the HP index by Hadlock and Pierce (2010) updated the KZ index using a new text-based sample, the WW index of (Whited & Wu, 2006), and the cash-flow sensitivity of cash Almeida, Campello, and Weisbach (2004), among others. However, using these indexes usually come with strong assumptions, such as the estimated coefficients being stable across time and industries. Such assumption is unlikely to hold when we apply them to the Chinese firms, who have drastically different characteristics than the firms in the United States.

However, when calculating difference indexes, different results generally find the same sign of loading on different firm characteristics. For example, the KZ index finds that the likelihood of financial constraints correlates positively with market-to-book, leverage, and negatively with cash flow, dividends and cash; similarly, HP index loads negatively on size, positively on size-squared, and negatively on age; WW index loads negatively on cash flow to asset, a dummy that indicates whether the firm pays dividend, size, sales growth, and positively on long-term debt to total assets and industry sales-growth. Therefore, the literature suggests that larger firms, firms with more profit, more cash flow, more cash balance and firms with less leverage are less likely to be financially constrained. In this section, we use these firm characteristics, along with earning smoothness –a measure commonly used in the accounting literature to proxy for earning manipulation – to test the hypothesis that more financially constrained firms use more trading halts, and the consequences of their trading halts are relatively more significant to investors.

For each firm, we take its accounting variables from its annual financial statements, and sort firms into one of the five quintiles for each year. Because we classify each quintile as having firms that meet the characteristic of the quintile, we allow firms to switch quintiles based on their yearly accounting numbers. Therefore, quintile 1 represents the firms that are most likely to be financially constrained within a given year, specifically, firms with less profit, less cash, less cash flow, less asset, more leverage and more earning smoothness. Quintile 5 represents the firms that are least likely to be financially constrained in that year. Over the entire sample, every quintile has the same number of firms. We sum the total number of halted days within each quintile, and report the result in Table 5. Table 5 displays an almost monotonic decrease in the total number of halted days, from more financially constrained firms to less financially constrained firms. This result

suggests that more financially constrained firms tend to implement trading halts with greater total length.

Having established the link between financial constraints and trading halts, we next explore whether differential effects of trading halts exist for firms having different levels of financial constraints. We replicate the test of Figure 2 for firms with different quintiles. Table 6 shows the *CAR* for different quintiles based on the different characteristics. For ease of eyeballing, we report *CAR-1* instead of *CAR*.

For all characteristics, we first observe the "run-up and reversal" pattern, with *CAR-1* above 0 for all Day-1 observations across all quintiles, but with a less magnitude on the fifth day after trading halts. This result confirms that the nature of trading halts persists for different kind of firms. Second, almost all characteristics, have steeper decreases of *CAR* after trading halts for quintile 1, and a smoother decrease of *CAR* after trading halts for quintile 5. The change in slope is also monotonic across quintiles. There is no obvious difference across quintiles before trading halts. This pattern suggests that when facing similar news and imposing similar actions, the trading halts of more financially constrained firms have a relatively larger impact on the market, pushing the return back to the original level faster, compared to the trading halts of less financially constrained firms.

6. Concluding Remarks

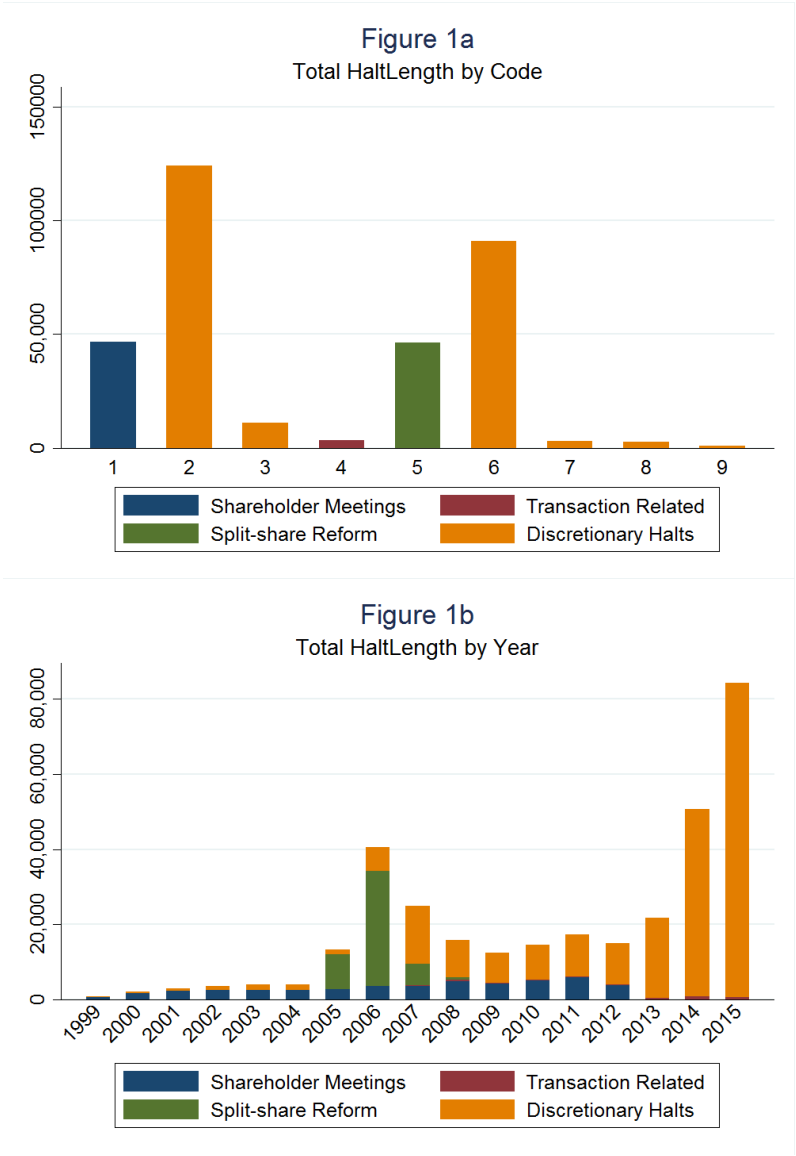
Our paper examines the consequences of a financial exchange giving firms the right to suspend trading in their common stock. Using data from 1999-2015 and focusing only on suspensions of one day or longer, we document that 5% of stock-trading days are suspended. We find that these suspensions are not predicted by the bid-ask spread but are predicted by measures

developed to estimate liquidity, specifically, the Corwin-Shultz high low measure, the FHT measure and the Amihud measure. Firms in China appear to be trading liquidity for price appreciation since these trading halts increase cumulative abnormal returns for longer term investors. Finally, financially constrained firms are more likely to use trading halts, and their trading halts appear to have a larger impact relative to less financially constrained firms.

Figures

Figure 2-1: Distribution of Trading Halts

The sample includes 62,490 Chinese A shares trading halts (of varying lengths) between June 1999 and December 2015. Panel A presents the sum of halted days by different code. Panel B presents the sum of halted days by year. Panel C presents the sum of halted days by month. Panel D presents the frequency of trading halts by different code.



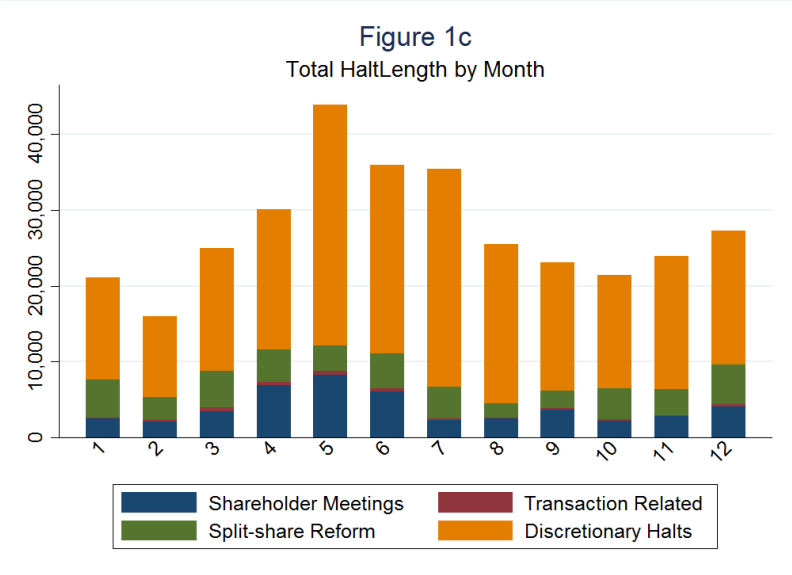


Figure 1c
Total HaltLength by Month
0
10,000
20,000
30,000
40,000
1
2
3
4
5
6
7
8
9
10
11
12
Shareholder Meetings
Transaction Related
Split-share Reform
Discretionary Halts

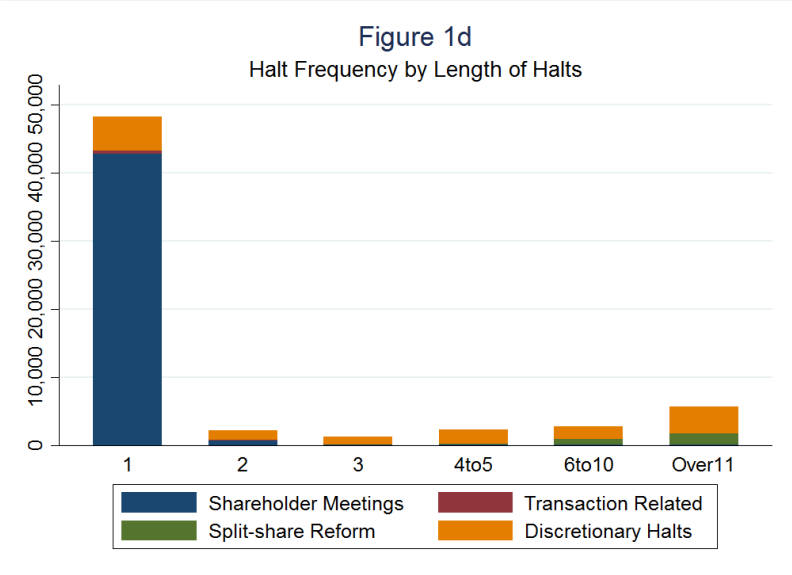
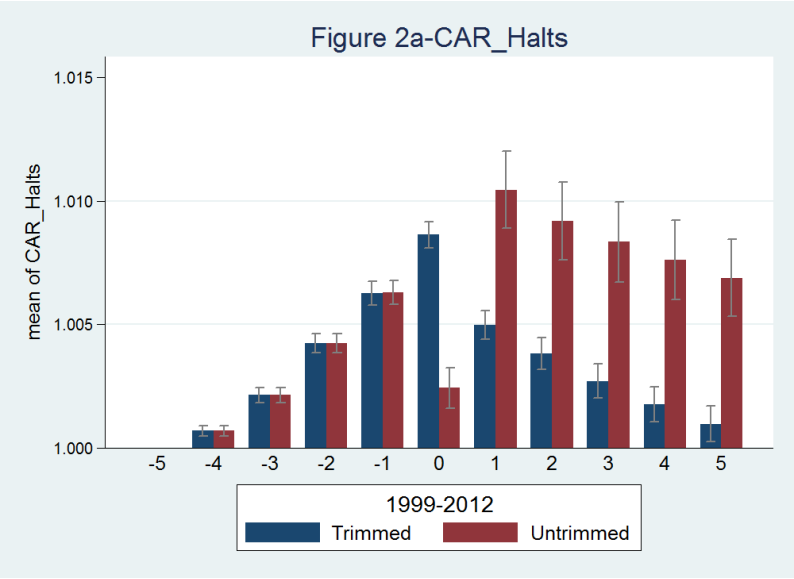
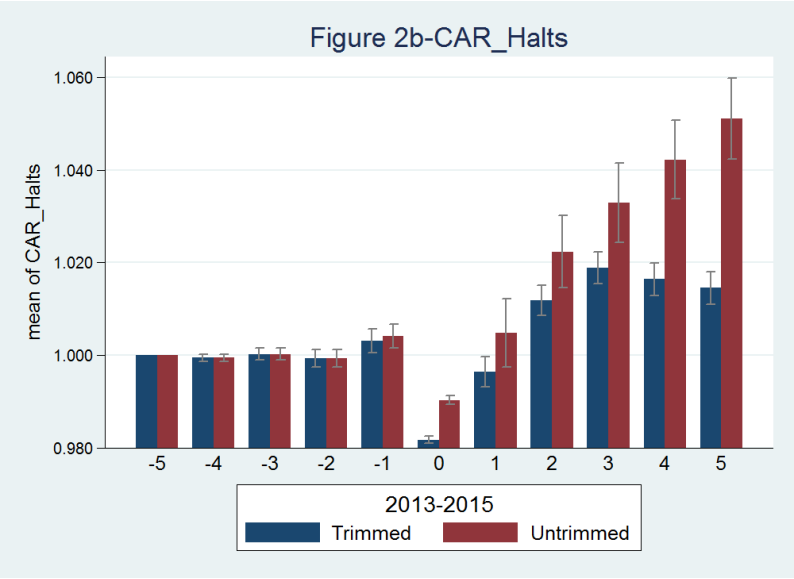


Figure 1d
Halt Frequency by Length of Halts
0
10,000
20,000
30,000
40,000
50,000
1
2
3
4to5
6to10
Over11
Shareholder Meetings
Transaction Related
Split-share Reform
Discretionary Halts

Figure 2-2: Variables of Interest Surrounding Trading Halts

The sample includes 57,036 Chinese A shares trading halts between June 1999 and December 2013. Figure 2 presents the daily mean of cumulative abnormal returns (CAR) for five days before trading halts and five days after trading halts. Trading halts are treated as day 0, regardless of the length. Blue bars represent untrimmed data, and red bars represent data trimmed at 1% and 99%. The figures also present confidence interval at 95%. Figure 2a includes data for the whole sample. Figure 2b excludes trading halts due to code 1, code 4 and code 5.





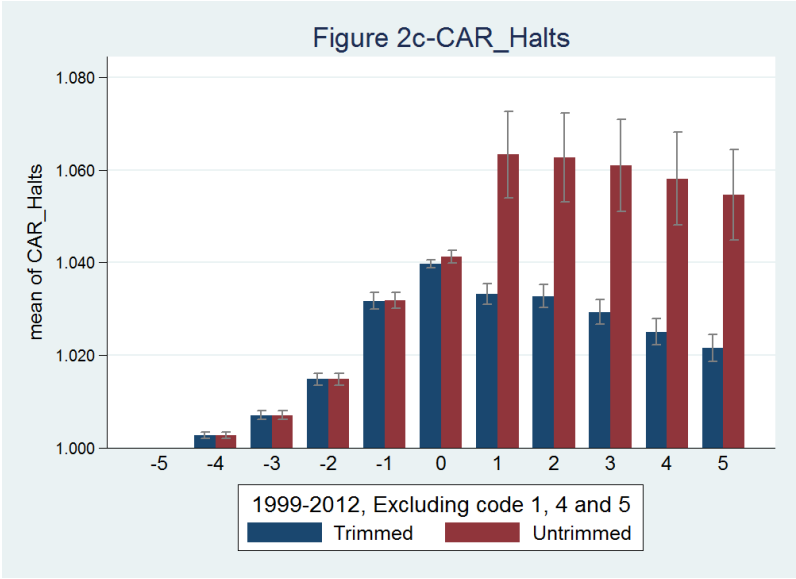


Figure 2c-CAR_Halts
mean of CAR_Halts
1.080
1.060
1.040
1.020
1.000
-5
-4
-3
-2
-1
0
1
2
3
4
5
1999-2012, Excluding code 1, 4 and 5
Trimmed
Untrimmed

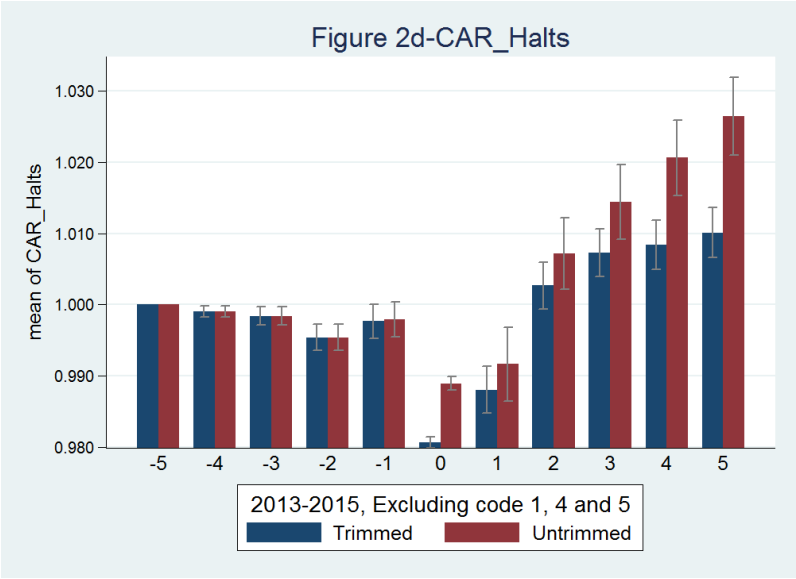


Figure 2d-CAR_Halts
mean of CAR_Halts
1.030
1.020
1.010
1.000
0.990
0.980
-5
-4
-3
-2
-1
0
1
2
3
4
5
2013-2015, Excluding code 1, 4 and 5
Trimmed
Untrimmed

Tables

Table 2-1: Definitions

Table 2-1a

Code, Reason and Keywords of Chinese A-Shares Trading Halts

The table provides the associated reason for each code of trading halts, as well as the keywords used to identify each reason. Each trading halt in the sample contains start day, end day, and the reasons for the halt. Based on the official regulations, we assign the reasons into nine categories based on the keyword used in the reported reasons.

Code	Category	Keywords																	
1	Shareholders' meeting	股东大会	股东会议	大会															
2	Important matters	重要	重大	事项															
3	Company report	临时报告	定期报告	提示性公告	董事会	年报	季报	指引披露	年度报告	股权激励	中报	季度	半年	临时公告	中期	公告			
4	Transaction related	异常波动	股价异动	配股	交易	股价													
5	Split share reform	股权分置改革	股改	对价方案															
6	Merge/Acquisition/Restructure	重组	增发	转让	发行	收购	控制人	法人	冻结	合并	配股	资产	股权						
7	Risk	特别处理	亏损	特别转让	退市	暂停	风险	预警	预亏	破产	担保	事故	宽限期	警示	恢复	特殊处理	诉讼	保留意见	重整
8	Media Report	媒体	传媒	报道	网站澄清公告	传闻更正公告	报纸刊登公告	报导有关公告	文章沟通结果	报告相关事项	相关信息								
9	Others	停牌	其他	相关公告															

Table 2-1b

Definitions of Variables

The table provides the definition of the variables used in this paper. The price, volume, and accounting data are obtained from CSMAR; the bid-ask spread is obtained from Datastream.

Variable Names	Definition
	Liquidity Variables
CS_0	The non-negative high-low spread from Corwin and Shultz (2011)
FHT_m	Measure developed by Fong, Holden and Trzcinka (2016)
baspread	Bid-ask spread: ask minus bid
amihud	Absolute daily return over daily volume
	Trading Variables
dnvaltrd	Daily level of volume
Daily_Return	Daily return
Price	Comparable closing price with dividend reinvested
delta_volume	Daily volume change
	Accounting Variables
ProfitRatio	Net profit / total assets
CashRatio	Cash holding / total assets
CashFlowRatio	Cash flow / total assets
TotalAssets	Total assets
Leverage	Total liabilities / total assets
CurrentRatio	Current assets / Current liabilities
smoothness	Earning smoothness: Earning volatility over cash-flow volatility

Table 2-2: Summary Statistics for Comparing Bid-Ask Spread and Trading Halts

The table lists information to compare bid-ask spread with trading halts. *clsprc* is the nominal daily closing price. Suspension is a dummy that equals to 0 for non-halted stock-day observations and equals to 1 for halted stock-day observations. *Haltlength* is a variable that equals to the number of halted trading days at the beginning of a trading halt, and is empty for the rest of the observations. *Baspread* is the daily ask price minus the daily bid price. *Baspreadoverp* is the bid-ask spread over the nominal daily closing price.

Panel A, 1999-2015

VARIABLES	N	Mean	SD	1%	25%	50%	75%	90%	95%
clsprc	6,341,577	13.35	12.31	2.3	6.49	10.06	15.89	25.26	34
suspension	6,669,781	0.0493	0.216	0	0	0	0	0	0
HaltLength	62,490	5.261	17.72	1	1	1	1	9	23
baspread	5,998,718	0.01	0.02	0.00	0.01	0.01	0.01	0.02	0.03
baspreadoverp	5,998,710	0.0013	0.0016	0.0000	0.0003	0.0009	0.0017	0.0028	0.0037

Panel B, 1999-2012

VARIABLES	N	Mean	SD	1%	25%	50%	75%	90%	95%
clsprc	4,652,660	12.36	10.81	2.23	6.21	9.55	14.88	22.98	30.37
suspension	4,822,854	0.0353	0.185	0	0	0	0	0	0
HaltLength	54,879	3.133	13.51	1	1	1	1	4	12
baspread	4,393,263	0.01	0.02	0.00	0.00	0.01	0.01	0.02	0.03
baspreadoverp	4,393,255	0.0013	0.0017	0.0000	0.0000	0.0010	0.0018	0.0029	0.0039

Panel C, 2013-2015

VARIABLES	N	Mean	SD	1%	25%	50%	75%	90%	95%
clsprc	1,688,917	16.07	15.39	2.56	7.35	11.72	19.11	31.44	41.81
suspension	1,846,927	0.0858	0.28	0	0	0	0	0	1
HaltLength	7,611	20.6	31.56	1	3	6	24	66	90
baspread	1,605,455	0.01	0.03	0.00	0.01	0.01	0.01	0.02	0.03
baspreadoverp	1,605,455	0.0011	0.0013	0.0000	0.0004	0.0009	0.0015	0.0024	0.0031

Table 2-3: Summary Statistics for Trading Variables, Liquidity Variables and Accounting Variables of the A-Shares Stocks

The table lists summary information for all A-share stocks, from June 1999 to December 2015. Panel A presents the correlation of the liquidity variables. Panel B summarizes the trading variables, the liquidity variables and the accounting variables. The trading variables and liquidity variables are at daily frequency, and the accounting variables are at yearly frequency.

Panel A: Correlation Matrix for Market Variables

	CS_0	FHT_m	BASpread	Amihud	Price	Daily_Return	Δ_Volume
FHT_m	-0.01						
BASpread	-0.01	0					
Amihud	-0.08	0.05	0.05				
Price	0.02	-0.12	0.04	-0.16			
Daily_Return	0.01	-0.01	-0.05	-0.09	0.02		
Delta_Volume	-0.01	0	0	0.01	-0.01	0.25	
Volume	0.11	-0.05	0	-0.25	0.35	0.09	0.12

Panel B: Correlation Matrix for Accounting Variables

	ProfitRatio	CashRatio	Delta_CashRatio	OperatingCashFlowRatio	TotalAssets	Leverage
CashRatio	0.34					
Delta_CashRatio	0.17	0.15				
OperatingCashFlowRatio	0.3	0.04	0.25			
TotalAssets	0	-0.05	0.03	0.04		
Leverage	-0.42	-0.41	0.04	-0.09	0.22	
smoothness	-0.03	-0.05	0	0.03	-0.08	-0.07

Panel C: Summary Statistics

VARIABLES	N	Mean	SD	5%	50%	95%
CS_0	6,276,196	0.0074	0.0119	0	0	0.0332
FHT_m	6,601,154	0.0019	0.0011	0	0.0018	0.0039
baspread	5,946,425	0.0103	0.0102	0	0.01	0.03
amihud	6,278,162	0.0016	0.0031	0	0.0005	0.0076
Price	6,214,747	45.23	65.69	6.81	25.23	147.41
Daily_Return	6,214,786	0.001	0.028	-0.046	0.001	0.049
delta_volume	6,151,348	-941,242	46,360,600	-67,038,200	-363,172	64,196,800
dnvaltrd	6,214,747	81,763,200	142,595,000	1,827,000	28,625,100	356,251,000
ProfitRatio	6,535,814	0.02	0.04	-0.03	0.02	0.08
CashRatio	6,530,569	0.17	0.13	0.03	0.14	0.44
OperatingCashFlow Ratio	6,535,681	0.02	0.05	-0.06	0.02	0.11
TotalAssets (Thousand)	6,535,705	6,164,430	16,718,600	457,844	1,949,320	22,478,700
Leverage	6,535,370	0.469	0.21	0.13	0.469	0.813
smoothness	6,537,998	0.581	0.528	0.111	0.435	1.556

Table 2-4: Hazard Estimation

The table is the hazard estimation. We estimate a proportional hazard function with time-varying covariates: $\lambda_i\big(t, x_i(t)\big) = \lambda_0(t)\mathrm{e}^{x_i(t)'\beta}$. Because there can be multiple failures for each firm, we cluster the standard errors at stock level. Panel A presents the regression including all liquidity measures lagged at one day. Panel B presents the regression including all liquidity measures lagged at three days. Panel B presents the regression with different account variables. Panel C combines daily liquidity measures and annual accounting variables. Sample period is from June 1999 to December 2015.

Panel A: One-day Lag

VARIABLES					
lag1_CS_0	1.974***	2.366***			
	(0.331)	(0.300)			
lag1_FHT_m	19.59***		4.919		
	(3.413)		(3.442)		
lag1_baspread	-0.508			-0.477	
	(0.429)			(0.426)	
lag1_amihud	0.232***				0.0402***
	(0.0361)				(0.00709)
Observations	4,725,694	4,999,477	5,256,705	4,726,686	5,000,911

Panel B: Three-days Lag

VARIABLES					
lag3_CS_0	2.273***	1.753***			
	(0.336)	(0.315)			
lag3_FHT_m	18.54***		5.301		
	(3.404)		(3.396)		
lag3_baspread	-0.0140			0.0463	
	(0.309)			(0.305)	
lag3_amihud	0.224***				0.0496***
	(0.0381)				(0.00723)
Observations	3,392,266	3,596,271	3,785,382	3,393,062	3,597,309

Panel C: Accounting Variables

VARIABLES							
lag1_CS_0	1.912***	1.974***	1.970***	1.977***	1.894***	1.972***	2.021***
	(0.331)	(0.331)	(0.331)	(0.331)	(0.331)	(0.331)	(0.331)
lag1_FHT_m	15.59***	19.58***	16.46***	19.82***	20.15***	19.54***	18.11***
	(3.312)	(3.412)	(3.301)	(3.517)	(3.452)	(3.410)	(3.280)
lag1_baspread	(0.58)	(0.51)	(0.48)	(0.48)	(0.52)	(0.51)	(0.66)
	(0.453)	(0.429)	(0.436)	(0.421)	(0.431)	(0.429)	(0.460)
lag1_amihud	0.243***	0.232***	0.229***	0.240***	0.231***	0.232***	0.232***
	(0.0353)	(0.0361)	(0.0359)	(0.0358)	(0.0361)	(0.0361)	(0.0361)
ProfitRatio	0.0013***	-0.002***					
	(0.00045)	(0.00075)					
CashRatio	-0.986***		-0.81***				
	(0.0899)		(0.0771)				
CashFlowRatio	1.555***			0.946***			
	(0.121)			(0.0939)			
TotalAssets	-0**				-0**		
	(0)				(0)		
Leverage	0.0054***					0.0056***	
	(0.00170)					(0.00158)	
smoothness	0.109***						0.125***
	(0.0192)						(0.0207)
Observations	4,721,472	4,725,661	4,721,573	4,725,572	4,725,661	4,725,661	4,725,649

Table 2-5: Sum of *HaltLengh* by Different Firm Characteristics Quintiles

The table lists the sum of *HaltLength* by different firm characteristics quintiles. The quintiles are organized so quintile 1 contains the firms that are expected to have the most trading halts, such as smaller firms, firms with lower cash balance, firms with lower cash flow, high earning volatility firms, etc.

Sum of HaltLength by Quintiles

Quintiles	ProfitRatio	Cash Ratio	Cash Flow Ratio	Total Assets	-Leverage	-Smoothness
1	90,966	80,921	67,621	88,244	72,184	63,479
2	61,246	61,212	66,043	69,492	62,624	59,230
3	55,282	61,911	68,068	58,250	60,891	61,187
4	57,803	60,361	59,262	54,336	60,211	61,647
5	51,397	56,618	61,231	47,855	59,039	73,297

Table 2-6: Cumulative Abnormal Return by Days to Trading Halts

The table tabulates *(CAR_Halts – 1)* for halted days, the first day after halts, and the fifth day after halts. *CAR_Halts* is defined in Equation (5). The results are sorted into 5 quintiles of financial constrain proxies. Quintile 1 group is most likely to be financially constrained, having least profit ratio, cash ratio, cash flow ratio, total assets, and highest leverage. Quintile 5 group is least likely to be financially constrained.

Panel A: CAR Halts, 1999-2015

	ProfitRatio			CashRatio			CashFlowRatio		
Financially Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	0.75%	0.17%	-0.48%	0.70%	0.28%	-0.15%	0.54%	0.14%	0.03%
2	0.55%	0.31%	0.21%	0.59%	0.46%	0.33%	0.60%	0.36%	0.04%
3	0.36%	0.20%	0.16%	0.56%	0.38%	0.31%	0.58%	0.33%	0.18%
4	0.55%	0.47%	0.47%	0.59%	0.39%	0.31%	0.53%	0.38%	0.19%
5(Less)	0.72%	0.88%	0.96%	0.50%	0.49%	0.47%	0.71%	0.78%	0.80%

	TotalAssets			-Leverage			-Smoothness		
Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	0.94%	0.68%	0.40%	0.74%	0.34%	0.02%	0.71%	0.48%	0.19%
2	0.52%	0.34%	0.17%	0.62%	0.46%	0.30%	0.70%	0.52%	0.32%
3	0.56%	0.28%	0.24%	0.51%	0.33%	0.32%	0.63%	0.39%	0.24%
4	0.44%	0.33%	0.27%	0.48%	0.43%	0.28%	0.55%	0.30%	0.27%
5(Less)	0.48%	0.37%	0.17%	0.58%	0.46%	0.38%	0.36%	0.31%	0.24%

Panel B: CAR Halts, 1999-2012

	ProfitRatio			CashRatio			CashFlowRatio		
Financially Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	0.79%	0.34%	-0.59%	0.70%	0.37%	-0.30%	0.58%	0.33%	-0.13%
2	0.62%	0.51%	0.06%	0.65%	0.56%	0.19%	0.66%	0.45%	-0.14%
3	0.47%	0.33%	0.05%	0.63%	0.52%	0.16%	0.59%	0.48%	0.08%
4	0.57%	0.50%	0.27%	0.66%	0.56%	0.19%	0.61%	0.50%	0.11%
5(Less)	0.67%	0.83%	0.76%	0.49%	0.48%	0.27%	0.70%	0.74%	0.56%

	TotalAssets			-Leverage			-Smoothness		
Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	0.89%	0.71%	0.14%	0.72%	0.40%	-0.13%	0.63%	0.53%	0.12%
2	0.66%	0.53%	-0.01%	0.69%	0.62%	0.22%	0.70%	0.60%	0.21%
3	0.67%	0.46%	0.14%	0.53%	0.42%	0.15%	0.63%	0.42%	0.04%
4	0.49%	0.44%	0.14%	0.59%	0.57%	0.13%	0.64%	0.51%	0.11%
5(Less)	0.45%	0.36%	0.07%	0.59%	0.49%	0.14%	0.53%	0.43%	0.00%

Panel C: CAR Halts, 2013-2015

	ProfitRatio			CashRatio			CashFlowRatio		
Financially Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	0.52%	-1.14%	0.32%	0.71%	-0.45%	1.06%	0.22%	-1.27%	1.24%
2	0.01%	-1.24%	1.45%	0.11%	-0.33%	1.48%	0.12%	-0.31%	1.49%
3	-0.49%	-0.85%	1.02%	0.07%	-0.72%	1.50%	0.48%	-0.80%	0.99%
4	0.44%	0.22%	2.09%	0.14%	-0.86%	1.25%	-0.04%	-0.60%	0.86%
5(Less)	1.06%	1.24%	2.59%	0.54%	0.56%	2.04%	0.76%	0.99%	2.64%

	TotalAssets			-Leverage			-Smoothness		
Constrained	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5	Day -1	Day 1	Day 5
1(More)	1.28%	0.49%	2.22%	0.87%	-0.20%	1.26%	1.27%	0.06%	0.84%
2	-0.39%	-1.00%	1.44%	0.10%	-0.86%	0.99%	0.64%	-0.10%	1.25%
3	-0.24%	-1.10%	1.04%	0.38%	-0.33%	1.54%	0.70%	0.11%	1.77%
4	0.11%	-0.61%	1.30%	-0.32%	-0.66%	1.38%	-0.14%	-1.29%	1.44%
5(Less)	0.83%	0.48%	1.15%	0.51%	0.28%	2.22%	-0.80%	-0.52%	1.95%

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Chapter 3

Sensation-Seeking and Excess Volume – Cross-Country Evidence

1. Introduction and Background

This study examines whether sensation-seeking is an important determinant of trading volume. Trading volume deserves attention because it represents significant economic activity, demonstrates the impact of information on individual investors, and unveils the impact of information on investor disagreement (Bamber, Barron, & Stevens, 2011). Although there is a rich literature on trading volume, the determinants of trading volume are still considered understudied (De Bondt & Thaler, 1994; Shleifer, 2000). For instance, empirical research on excess volume generally has very low explanatory power, suggesting that current work is far from revealing the entire picture.

Milgrom and Stokey (1982) and Tirole (1982) argued that trading volume should not exist under complete markets with rational investors, which is commonly known as the "no trade theorems." Tirole (1982), for instance, proposed three possibilities for the existence of nonzero volume: 1) risk-loving or irrational investors; 2) to increase diversification; 3) agents may have different prior beliefs. Black (1986) attributes the existence of trading activities to "noise." That is, investors exist who believe that noise is information. Notably, Black (1986) also proposed that people may trade simply because they enjoy it.

Scholars have developed various models to derive results after relaxing the assumptions that will lead to a no-trade situation and explain trading activities. In Kim and Verrecchia's (1991)

rational expectation model, for instance, excess volume around the announcement of trading news occurs because people have different prior information, which triggers trading. Their hypothesis has been supported by numerous studies (Ali, Klasa, & Zhen Li, 2008; Atiase & Bamber, 1994; Bamber, Barron, & Stober, 1997; Barron, Stanford, & Yu, 2009; Kandel & Pearson, 1995; Utama & Cready, 1997).

On the other hand, increasing evidence also suggests that part of the trading volume can be attributable to behavioral reasons. For instance, Ayers, Li, & Yeung (2011) find that different n groups of investors systematically responded differently to different kinds of earnings news. Breakthroughs from studying the behavioral factors offer new insights to the determinants of excess volume. For example, overconfidence has been identified as an important reason for people to trade (Daniel, Hirshleifer, & Subrahmanyam, 1998; Odean, 1998c), and this is supported by both archival evidence (Barber & Odean, 2000, 2001; Chuang & Lee, 2006; Glaser & Weber, 2007; Graham, Harvey, & Huang, 2005; Odean, 1998a, 1998b; Statman, Thorley, & Vorkink, 2006) and experimental evidence (Deaves, Lüders, & Luo, 2009; Gillette, Stevens, Watts, & Williams, 1999). However, there is no reason to believe that overconfidence is the only important personality trait that explains investor trading.

The focus of this study is on sensation-seeking. Sensation-seeking is a stable personality trait that has been studied in psychology for more than 40 years (Zuckerman, Kolin, Price, & Zoob, 1964). Zuckerman (1994) described it as "thrill and adventure seeking, disinhibition, experience seeking and boredom susceptibility." Sensation-seeking plays a natural role in explaining trading activities, because trading is a novel and dynamic experience that involves decision-making and financial risk. A sensation-seeking individual finds stock trading entertaining and derives utility from it. Sensation-seeking is a fundamentally different concept from overconfidence. In Odean's

(1998b) overconfidence theory of investor behavior, overconfident investors overestimate the precision of their private signals and thus create different prior information. In other words, overconfidence affects trading indirectly through the information channel. Overconfidence still complies with traditional asset pricing theory as overconfident investors trade to maximize their wealth. Sensation-seeking, on the other hand, affects trading directly as sensation-seeking investors simply enjoy trading and derive utility from it.

Sensation-seeking is also distinct from risk-taking (Grinblatt & Keloharju 2009). For example, putting all one's bets on one risky stock and holding it could fit a risk-taking investor's appetite, but a sensation-seeking investor would find such a strategy stale and uninteresting. Although risk-taking and sensation-seeking can occur independently (Pizam et al., 2004), there is a positive correlation between sensation-seekers and risk-taking activities in daily financial matters (Wong & Carducci, 1991). This paper tests such a relationship in section III to assure that this paper's result is not driven by a risk-taking property of sensation-seekers.

Grinblatt and Keloharju (2009) first examine sensation-seeking as a determinant of excess trading volume using a unique dataset from Finland on individual behavior. They use Finnish traffic-ticket history as a proxy for sensation-seeking and link individuals' traffic-ticket history to trading records. They find significant positive associations between whether a Finnish individual receives traffic tickets and her trading behavior, which is consistent with the sensation-seeking hypothesis.

My paper tests the sensation-seeking hypothesis with cross-country data. While the Finland data established a close link between sensation-seeking and trading volume at the individual level, there are two major limitations. First, traffic-ticket history is only a noisy proxy of sensation-seeking, as conceded by Grinblatt and Keloharju (2009): "Not all sensation seekers

are caught speeding, nor is sensation-seeking the only motivation for a speeding ticket." Second, it is not clear whether the evidence from Finland is generalizable to other countries with different cultures and market settings.

In addition to these issues of proxies and generalizability, another motivation for a cross-country analysis is to examine whether the individual effects of sensation-seeking play a role at the market level or, in other words, whether the market selection hypothesis applies here. The models under the market selection hypothesis suggest that in financial markets, investors may have different beliefs (for example, they may be overconfident or exhibit sensation-seeking), and only the investors with the correct belief and decision models can survive under a complete market (Alchian, 1950; Blume & Easley, 2006; Cootner, 1964; Fama, 1965; Friedman, 1953; Sandroni, 2000). Therefore, if the stock market is efficient to some extent, the impact of sensation-seeking investors might be negligible at the market level. In a cross-country study, Chui, Titman, & Wei (2010) find that overconfidence is a significant determinant of trading volume, which suggests that markets are not complete. It is not clear, however, how much of the effects of sensation-seeking on volume are incremental to those due to overconfidence.

In sum, this paper uses a clear identification of sensation-seeking to test whether it is a determinant of excess volume in a cross-country setting; I also address the challenges of using an alternative proxy for sensation-seeking, result generalizability, and market level impacts, and I disentangle the effects of overconfidence. This paper predicts, and confirms, that sensation-seeking serves as a significant determinant of excess volume around the world.

I test the following three hypotheses in this paper. First, I look for a positive association between the level of country-level sensation-seeking and total trading volume. Second, I test whether the market selection hypothesis applies to sensation-seeking investors. The market

selection hypothesis suggests that in financial markets, although investors may have different beliefs such as overconfidence, or have different preferences, such as sensation-seeking, only investors with correct beliefs and decision models can survive under a complete market (Alchian, 1950; Blume & Easley, 2006; Cootner, 1964; Fama, 1965; Friedman, 1953; Sandroni, 2000). Therefore, if the stock market is complete and investors are informed, the impact of sensation-seeking investors should become negligible at the market level. Third, I test for tail effect. Tail effect is directly derived from the conclusion of Grinblatt and Keloharju (2009), which suggests that the more sensation-seeking subgroup of the population, instead of the average investors, contributes disproportionally to excess volume.

2. Research Design

There is great variation in culture and personality among different countries (Hofstede, 2001). There is also notable variation of excess volume across different markets (Griffin, Nardari, & Stulz, 2007). Chui, Titman, and Wei (2010) used Hofstede's index of individualism as a measure of overconfidence, formed momentum strategy-based portfolios, and studied the association between overconfidence and momentum return. This paper follows their empirical strategy.

This paper relies on the following ordinary least squares (OLS) regression to test the sensation-seeking hypothesis:

$$LnVol_{it} = \alpha + \beta Sensation_i + \sum \delta_J Control_{Ji} + \varepsilon_i, \tag{3.1}$$

where $LnVol_i$ is the natural log of excess volume of country i in month t, $Sensation_i$ is the measure of country i's sensation-seeking, and $Control_{Ji}$ is a vector of J control variables. Our primary interest is the estimate of β. If sensation-seeking is an important determinant of trading, a country's level of sensation-seeking should be positively associated with country-level excess

volume (i.e., $\beta > 0$). Following Petersen (2009), I cluster standard error by both year and country, instead of using fixed effects.

This paper defines volume (Vol_{it}) as monthly turnover[17]. Turnover is measured by dividing total value traded over total market capitalization of country i during time t (e.g., Griffin et al., 2007). As Lo and Wang (2000) suggested, turnover controls for firm size and the growth of shares outstanding and shares traded. To be consistent with the previous literature on excess volume (Barber & Odean, 2000, 2001), this paper uses the natural logarithm of turnover as the dependent variable. The logarithm function standardizes the distribution of excess volume over different countries.

Our main variable of interest, $Sensation_i$, is the excitement-seeking score from the international survey conducted by McCrae and Terracciano (2005). Excitement-seeking is a good proxy because Zuckerman (1994) showed that excitement-seeking is identical to sensation-seeking, both theoretically and empirically. The international excitement-seeking score is constructed by McCrae and Terracciano (2005). I further discuss the details of their study in Section III.

In addition, my paper intends to distinguish risk-taking and sensation-seeking by testing two aspects of this concern: cultural effect and market effect. That is, do citizens in countries with a higher sensation-seeking score appear to take more risks, and do stock markets in countries with a higher sensation-seeking score show lower relative risk aversion? The difference is important because of the market selection hypothesis. For example, Blume and Easley (2006) point out that in a complete market, irrational investors will be driven out of the market, while the opposite is

[17] The excess volume has been defined either by the ratio of volume for a seven-day window near the release of earnings news, or by average stock turnover defined as volume over total shares outstanding. If we assume investors only obtain information near the release of earnings news, we can use the "earnings announcement" definition. If we assume that there is information flow, we can use the "average turnover" definition.

true in an incomplete market. Because there is international variation in market completeness, it is important to distinguish whether individuals are more behavior-biased or whether the markets are more behavior-influenced. To achieve such a goal, this paper includes two measures of risk: uncertainty avoidance from cultural studies by House, Javidan, Hanges, and Dorfman (2002), and relative risk aversion derived from consumption-based asset pricing models by Campbell (1999).

3. Data

3.1 Volume

I use securities data from Thomas Financial's Datastream International to calculate Vol_{it}; specifically, I use Datastream Global Equity Indices, which is a calculated index by Datastream that covers a minimum of 75-80% of total market capitalization. After matching Datastream volume data with excitement seeking scores, my sample includes 36 countries over the period from the base years to June 2013 (Table 3–1). Datastream uses the following equations to calculate market value and volume traded:

Datastream defines market value by:

$$MV_t = \sum_1^n (P_t * N_t), \tag{3.2}$$

where N_t is the number of shares in issue on day t, P_t is price on day t, and n is number of constituents in index.

Datastream defines volume traded by:

$$VA_t = \sum_1^n (VO_t * P_t), \tag{3.3}$$

where VO_t is volume of shares traded on day t, P_t is price on day t, and n is number of constituents in index.

The turnover, Vol_{it}, is obtained by taking the ratio of volume traded overmarket volume for country i in time t. $LnVol_{it}$ is the natural logarithm of Vol_{it}.

3.2 Sensation-seeking

One of the contributions of this paper is its use of the country-level excitement-seeking score from McCrae and Terracciano (2005), which identify and measures sensation-seeking directly. In their study, McCrae and Terracciano (2005) and their 78 collaborators from 51 cultures gathered observer-rating data from college students who rated college-age people or adults that they know. The study use a single questionnaire, the NEO-PI-R, to assess personality (NEO-PI-R manual, see Costa & MacCrae (1992)), and include 12,156 individuals.

The survey is designed based on a five-factor model, which is one of the most commonly used models in personality psychology (Digman, 1990). The five factors are Extraversion, Agreeableness, Conscientiousness, Neuroticism, and Openness to Experience. Excitement-seeking is one of the sub-traits, or facets, of Extraversion. McCrae and Terracciano described their methodology, analysis, and representativeness in two papers (McCrae & Terracciano, 2005a, 2005b). They reported the details of their surveys, including the means and standard deviations of excitement-seeking scores, for each culture in Chapter 10 of Vijver (2008). $Sensation_i$ is defined as the mean of excitement seeking scores of the culture in country i.

3.3 Control Variables

My paper follows Chui et al.'s (2010) model to define $Control_{Ji}$, which includes 1) degree of financial market development, FMD_{it}, measured by the ratio of stock market capitalization to gross domestic products (GDP) (Stulz & Williamson, 2003); 2) level of overconfidence, IDV_i, measured by individualism (Hofstede, 2001); and 3) GDP growth, $Growth_{it}$. The stock market

capitalization comes from Datastream, and GDP data come from The World Bank (The World Bank, 2013). The data for GDP and GDP growth are annual data measured in 2005 US dollars.

3.4 Measuring Risk Appetite

The measurement of the cultural aspect of risk-taking is the Uncertainty Avoidance from the GLOBE (Global Leadership and Organizational Behavior Effectiveness) project, denoted as UAI_i in this paper. The GLOBE project is also an international collaboration where scholars from 61 countries measure nine cultural dimensions of thousands of middle managers in various organizations (House et al., 2002). Higher UAI_i indicates higher uncertainty avoidance, and thus a higher degree of risk aversion, for an average citizen in country i.

The measurement of the market aspect of risk-taking comes from Campbell (1999). He derives a consumption-based asset-pricing model and calculates "relative risk aversion" with a measured return and the reported interest rate. Particularly, I chose to use his *RRA2* measure, which can be described with the following equation:

$$RRA_i = \frac{\overline{\alpha er_e}}{\sigma(er_e)\sigma(\Delta c)}, \tag{3.4}$$

where $\overline{\alpha er_e}$ is the average excess log return on stock over risk free rate, plus one half the variance of this excess return: $\overline{\alpha er_e} = \overline{r_e - r_f} + \sigma^2(r_e - r_f)/2$, $\sigma(er_e)$ is the standard deviation of excess return. $\sigma(\Delta c)$ is the standard deviation of the log consumption growth rate[18]. A higher $RRA2_i$ indicates a higher risk aversion of the market in a country that I indicated by the consumption CAPM.

[18] The reason I chose to report RRA2 rather than RRA1 is because as Campbell pointed out, RRA2 assumes a perfect correlation between excess return and consumption growth; it indicates the extent of an individual's risk aversion, rather than the low correlation between consumption and stock returns.

Campbell (1999) calculated risk aversion for 11 developed countries. Due to the coverage of his data source (International Financial Statistics from International Monetary Fund and bond data), an exact replication of his study with the same data source cannot be applied to all countries in my sample. Instead, this paper obtains the MCSI return index from Datastream to calculate r_e, the real interest rate from The World Bank to calculate r_f (The World Bank, 2013) and consumption volume from the OECD to calculate Δc (Organisation for Economic Co-Operation and Development). The calculated RRA_i has 25 observations.

3.5 Summary Statistics

For each country, Table 3–1 reports individualism scores obtained from Hofstede's study, excitement-seeking scores obtained from McCrae's study, $LnVol_{it}$ as defined above, the start month, and the country's $LnVol_{it}$ in the beginning month, or when t equals zero.

Table 3-1

4. Result and analysis

4.1 Sensation-seeking and risk-taking

To confirm that $Sensation_i$ does not pick up the variation in investors' different risk preferences, this paper measures the correlation among $Sensation_i$, uncertainty avoidance from the GLOBE project, and two relative risk-aversion measures reported in Campbell (1999). Table 3–2 reports the correlation matrix. If sensation-seeking investors consistently have higher risk tolerance, in either the cultural aspect or market aspect, one should observe a significant negative correlation between $Sensation_i$ and one of the risk-aversion measures.

Table 3-2

As shown in Table 3-2, the correlations between $Sensation_i$ and all three risk-aversion measures are not significant. Additionally, none of the three correlations exceed 0.35 in absolute value. Therefore, the main variable in this paper, $Sensation_i$, is not driven by investors' risk preference variations.

4.2 Sensation-seeking and excess volume

Table 3-3 reports the main result of this paper, i.e. the result of estimating equation (3.1). Following Chui et al. (2010), my paper uses a two-way cluster standard error for country and month. The reason for applying a two-way cluster is that we expect both a country effect and a time effect for excess volume. In other words, excess volume for a particular country in different months could be persistent; meanwhile, for a particular month, there could be events that cause the residuals of excess volume for a given month to be correlated across countries. Petersen (2009) suggests procedures to check for the existence of time or country effect by estimating the regression without clustering, and to cluster one-way. This paper reports results and with clustering in only one dimension and without clustering. As shown in Appendix A, the result from clustering by country is almost identical to clustering by both country and month, while no clustering and clustering by month shows dramatically different results. The results suggest that the time effect in this scenario was negligible, and the country effect was very strong.

As a consequence of clustering, the standard errors increase (under the full specification, the standard error of the coefficient of sensation-seeking increases from 0.00484 with no clustering to 0.0319 with two-way clustering). Because the cross-country sample size is very limited (36 countries with excitement-seeking score), if we estimate equation (3.1) with all control variables at once, the power of our test will be affected negatively. Accordingly, this paper also reports the results from separately estimating $Sensation_i$ with each control variable. Column (5) is the

estimation of equation (3.1) with full specification. Column (1) is the estimation of equation (3.1) without control variables. Column (2) to column (4) are estimations of equation (3.1) each using only one control variable: individualism, financial market development, and GDP growth, respectively.

Table 3-3

The sign on individualism is positive. This is consistent with Odean's (1998c) results that overconfidence is a determinant of trading volume. However, Chui et al. (2010) found a significant coefficient of individualism on excess volume, but the coefficient of individualism in this paper was not significant. This paper estimated equation (3.1) using the same sample period as in Chui et al. (2010), but I still did not get a significant coefficient. I requested sample data from Professor Chui. Comparing the sample data he provided, I obtained the key variables as he did for Hong Kong and Japan for the monthly turnover from the base years to June 2003. Therefore, I suspect this lack of significance is due to different control variables, because the correct control variable can reduce the noise reflected in the individualism index. Also, in Table 3-3, the t-statistic varies substantially under different specifications, which suggests that the result is very sensitive to different control variables. Unfortunately, at the time of writing this paper, data required to calculate some control variables, such as the Political Risk Index, were not available at Cornell University. I will include control variables used in Chui et al. (2010) in subsequent versions once the data become available.

The sign on financial market development was positive. This is consistent with the idea that a country with a better developed financial market should have more people involved in trading and, on average, should have a lower cost of capital that encourages trading activity. The

sign on GDP growth is negative, which suggests that developing countries have higher excess volume than developed countries.

This paper finds a positive coefficient of sensation-seeking on excess volume, but the coefficient is not significant. This contradicts the hypothesis that sensation-seeking is a determinant of trading volume. The result seems particularly surprising given the robust findings of Grinblatt and Keloharju (2009). This paper proposes three explanations for this lack of significance.

The first explanation is the market selection hypothesis. One of the objectives of this paper is to test whether the market selection hypothesis applies to sensation-seeking investors. Although the impact of sensation-seeking investors at the market level is small in Grinblatt and Keloharju (2009), this cannot be attributed to the market selection hypothesis because speeding-ticket history only captures a subset of sensation-seeking investors. The cross-country setting in this paper is a more direct test of the market selection hypothesis. The robust findings of Grinblatt and Keloharju (2009) supports the existence of a sensation-seeking effect on excess volume at the individual level. The non-significant result in this paper fails to reject the null hypothesis that sensation-seeking is not an important determinant of excess volume at the market level. The combination of these two findings supports the market selection hypothesis: sensation-seeking investors trade more than average at the individual level, but the share of wealth in the market by sensation-seeking investors is not enough to produce excess volume at the market level.

Griffin, Kelly, and Nardari (2010) suggested that there exists cross-country variation in market efficiency. They further argue that the level of market efficiency within a country can be quantified by the returns to trading strategies based on past returns and earnings announcements. The momentum strategy return in Chui et al. (2010) is one of the returns to trading strategies and

thus serves as a proxy for measuring the degree of market efficiency. Table 3-4 tests the prediction of a greater sensation-seeking effect in countries with a lower degree of market efficiency. My paper first ranked countries based on momentum strategy return calculated in Chui et al. (2010), then assigned a dummy variable *Low*. The dummy variable equals zero for the 19 countries with a higher momentum return and equals one for the 18 countries with a lower momentum return[19] to indicate a lower momentum return, and, thus, higher market efficiency. The prediction is that sensation-seeking investors have a greater impact on trading volume in less efficient markets, so we should expect sensation-seeking in a country with a lower momentum return (higher market efficiency) to have a lower effect on excess volume. In other words, the interaction of $Sensation_i$ and *Low* should have a significantly negative coefficient.

Table 3-4

The result in Table 3-4 is insignificantly negative. There are two features in the results that deserve our attention. First, the t-statistic for the coefficient of $Sensation_i$*Low is significantly greater than that of the coefficient for $Sensation_i$ in Table 3-3. This increase suggests that the degree of market efficiency indeed affects the sensation-seeking effect on excess volume. Second, the t-statistic for the individualism index dramatically increases and the coefficient is significant at 1% level under the new specification. This change of significance provides more evidence that results are sensitive to different control variables.

One should be careful about crediting this result to a correct prediction of the market selection hypothesis. The market selection hypothesis makes strong assumptions that investors are

[19] This paper also tested the estimation by assigning zero to low for the 18 higher momentum return countries and 1 for the 19 lower return countries, as well as dropping the three negative momentum return countries. The results do not change.

smart and rational—they utilize all information, use Bayesian logic to update their beliefs, maximize utility with rational expectations, and know the true value of parameters such as discount factors. Such investors may not exist. Meanwhile, the definition of market "completeness" in these models is different than the definition of market "efficiency" that Griffin et al. (2010) used. The difference between a complete market and an incomplete market is that in complete markets there are enough assets to span the states space, while in an incomplete market an investor cannot insure against all odds. The difference between an efficient market and an inefficient market in Griffin et al. (2010) depends on whether investors can gain extraordinary risk-adjusted returns from past public information, which is commonly known as "semi-strong form efficiency." The market selection hypothesis suggests that investors who are not smart or rational will vanish in complete markets, yet might survive or even dominate in incomplete markets. For example, in Blume and Easley (2006), an overconfident investor in an incomplete market could dominate the market eventually because she saves too much. These results, however, cannot be directly interpreted using a market efficiency idea. For instance, if a behavior-biased investor loses less in a less efficient market and loses more in a more efficient market, then, by the intuition of the market selection hypothesis, such an investor should make a larger impact in a more efficient market than in a less efficient market.

Without theoretical models, the estimation in Table 3-4 alone cannot establish a direct relationship between the market selection hypothesis and the non-significant coefficient on $Sensation_i$ in Table 3-3. Table 3-4 does tell us, however, that the sensation-seeking effect is greater in less efficient markets.

The second explanation is tail effects. Grinblatt and Keloharju (2009) showed that Finnish investors with speeding-ticket histories are more likely to trade. In my paper, one hypothesis that

predicts a significant coefficient of $Sensation_i$ is the following: countries where people have higher sensation-seeking scores on average are more likely to have higher excess volume. Grinblatt and Keloharju's (2009) conclusion is not equivalent to this assumption: people with a history of speeding tickets are not average people:they are the more sensation-seeking group of the population or, in other words, the tail. It is possible that trading volume is generated by this subgroup of people who are more sensation-seeking than average individuals.

To test this possibility, I construct $Sensation\_tail_i$, which equals the mean of the excitement-seeking score of country i plus two times the standard deviation of the excitement-seeking score of country i. I then re-estimate equation (3.1) but substitute $Sensation\_tail_i$ for $Sensation_i$.

Table 3-5

In the full specification in column (5), the coefficient of $Sensation\_tail_i$ is not significant; t-statistics of $Sensation\_tail_i$ as well as the adjusted R^2 increased under all specifications. Particularly, the coefficient of $Sensation\_tail_i$ in specification (1) is significantly positive at the 5% level. This paper also reports the estimation result by defining $Sensation\_tail_i$ as the mean of the excitement-seeking score of country i plus the standard deviation of the excitement-seeking score of country i, and mean of excitement-seeking score of country i plus three times the standard deviation of excitement-seeking score of country i in Appendix B. The t-statistics of the coefficient of $Sensation\_tail_i$ monotonically increases when adding more multiples of standard deviation of the excitement-seeking scores. For example, if we defined $Sensation\_tail_i$ as the mean of the excitement-seeking score of country i plus seven times the standard deviation of the excitement-seeking score of country i, the coefficient of $Sensation\_tail_i$ under specification (1) will be

significant at 1%, and the coefficients of $Sensation\_tail_i$ under specification (3) and (4) will be significant at 5%. The results suggest that the tail effect might explain part of the story here, as $Sensation\_tail_i$ proxies for scores of the more sensation-seeking subgroup of the population. However, because the constructed variable $Sensation\_tail_i$ may not be a proxy for the more sensation-seeking subgroup of the population, it is premature to claim that the sensation-seeking group of investors generates excess volume. To confidently identify that the "tail effect" is the key to understanding sensation-seeking and excess volume, we need more detailed data that classify the population into finer subgroups in terms of level of sensation-seeking, and then to study the sensation-seeking subgroups separately.

The third possible explanation is that sensation-seeking is simply a behavior attribute and cannot directly affect trading volume. Without a larger sample size, the non-significant result in this paper only suggests that we failed to reject the null hypothesis, but the power of our test is not strong enough to disqualify sensation-seeking as an important determinant of trading volume. If we are able to find a time-varying measure that can correctly represent the sensation-seeking difference in each country at different time periods, the test will be substantially more powerful. Alternatively, if scholars conduct cross-cultural studies similar to those of McCrae and Terracciano (2005a) that measures finer subgroups in each culture, we can also increase the power of our test.

Meanwhile, better control variables can also increase the accuracy of the estimation of sensation-seeking. As indicated in the analysis of Table 3-3 and Table 3-4, the most probable reason for the different level of significance in the individualism index in this paper compared to Chui et al. (2010) is the difference in control variables. Therefore, if the data required to replicate the control variables in Chui et al. (2010) become available, this paper can estimate the effect of sensation-seeking on excess volume more accurately.

5. Conclusion and directions for future research

This paper tests the hypothesis that sensation-seeking is a determinant of excess volume in a cross-country setting. After running an ordinary least squares regression of turnover on the excitement-seeking score and several control variables, this paper does not find a significant coefficient for sensation-seeking, suggesting that excess volume is significantly related to sensation-seeking in a cross-country setting. Subsample analysis further indicates that this lack of significance might be explained by different degrees of market efficiency, and might be explained by the tail effect. Although this paper is not able to validate the market selection hypothesis and the tail effect hypothesis, it provides tests that support the predictions of these hypotheses.

Besides the main result, this paper has three implications. First, sensation-seeking is not the same as risk-taking. Second, the cross-country results are sensitive to control variables. Third, the country effect predominates over turnover data.

Future research can focus on investigating different subsamples to identify where sensation-seeking affects trading activities and which subgroups of investors generate the results. For instance, recent work suggested that institutional investors generate a majority of trading volume in the United States. Jones and Lipson (2004) found that in 2002 non-retail trading was responsible for 94% of NYSE trading volume. However, the size of the holdings of cross-country institutional investors vary. Griffin et al. (2007) found institutional investors in Japan, Taiwan, Korea, and Thailand were responsible for 50, 18, 8, and 7% of all trading volumes, respectively. In addition, institutional investors held 44% of market capitalization of China A Share, which is the primary exchange in the country (Lu, 2007). The common perception is that institutional investors are sophisticated and thus less subject to behavior biases, such as sensation-seeking. If such an assumption is true, there might be a more significant effect of sensation-seeking on trading

volumes in countries where there is a low percentage of holdings by institutional investors. This paper was not able to find a sample with enough cross-country variation in institutional holdings. Nevertheless, the measurement of institutional ownership by large firms documented by Porta, Lopez- De- Silanes, and Shleifer (1999) also has the potential to serve as a proxy for institutional holdings and can be utilized to explain sensation-seeking effect.

Research can test the relationship between the market selection hypothesis and sensation-seeking effect on excess volume by comparing the performance of sensation-seeking investors across different countries. If it is indeed the case that sensation-seekers perform worse relative to the market in more efficient markets than in less efficient markets, we will be able to more confidently credit the lack of significance of the sensation-seeking effect on excess volume to the market selection hypothesis. Meanwhile, more data on sensation-seeking or on trading behavior are always valuable.

Last but not least, almost all theoretical financial models are based on the assumption that investors trade to maximize consumption and minimize risk. The possibility of other payoff functions, such as trading for sensation-seeking, deserves more consideration. Also, most finance models study complete markets versus incomplete markets, while not enough attention is paid to efficient markets versus inefficient markets. Researchers will truly understand the implications and importance of sensation-seeking effects only with good theoretical models that yield correct insights.

Tables

Table 3-1: Summary Statistics

Our sample consists of data on stock turnover from 36 markets around the world. We require each country in our sample to have a score on McCrae and Terracciano's cross-country survey. The turnover data are from Datastream. This table reports the name of the country/area. For each country/area, this table also reports its beginning month where data are available; its individualism index from Hofstede's study, $\mathrm{IDV_i}$; the excitement seeking score from McCrae and Terracciano's study, $\mathrm{Sensation_i}$; the average turnover across sample period, $\overline{\mathrm{LnVol_t}}$; and the turnover at beginning month, $\mathrm{LnVol_{i0}}$.

Countries	$\mathrm{IDV_i}$	$\mathrm{Sensation_i}$	$\overline{\mathrm{LnVol_t}}$	Beginning Month	$\mathrm{LnVol_{it}}$ at Beginning Month
Argentina	46	49.5	2.1	1993m7	1.5
Australia	90	54.9	3.8	1984m1	2.7
Austria	55	47.1	3.5	1986m8	2.1
Belgium	75	50.6	2.9	1986m1	1.6
Brazil	38	52.9	3.6	1999m1	3.7
Canada	80	52.7	3.2	1973m6	0.8
Chile	23	49.1	2.2	1989m7	2.9
China	20	47.9	4.2	1991m8	2.4
Czech Republic	57	42.9	3	1993m11	-6.3
Denmark	74	47.2	3	1988m4	-1.2
Finland	63	50.5	3.3	1988m3	-0.6
France	71	47.5	3.6	1988m6	-1.1
Germany	67	46.1	2.7	1988m6	4
Hong Kong	25	47.5	3.6	1988m6	3.6
India	48	50.4	3.3	1995m1	1
Indonesia	14	49.7	2.9	1990m4	3.3
Ireland	70	52.9	3.8	2001m1	1.8

Italy	76	46.5	3.7	1986m7	1.6
Japan	46	50	3.9	1990m12	3.1
Korea	18	49.4	4.4	1987m9	3.9
Malaysia	26	49.1	2.8	1986m1	1.4
New Zealand	79	54.9	3.3	1990m1	2.4
Peru	16	51	2	1994m1	3.4
Philippines	32	51.2	2.5	1990m1	0.8
Poland	60	47.8	3.4	1994m3	4.8
Portugal	27	52.6	3.1	1990m1	-1.2
Russia	39	49	2.9	1998m1	0.8
Slovenia	19	46.3	2.5	1999m1	3.6
Spain	51	46.3	4.2	1990m2	2.2
Switzerland	68	45.2	3.9	1989m1	0.2
Thailand	20	49.8	3.7	1987m1	3.9
Turkey	37	51.1	4.1	1988m1	1.3
UK	89	53.2	4.1	1986m10	2.1
US	91	54.2	4.2	1973m6	2.2

Table 3-2: Correlations Between Sensation-seeking and Different Risk-taking Measures

The pairwise correlation matrix between three risk aversion measures and Sensation_i. RRA1_i and RRA2_i were obtained from Campbell (1999). UAI_i is the uncertainty avoidance index reported in the GLOBE project. For each correlation, p-value is reported in parentheses and number of observations is reported in the third row.

	RRA1_i	RRA2_i	UAI_i	Sensation_i
RRA1_i	1			
(p)				
n	11			
RRA2_i	-0.0752	1		
(p)	(0.8262)			
n	11	11		
UAI_i	0.327	0.138	1	
(p)	(0.3267)	(0.6860)		
n	11	11	39	
Sensation_i	-0.341	0.309	-0.127	1
(p)	(0.3694)	(0.4184)	(0.5122)	
n	9	9	29	34

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3-3: Sensation-seeking and Excess Volume

OLS estimates the coefficients related to trading volume under different specifications. The market trading volume of country i in month t ($\mathrm{Vol_{it}}$) is measured as the market's value traded in month t, aggregated by daily value traded, divided by the market's capitalization in month t, both in local currency. The natural logarithm of monthly market trading volume of country i in month t ($\mathrm{LnVol_{it}}$) is regressed on McCrae and Terracciano's excitement seeking score ($\mathrm{Sensation_i}$), Hofstede's individualism index ($\mathrm{IDV_i}$), financial market development measured by market i's market capitalization divided by its GDP ($\mathrm{FMD_{it}}$), and GDP growth ($\mathrm{Growth_{it}}$). Column (5) is the estimation of $\mathrm{LnVol_{it}}$ for all four variables. Column (1) is the estimation of $\mathrm{LnVol_{it}}$ for $\mathrm{Sensation_i}$. Column (2) to column (4) are estimations of $\mathrm{LnVol_{it}}$ for $\mathrm{Sensation_i}$ and one control variable: individualism, financial market development and GDP growth, respectively. The sample period is from base year for each market reported in Table 3-1 to January 2013. This paper follows Petersen (2009) and Chui et al. (2010) and clusters standard error by both country and month. Robust t-statistics are in parentheses.

	(1)	(2)	(3)	(4)	(5)
	$\mathrm{LnVol_{it}}$	$\mathrm{LnVol_{it}}$	$\mathrm{LnVol_{it}}$	$\mathrm{LnVol_{it}}$	$\mathrm{LnVol_{it}}$
$\mathrm{Sensation_i}$	0.0290	0.0122	0.0248	0.0368	0.0180
	(0.93)	(0.42)	(0.80)	(1.11)	(0.57)
$\mathrm{IDV_i}$		0.00673			0.00644
		(1.45)			(1.40)
$\mathrm{FMD_{it}}$			510.2		1122.2
			(0.38)		(0.94)
$\mathrm{Growth_{it}}$				-0.0622	-0.0422
				(-1.24)	(-0.86)
Constant	1.932	2.419	2.166	1.692	2.214
	(1.24)	(1.75)	(1.42)	(1.04)	(1.47)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.005	0.025	0.006	0.024	0.041

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3-4: Market Selection Hypothesis

OLS estimates the coefficients related to trading volume under different specifications as in Table 3-3, with the addition of a dummy variable, Low, and the product of Sensation_i and Low, Sensation_i*Low. Low takes a value of one for the 18 markets with lower momentum returns in Chui et al. (2010) and Low takes a value of zero for the 19 markets with higher momentum returns.

	(1)	(2)	(3)	(4)	(5)
	LnVol_{it}	LnVol_{it}	LnVol_{it}	LnVol_{it}	LnVol_{it}
Sensation_i	0.0347	0.000179	0.0303	0.0569	0.00706
	(1.05)	(0.01)	(0.89)	(1.60)	(0.23)
Sensation_i*Low	-0.0307	-0.0644	-0.0341	-0.0683	-0.0830
	(-0.40)	(-1.38)	(-0.48)	(-0.94)	(-1.64)
Low	1.828	3.944	2.102	3.914	5.002
	(0.48)	(1.65)	(0.59)	(1.10)	(1.92)
IDV_i		0.0159***			0.0172***
		(4.28)			(5.49)
FMD_{it}			350.6		1504.3*
			(0.32)		(2.31)
Growth_{it}				-0.0896*	-0.0431
				(-1.99)	(-1.14)
Constant	1.509	2.195	1.727	0.529	1.808
	(0.92)	(1.76)	(1.02)	(0.30)	(1.25)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.023	0.096	0.037	0.075	0.144

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3-5: Tail Effect

This table reports the OLS estimates the coefficients related to trading volume under different specifications as in Table 3-3, but substitute Sensation_i with Sensation_tail_i . Sensation_tail_i is constructed by summing Sensation_i and two times standard deviations of excitement seeking scores reported in Vijver (2008).

	(1)	(2)	(3)	(4)	(5)
	LnVol_{it}	LnVol_{it}	LnVol_{it}	LnVol_{it}	LnVol_{it}
Sensation_tail_i	0.0478*	0.0330	0.0435	0.0427	0.0293
	(1.98)	(1.26)	(1.81)	(1.77)	(1.04)
IDV_i		0.00497			0.00475
		(1.03)			(0.93)
FMD_{it}			550.2		1031.0
			(0.43)		(0.89)
Growth_{it}				-0.0530	-0.0429
				(-1.11)	(-0.89)
Constant	0.0745	0.836	0.399	0.562	1.186
	(0.04)	(0.49)	(0.24)	(0.34)	(0.66)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.024	0.033	0.023	0.036	0.047

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix

Appendix A

Relationship Between Calculated $\mathbf{RRA2_i}$ and $\mathbf{Sensation_i}$

The relationship between calculated $RRA2_i$ and $Sensation_i$. $RRA2_i = \frac{\overline{\alpha er_e}}{\sigma(er_e)\sigma(\Delta c)}$, where $\overline{\alpha er_e}$ is the average excess log return on stock over risk free rate, plus one half the variance of this excess return: $\overline{\alpha er_e} = \overline{r_e - r_f} + \sigma^2(r_e - r_f)/2$. $\sigma(er_e)$ is the standard deviation of excess return. $\sigma(\Delta c)$ is the standard deviation of the log consumption growth rate. Higher RRA_i indicates higher risk aversion .This paper obtains MCSI return index from Datastream to calculate r_e, real interest rate from World Bank to calculate r_f, and consumption volume from OECD to calculate Δc . Panel A reports the summary statistics, and Panel B reports the OLS estimation of $Sensation_i$ on $RRA2_i$. Robust t-statistics is in the parentheses.

Panel A: Summary Statistics

Countries	$RRA2_i$	$Sensation_i$
Australia	-12.1	54.9
Austria	-5.6	47.1
Brazil	-2.8	52.9
Canada	-6.3	52.7
Chile	-2.4	49.1
Czech Republic	-1.5	42.9
Denmark	-1.4	47.2
Finland	-3.3	50.5
France	-9	47.5
Germany	-1.4	46.1
Indonesia	-4	49.7
Ireland	-11.1	52.9
Italy	-2	46.5
Japan	-2.5	50

Korea	-4.3	49.4
Mexico	-5.8	49.3
New Zealand	4.6	54.9
Poland	-4	47.8
Portugal	-2.2	52.6
Slovenia	-2.7	46.3
Spain	-11.7	46.3
Switzerland	-0.2	45.2
Turkey	-4.2	51.1
United Kingdom	-3.4	53.2
United States	-13.2	54.2

Panel B: Regression

	$Sensation_i$
$RRA2_i$	-0.185
	(-0.82)
Constant	48.78***
	(36.77)
Observations	25

Appendix B

Different Clustering Treatments

OLS estimates the coefficients related to trading volume under different specifications as in Table 3-3, with different clustering treatments. Panel A reports the estimation results without clustering. Panel B reports the estimation results clustered by month. Panel C reports the estimation results clustered by country.

Panel A: No Cluster					
	(1)	(2)	(3)	(4)	(5)
	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$
$Sensation_i$	0.0290***	0.0122**	0.0248***	0.0368***	0.0180***
	(7.12)	(2.66)	(6.46)	(9.61)	(3.73)
IDV_i		0.00673***			0.00644***
		(13.66)			(9.97)
FMD_{it}			510.2***		1122.2***
			(4.72)		(10.42)
$Growth_{it}$				-0.0622***	-0.0422***
				(-11.48)	(-6.88)
Constant	1.932***	2.419***	2.166***	1.692***	2.214***
	(9.39)	(11.13)	(11.14)	(8.79)	(10.16)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.005	0.025	0.006	0.024	0.041

Panel B: Cluster by Month					
	(1)	(2)	(3)	(4)	(5)
	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$
$Sensation_i$	0.0290***	0.0122**	0.0248***	0.0368***	0.0180***
	(7.12)	(2.66)	(6.46)	(9.61)	(3.73)
IDV_i		0.00673***			0.00644***
		(13.66)			(9.97)
FMD_{it}			510.2***		1122.2***
			(4.72)		(10.42)
$Growth_{it}$				-0.0622***	-0.0422***
				(-11.48)	(-6.88)

Constant	1.932***	2.419***	2.166***	1.692***	2.214***
	(9.39)	(11.13)	(11.14)	(8.79)	(10.16)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.005	0.025	0.006	0.024	0.041

Panel C: Cluster by Country

	(1)	(2)	(3)	(4)	(5)
	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$
$Sensation_i$	0.0290	0.0122	0.0248	0.0368	0.0180
	(0.92)	(0.42)	(0.80)	(1.10)	(0.56)
IDV_i		0.00673			0.00644
		(1.45)			(1.39)
FMD_{it}			510.2		1122.2
			(0.38)		(0.94)
$Growth_{it}$				-0.0622	-0.0422
				(-1.24)	(-0.86)
Constant	1.932	2.419	2.166	1.692	2.214
	(1.24)	(1.74)	(1.41)	(1.04)	(1.46)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.005	0.025	0.006	0.024	0.041

Appendix C

Alternative Definitions of $Sensation\_tail_i$

OLS estimates the coefficients related to trading volume under different specifications as in Table V, with different definitions of $Sensation\_tail_i$.

Panel A: $Sensation\_tail_i$=$Sensation_i + Sensation\_SD_i$					
	(1)	(2)	(3)	(4)	(5)
	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$
$Sensation\_tail_i$	0.0438	0.0263	0.0392	0.0432	0.0265
	(1.57)	(0.92)	(1.42)	(1.50)	(0.85)
IDV_i		0.00584			0.00549
		(1.22)			(1.11)
FMD_{it}			535.0		1077.2
			(0.41)		(0.92)
$Growth_{it}$				-0.0584	-0.0440
				(-1.19)	(-0.90)
Constant	0.770	1.509	1.074	0.956	1.593
	(0.46)	(0.94)	(0.65)	(0.57)	(0.92)
Observations	10064	10064	9193	9189	9189
Adjusted R^2	0.015	0.028	0.014	0.031	0.044

Panel B: $Sensation\_tail_i$=$Sensation_i + 3 * Sensation\_SD_i$					
	(1)	(2)	(3)	(4)	(5)
	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$	$LnVol_{it}$
$Sensation\_tail_i$	0.0461*	0.0343	0.0424*	0.0395	0.0287
	(2.23)	(1.49)	(2.05)	(1.95)	(1.19)
IDV_i		0.00433			0.00428
		(0.90)			(0.83)
FMD_{it}			557.6		994.4
			(0.44)		(0.87)
$Growth_{it}$				-0.0481	-0.0406
				(-1.02)	(-0.86)
Constant	-0.249	0.449	0.0672	0.391	0.971
	(-0.15)	(0.26)	(0.04)	(0.25)	(0.56)
Observations	10064	10064	9193	9189	9189

Adjusted R^2	0.030	0.037	0.029	0.040	0.048

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