What is the Causal Effect of IPO on Firms' Investment?

Evidence from a Natural Experiment[1]

Yuzheng Sun[2]

This Draft: October 5th, 2016

Abstract

This paper provides strong evidence of a causal effect of IPO on firms' investment. Utilizing the 2012 Chinese IPO moratorium, an event that independently sorted IPO permission to two groups of similar IPO applicant firms, allowing 154 firms to go public on schedule, while delaying the IPO process for over a year of another 332 firms, I revisit the question of whether firms invest more after they go public. Using a difference-in-difference method to quantify the treatment effect, I find that firms who go public on schedule make significantly more investment on fixed assets after their IPO compared to similar firms who faced a one-year delay in IPO process. The increase in investment is pronounced, on average is equal to 18.5% of the firms' pre-IPO fixed asset, thus generating policy implications. My findings suggest that a loosening of financial constraints outweighs potential agency problems during the process of IPOs.

JEL Classification Number: G31, C21, D22

Keywords: Financial constraints, IPO, governmental intervention, difference-in-difference

---

[1] I am very grateful to my dissertation committee: Crocker Liu (chair), Warren Bailey, Andrew Karolyi and Charles Trzcinka. In addition, I am extremely indebted to Roni Michaely, Murillo Campello, Ting Xu and Fang Zhang for all their suggestions. I have benefited from the input of seminar participants at Cornell University, and conference participants at the London Business School TADC and the 2015 Northern Finance Association meeting, as well as discussions with Zhuo Zhong, Janet Gao, Xiaomeng Lu, Joseph Pacelli and Shuai Ye. All errors are my own.

[2] Affiliation: Ph.D. candidate in Economics, Cornell University. Email: ys543@cornell.edu

What is the Causal Effect of IPO on Firms' Investment? Evidence from a Natural Experiment

1. Introduction

The Initial Public Offering (IPO) is one of the most significant events of a firm. An IPO drastically changes two aspects of a firm: financial constraints and the agency problems associated with public 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. Introducing public shares and forcing the firm to disclose their quarterly performance can exacerbate the firms' agency problem through issues like managerial short-termism.[3]

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 to avoid financial distress and the higher cost of capital (Almeida, Campello, & Weisbach, 2004; Fazzari, Hubbard, & Petersen, 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 behaviors can lead public firms to under-invest because some long-term investment can hurt short-term earnings (Asker, Farre-Mensa, & Ljungqvist, 2014; Graham, Harvey, & Rajgopal, 2005; Jensen & Meckling, 1976; Stein, 2003).

The purpose of this study therefore is to examine how does IPO changes a firm's investment behavior. The answer is not obvious, as the results of prior empirical studies are mixed. For example, Pagano, Panetta, and Zingales (1998) find that Italian firms decrease investment after their IPOs. Chemmanur, He, and Nandy (2010), on the other hand, find that U.S. firms increase investment after their IPO. Their explanation of these results represents the opposite predictions of the lifecycle theory of firms. This theory suggests that a firm conducts an IPO at a certain stage of its lifecycle. Yet, firms may reach their productivity peak and then initiate IPOs to monetize higher valuations or firms may initiate IPOs to raise capital in order to increase productivity. The former leads to a decrease in investment after IPOs and the latter leads to an increase after IPOs. Therefore, the causal effect of IPO on investment remains an empirical question.

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

THE CAUSAL EFFECT OF IPO ON FIRMS' INVESTMENT

Attempts to identify empirically the treatment effect of IPOs on investment are complicated given that firms endogenously choose whether to conduct an IPO and when to do so. Selection bias, such as the inherent difference between firms who undertake IPOs and those who 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 bridge the gap, I take advantage of an experimental event at the national level to explore the effect of IPO on a firm's investment behavior. I argue that this event is comparable to a randomized assignment of at least a one-year delay in the IPO event for firms that have applied for IPO status. These applicant firms are similar in both cross section and in time series. Using this exogenous event, I test the causal effect of IPOs on firms' subsequent investment behavior. The exogenous event in my study is the 2012 Chinese IPO moratorium: In November, 2012, the Chinese stock market supervising authority, the Chinese Securities Regulatory Commission (CSRC), initiated an unannounced IPO moratorium. The CSRC's Public Offering Review Committee (PORC), which reviews firms' IPO applications and grants IPO permissions, froze reviews. The reason for the moratorium is the CSRC's political mission – to keep the stock market stable. The CSRC views IPOs as a substitute for existing shares. When the stock market is not performing well, the CSRC halts IPOs in an attempt to stabilize the price for existing shares (Details of the moratorium are provided in Section 2). According to the disclosed list of the CSRC, over 600 Chinese firms had submitted their applications and were waiting for their review at the time of the moratorium. On December 19th, 2013, the PORC started their reviews again, signaling the end of the moratorium. Eventually, 379 moratorium-affected firms were listed in 2014 and 2015 (in Section 2, I address how the selection of firms could affect my results), but their IPO process was delayed by at least a year due to the moratorium. It denied the applicant firms' access to both the stock market and the bond market, as the Chinese corporate bond market is hardly available to private firms. In my study, I define the treatment as the event of listing, the treatment group as the 154 firms that were listed in 2012 before the moratorium, and the control group as the 332 moratorium-affected firms. In Section 3, I demonstrate that the treatment group and the control group have similar characteristics before the moratorium. Based on the PORC review meeting data, both the treatment group and the control group firms submitted their application within a year. Since, in the Chinese stock market, it generally takes three years to prepare for IPO applications and usually another year for application approval, I argue that firms could not predict the moratorium *ex ante*. Conditional on being a Chinese IPO applicant firm from the end of 2011 to the end of 2012, the treatment of an IPO is exogenous to the firms' characteristics and choices.

THE CAUSAL EFFECT OF IPO ON FIRMS' INVESTMENT

Utilizing a difference-in-difference (DID) strategy, I find that IPOs generate a positive effect on firms' investment. The results are both statistically and economically significant– the average treatment effect of an IPO on a treated firm's investment spending is equal to 18.5% of its level of fixed assets in 2011. As the moratorium is exogenous, the effect can be neatly attributed to the difference in receiving the IPO treatment between the two groups. This positive effect 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 the prediction of lessened financial constraints. I find that an IPO increases the treated firms' cash balance and reduces their cash spending on financing activities. These are two mechanical results following IPOs. They demonstrate that going public can reduce firms' distress risk and financial cost in order to reduce their financial constraints. Although the treated firms decrease their liabilities for the first year after their IPOs, they substantially increase their liabilities in subsequent years. The increase in liabilities is consistent with firms being previously financially constrained. 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 two results suggest that IPOs alleviate a firm's financial constraints not only by providing capital but also by enabling firms to raise more debt financing. This directly results in the IPO firms growing larger. 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.

The positive effect of an IPO on investment suggests that firms conduct IPOs to reach their productivity potential, rather than to monetize high valuation after their productivity has peaked. The setting of my study provides a unique opportunity to test this implication regarding the lifecycle theory of firms. The main empirical evidence of firms conducting an IPO after their productivity has peaked is their well-documented post-IPO operating underperformance. Empirical studies using return on assets (ROA) or net profit scaled by assets or sales to measure firms' operating performance find that it decreases after going public compared with either a firm's own pre-IPO levels or with similar private firms. Scholars usually interpret this pattern as indicating that firms reach their productivity peak prior to IPO; they choose to go public after the peak to capture higher valuation. The underlying assumption is that an IPO only serves as a signal and, without an IPO, firms would experience the same operating underperformance pattern. In my sample, however, I find that an IPO is the cause of this pattern rather than a signal. The treatment group experiences a decrease in operating performance after their IPO, while similar firms in the control group simultaneously maintain a steady growth. My result suggests that pre-

IPO firms do not reach their productivity peak. Why does an IPO cause operating underperformance? My interpretation is that investment can take multiple years to realize benefits while increasing depreciation and thus reducing earnings in short term. Therefore, the post-IPO operating underperformance can be due to increased investment. This result provides concrete empirical evidence for the lifecycle theory of firms, which holds that a firm's productivity peak, if it exists, is more likely to happen after rather than before its IPO.

Overall, the study makes three distinct contributions to the existing literature. First, it contributes to the IPO literature by providing a quasi-experimental setting to study the causal effect of IPOs on firms' investment and to quantify this effect. The large magnitude of my result – 18.5% of its 2011 fixed assets – suggests that an IPO can profoundly increase firms' investment and consequently affects the real economy, as corporate investment is one of the key drivers of economic growth. The study also provides causal evidence on the relationship of IPO to firms' productivity implied by the lifecycle theory of firms. I find that an IPO helps a firm to increase productivity rather than signaling that a firm's productivity has peaked and that its net profit, at least in the short term, is an inaccurate measure for firms' productivity.

Second, the results further findings in recent papers that compare the difference in investment behavior between public and private firms (Asker et al., 2014; GILJE & Taillard, 2016). By studying a particular group of firms – IPO applicants – I provide a more dynamic picture to their findings. My result indicates that for Chinese IPO applicants, a group of firms that are large, mature and profitable, financial constraints have a much larger effect than agency problems in determining post-IPO investment behaviors. This result stands in contrasts to much of the previous literature which assumes that financial constraints are less – or even not – binding for large firms.

Finally, as argued by Shan & Zhu (2015), problematic IPOs are one of the main contributors to China's poor stock market performance. Thus, this paper has potential policy implications. To the best of my knowledge, it is the first attempt to use quasi-experimental methods to quantify the real effect of the 2012 Chinese IPO moratorium. Because the CSRC's political mission is to stabilize the market, the IPO applicant firms' performance 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 and economic growth caused by the IPO moratorium. This study has particular relevance to the recent historically high amount of IPO applicant firms. Many previous papers talk about the positive effect of a healthy financial market to promote economic growth; my paper quantifies this effect 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 IPO. 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, Karolyi, and Stulz (2013), total Chinese IPO proceeds have cumulatively exceeded those of the U.S. since 2000 and China has become the world's largest and fastest growing IPO market. However, this vibrant market is heavily regulated, sometimes even suppressed, by the Chinese Securities Regulatory Commission (CSRC), which supervises the Chinese IPO market through a merit-based regulation system. Under this system, an IPO applicant firm has to satisfy certain size and profitability thresholds to qualify for an IPO application. The precise conditions are reported in Appendix A. This regulation ensures that only mature, profitable and large companies that are as good as public companies can apply for an IPO in China and is designed to protect retail investors who have an information disadvantage. The process of preparing to apply for an IPO usually 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. As the applicant firms all satisfy the required conditions, the approval decision is mostly based on the truthfulness of the reports and the PORC's judgement of the applicant's future earning abilities. The PORC consists 25 members, mostly lawyers, auditors, with some government officials and scholors.

Applications are reviewed in the order in which they were received. In 2012, the approval rate of the review meetings was over 80%. Before the 2012 moratorium, the wait time for the review meeting was usually three to six months, but it drastically increased following the moratorium. This unique regulatory procedure of the Chinese market limits the diversification of public companies but 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 structures of the Chinese IPO market ensure that the applicant firms within a short period, such as a year, are *qualitatively comparable* both in cross section and in time series. I argue that the length of the preparation stage and reviewing process made companies affected by the moratorium unlikely to expect it when they first decided to conduct their IPOs three or four years ago. I provide evidence for this claim in the robustness test section.

On Feb. 1st 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*. However, only the record of *the PROC review meeting date* is complete

for each company; the records of the other three dates are often not. In most cases, there is only one date out of the three recorded. The poor bookkeeping of these records also affects the accuracy of reported dates: the PORC started to disclose the applicant list on Feb. 1st 2012, and if they did not document the data before the disclosure, they recorded the feedback date as Feb. 1st 2012. This results in a cluster of records for Feb. 1st 2012 in my sample. To clearly identify firms that applied to IPO before the moratorium as the control group, I use the method that is most unfavorable to my hypothesis: I construct a variable, *Review Date*, that is equal to the earliest of the four dates for each applicant firm. *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 Oct. 19th 2012, the date of the last reviewing meeting before the moratorium, undoubtedly applied to IPO before the moratorium. I define the treatment group 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 Oct. 19th 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 *Listing Date* for both the treatment group and the control group. It demonstrates that 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 stage of Chinese IPO applications, the treatment group and the control group are at a similar stage of their lifecycles.

[Insert Figure 1 here]

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 Dec. 6th 2008 to June 29th 2009, one brief moratorium in mid-2012, one from Nov. 3rd 2012 to Dec. 30th 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 to the moratorium that occurred between Nov. 3rd 2012 to Dec. 30th 2013 and study only this moratorium.

Based on the IPO applicant list published by the PROC, there were over 600 firms that submitted their application and were waiting for the review meeting in Nov. 2012, while previous IPO moratoriums only affected dozens of firms. There was no official announcement of either the beginning 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 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 Oct. 19th 2012, and, without any official announcement, the PROC halted review meetings. The firms who got approved at the Oct. 19th meeting went

public on Nov. 3rd 2012. The market quickly interpreted the halt of review meetings as an IPO moratorium. However, 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 Nov. 3rd 2012 to Dec. 19th 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. Nevertheless, the motivation of the CSRC for first initiating the moratorium was clear and simple: their political mission is 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 put downward pressure on current stock prices (Sun & Tong, 2003). Therefore, the CSRC put a halt on IPOs in an attempt 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 Aug. 2012, yet by around Oct. 2012 it was regularly requested by the media and retail investors.

Whether an IPO moratorium can help stabilize the price of current stocks remains an open question (Packer & Spiegel, 2016), but 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 perspective of the CSRC strengthens the likelihood of the moratorium being an exogenous event to the applicant firms. Neither the firms' characteristics, nor their lobbying efforts, is likely to have much impact on the 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. With this list, I can identify which firms were affected by the moratorium and which firms applied to IPO before it. The second regulatory event occurred at the beginning 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 gives me the 2011 financial statements for all moratorium-affected firms, which is essential to ensuring the similarity of the treatment group and the control group.

2.3 Survival Bias:

Survival bias is a potential concern for the validity of my experiment. Specifically, moratorium-affected firms could choose to drop out of the pipeline, and 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, to buy shell companies, or to be acquired by other firms. [4] [5] 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 pipeline is a choice, it biases against my result: my study aims to test the causal effect of 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, and the IPO effect should be stronger among them. Excluding these firms in my sample implies that my findings are understated.

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 come from Wind Financial Terminal (WFT).[6] WFT could be considered the Chinese counterpart to Bloomberg, Datastream and Compustat. It serves more than 90% of financial enterprises in the Chinese market and over 75% of Qualified Foreign Institutional Investors.[7] Through WFT, I have access to 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, and 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 Oct. 19th, 2012, were affected by the moratorium, and eventually got listed. There are

[4] 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.

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

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

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

332 firms in the control group. Therefore, my sample contains the 486 firms that were listed after 2012 and applied to IPO between Jan. 1st 2011 and Oct. 19th 2012.

I combine the firms' pre-disclosed financial statements before their IPO with their disclosed financial statements after their IPO and construct a sample of financial statement variables from 2009 to 2015. My sample is an unbalanced panel data set, though. 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 I use are from firms' annual reports, measuring their end-of-year performance when applicable.

3.2 Variable Construction

My variable of interest is the change in investment. Following the convention of the corporate finance literature and adapting to the Chinese accounting practices and my sample, I construct a variable, *InvestmentRate*, to proxy for firms' investment spending. *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. *InvestmentRate* is equal to the ratio of the difference over the firm's level of fixed assets in 2011. I use *InvestmentRate* as the analog of capital expenditure that is commonly used in the corporate literature to measure firms' investment. 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 cash flow statements are relatively more credible than from 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, so studying the cash spent on fixed assets has more straightforward implications for the real impact of an IPO. The alternative of using *InvestmentRate* is the change in firms' fixed assets (ΔK), featured in Campello, Ribas, and Wang (2014) to proxy for investment for Chinese listed firms. However, I only have a short period (2011-2015 for the control group firms). For the purpose of identification in my study, the level of investment is as important as the change in investment in ensuring that the treatment group and the control group are comparable.

Thus, I choose to use the yearly investment rate, rather than the change in fixed assets, to keep the valuable information from 2011.

To ensure that the treatment group and the control group are comparable, I use a large number of covariates to control for potential investment opportunities, firm characteristics, and a firm's equity-dependent status. Specifically, my 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 industry Herfindahl index in terms of sales to represent industry competition. Because IPO drastically changes firms' characteristics, I only use the firms' characteristics in 2011 as control variables. For readers' convenience, I list all my variables with their definitions in Table 1.

[Insert Table 1 here]

I refer to two types of literature when selecting the control variables: literature that studies the real impact of financial constraints such as Hadlock and Pierce (2010) and literature that studies the Chinese stock market such as Campello et al. (2014). Chinese accounting practices are different than those of the United States. Thus, I choose variables that measure the elements that interest scholars studying the real impact of financial constraints and that also reflect the most stable and accurate information available through Chinese accounting practices. I also control for the listing conditions required by the CSRC.

3.3 Descriptive Statistics

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

[Insert Table 2a here]

[Insert Table 2b here]

From Panel A, 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, but 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.

Panel B 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. For our variable of interest, *InvestmentRate*, the ratio is at 95%. The other variables are at similar scales. 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 treatment of IPO causes the two groups to diverge.

3.4 Identification Strategy

Previous studies attempting to link IPOs and investment usually do not try to infer causality due to the endogeneity nature of 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 limits to the firms that are prone to market timing. In contrast, my study furthers our understanding of IPO firms by investigating a local, but precipitous, shock that made a larger impact on a broader group of firms. The firms in my sample were determined to conduct IPOs despite the dire market conditions[8] and the uncertain future posed by the moratorium.

The 2012 Chinese IPO moratorium is an exogenous shock on applicant firms' permission for listing. The moratorium independently sorts IPO permissions to two otherwise similar groups of firms. One group successfully went public on schedule before the moratorium, and the other group's IPO processes were delayed by the moratorium for over a year. As demonstrated in previous sections, the two groups are similar both qualitatively and quantitatively. Since the objective of this paper is to study the treatment effect of IPOs, I define the groups that were listed before the moratorium as the treatment group and define the moratorium-affected firms as the control group.

[8] 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.

THE CAUSAL EFFECT OF IPO ON FIRMS' INVESTMENT

As shown in Angrist and Krueger (1999), a 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. The difference is the definition of treatment time and different sample period. Equation (1) uses event time – year of IPO – as the treatment time:

$$y_{it} = \alpha + \beta d_{it} + wT_i + \theta d_{it} T_i + Z_{it}\delta + \varepsilon_{it} \tag{1}$$

Here, y_{it} is my variables of interest, *InvestmentRate*; T_i is an indicator variable that equals one if frims belong to the treatment group and zero otherwise; d_{it} is an event dummy that equals one for the year of and the years after firm *i* goes public and zero before. And Z_{it} is a vector of control variables. θ gives the DID estimate of the effect of an IPO on $\mathrm{y_{it}}$. 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 dynamic treatment effect equation estimates the treatment effect on a firm's investment of being able to conduct an IPO on schedule versus having to receive at least a one-year delay 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 my sample are listed. Thus, the treatment is not IPO vs. non IPO, but is on schedule IPO vs. delayed IPO. The shortcoming of equation (1) 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.

Equation (2) uses 2012 as the treatment time:

$$y_{it} = \alpha + \beta d_t + wT_i + \theta d_t T_i + Z_{it}\delta + \varepsilon_{it} \tag{2}$$

All variables follow the same definition as equation (1), but d_t no longer has subscript *i*, as all firms are considered treated after 2012. The sample period for equation (2) is 2011-2013. Equation (2) operates on a straightforward intuition: 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, and 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 can be fully credited to the differing status of their IPO. The treatment here is IPO vs. non IPO. 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 2b. However, due to data limitations, these pieces of evidence are not conclusive. For example, I do not have the control group's

data beyond 2011, so I cannot verify that the pre-tends of the two groups are the same. Therefore, I present the result of equation (2) and interpret it assuming the two prerequisites are true. You can decide how much to trust this assumption at your own discretion.

Following Bertrand et al. (2004) and Petersen (2009), I estimate robust standard errors clustered by each stock ticker. Estimating each equation, I use four empirical specifications. 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, which serve as the main specification because the coefficient of θ 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) but should not affect the treatment effect of equation (2) as the sorting of treatment status happens to the treatment group and the control group at the same time.

If the effect of financial constraints prevails over the effect of agency problem, we should predict that firms increase investment on fixed assets, and we can expect to observe that $\theta > 0$. Otherwise, if the effect of agency problems is more important, we would observe that $\theta < 0$.

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 2009-2015, by the treatment group, the control group with firms listed in 2014, and the control group with firms listed in 2015. Focusing on year 2011-2013, we observe that in 2011, the three groups have very similar *InvestmentRate*. In 2012, when the treatment group went public, it has a slightly higher *InvestmentRate*. But in 2013, *InvestmentRate* by the treatment group diverges significantly from that of the control group. The treatment group, benefiting from IPOs, increased investment drastically, while the control group, affected by the moratorium, sat tight. This difference marks the results of equation (2). 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 (1). An additional detail that strengthens the parallel trends assumption is the similarity of *InvestmentRate* in 2013 by the 2014-listed firms and the 2015-listed firms. Although these two groups of firms applied to IPO at different times, resulting in the differential years of getting listed, their pre-IPO *InvestmentRate*

follows the same trend over 2011-2013. The difference between the 2014-listed firms and the 2015-listed firms is similar to that of the treatment group and the control group: timing of application. Since the 2014-listed firms' *InvestmentRate* and the 2015-listed firms' *InvestmentRate* follow the same pre-IPO trend, I have more confidence in the pre-IPO trend being parallel between the treatment group and the control group.

[Insert Figure 2 here]

Tables 3a and 3b reports the DID results of equation (1) and equation (2), respectively. Using *InvestmentRate* as the dependent variable, I find that an IPO results in a statistically significant increase in firms' investment behavior in the 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 firm fixed effects and year fixed effects in equation (2), but in equation (1), year fixed effects absorbs the treatment effect as discussed earlier.

[Insert Table 3a here]

[Insert Table 3b here]

The results are economically meaningful as well. Interpreting the coefficients from the specification with control variables, Table 3a suggests that for an average firm, being able to conduct an IPO on schedule, compared to receiving a one-year delay in IPO process, invested more on fixed assets, intangible assets, and other long-term assets, and the increased amount is equal to 17.8% of its 2011 fixed assets. Similarly, Table 3b suggests that in 2012 and in 2013, a firm that could conduct an IPO on schedule, compared to a similar firm that was affected by the moratorium and had to wait, invested more on fixed assets, intangible assets, and other long-term assets, and the increased amount is equal to 18.5% of its 2011 fixed assets.

4.2 IPO, Financial Constraints and Agency Problems

The previous results establish that an IPO increases firms' investment, suggesting that the financial constraints effect outweighs the agency problems effect in determining firms' investment behavior. I develop an empirical test to probe these two competing channels.

To test whether an IPO lessens financial constraints, I examine the consequences of an IPO that are associated with lessening financial constraints. In Figure 3a and 3b, I find that an IPO significantly increased the treated firms' cash holdings and decreased their financial costs. These two mechanical results of an IPO are consistent with the financial constraints literature and indicate that IPOs reduce firms' financial constraints.

[Insert Figure 3a here]

[Insert Figure 3b here]

THE CAUSAL EFFECT OF IPO ON FIRMS' INVESTMENT

Are financial constraints binding for the IPO applicants? I test this question by looking at the liabilities of IPO applicants. An IPO not only raises a large amount of capital for the issuer but also can increase the issuer's access to debt financing. According to pecking order theory, firms prefer to use internal capital over 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 4 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 under financial constraints at the time of their IPOs and an IPO could lessen their financial constraints.

[Insert Table 4 here]

Establishing that IPOs reduce firms' previously binding financial constraints, I turn to test whether increasing investment led firms to suffer the consequences predicted by agency problems theory. The agency problems literature demonstrates that public firms under-invest due to managerial short-termism and their focus on earnings. It argues 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 4, that an IPO does cause net profit to decrease while increasing investment. My results contribute to the agency problems 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 suggested by agency problems theory, but they made a choice to increase investment in spite of the short-term negative effects, suggesting that the financial constraints effect plays a more important role in determining post-IPO firms' investment behaviors.

[Insert Figure 4 here]

In addition to the horse race between financial constraints effects and agency problems effects, my results also shed light on the lifecycle theory of firms. The theory in general explains that the motivation to conduct an IPO is a result of firms reaching a certain stage in their lifecycles, usually around their productivity peaks. However, the literature disagrees on whether firms conduct IPO to realize potential productivity or do so after their productivity peak to take advantage of investors' asymmetric information and to monetarize higher valuation. The empirical evidence often used to support the latter is firms' post-IPO operating underperformance – after IPO, firms are documented to have a worse operating performance, sometimes measured by profit over assets, than their pre-IPO levels. My study reconciles this finding by comparing post-IPO firms with similar moratorium-affected firms. If the post-IPO operating underperformance is due to firms reaching their productivity peak before IPOs, 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 in my setting, only occurs to the post-IPO firms. This result suggests that an IPO serves as the cause of operating underperformance through

increased investment. My evidence is consistent with firms conducting an IPO to raise funds to increase productivity rather than monetizing high valuations after their productivity peaks.

4.3 Robustness Tests

The first robustness check tests the validity of equation (2) by drawing an analog of the treatment group and the control group with the firms listed in 2014 and the firms listed in 2015. To infer causality, equation (2) relies on the assumption that the treatment group and the control group are identical and the time of IPO application is unrelated to the moratorium. Through summary statistics in Table 2b, I demonstrate that the treatment group and the control group are similar, but since I do not have the data from 2010 for the control group, I cannot directly prove that the treatment group and the control group followed the same trend before the treatment. However, if the pre-IPO trend is dependent on IPO application time, we should observe different pre-IPO trends for the 2014-listed firms and the 2015-listed firms in 2011, 2012 and 2013, before their IPO, as these two groups applied to IPO at different times. Table 5 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 IPO at different times results in different post-IPO investment behaviors, we should observe significant results from this estimation. Yet, I find no significant difference in *InvestmentRate* between the 2014-listed firms and the 2015-listed firms. Therefore, the result strengthens the validity of equation (2) by showing that the timing of an IPO application does not significantly impact the post-IPO investment behavior, and the sorting of the moratorium is independent to the outcome variable.

[Insert Table 5 here]

Public firms and private firms may face different investment opportunities, leading to the difference in the outcome variable. I test this alternative hypothesis by comparing the treated firms with a sample of matched public firms. If the treated firms' increased investment comes from lessening financial constraints, then the coefficient from this test should be similar to the main result. Otherwise, if the treated firms' increased investment is a consequence of being a public firm, then we should observe a smaller or no effect in this test. Table 6a and 6b estimate equation (1) and equation (2) using public firms as the control group. The coefficients are significantly positive. The result confirms that the treatment effect does not come from unobserved differences in investment opportunities between public firms and private firms, strengthening the channel of IPO increasing firms' investment through lessening financial constraints.

[Insert Table 6a here]

[Insert Table 6b here]

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 of 332 of them 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 DID approach, I find that, consistent with the effect of mitigating financial constraints outweighing the effect of agency problems in determining firms' post-IPO investment behavior, an IPO has a statistically significant and economically meaningful effect on firms' investment behavior: an average treated firm spent 18.5% more of its 2011 fixed assets on investment than an average control group firm did because it could conduct an IPO on time.

I also provide suggestive evidence to support the two proposed channels. I demonstrate that financial constraints were binding for pre-IPO firms and IPOs lessen firms' financial constraints; an IPO causes net profit to decrease, which suggests that the concern over agency problems 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. I also find evidence that supports that an IPO causes firms' operating underperformance through increased investment, while rejecting that an IPO serves as a signal of a productivity peak.

Apart from its theoretical contributions and its quantification of the effect of IPOs, my study also generates policy implications. First, firms in my sample are large, mature and profitable. Many previous financial constraints studies assume that such firms are immune to financial constraints. Using a local shock, I find that these firms are subject to financial constraints as well because an IPO can significantly increase their subsequent investment. This finding reinforces that access to equity markets has a huge potential to improve firms' investment and productivity and has a profound impact on the real economy. The IPO literature documents the expensive fixed fee associated with an IPO and is one of the biggest hurdles for companies seeking to get listed. My finding suggests that regulators should consider reducing these barriers more aggressively.

Second, to the best of my knowledge, my study is the first to quantify the real impact of the 2012 Chinese IPO moratorium. I find a large treatment effect of IPOs on the treated firms; the negation of that is a huge opportunity cost caused by the IPO moratorium on the control group firms. As explained in Section II, the welfare of the IPO applicant firms is only a secondary consideration of the CSRC. But the CSRC should be aware of the

impact of its administrative measures, and my study provides a reference to help them make a more informed decision.

Tables

Table 1

List of Variables and Definition

Variable	Description
Variables of Interest	
InvestmentRate	(*Cash outflow for fixed, intangible and other long term assets - cash inflow for fixed, intangible and other long term assets*) / $FixedAssets_{2011}$
CashRate	*Ending balance of cash* / $TotalAssets_{2011}$
FinancingRate	*Cash outflow for financing activities* / $TotalAssets_{2011}$
LiabilitiesRate	*Liabilities* / $TotalAssets_{2011}$
NetProfitRate	*Net profit* / $TotalAssets_{2011}$
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 2

Descriptive Statistics

The table lists descriptive information for all A-share stocks from 2011 to 2015. Panel A compares all public firms and firms in my sample, which are the treatment group, firms listed in 2012 before the moratorium, and the control group, firms that applied to IPO before the moratorium and eventually got listed. Panel B compares the two groups of firms. In order to draw meaning from the statistics, no logarithm operations have been applied to the variables. Variables are in million RMB with the exception of Staff Headcount and Age. Standard deviations are in parentheses.

Panel A

	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)
FixedInvestment	310.56	356.38	133.74	343.81	176.15
	(523.4)	(583.2)	(291.9)	(564.0)	(334.9)
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

Panel B

	Whole Sample 2011-2015	Control Group 2011	Treatment Group 2011	Control Group 2015	Treatment Group 2015
Total Assets	2464.95	1462.32	1599.12	3497.61	4270.00
	(6368.8)	(4130.3)	(4076.8)	(8834.6)	(8845.1)
Fixed Assets	449.89	283.07	326.86	570.24	726.50
	(1141.5)	(786.4)	(828.8)	(1412.5)	(1386.8)
FixedInvestment	146.87	128.49	146.02	167.95	193.53
	(296.7)	(276.3)	(326.5)	(339.2)	(326.2)
Liabilities	1251.08	828.39	807.76	1822.05	1888.99
	(4155.0)	(2625.5)	(2252.4)	(6056.6)	(5388.7)
Total Revenue	1428.17	1024.07	1363.93	1720.75	1987.11
	(2799.4)	(1949.1)	(2641.8)	(3372.0)	(3945.9)
Total Operating Cost	1241.74	868.78	1218.41	1426.71	1818.95
	(2446.8)	(1668.1)	(2533.6)	(2511.2)	(3700.3)
Cash In Sales	1314.86	898.53	1370.41	1438.57	1916.85
	(2425.8)	(1503.2)	(2584.3)	(2362.2)	(3635.7)
Net Profit	148.88	121.48	152.79	173.81	171.39
	(251.7)	(203.3)	(230.0)	(325.8)	(319.6)
Cash Out Fixed Assets	132.56	110.40	121.33	153.76	178.85
	(268.6)	(242.3)	(272.3)	(301.7)	(291.4)
Cash Out Financing	521.98	274.67	374.77	800.17	843.19
	(1581.6)	(615.5)	(980.5)	(2516.5)	(2338.6)
Cash Flow	66.53	34.72	47.45	126.48	95.37
	(257.8)	(142.5)	(208.7)	(362.1)	(397.4)
Cash	419.93	247.66	306.10	558.83	676.09
	(896.0)	(679.9)	(730.3)	(1132.7)	(1301.6)
Staff Headcount	1801.07	1421.85	1609.00	2022.03	2512.05
	(2784.1)	(2226.9)	(2565.6)	(2959.5)	(3551.3)
Age	12.45	10.26	10.87	14.26	14.87
	(5.186)	(5.251)	(4.376)	(5.251)	(4.376)
Observations	2430	332	154	332	154

Table 3a

Difference-in-differences (DID) regressions: effect of IPO on firms' investment

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 *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	0.175**	0.178**	0.141**	0.147
	(0.0797)	(0.0793)	(0.0708)	(0.0904)
time	0.456***	0.430***	0.408***	0.103*
	(0.0555)	(0.0547)	(0.0470)	(0.0610)
treatment	-0.259***	-0.252***		
	(0.0370)	(0.0412)		
Constant	0.654***	0.702***	0.583***	0.349***
	(0.0324)	(0.132)	(0.0154)	(0.0545)
Observations	2,121	1,942	2,121	2,121
R-squared	0.092	0.159	0.154	0.197
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 3b

Difference-in-differences (DID) regressions: effect of IPO on firms' investment

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., 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 *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	0.180***	0.185***	0.193***	0.195***
	(0.0627)	(0.0653)	(0.0625)	(0.0625)
time	0.104***	0.0989***	0.0915***	
	(0.0330)	(0.0357)	(0.0333)	
treatment	0.00874	0.0161		
	(0.0460)	(0.0498)		
Constant	0.555***	0.613***	0.563***	0.564***
	(0.0259)	(0.115)	(0.0195)	(0.0195)
Observations	1,102	1,004	1,102	1,102
R-squared	0.031	0.128	0.054	0.087
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 4

Difference-in-differences (DID) regressions: effect of IPO 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

Table 5

Difference-in-differences (DID) regressions: 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 2012. *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 2009-2013. 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	0.0404	0.0793	0.0392	0.118
	(0.105)	(0.104)	(0.0942)	(0.113)
time	0.451***	0.389***	0.388***	0.0951
	(0.0797)	(0.0777)	(0.0680)	(0.0932)
treatment	-0.0653	-0.0327		
	(0.0617)	(0.0594)		
Constant	0.675***	0.355**	0.667***	0.601***
	(0.0423)	(0.154)	(0.0135)	(0.0286)
Observations	1,298	1,148	1,298	1,298
R-squared	0.065	0.164	0.121	0.141
Control Variables	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 6a

Difference-in-differences (DID) regressions: public firms as alternative 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 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	0.581***	0.427***	0.0394	0.0525
	(0.0618)	(0.0639)	(0.0585)	(0.0595)
time	0.0501**	0.177***	0.509***	0.305***
	(0.0235)	(0.0280)	(0.0251)	(0.0262)
treatment	0.00529	0.0639*		
	(0.0281)	(0.0346)		
Constant	0.390***	0.523***	-0.0102	0.00333
	(0.0217)	(0.0349)	(0.0222)	(0.0213)
Observations	15,341	14,695	15,341	15,341
R-squared	0.031	0.094	0.071	0.124
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Table 6b

Difference-in-differences (DID) regressions: effect of IPO on firms' investment

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	0.179***	0.191***	0.209***	0.209***
	(0.0541)	(0.0554)	(0.0536)	(0.0536)
time	0.104***	0.0932***	0.0762***	
	(0.0103)	(0.0102)	(0.00940)	
treatment	0.161***	0.0761*		
	(0.0389)	(0.0421)		
Constant	0.403***	0.716***	0.429***	0.429***
	(0.00852)	(0.0336)	(0.00629)	(0.00630)
Observations	6,695	6,395	6,695	6,695
R-squared	0.024	0.107	0.027	0.028
Controls	No	Yes	No	No
Firm FE	No	No	Yes	Yes
Year FE	No	No	No	Yes

Figures

Figure 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 dots are control firms and blue dots are treated firms. The area between the red lines represents the moratorium.

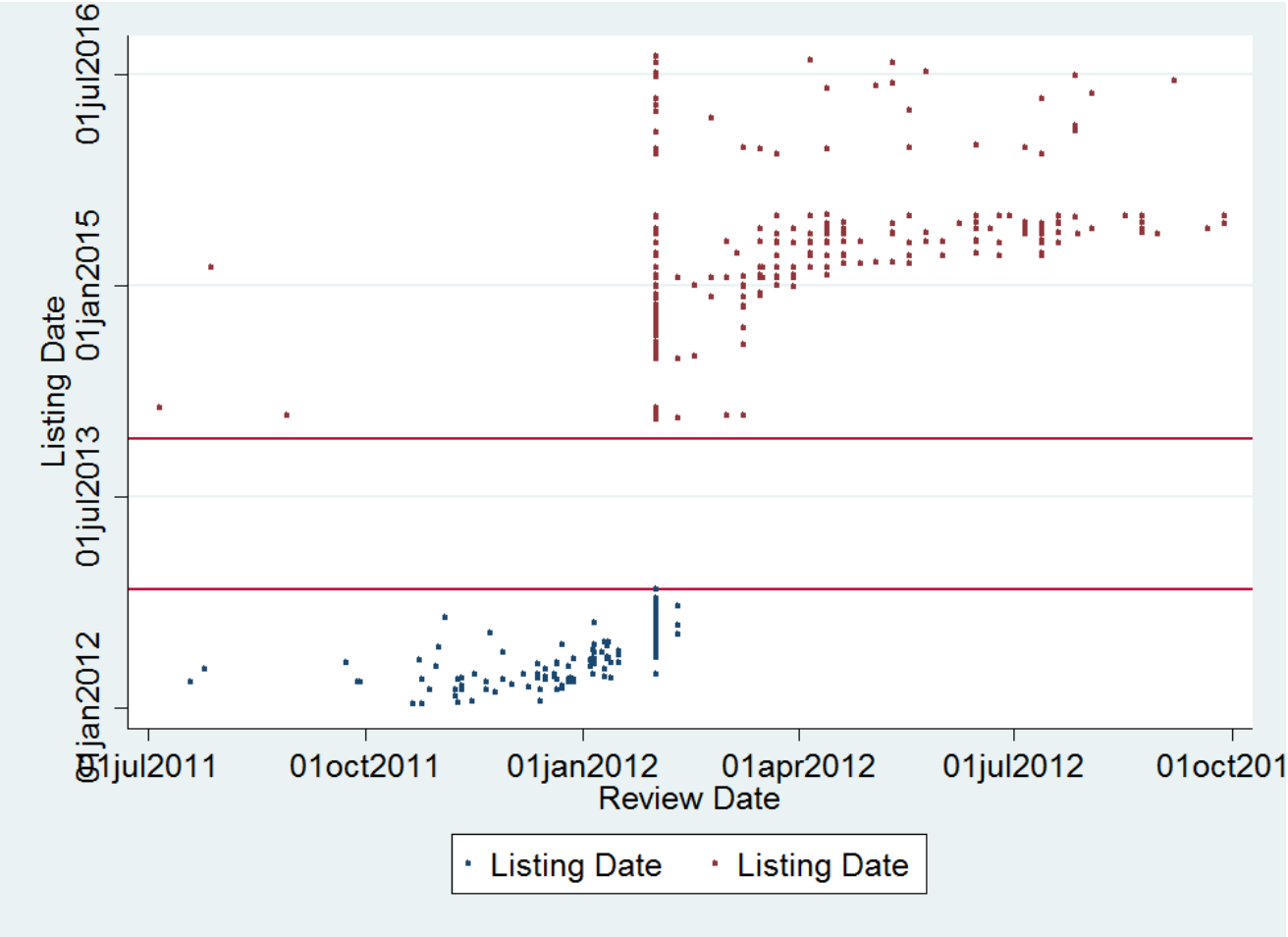


Figure 2

InvestmentRate around IPO

Figure 2 plots the average *InvestmentRate* along with 95% confidence intervals for the treatment group firms that were listed in 2012, the control group firms that were listed in 2014, and the control group firms that were listed in 2015. The bars represent the means, and the lines represent the 95% confidence interval.

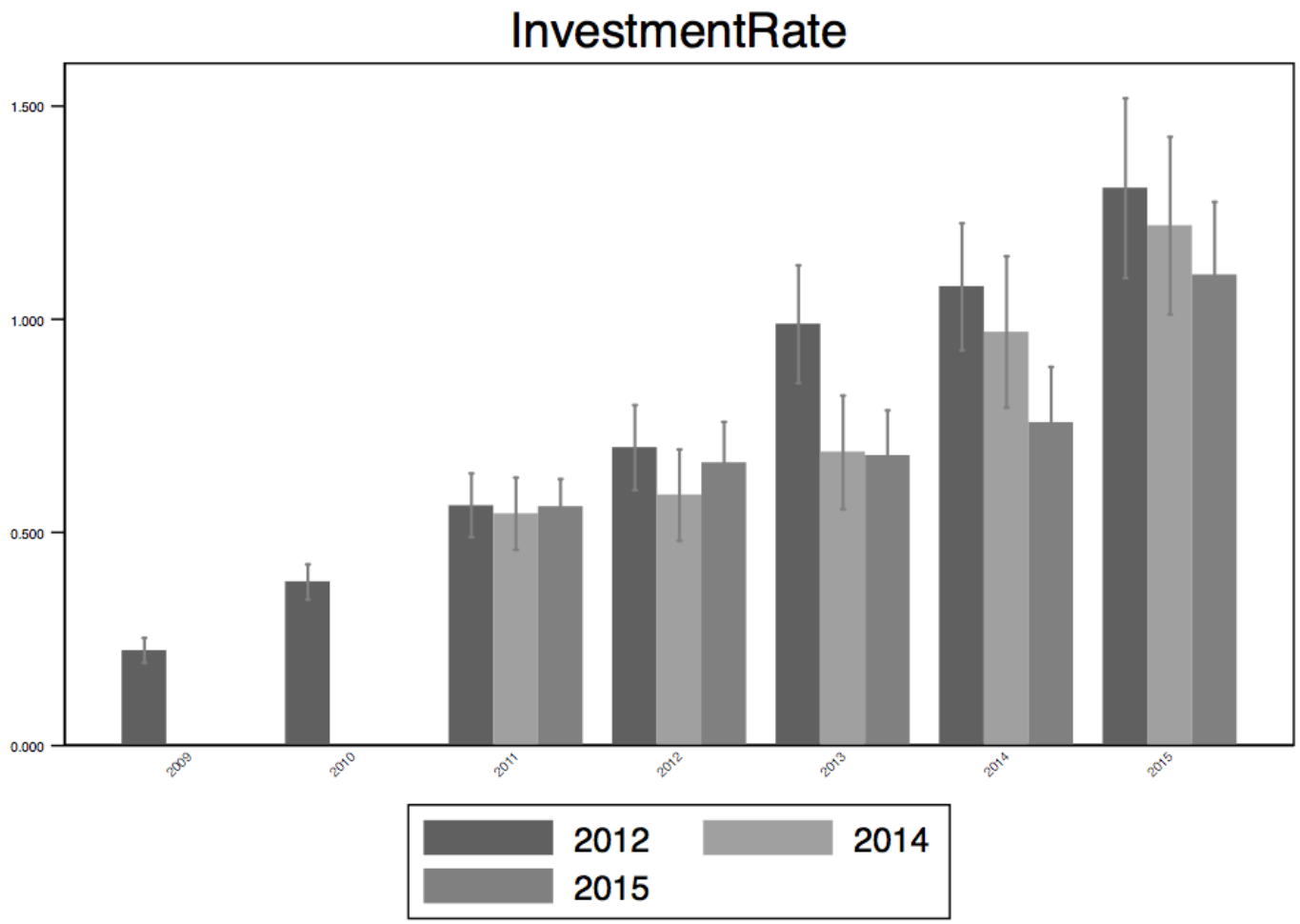


Figure 3a

FinancingRate around IPO

Figure 3a plots the average *FinancingRate* along with 95% confidence intervals for the treatment group firms that were listed in 2012, the control group firms that were listed in 2014, and the control group firms that were listed in 2015. The bars represent the means, and the lines represent the 95% confidence interval.

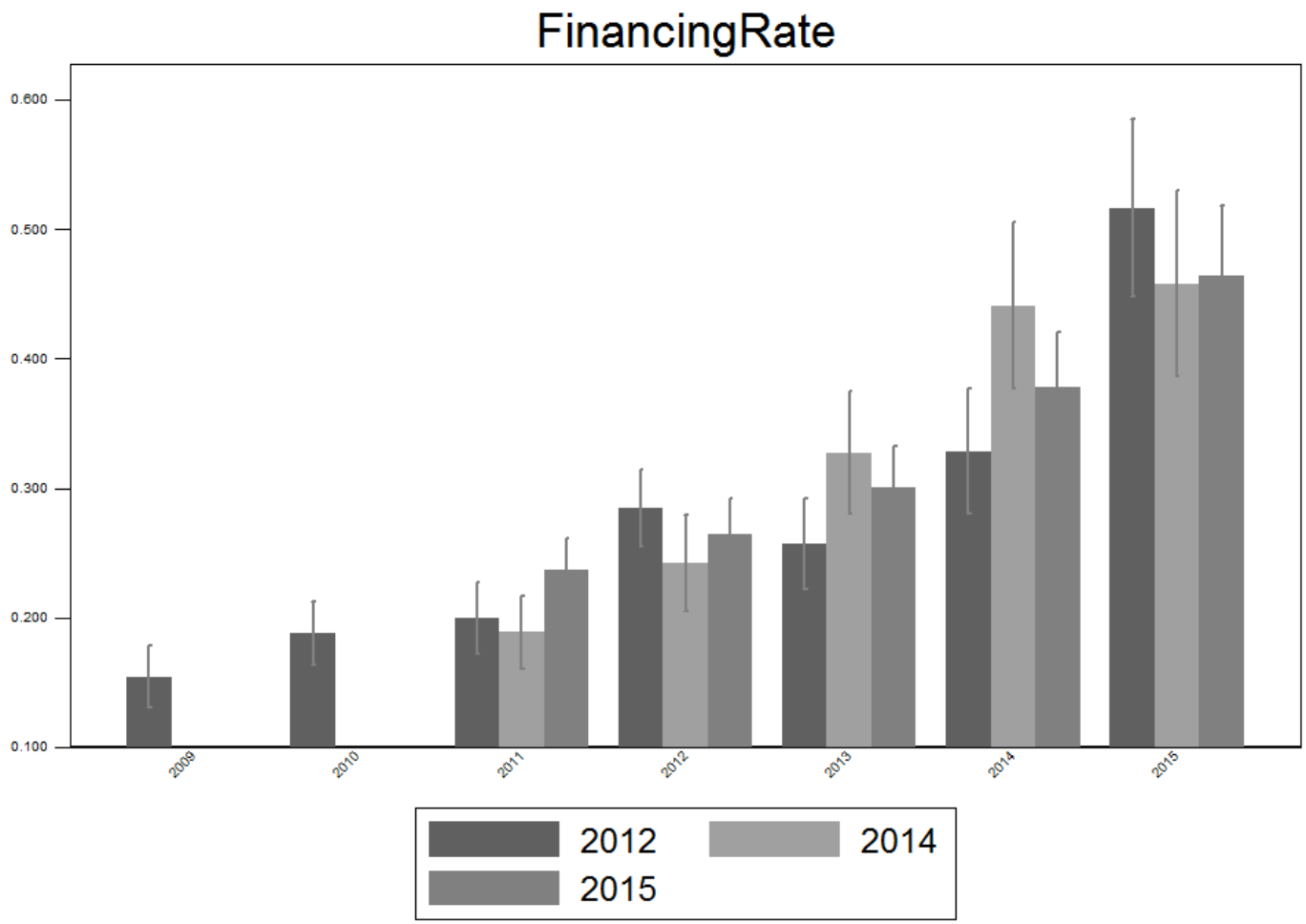


Figure 3b

CashRate around IPO

Figure 3b plots the average *CashRate* along with 95% confidence intervals for the treatment group firms that were listed in 2012, the control group firms that were listed in 2014, and the control group firms that were listed in 2015. The bars represent the means, and the lines represent the 95% confidence interval.

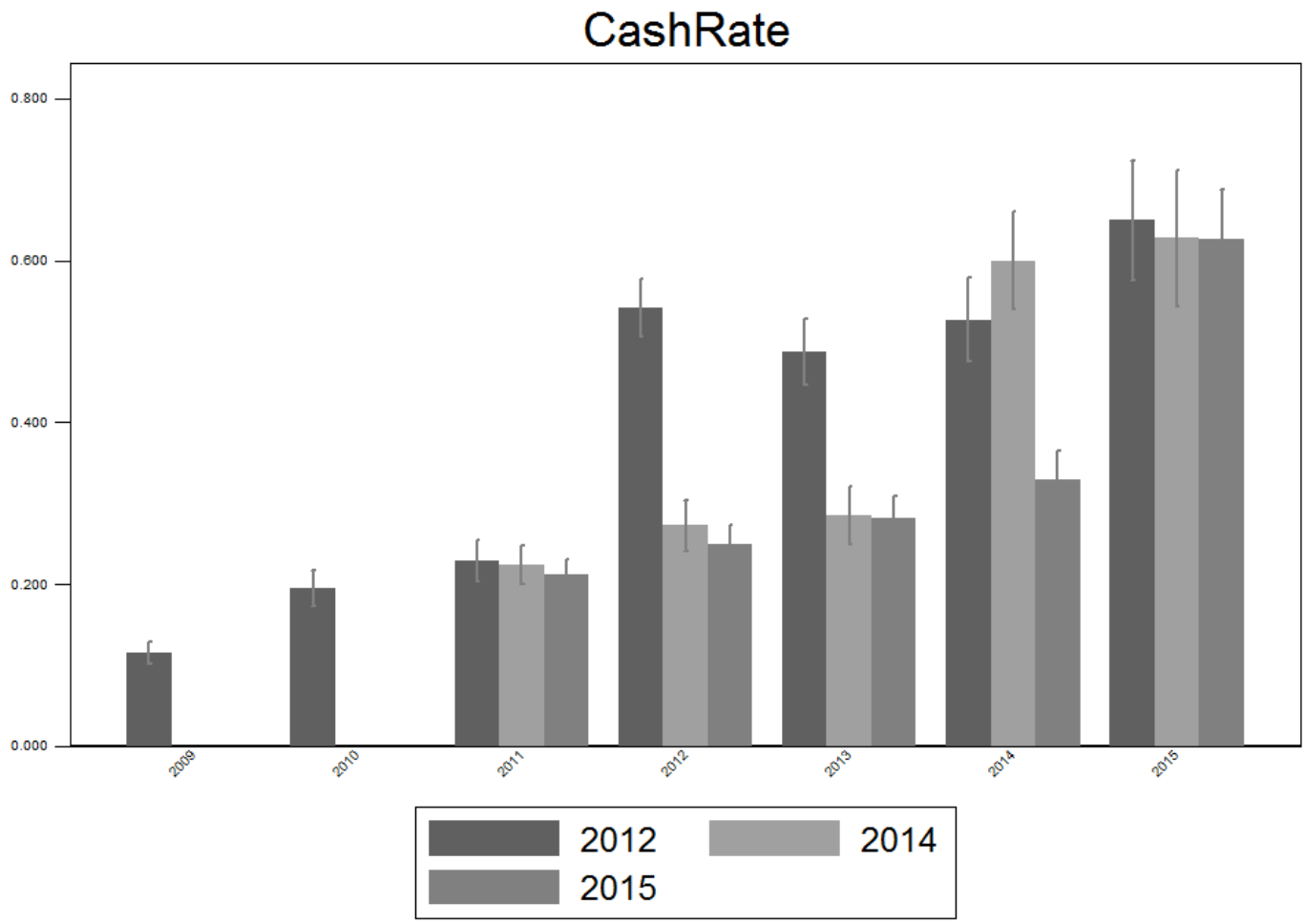
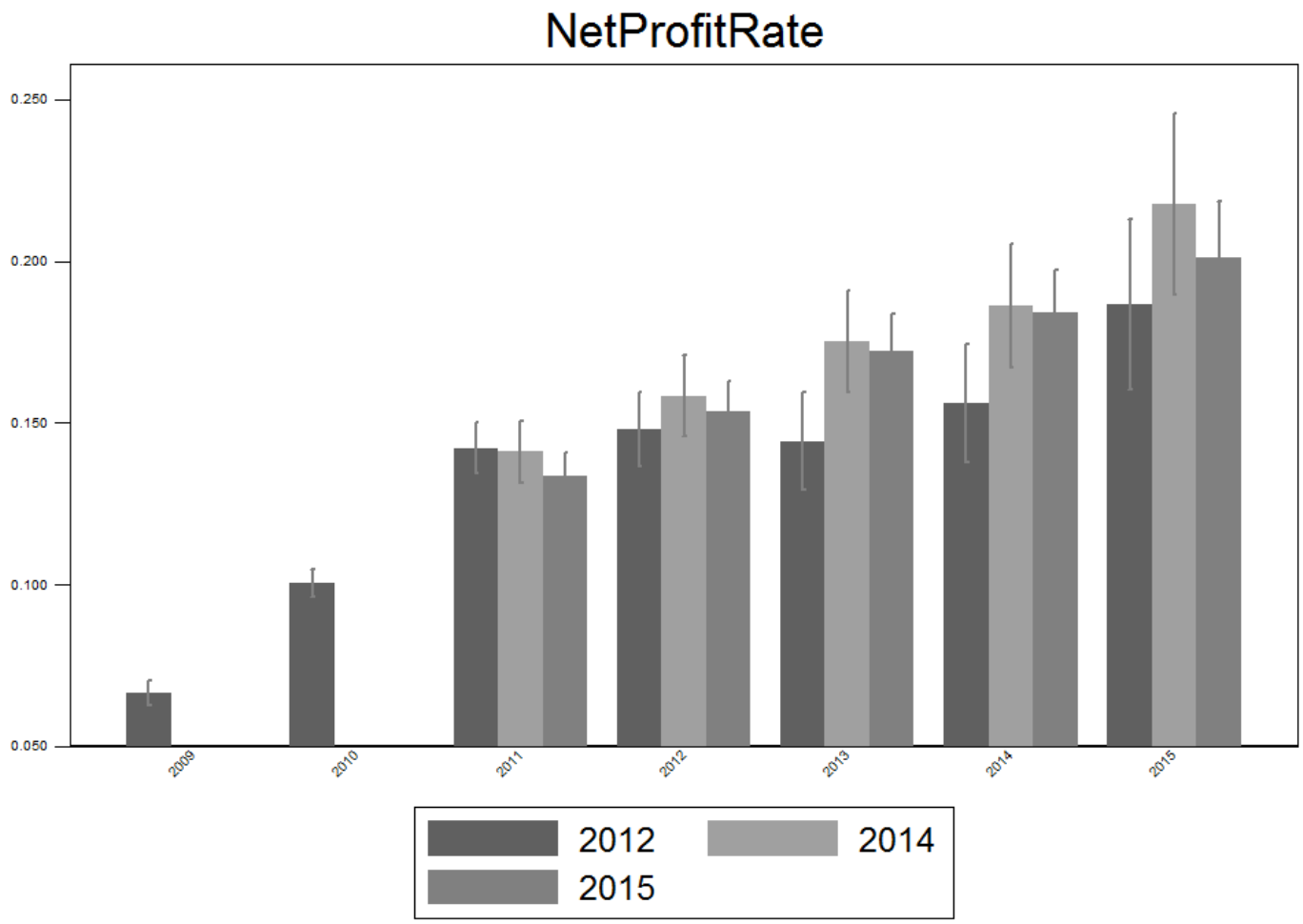


Figure 4

NetProfitRate around IPO

Figure 4 plots the average *NetProfitRate* along with 95% confidence intervals for the treatment group firms that were listed in 2012, the control group firms that were listed in 2014, and the control group firms that were listed in 2015. The bars represent the means, and the lines represent the 95% confidence interval.



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 this 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.

References

Almeida, H., Campello, M., & Weisbach, M. S. (2004). The cash flow sensitivity of cash. *The Journal of Finance, 59*(4), 1777-1804.

Angrist, J. D., & Krueger, A. B. (1999). Empirical strategies in labor economics. *Handbook of labor economics, 3*, 1277-1366.

Asker, J., Farre-Mensa, J., & Ljungqvist, A. (2014). Corporate investment and stock market listing: a puzzle? *Review of Financial Studies*, hhu077.

Bernstein, S. (2015). Does going public affect innovation? *The Journal of Finance, 70*(4), 1365-1403.

Brav, O. (2009). Access to capital, capital structure, and the funding of the firm. *The Journal of Finance, 64*(1), 263-308.

Campello, M., Ribas, R. P., & Wang, A. Y. (2014). Is the Stock Market Just a Side Show? Evidence from a Structural Reform. *Review of Corporate Finance Studies*. doi:10.1093/rcfs/cfu006

Chemmanur, T. J., He, S., & Nandy, D. K. (2010). The going-public decision and the product market. *Review of Financial Studies, 23*(5), 1855-1908.

Dechow, P., Ge, W., & Schrand, C. (2010). Understanding earnings quality: A review of the proxies, their determinants and their consequences. *Journal of accounting and economics, 50*(2), 344-401.

Doidge, C., Karolyi, G. A., & Stulz, R. M. (2013). The US left behind? Financial globalization and the rise of IPOs outside the US. *Journal of Financial Economics, 110*(3), 546-573.

Fazzari, S., Hubbard, R. G., & Petersen, B. C. (1988). Financing constraints and corporate investment: National Bureau of Economic Research Cambridge, Mass., USA.

GILJE, E. P., & Taillard, J. P. (2016). Do private firms invest differently than public firms? Taking cues from the natural gas industry. *The Journal of Finance*.

Graham, J. R., Harvey, C. R., & Rajgopal, S. (2005). The economic implications of corporate financial reporting. *Journal of accounting and economics, 40*(1), 3-73.

Jensen, M. C., & Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics, 3*(4), 305-360.

Maksimovic, V., Phillips, G., & Yang, L. (2013). Private and public merger waves. *The Journal of Finance, 68*(5), 2177-2217.

Packer, F., & Spiegel, M. (2016). China's IPO activity and equity market volatility. *FRBSF Economic Letter, 2016*, 18.

Pagano, M., Panetta, F., & Zingales, L. (1998). Why do companies go public? An empirical analysis. *The Journal of Finance, 53*(1), 27-64.

Petersen, M. A. (2009). Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies, 22*(1), 435-480.

Ritter, J. R., & Welch, I. (2002). A review of IPO activity, pricing, and allocations. *The Journal of Finance, 57*(4), 1795-1828.

Saunders, A., & Steffen, S. (2011). The costs of being private: Evidence from the loan market. *Review of Financial Studies, 24*(12), 4091-4122.

Stein, J. C. (2003). Agency, information and corporate investment. *Handbook of the Economics of Finance, 1*, 111-165.