Liquidity Value and IPO Underpricing

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Abstract

Initial public offerings (IPOs) transform private firms into publicly traded ones, thereby improving liquidity of their shares. Better liquidity increases firm value, which we call "liquidity value". We develop a model and hypothesize that issuers and IPO investors bargain over the liquidity value, resulting in a discounted offer price, i.e., IPO underpricing. Consistent with the model, we find that underpricing is positively related to the expected post-IPO liquidity of the issuer. The relation is stronger when firms are financed by venture capital investors, when the underwriter has more bargaining power, or when a smaller fraction of the firm is sold. We also explore two regulation changes as exogenous shocks to issuers' liquidity before and after IPO, respectively. With a difference-in-difference approach, we find that underpricing is more pronounced with better expected post-IPO liquidity or lower pre-IPO liquidity.

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"IPOs, represent a gift to Wall Street banks, who get to handpick the new investors and offer them a stake in Silicon Valley's shiny new object at what amounts to a steep discount because of the built-in first-day pop."

– Bill Gurley, board director of Uber, general partner of Benchmark Capital Holdings Co.[1]

1. Introduction

Investors demand lower returns and give higher valuations to more liquid assets (Amihud and Mendelson 1986).[2] IPOs transform private companies into publicly traded ones and therefore significantly improve the liquidity of their shares. Better liquidity can raise investors' willingness to invest, and directly increase firms' market value. In addition, founders and early investors benefit from better liquidity because they can exit their investments with smaller price impact in a liquid public market.[3] We refer to such increased payoffs to founders and investors due to improved liquidity as "liquidity value" in this paper. Despite IPOs' significant impact on the liquidity of stocks, little attention has been paid to the relation between liquidity value and the determination of IPO offer prices. In this study, we posit that, issuers share the benefit of liquidity value with IPO investors by setting a discounted offer price, i.e., IPO underpricing – a stylized fact that IPO stocks typically yield large first-day returns after going public.[4]

Our paper is closely related to Loughran and Ritter (2002) in spirit, (i) we take the stand that the underwriting business is not completely competitive, because there are significant barriers to entry, due to the limited supply of influential analysts; (ii) we explore the conflict of interest between underwriters and issuers. Since the IPO offer price is determined during the book building process, and underwriters have the discretion in share allocation, underwriters could intentionally leave more money on the table than necessary and allocate these shares to favored regular buy-side clients. The key is why issuers appear content and allow this to happen. Loughran and Ritter (2002) explain this using the prospect theory, which shows people care about the change of wealth,

[1] See the CNBC report in Oct, 2019, https://www.cnbc.com/2019/10/06/bill-gurleys-plan-to-move-from-tech-ipos-to-direct-listings.html. Mr. Gurley is considered a top dealmaker in technology.
[2] Relatedly, Balakrishnan, Billings, Kelly, and Ljungqvist (2014) show that managers actively shape firms' information environments in order to improve the liquidity of their shares and achieve higher valuation.
[3] Aggarwal, Krigman, and Womack (2002) point out that managers and early investors of issuers do not sell their own stakes at IPOs, but rather wait until the end of the lockup period, which is typically six months after IPO.
[4] In our sample period of 1981-2015, the average underpricing is 21.5%.

rather than the level of wealth. They argue that with great recent increase in wealth, founders and early investors spend little bargaining effort in their negotiations over the offer prices with underwriters. Instead, we model the determination of offer price in a standard Nash bargaining framework. In the model, the underwriter, on behalf of his buy-side investors, negotiates with a rational founder to share the expected liquidity value. The model shows that underpricing, which is the payoff to IPO investors, is positively driven by the expected liquidity value, the bargaining power of the investment bank (or equivalently IPO investors), and negatively related to the issuance size. Intuitively, the rational entrepreneur is willing to leave money on the table for IPO investors because he relies on the underwriter and IPO investors to have a successful IPO, in order to realize the higher firm value caused by public listing.[5] Hence the higher the total value gain, the larger the payoffs received by IPO investors.

Admittedly, conditional on the fact that issuers choose to go public, the source of increase in firm value includes, but is not limited to, improved liquidity. For example, firms can benefit from less constraints in financing which enables them to invest in more positive-NPV projects, lower cost of capital with reduced information asymmetry, and/or attracting more faith from banks, consumers, and suppliers with its public status. In the Nash bargaining game modeled in the paper, value gains in all the above channels are shared with IPO investors by the issuer. Some of these channels, such as the easiness to raise capital and the reduced information asymmetry once public, could also be innately related to liquidity improvement.[6] We choose to focus on the aspect of liquidity only, because it is the most tractable and testable one of all the channels. If the liquidity value does not make up a significant part of the value gain, it will simply bias against us finding any results.

In the framework of the bargaining game, we take the liquidity improvement as exogenous. The theoretical justification for this stand is based on Kyle (1985), which shows that liquidity in a financial market is generated by liquidity traders who trade for liquidity/diversification needs rather than information reasons. In the extreme case where traders only trade for information

[5] Enough subscription interests from IPO investors are crucial for IPO success. Around 20% of IPO are withdrawn in the book-building process, and withdrawn issuers receive lower valuations when they return to the market later. See Dunbar and Foerster (2008), among others.
[6] One benefit associated with public listing is analyst coverage and more information production about the firm, which reduces information asymmetry between the firm and the market participants. Less information asymmetry improves liquidity and reduces the cost of capital of financing.

reasons, the market breaks down because every trader has a legitimate concern that the counter party only initiates a trade when he has an information advantage (Milgrom and Stokey, 1982). IPOs provide issuers access to a crowd of liquidity traders in the secondary market, which enables informed traders to hide their trades, and decreases the price impact of any trade.

The degree of liquidity improvement is equal to the difference between the issuer's expected post-IPO liquidity and its pre-IPO liquidity. Empirically we test the relation between underpricing and the expected post-IPO liquidity (henceforth, "expected liquidity", for brevity) in the baseline regressions, since the pre-IPO liquidity cannot be directly measured. We measure expected liquidity by spread, turnover, and log AIM (Amihud's Illiquidity Measure) of the issuer's peer public firms in the 12 months before the issuer's IPO time (henceforth, referred to as *Peer Spread*, *Peer Turnover*, and *Peer AIM*). We use peer liquidity before the IPO time to measure expected liquidity because it wards off reverse causality concerns. An issuer's IPO price and its subsequent secondary market first-day price of are unlikely to affect its public peers' trading environment on the secondary market a year before the IPO time. Figure 1 summarizes the key takeaway of the baseline regression. It plots the average underpricing across subsamples of issuers in five quintiles sorted by expected liquidity. Underpricing is monotonically increasing in expected liquidity across the five groups and the pattern is economically large. The difference in underpricing exceeds 25% between the lowest and highest quintile. We find similar results in multi-variate regressions, controlling for various deal and issuer characteristics, and time-varying industry effects. For instance, one standard deviation increase (decrease) in peer turnover (AIM) increases underpricing by 13.3% (9.6%), in absolute terms. These results hold in various robustness checks.

We then provide cross-sectional tests of the baseline specification, conjecturing that some issuers may benefit more from the improved liquidity compared to others. First, venture capital (hereafter, VC) funds typically have limited duration, and therefore have stronger needs to unload shares in their portfolio companies in time. Hence, we hypothesize that the relation between underpricing and expected liquidity is stronger for VC-backed issuers, compared to non-VC-backed issuers.[7] In the data, we find that the interaction term between the dummy variable of being VC-backed and expected liquidity has a positive and significant effect on underpricing. Second,

[7] Black and Gilson (1998) show that IPOs are not so much exits for founders as they are for VC investors, as founders often regain control from VC investors in VC-backed companies at IPO.

our theoretical model shows that the relation between underpricing and expected liquidity should be stronger when the underwriter has more bargaining power or when the issuance size is smaller. We find consistent results supporting these predictions.

One alternative interpretation of the baseline results could be that some underlying unobserved variables drive both peer liquidity and underpricing simultaneously, even when reverse causality is ruled out. In addition, since we leave out the pre-IPO liquidity in the regression, it could cause estimation bias if expected liquidity is correlated with pre-IPO liquidity. Using peer liquidity as the measure for expected liquidity is unlikely to be the case. Still, to thoroughly address these concerns, we explore two regulation changes as exogenous shocks to firms' expected liquidity value, one to the expected post-IPO liquidity, and the other to the pre-IPO liquidity.

We use the enactment of changes in Order Handling Rules (hereafter, OHR) at Nasdaq in 1997 as an exogenous shock to expected liquidity of issuers listed on Nasdaq. Triggered by a scandal that Nasdaq dealers collude to maintain large bid-ask spread, SEC mandated several changes of OHR for all Nasdaq securities in 1997. Most changes aim to decrease quoted spread and promote trading. For example, one of the changes requires a limit order to be posted on the trading system if it is better than a dealer's quotes. These changes in OHR promoted the competition between limit orders and dealer quotes on Nasdaq and improved the liquidity of stocks listed on Nasdaq (see Bessembinder 1999, Barclay et al. 1999, among others). Meanwhile, other exchanges did not undergo a similar regulation change and IPO stocks listed on those exchanges were not subject to the same liquidity shock. More importantly, this change in liquidity of public stocks does not affect the pre-IPO liquidity should the firm stay private. Using a difference-in-difference approach, we show that Nasdaq IPOs exhibit more underpricing after changes to OHR compared to non-Nasdaq IPOs, due to their improved liquidity, and the magnitude is economically large. We find that, on average, underpricing of Nasdaq deals is less than that of non-Nasdaq deals by 5.6% before 1997, but higher by 15% after 1997, and both differences are quoted in absolute terms.[8]

Second, we use the passage of the National Securities Market Improvement Act (hereafter, NSMIA) in 1996 as an exogenous shock to the pre-IPO liquidity of issuers. Liquidity of public

[8] One potential concern is that this law change coincides with the tech bubble period of 1998-2000 and many tech stocks are listed on Nasdaq. We repeat the analysis excluding all tech firms and results remain. We follow the definition in Loughran and Ritter (2004) for tech firms.

shares is measured by how easily transactions take place among existing shareholders and other investors, which is bid-ask spread or price impact of trades. However, since a secondary market for private shares does not widely exist, we measure pre-IPO liquidity by how easily founders can sell part of their ownership and raise capital, specifically by how large the pool of potential investors is.[9] When it is easier for founders to sell ownership in exchange for external equity capital, the private firm receives higher valuations, which is equivalent to the concept of smaller price impact for public shares. In the era before NSMIA, if a private firm raises capital in multiple states, it must comply with varying state-level disclosure and registration rules in each state. NSMIA lowers the regulatory barriers for private firms to raise capital from multiple states by exempting firms from those rules (Ewens and Farre-Mensa, 2017). Furthermore, NSMIA also expanded exemptions that VC and PE funds typically use to avoid registering with SEC and subsequent disclosures. It made it possible for these funds to manage more capital. Hence the adoption of NSMIA facilitates financing by private firms, broadens the pool of potential buyers for private shares, and enhances their liquidity.

We observe that, even though NSMIA is a federal regulation impacting all private firms in US, firms located in states with scarce in-state capital should be affected more than firms located in states with abundant in-state capital. We regard issuers located in the former states as the treatment group, and issuers in the latter states as the control group, and conduct a difference-in-difference test. We find that the treatment group exhibit less underpricing than the control group post NSMIA, while controlling for expected liquidity. For example, when we use deals in the top four states with the largest number of VC and PE firms as the control group (CA, NY, MA, and TX) and deals in all other states as the treatment group, we find that underpricing of the treatment group is 14% lower than the control group post-NSMIA, in absolute terms. Overall, the evidence from the empirical analysis supports the model prediction that underpricing is positively related to the issuer's expected liquidity and negatively related to pre-IPO liquidity.

The traditional IPOs fundamentally serve two roles for the issuer: raising capital and improving the liquidity of its shares. Traditional theories in the IPO literature tends to focus on the first role but had little to say about the latter. However, given the recent trend of direct-listing promotion in

[9] Most private firms have restrictions on shareholders selling their shares to a third party, for concerns of control rights. Normally the agreement between the company and shareholders (the stock issuance agreement) stipulates that the company has a right of first refusal over their shares. A right of first refusal requires shareholders to offer their shares to the company to purchase before they can sell those shares to the third party. See Belt (2018).

the community of successful private firms and the ever-growing amount of capital under the management of VC and PE funds, we believe analyzing IPO phenomena only through the lens of raising capital is somewhat restricted. Our paper recognizes the liquidity improvement role via IPO and shows that it is relevant in determining a key aspect of IPO: underpricing.

The remainder of the paper is organized as follows. Section 2 summarizes the IPO underpricing literature and discusses the relation of our paper to previous studies. Section 3 presents a model of underpricing based on a Nash bargaining game and develops hypotheses. Section 4 describes the sample and variable construction in the study. Section 5 describes empirical tests, presents main findings and conducts robustness checks. Section 6 concludes.

2. Relation to previous work

IPO underpricing is a longstanding puzzle in finance, and there is a large literature aiming to explain the phenomenon. Ritter and Welch (2002) provide an excellent and extensive review. The early studies focus on information asymmetry issues. For example, Rock (1986) models information asymmetry among investors, which causes the winner's curse for uninformed investors, and argue that average underpricing is necessary to attract uninformed investors to invest in IPOs. Other studies model information asymmetry between firm insiders and IPO investors, and view underpricing as a credible signal for firm quality, since high-quality firms can benefit from returning to the equity market for subsequent financing (Welch, 1989), receiving more favorable market reactions to future dividend news (Allen and Faulhaber, 1989), or attracting more attention to stimulate information production (Chemmanur, 1993). Benveniste and Spindt (1989) model information asymmetry between informed investors and the underwriter during the book-building process, and argue that underpricing is a compensation to informed investors for them to reveal private information. Some other explanations do not rely on information asymmetry, such as Hughes and Thakor (1992), which argue that issuers use underpricing to reduce legal liabilities.

Our paper is different from these studies in that we take the determination of IPO offer prices as a result of negotiation between the underwriter acting on behalf of their buy-side clients and the issuer. This approach is closely related to Loughran and Ritter (2002), which also focuses on the conflicts of interests between the underwriter and the issuer. This view is based on two bodies of empirical evidence. First, the IPO market is not completely competitive, otherwise the underwriter does not have any bargaining power. Krigman, Shaw, and Womack (2001) base their studies on questionnaires to firms and document that the two most important determinants of choosing underwriters are underwriter prestige and analyst coverage, instead of the magnitude of underpricing. Similarly, Loughran and Ritter (2002) argue that the perceived importance of analyst coverage allows high-prestige investment banks to attract issuers despite underpricing. Liu and Ritter (2011) show that the IPO market is characterized by local underwriter oligopolies, because issuers care about non-price dimensions such as all-star analyst coverage and industry expertise. Bradley et al. (2004) study whether the offer price is an integer and conclude that the office price is likely to be the result of negotiation and bargaining between issuers and IPO investors.

Second, the underwriter has an incentive to leave money on the table to benefit its buy-side clients. There is abundant evidence showing that underwriters and institutional IPO investors are no strangers to each other. Aggarwal, Prabhala, and Puri (2002) show that institutional investors get more allocations in IPOs with strong premarket demand. Cornelli and Goldreich (2001) document that regular investors receive favorable allocations, especially when the issue is heavily oversubscribed. Underwriters in general rely on the same group of investors for future deals, so the interaction between investment banks and IPO investors can be characterized as repeated games. Thus, it is beneficial for investment banks to "leave a good taste in investors' mouth". In addition, Loughran and Ritter (2002) point out that underwriters benefit from underpricing because investors engage in rent-seeking behavior to receive favorable allocations in hot IPOs. The observation that underwriters can act in the interests of IPO investors is also supported by the "partial adjustment phenomenon" during book building. Hanley (1993) first documents that underwriters do not fully adjust the offer price based on information collected when the demand is strong. Bradley and Jordan (2002), Loughran and Ritter (2002), and Lowry and Schwert (2004) all find that the offer price is not fully adjusted to reflect publicly available information either, proxied by recent market rally. So underwriters seem to leave money on the table on purpose.

Our paper contributes to the literature in several ways. First, we formally model the negotiation between the underwriter and the issuer with a Nash bargaining game. The first-order prediction of any bargaining game is that, each party's payoff is proportional to the total value gain conditional on the success of the transaction, and the proportions are determined by their bargaining power. Guided by this, we are able to explore a novel factor determining the magnitude of underpricing:

the size of the firm value gain via IPO. We test this channel by focusing on the liquidity value when the firm is transformed from a private to a public one. Our empirical analysis shows that this is an economically significant factor for underpricing magnitude.

Second, our paper deviates from several previous papers that study liquidity and underpricing, as they mostly focus on the issuer's realized liquidity in the secondary market and the theory is vastly different. For example, Booth and Chua (1996) model how the issuer's needs for ownership dispersion and secondary market liquidity jointly determine the equilibrium level of underpricing with asymmetric information. Issuers achieve broad ownership dispersion through over-subscription, which increases both the secondary-market liquidity and information costs borne by investors, who are compensated through larger underpricing. Their model predicts that more underpricing causes better secondary-market liquidity through the channel of diverse ownership. In their empirical analysis, they do not test the relation between underpricing and liquidity directly, but focus on the relation between underpricing and over-subscription. However, given that some IPO investors quickly turn around to sell their shares (dubbed as "flipping"), it is unclear how the ownership structure at IPO benefits control-seeking issuer executives and facilitates secondary-market trading.[10] Furthermore, the direction of causality studied in their paper is the opposite to ours, and we carefully select peer liquidity as expected liquidity measures before the IPO time to rule out reverse causality concerns.

Ellul and Pagano (2006) hypothesize that deals with higher liquidity risk and less post-IPO liquidity need more underpricing to attract investors to participate, extending the line of research linking information asymmetry with underpricing. Their theory predicts a negative relationship between post-IPO liquidity and underpricing, that is, shares that are expected to be illiquid should command more underpricing. This is opposite to the hypothesis in our paper. They use IPO data from UK and confirm the negative relation, by documenting a positive relation between underpricing and the PIN variable measuring asymmetric information. Our study helps shed light on this relation by focusing on *expected* liquidity of the issuer and using exogenous shocks to post-IPO expected liquidity and pre-IPO liquidity for identification. Our empirical finding is opposite

[10] Field and Sheehan (2004) find that the link between underpricing and ownership structure is weak.

to Ellul and Pagano (2006), possibly due to the fact that they use UK data and focus on the relation between underpricing and liquidity risk rather than liquidity value.[11]

Lastly, our paper adds to the literature on why firms go public. Zingales (1995) models the profit maximization problem of an entrepreneur selling his stake in the firm, and argues that the public status enables him to maximize proceeds from selling cash flow rights and the private status enables him to maximize proceeds from selling control rights. Hence the decision to go public depends on the initial ownership structure. Chemmanur and Fulghieri (1999) develop a life cycle theory of the going public decision based on the pros and cons of diverse ownership brought by the public status. Maksimovic and Pichler (2001) point out that public trading can inspire more faith in the firm from investors, customers, creditors, and suppliers. There are also market-timing theories that firms go public when market valuations are high (Lucas and McDonald, 1990, among others). We argue that IPOs provide issuers access to liquidity traders in the secondary market and improve the firm's liquidity, which increases firm value and decreases the price impact of investment exit for founders and early investors.

3. A model of underpricing with bargaining

Consider a private firm that is 100% owned by its founder. The firm value is V and the total number of shares is normalized to one. The pre-issuance share price is thus equal to V. The firm goes public by issuing N new shares at some offer price P, which will be determined in equilibrium. Once public, the shares of the company become much more liquid and we assume the firm value net of the newly raised capital improves to $k \cdot V$, where $k > 1$, and $(k-1)V$ represents the expected liquidity value. Therefore, the total firm value after the IPO is given by $kV + NP$.

Throughout the paper, we assume that k is exogenous, and not within the control of the private firm, the underwriter, or IPO investors. This assumption is drawn from Kyle (1985), which shows that the illiquidity measure "Kyle's λ" (price impact of an order flow) decreases in the amount of noise traders' trade orders. The price impact of a trade is driven by the fear of trading with an trader with superior information. With the presence of noise traders, informed traders can hide

[11] For our own curiosity, we download the PIN measure for information asymmetry from Prof. Stephen Brown's website: http://scholar.rhsmith.umd.edu/sbrown/pin-data?destination=node/998 and replicate Ellul and Pagano (2006) with US IPO data. We find the opposite result: the relationship between underpricing and PIN is negative, instead of positive.

their trades among noise traders' trades. In the extreme case where traders only trade for information reasons, the market breaks down and there is zero liquidity (Milgrom and Stokey 1982). When a firm becomes publicly listed, it gains access to large numbers of noise traders in the secondary market. Investors might want to buy or sell the shares simply for diversification or liquidity needs.[12] We thus view the availability of noise traders to issuers in the secondary market as the source of liquidity improvement from IPOs. Better liquidity can improve the firm value through at least two channels. First, investors could demand lower rates of returns on more liquid assets, ceteris paribus. Second, when early investors of private firms sell their stakes in the public market, the total proceeds will be higher when the price impact associated with such selling is smaller due to better liquidity.

In a typical IPO, issuers hire underwriters to connect with potential IPO investors. The underwriter relies on his network of buy-side investors for participation and negotiates with the issuer on behalf of investors. For simplicity, we assume away any friction between the underwriter and the investors who are usually long-term relationship clients of the underwriters. On the other hand, the underwriting market is not perfectly competitive due to limited number of reputable investment banks. As reviewed in Section 2, existing empirical evidence suggests that underwriters normally have bargaining power against issuers, featuring local oligopolies. The offer price P is therefore determined by the negotiation between the founder and the underwriter (or equivalently the IPO investors). Assume the bargaining power of the founder is β, and the bargaining power of the underwriter is $1-\beta$.

Admittedly, the choice of the underwriter is a deliberate decision by the issuer, and the issuer must be aware of the investment bank's bargaining power. The interesting question then is why an issuer is willing to choose a prestigious investment bank, which leads to leaving more money on the table. Logue et al. (2002) show that the choice of underwriter is typically determined by the issuer's size and industry, and the underwriter's prestige and expertise. Krigman, Shaw, and Womack (2001) document that firms switch underwriters after IPOs at subsequent offerings for more underwriter prestige and better analyst coverage, not caring so much about the magnitude of underpricing at IPOs. Given these evidences, we argue that even though the choice of the

[12] For example, passive index funds or smart beta funds may add stocks of IPO firms to their portfolios, because they need to hold the market, a particular sector, or a particular style, instead of having positive information regarding the firms' outlook.

underwriter is deliberate, since the variable of interest in this study is the magnitude of underpricing and we control for size and industry, the bargaining power of underwriters can be viewed as exogenous. In another word, given the issuer's size and industry, and its need for underwriter prestige, it does not really have much choice about which underwriter to pick.

Denote by P' the post-issuance secondary market price on the first trading day, and we have $P' = \frac{kV+NP}{1+N}$. The founder's gain in wealth via going public W is given by:

$$W(P) = P' - V = \frac{kV+NP}{1+N} - V. \tag{1}$$

The offer price P can be solved with a standard Nash bargaining game between the founder and the underwriter over the expected liquidity value. In equilibrium, the payoff to each party is equal to their bargaining power multiplied by the total surplus, which is the liquidity value $(k-1)V$. From the founder's perspective, we have:

$$W(P) = \beta(k-1)V. \tag{2}$$

Substituting Equation (1) to (2), we can solve for the offer price P,

$$P = \left[\beta k + (1-\beta) - \frac{(1-\beta)(k-1)}{N}\right]V. \tag{3}$$

$P' - V$ captures the dollar gain per share for the founder, and $P' - P$ captures the dollar gain per share for IPO investors. It is straightforward to verify that $P' > V$, and $P' > P$. Both the founder and IPO investors benefit from the IPO. Denote underpricing per share in the percentage term by U. Consistent with the literature, U is calculated as the difference between the post-IPO closing price P' and the IPO price P, and divided by P.

$$U = \frac{P'-P}{P} = (\frac{kV+NP}{1+N} - P)/P = \frac{1-\beta}{\frac{N}{k-1}+(N+1)\beta-1}. \tag{4}$$

With Equation (4), we can calculate the first derivative of underpricing U with respect to the (percentage) liquidity value $(k-1)$, the founder's bargaining power β, and new shares issued N:

$$\frac{\partial U}{\partial k} > 0, \frac{\partial U}{\partial \beta} < 0, \frac{\partial U}{\partial N} < 0. \tag{5}$$

Expressions in (5) show that the magnitude of underpricing is increasing in the expected liquidity benefit k, decreasing in the issuer's bargaining power β, and decreasing in new shares issued N. In the empirical analysis, we focus on the relation between underpricing and the liquidity benefit k, controlling for bargaining power and new shares issued.

The liquidity value is generated by the difference between the issuer's expect liquidity and its pre-IPO liquidity. Expected liquidity is the market's consensus expectation at the time of IPO for the issuer's post-IPO liquidity. Formally, we test the following two hypotheses.

H1: The magnitude of IPO underpricing should be positively related to the expected liquidity of the issuer, ceteris paribus.

H2: The magnitude of IPO underpricing should be negatively related to the pre-IPO liquidity of the issuer when it is private, ceteris paribus.

The expected liquidity can be measured using the "comparables" approach, while the issuer's pre-IPO liquidity as a private firm is not observable. In the next two sections, we describe in detail the data and empirical methodology employed to test these two hypotheses.

4. Sample and variable construction

We start sample construction by identifying US firm-commitment IPOs from Thomson Financial's SDC Global New Issues database from 1981 to 2015.[13] Following the literature, we exclude deals with offer prices less than \$5, unit offerings, ADRs, financial and utility offerings (SIC codes 6000–6999 and 4900–4999), certificates, shares of beneficial interest, companies incorporated outside the U.S., Americus Trust components, closed-end funds, REITs, and limited partnerships. Variables related to the deal and issuer characteristics (pre-issue assets, offer price, underwriters, VC-backed or not, and proceeds) are from SDC. Stock price and liquidity data (first-day closing price, bid price, ask price, number of shares outstanding, stock exchange, and trading volume) are from CRSP. We use underwriter ranking data on Prof. Jay Ritter's website. The monthly market sentiment data are from Prof. Jeffrey Wurgler's website (Baker and Wurgler 2006). We download the Fama-French 10-industry classification from Prof. Kenneth French's

[13] Some IPO studies examine both firm-commitment and best-effort issues (see Ritter, 1987; Booth and Chua, 1996) Meanwhile, many other IPO studies only examine firm-commitment issues (Carter and Manaster, 1990; Loughran and Ritter, 2004, among others). We exclude best-effort offers because best-effort offers are typically very small offerings.

website. The final sample consists of 3,775 deals.

Panel A of Table 1 presents the distribution of IPO deals by listing venues. There are three exchanges that issuer stocks are listed on: the New York Stock Exchange (NYSE), the American Stock Exchange (ASE), and Nasdaq. Nasdaq IPOs account for 85.8% of the whole sample, while NYSE deals account for 11.6%. The imbalance across exchanges is more severe during the tech bubble period in the 1990s. Over time, there are hot IPO periods such as the 1990s, and cold years such as 2001, 2002, and 2003, in the aftermath of bubble burst. Panel B shows the distribution of IPOs in each industry over time.[14] Consistent with stylized facts, the two industries with largest numbers of IPOs in the sample are Business Equipment (Computers, Software, and Electronic Equipment) and Healthcare. They each account for 34.3% and 17.8% of all deals.

The key variables in the baseline analysis are underpricing and expected liquidity. Underpricing is the percentage change from the offer price to the first-day closing price. Liquidity is measured by spread, turnover, or log AIM. Spread is the difference between ask price and bid price divided by the average of the two prices (mid-point price). Turnover is the daily trading volume divided by the number of shares outstanding. AIM is the Amihud's (2002) illiquidity measure, which is the absolute value of daily returns divided by daily dollar volumes, scaled by 10,000,000. Peer firms are defined by industry, size, and the listing stock exchange.[15] We divide the COMPUSTAT-CRSP universe of stocks into five quintiles by market capitalization each year, and select publicly traded stocks that are in the same size quintile, with the same two-digit SIC code, and listed on the same stock exchange as the issuer. For each issuer, its expected liquidity is defined as the average daily liquidity measures across its peer firms over a 12-month window before the IPO date, denoted by *Peer Spread*, *Peer Turnover*, and *Peer AIM* respectively. These measures are observable to the issuer and investors during book-building when the offer price is determined, so there is no look-ahead bias. In addition, they are unlikely to cause reverse causality concerns, because the issuer's offer price and its subsequent first-day close price on the secondary market should not affect its peer public firms' trading environment *before* its IPO time.

[14] Nine industries are presented because Industry nine (Utilities) are excluded.
[15] A common practice to identify peer firms is to use just industry and size (Albuquerque, 2009, among others). Our results are robust to constructing peer firms only by industry and size. We include listing exchanges because some studies suggest that different institutional designs in different stock exchanges can affect liquidity of listed stocks (see Huang and Stoll, 1996; Bessembinder and Kaufman, 1997).

The control variables are as follows. The model in Section 3 predicts that the bargaining power of the underwriter and the issuance size matter too. We use *Top Underwriter* as a proxy for bargaining power, which is a dummy variable that is one if the lead underwriter has an updated Carter and Manaster (1990) rank of eight or more, and zero otherwise. The literature also document a significant relation between the prestige of the underwriter and underpricing (see Carter and Manaster, 1990; Beatty and Welch ,1996; Liu and Ritter, 2011, among others). Habib and Ljungqvist (2001) propose that the dilution of shares as a result of new issuance should matter for IPO underpricing. To measure issuance size, we define *New Shares Ratio* as the percentage of IPO shares in the firm's total number of shares outstanding. Computationally, it is equivalent to the ratio of IPO proceeds divided by the product of offer price multiplied by the number of shares outstanding. Bradley et al. (2004) hypothesize that the integer versus fractional dollar offer prices are results of negotiations between issuers and underwriters. They find that deals with integer offer prices are associated with more underpricing. We include the dummy variable of *Integer Price*. To control for investor sentiment, we include *Sentiment* in the regression, which is equal to the monthly market sentiment index downloaded from Prof. Jeffrey Wurgler's website at IPO time.[16] The literature has shown that whether the issuer is backed by VC funds is related to underpricing (see Lee and Wahal, 2004, among others). We include the dummy variable *VC-backed* as a control. Finally, we also control for the issuer's asset size and age. More details of variable construction can be found in Appendix. All variables are winsorized at the 1st and 99th percentile level.

Table 2 presents summary statistics of variables. Panel A describes the distribution of each variable and Panel B is the correlation matrix. The average underpricing is 21.5%, with a standard deviation of 38.8%. The average daily spread and turnover of public peer firms are 3.50% and 0.80% in the 12-month period before the issuer's IPO time. About 60.7% of IPO underwriters are large investment banks with the *Top Underwriter* status. 82.1% of IPOs have integer offer prices. On average, the issuer sells 32.2% of the ownership to raise capital in IPOs. The average sentiment is 0.32. The average asset value of issuers is $184 million, with a large variation as its standard deviation is $578 million. The average age of issuers is 14.8 years. Around 44.7% of sample issuers are backed by VC funds.

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[16] Lee, Shleifer, and Thaler (1991) argue that individual investor sentiment is an important factor that determines when companies go public. Ljungqvist, Nanda, and Singh (2006) hypothesize that regular investors sell IPO stocks to sentiment investors.

Panel B shows that underpricing has negative correlations with *Peer Spread* (-0.23 for Pearson correlation, and -0.22 for Spearman correlation), positive correlations with *Peer Turnover* (0.47 for Pearson correlation, and 0.31 for Spearman correlation), and negative correlations with *Peer AIM* (-0.30 for Pearson correlation, and -0.32 for Spearman correlation). Additionally, underpricing is positively correlated with *Top Underwriter, Integer Price*, and *VC-backed*, and negatively correlated with *New Shares Ratio*, *Sentiment*, and *Age*.

5. Empirical tests and results

In this section, we test the two hypotheses developed in the illustrative model in Section 3. First, we develop a baseline regression testing the relation between underpricing and expected liquidity. We then test the cross-sectional variation of this relation based on certain firm characteristics. Next, we use changes to OHR at Nasdaq in 1997 as an exogenous shock to expected liquidity of Nasdaq deals, so we can study the effect of expected liquidity on underpricing in a difference-in-difference setting. Lastly, we use NSMIA as an exogenous shock to the pre-IPO liquidity of issuers to test how pre-IPO liquidity affects underpricing, while controlling for expected liquidity.

5.1 The baseline

The baseline regression tests the hypothesis of H1 directly by investigating whether there is a positive relation between underpricing and the issuer's expected liquidity. We regress underpricing on expected liquidity and control variables. The regression equation is specified as follows,

$$Underpricing_{ijt} = \alpha_{jt} + \beta_1 Expected\ Liquidity_{ijt} + \gamma' X_{ijt} + \varepsilon_{ijt}, \tag{6}$$

where i, j, t index firms, industries, and years, respectively. X_{ijt} is a vector of control variables, explained in the last section. H1 predicts that $\beta_1 > 0$. We use public peers' liquidity measures *Peer Spread*, *Peer Turnover*, and *Peer AIM* to proxy for the expected liquidity of the issuer. These measures can ward off reverse causality concerns, as it is unlikely that the offer price and the later secondary market first-day price of an issuer can affect its public peers' trading environment on the secondary market a year before IPO time. To further address endogeneity concerns, we control for any unobserved time-varying industry effects that can affect both underpricing and expected liquidity by including the industry-year fixed effects α_{jt}.[17] To address for potential correlations of error terms among deals in the cohort of the same industry and year, we cluster standard errors in every industry-year group.

The results of regression Equation (6) are presented in Table 3. Expected liquidity is measured by *Peer Spread* in Columns (1) and (2), by *Peer Turnover* in Columns (3) and (4), and by *Peer AIM* in Columns (5) and (6). For each measure, we run the regression with two specifications: with and without the interacted industry-year fixed effects α_{jt}. In all specifications, underpricing is found to be positively related to expected liquidity, as the coefficients on *Peer Spread* and *Peer AIM* are negative, and the coefficient on *Peer Turnover* is positive. The coefficients are statistically and economically significant across all specifications. In Column (2), one standard deviation increase in peer spread (3%) reduces underpricing by 6.5% (=3%×-2.152) in absolute terms. Considering that the sample average underpricing is 21.5%, this is a 30% reduction from the mean (=6.5/21.5). In Column (4), one standard deviation increase in peer turnover (0.5%) increases underpricing by 13.3% (=0.5%×26.584), which is a 61.9% increase from its mean (=13.3/21.5). In Column (6), one standard deviation increase in peer AIM (1.6) reduces underpricing by 9.6% (=1.6×-0.06), which is a 44.7% reduction from the mean (=9.6/21.5). The results on expected liquidity are robust to the inclusion of industry-year fixed effects, suggesting that underpricing is higher when an issuer has better expected liquidity than peer issuers in the same industry in the same IPO year.

We also find supporting evidence for model predictions regarding relations between underpricing and the underwriter's bargaining power and the issuance size. Consistent with the theory, *Top Underwriter* is positively related to underpricing, and its coefficient is statistically significant in all specifications. *New Shares Ratio* is negatively and significantly related to underpricing, that is, the larger the fraction of the ownership an issuer sells at IPO, the smaller the underpricing. Results on other control variables are also consistent with existing literature. When the offer price is an integer, there is more underpricing. Investor sentiment is found to be negatively related to underpricing. In most specifications, larger and older issuers are associated with lower

[17] We use Fama-French 10-industry classification, since with the industry-year interaction fixed effects, using Fama-French 48-industry classification generates too many explanatory variables and lowers the power of the test.

underpricing, which is likely due to less asymmetric information, or more bargaining power of the issuer. VC-backed IPOs have higher underpricing compared to non-VC-backed ones.

We conduct the following robustness checks to the baseline and the results remain. (i) We use the issuer's own secondary-market liquidity measures (*Issuer Spread*, *Issuer Turnover*, and *Issuer AIM*) in the 12-month period following IPO as proxies for expected liquidity.[18] (ii) We follow the definition of tech firms in Loughran and Ritter (2004), and find that about 42.4% of the deals are tech stocks.[19] We exclude these deals from the sample, to address the possibility that one particular industry drives the results, or that the results are driven by the tech-bubble period. (iii) We use alternative time horizons of six months or nine months prior to the IPO date, when we construct peer liquidity measures. The results are robust to all the above alternative specifications.

5.2 Cross-sectional analysis of the baseline

Having established the positive relationship between underpricing and expected liquidity, in this section we explore cross-sectional variations in this relation. VC funds typically have limited investment horizon (mostly up to ten years) and therefore often have stronger incentives to exit their investments compared to founders or employees after IPO. Better liquidity of the issuers' shares in the secondary market is particularly valuable for venture capitalists, because the unwinding of their positions would generate less price impact and higher payoffs. Hence, we expect a stronger relationship between expected liquidity and underpricing for VC-backed issuers than non-VC-backed issuers. We test this conjecture by adding the interaction term of the dummy variable of VC-backed and expected liquidity in the baseline regression.

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[18] Using the issuer's realized liquidity post-IPO assumes that the issuer and the underwriter can predict its post-IPO liquidity on the secondary market accurately. Peer liquidity and issuer liquidity measures are highly correlated. For example, the correlation between *Peer Spread* and *Issuer Spread* is 0.73 and they have similar magnitudes. All results with the baseline remain and some are stronger with these alternative measures. Admittedly, using the issuer's own realized liquidity involves look-ahead bias and is likely to generate endogeneity concerns. That's why we do not include this analysis in the main text of the paper.

[19] In Loughran and Ritter (2004), tech stocks are defined as those in SIC codes 3571, 3572, 3575, 3577, 3578 (computer hardware), 3661, 3663, 3669 (communication equipment), 3671, 3672, 3674, 3675, 3677, 3678, 3679 (electronics), 3812 (navigation equipment), 3823, 3825, 3826, 3827, 3829 (measuring and controlling devices), 3841, 3845 (medical instruments), 4812, 4813 (telephone equipment), 4899 (communications services), and 7371, 7372, 7373, 7374, 7375, 7378, and 7379 (software).

We also rely on the theoretical model in Section 3 for guidance of more cross-sectional tests. Expressions in (5) show the first-order derivative of underpricing with respect to liquidity value k, the founder's bargaining power β, and shares of new issuance N. Since we are interested in how the relation between underpricing and liquidity value varies in the cross section, we can further take derivatives of $\partial U/\partial k$ with respect to β and N, and show

$$\frac{\partial^2 U}{\partial k \partial \beta} < 0, and\ \frac{\partial^2 U}{\partial k \partial N} < 0. \tag{7}$$

We test the cross-sectional predictions in (7) in a regression by interacting expected liquidity with *Top Underwriter* and *New Shares Ratio*, respectively. In the model, the initial number of shares outstanding is normalized to one, so the term N is empirically equivalent to *New Shares Ratio.* Since the underwriter's bargaining power is $1 - \beta$, we expect a positive coefficient on the interaction term of *Top Underwriter* and expected liquidity.

The regression results are presented in Table 4. For the sake of brevity, regression results on control variables including the dummy variable of integer price, sentiment, asset size, age, and the constant term are not presented in the table. We continue to find strong support for hypothesis H1 across all three liquidity measures (*Peer Spread*, *Peer Turnover*, and *Peer AIM*). While the coefficient on the *VC-backed* dummy is positive when the interaction terms are not included, Columns (1), (4), and (7) show a significantly positive (negative) relationship between underpricing and the interaction term of *VC-backed* and liquidity (illiquidity). This implies that on average, VC-backed issuers leave more money on the table, but that positive relation mostly come from the subset of issuers whose expected liquidity value is high via IPO. Take Column (7) for example, for issuers with expected AIM around 0.36 (25 percentile in the sample), the combined coefficient on *VC-backed* is 0.166 (=0.197-0.087×0.36); for issuers with expected AIM around 2.89 (75 percentile in the sample), the combined coefficient on *VC-backed* is -0.054 (=0.197-0.087×2.89). This is consistent with our conjecture that VC funds are willing to leave more money on the table when IPOs are more beneficial in terms of improved liquidity, due to their needs to exit investments in time.

Columns (2), (5), and (8) show that the coefficients on *Expected Liquidity*×*Top Underwriter* are significantly positive (negative when measured by illiquidity). This finding indicates the relation between underpricing and liquidity is stronger when the underwriter has stronger bargaining power, and supports the prediction in Equation (7). When the underwriter's bargaining power is stronger, he can negotiate for more payoffs in the form of underpricing for IPO investors, per unit on liquidity value gain. In Columns (3), (6), and (9), we find that the coefficients of the interaction terms between liquidity (illiquidity) and *New Shares Ratio* are significantly negative (positive). The relation between underpricing and liquidity is stronger when fewer new shares are issued in an IPO. This is also consistent with the prediction in Equation (7).

5.3 Changes in the Order Handling Rules at Nasdaq

In the baseline, we show that underpricing is positively related to the issuer's expected liquidity, and we measure expected liquidity with the issuer's public peer firms' liquidity *prior to* the IPO time. With this measure, there is unlikely a reverse causality problem. We also include the industry-year fixed effects, to control for any unobserved time-varying industry effects driving both public peers' liquidity and underpricing. Still, this might not be a complete solution for the endogeneity concern. Also, there is a potential measurement error issue as the pre-IPO liquidity is not included in the baseline regression. In this section, we conduct a difference-in-difference test for hypothesis H1 with an exogenous shock to expected liquidity to a subset of issuers using an important regulation change, to formally address these concerns.

The regulation change involves the change to Order Handling Rules (OHR) on Nasdaq in 1997. Christie and Schultz (1994) first expose the lack of odd-eighth quotes on Nasdaq, which help reveal the scandal of Nasdaq dealers colluding to enhance the profitability of their market-making business. Specifically, some dealers did not include competitive limit orders from customers when these orders are better than their own quotes. By doing so, they managed to artificially maintain higher spread of stocks, which suppressed liquidity in the market. In the aftermath of the scandal, SEC enacted several major changes to OHR. First, the Limit Order Display Rule was phased in for all Nasdaq National Market System issues from January 20, 1997 to October 13, 1997. The rule requires that limit orders should be displayed in the Nasdaq BBO (i.e., best bids and offers) when they are better than quotes posted by market makers. This allows the general public to compete directly with Nasdaq dealers. Second, the Quote Rule requires market makers to publicly display their most competitive quotes. Third, the Actual Size Rule reduces the minimum quote size of market makers from 1000 shares to 100 shares and thereby decreases dealers' market-making risk, and encourages them to maintain more competitive quotes. Lastly, the Excess Spread Rule is amended so that dealers' average spread during each month must be smaller than 150% of the average of the three narrowest spreads over the month. Prior to this, dealers face a similar requirement but on a continuous basis. Changing it to a monthly basis poses less restriction on dealers' ability to change their spreads. All these changes help improve the liquidity of stocks listed on Nasdaq (see Bessembinder 1999, and Barclay et al. 1999).

Using the changes of OHR on Nasdaq in 1997 as an exogenous shock to the expected liquidity of IPOs listed on Nasdaq, we can test the relation between expected liquidity and underpricing in a difference-in-difference framework. The first level of difference is the difference in the magnitude of underpricing before and after 1997. The second level of difference is the difference of the first-level difference among IPO deals listed on Nasdaq and non-Nasdaq exchanges. If we find that after 1997, Nasdaq deals tend to have more underpricing than before 1997, it could be due to a common time trend that is unrelated to the liquidity shock. Only if non-Nasdaq IPOs do not experience the same level of increase in underpricing after 1997, we can rule out the possibility of a common time trend. By using Nasdaq deals as the treatment group and non-Nasdaq deals as the control group, we can identify the impact of expected liquidity on underpricing. We estimate the following regression equation.

$$Underpricing_{ijt} = \alpha_j + \beta_1 Nasdaq_i \times Post_t + \beta_2 Nasdaq_i + \beta_3 Post_t + \gamma' X_{ijt} + \varepsilon_{ijt}, \quad (8)$$

Where i, j, t index firms, industries, and years, respectively. *Nasdaq* is the dummy variable that is equal to one if the issuer is listed on Nasdaq and zero otherwise; *Post* is the dummy variable if IPO occurs after 1997 and zero otherwise. X_{ijt} is the same vector of control variables as in Equation (6), including *Top Underwriter*, *New Shares Ratio, Integer Price*, *Sentiment, Log(Assets), Log(1+Age),* and *VC-backed*. Standard errors are clustered at the level of industry-year groups. H1 predicts that $\beta_1 > 0$, that is, underpricing should increase more (or, decrease less) after 1997 for Nasdaq-listed deals than non-Nasdaq-listed deals, due to improved liquidity of Nasdaq shares after 1997. Because this is essentially an event study, we take the sample years of 1994-2000, which covers the six-year period before and after 1997. Issuers that went public in 1997 are excluded. We include only industry fixed effects but not year fixed effects, due to the relatively shorter time window compared to the full sample period, and the inclusion of *Post*, which is a dummy variable indicating years before and after the exogenous shock.[20]

As a preliminary investigation, we plot the average underpricing of deals listed on Nasdaq and non-Nasdaq exchanges in each year from 1994 to 2000 in Panel A of Figure 2. The figure shows that both the time trend and levels of underpricing are extremely similar for Nasdaq and non-Nasdaq deals before 1997. After 1997 underpricing increases in both groups, but the increase is significantly higher for Nasdaq IPOs. In particular, from 1997 to 1998, shortly after the enactment of OHR changes, underpricing at Nasdaq increased tremendously from the previous year while underpricing at other exchanges actually decreased. This visual examination suggests that there is a real impact of OHR changes on underpricing.

Since the post-1997 era coincides with the tech stock bubble in 1998 and 1999, and many tech stocks go public on Nasdaq, we draw the same plot but excluding all tech stocks as defined in Loughran and Ritter (2004), in Panel B of Figure 2. The figure shows that Nasdaq IPOs have lower underpricing than non-Nasdaq deals before 1997, but higher underpricing than non-Nasdaq deals after 1997. The pattern that OHR affects underpricing is even more pronounced. This rules out the possibility that the result is driven by the tech bubble.

We then carry out the regression analysis of Equation (8) and present the results in Table 5. We compare underpricing in the periods of three years before and after 1997 (1994-2000), and two years before and after 1997 (1995-1999) in Column (1) and (2) respectively. There are 1,273 Nasdaq deals and 337 non-Nasdaq deals in Column (1), and 897 Nasdaq deals and 251 non-Nasdaq deals in Column (2). Consistent with the theory, the coefficient on *Nasdaq*×*Post* is significantly positive in both sample periods. This suggests that Nasdaq IPOs exhibit more underpricing post 1997 compared to non-Nasdaq IPOs. Combining the coefficient on *Nasdaq*×*Post* with the one on *Nasdaq*, we find that, Nasdaq IPOs experience less underpricing prior to 1997, but more underpricing after 1997. Taking Column (1) as an example, we can compute that average underpricing of Nasdaq IPOs is 5.6% less than that of non-Nasdaq IPOs before 1997 (based on the coefficient of -0.056 on *Nasdaq*), and is 15% higher after 1997 (=0.206-0.056, where 0.206 is the

[20] As a robustness check, we replace the *Post* dummy variable with individual year fixed effects, and the results remain. Using the *Post* dummy variable enables direct comparison of average underpricing before and after the treatment event.

coefficient on *Nasdaq*×*Post*). Combining the coefficient on *Nasdaq*×*Post* with the one on *Post* also enables us to confirm the visual finding in Figure 2: average underpricing of Nasdaq deals increased significantly after 1997 (the coefficient on *Nasdaq*×*Post* is 0.206), while that of non-Nasdaq deals of the same time window stayed almost flat (the coefficient on *Post* is 0.049 and not significant), controlling for issuer and deal characteristics. We hence conclude that the economic impact of OHR changes on the underpricing of Nasdaq IPOs is large and significant. The results on control variables remain the same as in earlier analysis.

To address the concern that firms endogenously choose where to be listed and thus Nasdaq IPOs and non-Nasdaq IPOs can be fundamentally different, we run the same regression using a matched sample. We match each Nasdaq IPO with a non-Nasdaq deal from the same year in the same industry (SIC two-digit code) and with a similar size (market capitalization). For size matching, each year we select all IPO deals and divide them into five quintiles by ranking their market capitalization in the first year post IPO. If there are multiple matches, we select the one with the smallest size difference. Only Nasdaq deals with a matched control deal are included in the sample. There are 683 (531) Nasdaq deals and an equal number of matched non-Nasdaq deals in the period of 1994-2000 (1995-1999). Note that there are more non-Nasdaq deals in the matched sample compared to the full sample. This is because there are more Nasdaq IPOs than non-Nasdaq IPOs in the unmatched sample. In contrast, in the matched sample, one non-Nasdaq deal can be shared by multiple Nasdaq deals as a control, so it can appear multiple times in the data and is counted each time as a separate observation.

Regression results of the matched sample are presented in Columns (3) and (4) of Table 5, with the two sample periods of 1994-2000 and 1995-1999. The main finding of a positive and significant coefficient on *Nasdaq*×*Post* remains robust in both specifications. Of the two sample periods, the smaller estimate is $\beta_1 = 0.177$ in Column (3), which implies a relative increase of 17.7% in underpricing after 1997 for Nasdaq IPOs compared to non-Nasdaq IPOs. Based on the average IPO underpricing of 21.5%, the marginal effect of OHR measured by β_1 features an 82.3% (=17.7/21.5) increase of underpricing from its mean level. Combining the coefficient on *Nasdaq*×*Post* and the ones on *Nasdaq* or *Post*, we reach similar conclusions to what we have with the full sample, shown in the first two columns of Table 5.

Next, we conduct placebo tests to show that the significant change in the trend and level of underpricing for Nasdaq and non-Nasdaq deals occurring in 1997 is not just a fluke of data or due to fundamental differences between Nasdaq and non-Nasdaq deals. We take the pre-treatment period of the sample from 1981 to 1996 and select moving windows of seven years with a pseudo event occurring in the 4th year, when only Nasdaq stocks are assumed to undergo an exogeneous shock in liquidity. There are in total ten such event windows, with the pseudo event years being each year between 1984 and 1993. We run the same regression equation (8) for the full sample in these event windows with three years before and after the pseudo event year. We do not find any significant coefficients for the interaction term *Nasdaq×Post.* The results are presented in Table 6.

Another potential issue is that changes to OHR at Nasdaq, as a shock to the expected post-IPO liquidity, may create some selection bias because firms choose whether to go public strategically and such decisions may be affected by the liquidity value of the deal. However, we argue this selection channel should bias against finding a result. After the positive shock of OHR, the liquidity value of going public increases, which motivates some firms with low liquidity gain to go public who would otherwise choose to stay private. The entry of these marginal firms into the IPO market would contaminate the treated group and reduce the treatment effect. Hence, we believe this possible selection channel in fact makes our results stronger.

Lastly, we verify that changes to OHR at Nasdaq are indeed shocks to expected liquidity of issuers at Nasdaq and issuers at other exchanges are not subject to the same shock. We test the relation between the regulation change and expected liquidity directly. We use the monthly spread, turnover, and log AIM (averages of daily data) of each individual public peer as the dependent variable, instead of the average value across peer firms, for more accuracy of the test. To differentiate from the variables of *Peer Spread*, *Peer Turnover*, and *Peer AIM* defined earlier as the average liquidity across peer firms, we name them *Individual Peer Spread*, *Individual Peer Turnover*, and *Individual Peer AIM*. We run the following regression equation.[21]

$$Indiviual\ Peer\ Liquidity_{ijt} = \alpha_j + \beta_1 Nasdaq_i \times Post_t + \beta_2 Nasdaq_i + \beta_3 Post_t$$

$$+\gamma' Z_{ijt} + \varepsilon_{ijt}, \qquad (9)$$

[21] In Equation (9), some control variables have monthly frequencies, some firm-level characteristics have annual frequencies, and we run monthly regressions.

where liquidity is measured by spread, turnover, or log AIM, and i, j, t index firms, industries, and months, respectively. Z_{ijt} is a vector of control variables shown in previous studies that are related to firm liquidity. The literature documents commonality of liquidity, so we control for market level variables such as the market return, the lagged market return, the variance of daily market returns, market sentiment, and the interest rate measured by the three-month T-bill rate (see Huberman and Halka, 2001; Chordia, Roll, and Subrahmanyam, 2000, 2001, among others). All market level variables are of monthly frequency.[22] We also control for peer firms' characteristics such as the log of peer firm age as a public firm, the log of sales, the log of market capitalization, and the number of shareholders as a measure for ownership diversity. All firm characteristics are annual observations downloaded from COMPUSTAT. Industry fixed effects are included. Standard errors are clustered at the firm level as the dependent variable is a firm-specific liquidity measure. We also run the regression for two periods of 1994-2000 and 1995-1999, excluding the year of 1997.

The results are presented in Table 7. Columns (1) and (2) show that in both periods, individual peer spread drops more for Nasdaq issuers than non-Nasdaq issuers and the difference is highly significant. Based on the results in Column (1), we document that peer firms of Nasdaq issuers experience a drop of 3.3% in quoted spread relative to those of non-Nasdaq issuers after 1997, in absolute terms. This is economically large as the average peer spread is 3.5%. Taking the coefficient on *Nasdaq*×*Post* and the one on *Post* in Column (1), we estimate that after the changes to OHR, average individual peer spread decreased 1.9% (= -0.033 + 0.014 = -0.019) for Nasdaq issuers, while average individual peer spread increased 1.4% (= 0.014) for non-Nasdaq issuers, in the period of 1994-2000. Column (2) shows similar patterns with slightly different magnitudes in the period of 1995-1999. We conclude that changes to OHR impact Nasdaq issuers' expected trading spread, in an economically significant way. This is consistent with the purpose of the regulation change, as changes to OHR are designed to reduce quoted spread on Nasdaq.

Columns (3) and (4) report the results when liquidity is measured by turnover. The coefficient on *Nasdaq*×*Post* is statistically significant and positive in both periods. Using the coefficient

[22] The market return is the value-weighted NYSE/AMEX/NASDAQ/ARCA return reported by CRSP. The variance of market returns is the variance of daily market returns in a given month. Monthly market sentiment is downloaded from Prof. Jeff Wurgler's website. The monthly three-month T-bill rate is download from the Federal Reserve Bank's website.

estimates of *Nasdaq*×*Post* and *Nasdaq* in Column (3), we estimate that on average, average peer turnover of Nasdaq issuers is 0.3% higher than that of non-Nasdaq issuers before 1997, but even more so after 1997, when the difference becomes 0.4% (=0.001+0.003). Given that average peer turnover is 0.5%, the impact of changes to OHR at Nasdaq on turnover is also economically large.

Lastly, Columns (5) and (6) show that the price impact of trades becomes smaller for Nasdaq shares after 1997 compared to non-Nasdaq shares. Taking Column (5) for example, combining the coefficients on *Nasdaq*×*Post* and *Nasdaq*, we can see that the price impact measured by log AIM at Nasdaq was higher than that at NYSE and ASE by 0.182 before 1997, but became lower than the latter after 1997 by 0.055 (=-0.237+0.182). The coefficient on the *Post* dummy also shows that the price impact on NYSE and ASE increased after 1997 (their average log AIM increased by 0.114), but it decreased on Nasdaq (its average log AIM decreased by 0.123, which is -0.237+0.114). In summary, Table 7 shows that changes to OHR at Nasdaq indeed improves Nasdaq issuers' expected liquidity, measured by individual spread, turnover, and price impact of trades of its peer firms listed on Nasdaq. To make sure that the tech bubble is not driving these results, we drop all tech firms as defined in Loughran and Ritter (2004) and repeat the analysis in Table 5 and Table 7, and the results remain.

5.4 The National Securities Market Improvement Act (NSMIA)

Based on the model in Section 3, the hypothesis H2 predicts a negative relation between underpricing and the issuer's pre-IPO liquidity. Intuitively, if the pre-IPO liquidity of issuers is better, the liquidity value gained via IPO is lower, reducing the surplus in the negotiation as well as the need for underpricing. However, testing H2 is more complicated than testing H1, because a secondary market for private shares does not widely exist. Most private firms have restrictions on shareholders selling their shares to a third party, for concerns of control rights. And such transactions are normally privately negotiated if they occur. As a result, we cannot construct liquidity measures for private shares the same way as we do for public shares such as spread, turnover, or AIM, and subsequently test a similar baseline for H2 to the one for H1 specified by Equation (6).

Meanwhile, the most common type of trading for private shares is by founders themselves when they raise capital from private share investors. Hence, we measure the pre-IPO liquidity of private firms by how easily founders can raise capital. When it is easier for founders to sell ownership in exchange for external equity capital, the private firm receives higher valuations, which is equivalent to the concept of smaller price impact for public shares. One important factor determining how easily private firms can raise capital is the size of the pool of potential investors available, which we use as the proxy for the liquidity of private shares in this section. Based on this measure, we test H2 by exploiting a law change that asymmetrically affects private firms' access to VC and PE investors and adopting a difference-in-difference approach. Even though the firm-specific pre-IPO liquidity of issuers is not directly observable, we can use the law change as an exogenous shock to pre-IPO liquidity and compare underpricing before and after the law change across deals. The law change examined is the National Securities Market Improvement Act (NSMIA), passed in October 1996. Ewens and Farre-Mensa (2017) provide an excellent and detailed description of the law. We describe and summarize the law as follows.

NSMIA brings two major changes to the issuance and trading of private securities. First, before the law change, a private firm seeking to raise capital needs to comply with state regulations known as blue-sky laws, in addition to federal regulations such as Regulation D. Since these state regulations are often complex and different from each other, any private firm raising capital from multiple states faces significant regulatory burdens. NSMIA creates certain federal provisions that exempt qualified private security issuers from having to comply with these blue-sky laws in each state where they raise capital. Specifically, securities sold under Rule 506 of Regulation D, which allows private firms to raise unlimited amount of capital when the investors are "accredited investors", are exempted.[23] This exemption also applies to the fundraising of many VCs and PEs.

Second, NSMIA affects VC and PE funds directly through changes to the Investment Company Act of 1940. The Act mandates that most investment advisors must register with the SEC, regularly disclose their investment positions, and limit their use of leverage. VC and PE funds have often relied on the Act's exemption to avoid having to comply with its costly registration and disclosure requirements. NSMIA expanded these exemptions and made it easier for VC and PE funds to

[23] "Accredited investors" are institutions, individuals with annual income above $200K ($300K for couples), or individuals and couples with net worth above $1 million excluding the primary residence.

satisfy the exemption criteria. The law effectively removes the 100-investor cap in private investment funds, allowing these funds to raise capital from a larger number of investors and prompting the rise of large VC and PE funds. This directly improves the liquidity of private securities by broadening the pool of potential buyers and increasing the amount of equity capital available for private firms. The market for private securities has also become more professionalized, with VC and PE funds and operating businesses all vying for opportunities to invest in private companies or to acquire them outright (see De Fontenay, 2017).

Both features of NSMIA improve the liquidity environment of private firms. They not only make it easier for private firms to raise capital, but also directly expand the pool of potential investors in private firms. Even though the law impacts all private firms in the U.S. at the national level, we conjecture that the effect is more pronounced for private firms located in states with less local VC and PE funds. For example, consider a private firm located in San Francisco versus another one in North Dakota. For the firm in San Francisco, raising capital only within the state of California is likely to satisfy all of its capital needs, and thus the passage of NSMIA hardly makes any difference. While for the firm in North Dakota, due to the lack of in-state private capital and investors, it needs to face heavy compliance obstacles dealing with other state blue-sky laws prior to NSMIA, and the passage of NSMIA alleviates this burden substantially. Meanwhile, by removing the 100-investor cap, NSMIA should increase the amount of capital managed by VC and PE firms more in states with more such firms to begin with. As a result, NSMIA should have a larger impact on the North Dakota firm's pre-IPO liquidity than that of the San Francisco firm. Motivated by the regulation's differential impact on firms located in different states, we adopt the difference-in-difference approach. We construct the treatment group as issuers located in states with few in-state VC and PE investors, and the control group as issuers in states with abundant private share investors. We can then compare the change of underpricing of the treatment group with that of the control group.

We collect the number of VC and PE firms by state and year from Thomson Reuters Eikon. Since NSMIA is enacted towards the end of 1996, we take 1996-1997 as the event time, and focus on the period of three years before and after the law change (1993-2000).[24] We then rank states in

[24] Unlike changes to OHR at Nasdaq that affect the liquidity of public firm listed there immediately, it could take NSMIA longer time to impact the liquidity of private firms and the magnitude of IPO underpricing.

this period by the total number of these firms in the event period of 1993-2000. The ranking is shown in Table 8. Not surprisingly, we find that the number of VC and PE firms from the top four states of CA, NY, MA, and TX together account for 57.89% of all such firms in the entire country. Issuers located in these states are more likely to have larger number of potential investors within the state, and issuers located in other states have relatively fewer potential investors locally and therefore are more likely to raise capital from other states. We take issuers in the top four states as the control sample, and issuers outside these states as the treatment sample.

As a first look at the data, we plot the average underpricing of deals in these two subsamples in each year from 1993 to 2000 in Panel A of Figure 3. The figure shows that both the time trend and levels of underpricing are extremely similar in the two groups before 1996. After 1997 underpricing increases in both groups, but the increase is significantly lower for the treatment group, which is consistent with our conjecture. For concerns that the tech bubble period drives the results, we replot the figure excluding tech firms in Panel B of Figure 3, and find a similar pattern. The smaller magnitude of underpricing for the treatment sample is especially pronounced in 1998 and 2000. We hypothesize that IPO underpricing should decrease more (or, increase less) for issuers in the treatment sample after the passage of NSMIA, controlling for expected liquidity. We run the following difference-in-difference regression.

$$\begin{aligned} Underpricing_{ijt} = \alpha_j + \beta_1 Treated \times Post + \beta_2 Treated + \beta_3 Post + \\ \beta_4 Expected\ Liquidity_{ijt} + \gamma' X_{ijt} + \varepsilon_{ijt}, \end{aligned} \tag{10}$$

where i, j, t index firms, industries, and years, respectively. *Treated* is the dummy variable that is equal to one if the issuer is headquartered outside of the control states (CA, NY, MA, and TX), and zero otherwise. We also explore alternative control samples with the top eight states (CA, NY, MA, TX, IL, CT, PA, and NJ), or the top two states (CA and NY). *Post* is the dummy variable if IPO occurs after 1997 and zero if it occurs before 1996. Standard errors are clustered at the level of industry-year groups.

H2 predicts that $\beta_1 < 0$. We explicitly control for the post-IPO expected liquidity as the shocks here are to pre-IPO liquidity. X_{ijt} is the same vector of control variables as in Equation (6), which are *Top Underwriter*, *New Shares Ratio, Integer Price*, *Sentiment, Log(Assets), Log(1+Age),* and *VC-backed*. For the same reasons explained in Equation (8), we do not include year fixed effects but just industry fixed effects in Equation (10). Replacing the *Post* dummy with year fixed effects does not change the key results. We investigate two event windows around the event: three years before and after the law change (1993-2000) and two years before and after the law change (1994-1999), both excluding 1996 and 1997.

The regression results are presented in Table 9. We control for *Peer Spread*, *Peer Turnover*, and *Peer AIM* in Panel A, B, and C, respectively. The three panels are otherwise identical except for the definition of expected liquidity. Columns (1) and (2) use issuers from the top eight states with the largest number of VC and PE firms (CA, NY, MA, TX, IL, CT, PA, and NJ) as the control firms, whereas Columns (3) and (4) use issuers from the top four states (CA, NY, MA, and TX) and Columns (5) and (6), the top two states (CA and NY). Across all 18 specifications (two sample periods × three measures of liquidity × three control samples), we find negative coefficient consistently on *Treated×Post,* statistically significant in all but two specifications.[25] Since all specifications generate qualitatively similar results, we only describe Panel A in detail below.

Combining the coefficients on *Treated×Post* with the ones on *Treated* or *Post*, we reach two interesting conclusions. First, before the enactment of NSMIA, issuers located in states with less potential investors (the treatment group) experience larger underpricing than issuers located in states with more investors (the control group). This is reflected in the positive coefficient on *Treated* across all columns, which is also statistically significant in six out of 18 specifications. Based on these results, we estimate that underpricing for these issuers are about 1% to 4% higher. This itself is an intuitive finding, as it implies that IPO is especially important and beneficial for firms located in states without a large pool of potential investors before 1996, and these issuers are thus willing to leave more money on the table while going public. After the enactment of NSMIA, the pattern flipped, as issuers in the treatment group experience smaller underpricing than issuers in the control group. This is implied by the negative coefficient on *Treated×Post* and the positive coefficient on *Treated*, and the fact that the magnitude of the former is always larger than the magnitude of the latter. For example, Column (3) shows that the difference is -14% (=-0.178+0.041).

[25] The only exceptions are Columns (2) and (5) in Panel B, where the t-statistics of *Treated* × *Post* are -1.32 and -1.61, respectively.

Second, the coefficient on the *Post* dummy is positive and statistically significant in 14 out of 18 specifications. This suggests that, during the sample period, underpricing increased for the control sample, but it increased less or even decreased for issuers in the treatment group. For example, the estimates in Column (3) of Panel A show that underpricing increased by 22.2% for issuers in the control group in 1993-2000, but only increased by 4.4% (=0.222-0.178) for issuers in the treatment group in the same period. Column (4) shows that while underpricing for the control group increased by 12.9% in 1994-1999, it actually decreased for the treatment group by 0.5% (=0.129-0.134). The coefficient estimates on control variables remain consistent with earlier analysis. Overall the evidence suggests that after the passage of NSMIA, the liquidity benefit provided by going public becomes smaller for issuers in the treatment group compared to issuers in the control group. And the compensation received by IPO investors from these issuers, which is measured by underpricing, becomes lower than that from the issuers in the control group. We conclude that NSMIA has significantly different economic impact on issuers in the treatment sample versus the control sample.

Similar to the analysis in Section 5.3, we also conduct placebo tests using the pre-treatment period before 1996. We use the top eight states (CA, NY, MA, TX, IL, CT, PA, NJ) as the control sample and the rest of the states as the treatment sample. We select moving windows of eight years with a pseudo event occurring in the 4th and 5th year (as the NSMIA event years include two years), when only private firms in treatment states are assumed to undergo an exogeneous shock in liquidity. There are in total nine such event windows, with the pseudo event years being every two years between 1984 and 1993. We run the same regression equation (10) in these event windows with three years before and after the pseudo event years. We do not find any significant coefficients for the interaction term *Treated*×*Post.* We present the results when expected liquidity is proxied by *Peer AIM* for brevity in Table 10.[26]

One might be concerned with the endogenous choice of locations by private firms, and argue that the treatment effect is not randomly assigned among issuers. In particular, private firms may deliberately choose to be close to potential investors and be around where the capital is. While we completely agree with this point, we argue that the self-selection problem is only likely to attenuate

[26] We find similar results with alternative specifications such as using top four or top two states instead of top eight as the control sample, restricting the event window to be two years instead of three years before and after pseudo event years, using *Peer Spread* or *Peer Turnover* as the proxy for expected liquidity.

our results. Location choices of a start-up company could be a rational decision determined by many factors such as the hometown of the founders, the proximity to valuable human capital, the friendliness of the business environment, besides the proximity to capital. For firms that choose to be physically close to potential investors, capital must be one of the most important factors. Hence, one can infer that raising capital should be more crucial for issuers from San Francisco, who choose to be in the same city as numerous VC and PE funds than issuers from North Dakota, who have to cross state borders to reach a large private investor. Therefore, NSMIA, which is designed to facilitate capital raising by private firms, should have a larger impact on issuers from San Francisco than issuers from North Dakota, if these two firms were randomly assigned to the same state. Self-selection thus should weaken our results on *Treated* × *Post*. Hence the main finding in this section is unlikely due to self-selection issues, but directly supports H2.[27]

Another potential selection bias related to the passage of NSMIA is similar to the one we discussed about OHR in Section 5.3. NSMIA provides private firms with better access to capital and expands the pool of potential investors, which may affect these firms' incentives to go public. As in Section 5.3, we argue this selection effect should bias against finding a result. NSMIA improves pre-IPO liquidity, thereby reducing the liquidity value of going public. This effect in turn encourages some firms with low liquidity value gain to stay private who would otherwise go public. The exit of these marginal firms from the IPO market improves the quality of the remaining treated firms, creating a positive bias for the liquidity value in the treated IPO sample against finding a negative β_1. Hence, we believe this possible selection effect in fact makes our results stronger.

Lastly, instead of dividing the deals into the control sample and the treatment sample explicitly, we test whether issuers incorporated in states with less number or lower percentage of VC and PE firms experience less underpricing after 1997. We use *Rank* to denote the rank of the states, and *Percentage* to denote the percentage of VC and PE firms of the states in US, as presented in Table 8. For example, for state NJ, *Rank=8* and *Percentage=2.66*. The regression equation is as follows,

[27] When firms select their locations for factors unrelated to capital, for example, the founder's birthplace or the proximity to human capital, there are no issues with the estimation, as these factors are uncorrelated with the different impact of NSMIA on firms in different locations.

$$Underpricing_{ijt} = \alpha_j + \beta_1 Rank\ (or\ Percentage) \times Post + \beta_2 Rank (or\ Percentage) + \beta_3 Post + \beta_4 Expected\ Liquidity_{ijt} + \gamma' X_{ijt} + \varepsilon_{ijt}, \quad (11)$$

where i, j, t index firms, industries, and years, respectively. Standard errors are clustered at the level of industry-year groups. The coefficient *Rank*×*Post* is expected to be negative, and the coefficient on *Percentage*×*Post* is expected to be positive. Table 11 summarizes the results. In the three panels, we control for expected liquidity with *Peer Spread*, *Peer Turnover*, or *Peer AIM*. The finding is consistent with our prediction. The coefficient on *Rank*×*Post* is negative and significant in five out of six specifications, and the coefficient on *Percentage*×*Post* is positive and significant in all six specifications. The effect is also economically large. For example, when *Rank* is increased by one, underpricing post-1997 is lowered by 0.7% to 0.9% in the period of 1993-2000. For every 1% decrease in *Percentage*, underpricing post-1997 is lowered by 0.5% to 0.6%.

For robustness, we repeat our analysis in Table 9 and Table 11 by dropping the state of California, which has the highest number of VC and PE firms in the sample period, to rule out the possibility that the results are driven by one particular state. We also repeat the analysis excluding all tech firms to address the concern that results are driven by the tech bubble. The results remain with both exercises.

5.5 Discussion: Changes to OHR on Nasdaq and the passage of NSMIA

In sections 5.3 and 5.4, we use changes to OHR on Nasdaq in 1997, and the passage of NSMIA in October 1996, as two separate exogenous shocks to issuer's expected liquidity with the difference-in-difference approach. Since these two regulations are close to each other in time, regression Equation (9) and Equation (10) can share the same dummy variable of *Post*. If the dummy variables indicating the two treatment deals *Nasdaq* and *Treated (NSMIA)* are also highly correlated, then empirically these two experiments could be just the same one in nature. For example, if all Nasdaq deals post 1997 are also issuers headquartered in CA and NY, Section 5.4 is just presenting the same set of results with a different name of variable.

To rule out this possibility, and since the variable of interests are $Nasdaq \times Post$ and *Treated* $(NSMIA)$ ×*Post,* we investigate the distribution of the two dummy variables *Nasdaq* and *Treated* among observations used in the two regressions of Equations (9) and (10), after 1997 (*Post*

= *1*), in the years of 1998, 1999, and 2000. We construct a 2 × 2 matrix, showing the number of observations in four groups: *Nasdaq=1* and *Treated (NSMIA)=1, Nasdaq=1* and *Treated (NSMIA)=0, Nasdaq=0* and *Treated (NSMIA)=1,* and *Nasdaq=0* and *Treated (NSMIA)=0*. Since the *Treated(NSMIA)* dummy changes when we use three alternative control samples: deals in top eight states (CA, NY, MA, TX, IL, CT, PA, and NJ), deals in top four states (CA, NY, MA, and TX), and deals in top two states (CA and NY), we present three panels of this matrix, in Table 12.

These matrices show that there is no strong correlation between *Nasdaq* and *Treated* ($NSMIA$). There are more observations with *Treated (NSMIA)=1* (177 obs) than *Treated (NSMIA)=0* (412 obs) when *Nasdaq=1* in Panel A, but the pattern is flipped in Panel C (338 vs. 251 obs). Panel B shows a very balanced distribution of the two variables (247 vs. 342 obs). Hence we conclude that these two regulation changes represent two independent events that we can reply on for separate difference-in-difference analysis.

6. Conclusion

Traditionally, IPO underpricing has been explained by theories based on asymmetric information. In these theories, underpricing is the compensation to IPO investors for their information disadvantage or a tool of signaling by high-quality firms. In this paper, we argue that, IPOs enable issuers to access the secondary market via public-listing and enhance firm value, which we call liquidity value. We model underpricing as the negotiation result between the underwriter on behalf of IPO investors and the issuer, splitting the liquidity value. The model predicts a positive relation between the size of the liquidity value and underpricing.

We thus conjecture that underpricing is positively related to the expected post-IPO liquidity of the issuer, and negatively related to the issuer's pre-IPO liquidity. We first test a baseline specification investigating the relation between underpricing and expected liquidity. Consistent with the theory, we find a positive and significant coefficient when regressing underpricing on expected liquidity. We conduct cross-sectional analysis for the baseline regression, and find that the relation is stronger for issuers with VC investors involved, and when the underwriter has more bargaining power and the fraction of new issuance is smaller.

We then exploit two regulation changes as exogenous shocks to the expected post-IPO liquidity and the pre-IPO liquidity, respectively. The first one is the changes to OHR at Nasdaq in 1997, which improves the liquidity of Nasdaq-listed stocks but not for stocks on other exchanges. With a difference-in-difference approach, we use Nasdaq IPOs as the treatment sample and non-Nasdaq IPOs as the control sample. The result shows that Nasdaq IPOs exhibit more underpricing after the regulation change. The second one is the enactment of NSMIA, which removes compliance burdens for private firms raising capital from different states. We argue that this is an exogenous positive shock to pre-IPO liquidity. We further conjecture that the law has more impact on issuers located in states where private equity capital is more scarce, proxied by fewer in-state VC and PE firms. Using the issuers in states with less private equity investors as the treatment sample and the ones in states with the more private equity investors as the control sample, we find that the treatment sample shows less underpricing than the control sample post NSMIA.

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Appendix

Variables	Definition	Source
Offer Price	IPO Offer Price	SDC
Underpricing	Percentage change from the offer price to the first-day closing price	SDC & CRSP
Peer Spread	We select the issuer's peer firms as the publicly traded ones with the same industry (SIC 2-digit code), similar size, and listed on the same exchange. Daily spreads of each peer firm is calculated in the 12 months preceding the IPO time. We then take an average of the daily spreads, and average across peer firms to construct peer spread.	CRSP
Peer Turnover	We select the issuer's peer firms as the publicly traded ones with the same industry (SIC 2-digit code), similar size, and listed on the same exchange. Daily turnover of each peer firm is calculated in the 12 months preceding the IPO time. We then take an average of the daily turnover, and average across peer firms to construct peer turnover.	CRSP
Peer AIM	We select the issuer's peer firms as the publicly traded ones with the same industry (SIC 2-digit code), similar size, and listed on the same exchange. Following Amihud (2002), we use daily CRSP data (CRSP variables *ret, prc,* and *vol*) to calculate the ratio of absolute stock return to dollar volume [10,000,000 × | *ret* | ÷ (*prc* × *vol*)] for each day in the 12-month period before the IPO for each peer firm. We then average over the period and average across peer firms, and the final measure is the natural log of one plus the average peer AIM.	CRSP
Top Underwriter	A dummy variable that is equal to one if the lead underwriter has an updated Carter and Manaster's (1990) rank of eight or more, and zero otherwise.	Prof. Jay Ritter's website
Integer Price	A dummy variable that is equal to one if the offer price is an integer and zero otherwise.	SDC
New Shares Ratio	It is the fraction of the ownership the issuer sells during the IPO. It is calculated as IPO proceeds / (IPO price × number of shares outstanding).	SDC & CRSP
Sentiment	It's the monthly market sentiment index based on the closed-end fund discount, the NYSE share turnover, the number of IPOs, the share of equity issuance in total equity and debt issuance, and the dividend premium, constructed in Equation (2) of Baker and Wurgler (2006).	Prof. Jeffrey Wurgler's website
Assets	Firm's pre-issue book value of assets, in millions of dollars.	SDC
Age	Calendar year of offering minus the calendar year of founding.	Prof. Jay Ritter's website

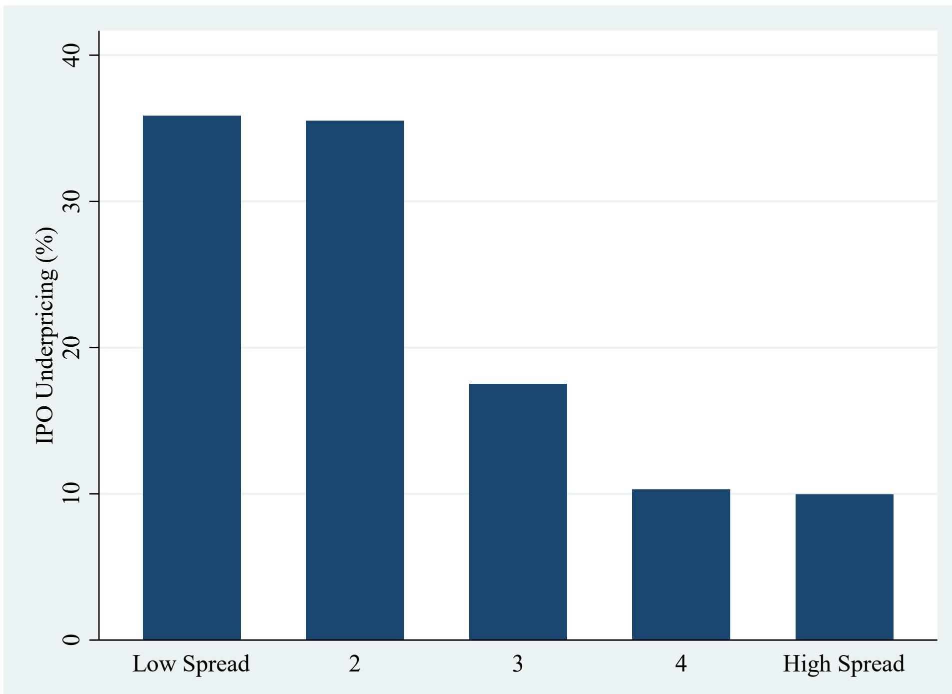
VC-backed	Equals one (zero otherwise) if the IPO was backed by venture capital.	SDC
Peer Age	Number of years that the peer firm has been public.	CRSP
Sales	Peer firm's annual sales, in millions.	COMPUSTAT
Market Cap	We first obtain daily market capitalization, which equals daily price times number of shares outstanding. We then take the average of daily values within a given month to reach monthly value.	CRSP
Number of Shareholders	Number of shareholders, in millions	COMPUSTAT
Market Return	NYSE/AMEX/NASDAQ/ARCA monthly market return.	CRSP
Lagged Market Return	Market return of the same month in the previous year.	CRSP
Variance of Market Return	Square of the standard deviation of daily market return within a given month.	CRSP
Interest Rate	Monthly three-month treasury bill rate.	The Federal Reserve Bank's website

Figure 1

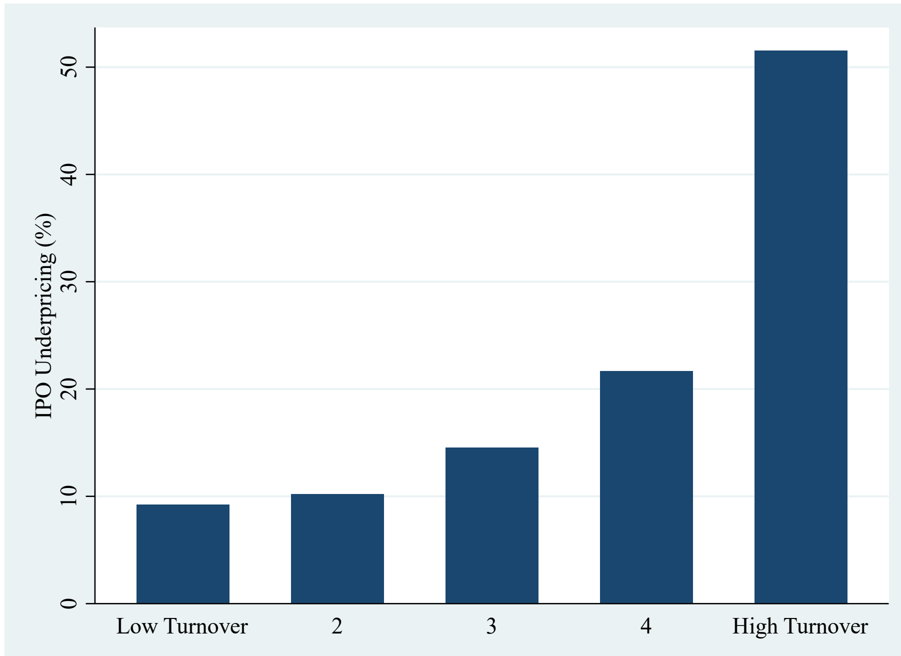
Average underpricing across quintile subsamples sorted by expected liquidity

The figure shows the average underpricing of deals in five quintiles in 1981-2015, sorted by *Peer Spread, Peer Turnover,* or *Peer AIM*. Peer firms are publicly traded firms that are in the same industry (SIC two-digit code), listed on the same exchange, and within the same size quintile by market capitalization in the COMPUSTAT-CRSP universe as the issuer. *Peer Spread (Turnover)* is the peer public firms' average daily spread (turnover) in the 12-month period prior to the issuer's IPO time. *Peer AIM* is the natural log of one plus peer public firms' average daily AIM in the 12-month period prior to the issuer's IPO time.

Panel A: Underpricing and Peer Spread



Panel B: Underpricing and Peer Turnover



Panel C: Underpricing and Peer AIM

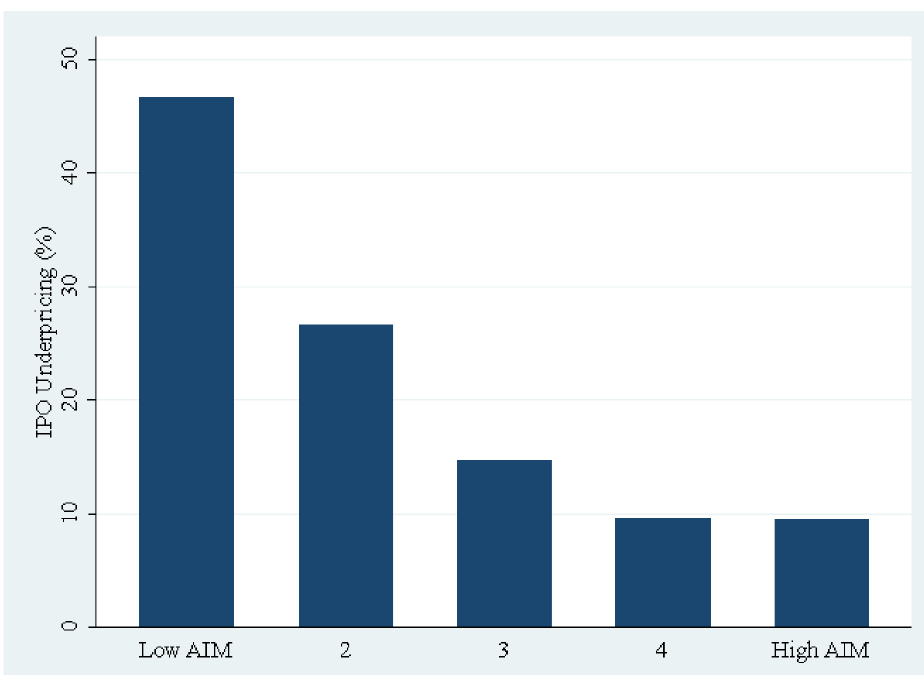


Table 1
Distribution of IPOs by year, industry, and exchange

The table shows sample distribution of IPOs in US across year, industry, and exchange in 1981-2015. Panel A shows the distribution by year and exchange, and Panel B shows the distribution by year and industry. The exchanges include the New York Stock Exchange (NYSE), American Stock Exchange (ASE), and Nasdaq. Industries are defined by the Fama-French 10-industry classification. Nine industries are presented because we exclude utilities firms.

Panel A: Distribution by year and exchange

Year	NYSE	ASE	Nasdaq	Total	%
1981	0	0	3	3	0.1%
1983	0	1	4	5	0.1%
1984	0	0	2	2	0.1%
1985	0	1	12	13	0.3%
1986	8	16	166	190	5.0%
1987	6	12	129	147	3.9%
1988	5	5	41	51	1.4%
1989	4	3	44	51	1.4%
1990	3	3	39	45	1.2%
1991	20	3	112	135	3.6%
1992	20	1	185	206	5.5%
1993	19	1	233	253	6.7%
1994	15	3	202	220	5.8%
1995	13	5	212	230	6.1%
1996	26	11	352	389	10.3%
1997	31	5	229	265	7.0%
1998	29	4	147	180	4.8%
1999	15	3	331	349	9.2%
2000	10	2	230	242	6.4%
2001	9	2	27	38	1.0%
2002	13	1	27	41	1.1%
2003	7	1	28	36	1.0%
2004	16	3	79	98	2.6%
2005	22	5	49	76	2.0%
2006	10	3	56	69	1.8%
2007	15	0	58	73	1.9%
2008	3	0	4	7	0.2%
2009	6	0	10	16	0.4%
2010	14	2	29	45	1.2%
2011	16	1	27	44	1.2%
2012	23	0	24	47	1.2%
2013	27	0	45	72	1.9%
2014	19	1	66	86	2.3%
2015	13	0	38	51	1.4%
Total	437	98	3,240	3,775	
%	11.6%	2.6%	85.8%		

Panel B: Distribution by year and industry

Year	Consumer Non-durables	Consumer Durables	Manu-facturing	Energy	Business Equipment	Telecom-municiatons	Wholesale and Retail	Healthcare	Others	Total	%
1981	0	0	0	0	2	0	1	0	0	3	0.1%
1983	0	1	1	0	0	0	1	0	2	5	0.1%
1984	0	0	0	0	0	0	2	0	0	2	0.1%
1985	3	0	1	0	4	0	4	1	0	13	0.3%
1986	17	9	32	0	38	8	43	16	27	190	5.0%
1987	13	1	26	2	32	5	26	16	26	147	3.9%
1988	4	2	13	0	12	2	9	6	3	51	1.4%
1989	2	0	6	2	15	1	6	8	11	51	1.4%
1990	2	2	2	4	10	0	7	13	5	45	1.2%
1991	9	5	10	2	38	3	24	38	6	135	3.6%
1992	16	8	10	2	43	9	47	54	17	206	5.5%
1993	18	12	41	9	56	14	42	27	34	253	6.7%
1994	9	9	29	4	61	14	35	26	33	220	5.8%
1995	9	3	20	2	98	11	22	36	29	230	6.1%
1996	16	6	21	9	145	14	55	64	59	389	10.3%
1997	17	4	25	4	100	5	33	40	37	265	7.0%
1998	13	2	12	2	66	12	28	16	29	180	4.8%
1999	8	3	5	1	212	35	23	11	51	349	9.2%
2000	0	1	6	2	137	20	10	42	24	242	6.4%
2001	1	0	4	1	13	0	3	11	5	38	1.0%
2002	2	0	2	1	13	1	7	7	8	41	1.1%
2003	0	1	2	1	12	2	4	7	7	36	1.0%
2004	2	3	5	4	28	3	10	31	12	98	2.6%
2005	5	2	7	5	21	4	8	18	6	76	2.0%
2006	3	2	8	2	17	4	4	21	8	69	1.8%
2007	1	1	7	6	22	2	4	21	9	73	1.9%
2008	0	0	3	0	2	0	0	1	1	7	0.2%
2009	1	0	0	0	3	0	4	4	4	16	0.4%
2010	0	2	4	0	11	2	5	11	10	45	1.2%
2011	0	0	2	3	17	0	7	11	4	44	1.2%
2012	1	1	5	4	15	1	10	8	2	47	1.2%
2013	1	1	5	2	21	0	6	27	9	72	1.9%
2014	1	1	1	2	22	0	6	47	6	86	2.3%
2015	2	0	1	0	8	0	5	33	2	51	1.4%
Total	176	82	316	76	1,294	172	501	672	486	3,775	
%	4.7%	2.2%	8.4%	2.0%	34.3%	4.6%	13.3%	17.8%	12.9%		

Table 2
Summary statistics

The table provides summary statistics of variables. Panel A shows the distribution statistics, and Panel B presents the correlation matrix. In Panel B, Pearson correlations are below diagonal and Spearman correlations are above diagonal. Variables construction is described in Appendix. We winsorize all variables at the 1st and 99th percentile levels.

Panel A: Distribution statistics

	N	Mean	Median	Std. Dev.	P25	P75
IPO Underpricing	3,775	21.5%	8.7%	38.8%	0.0%	25.4%
Peer Spread	3,658	3.50%	2.90%	3.00%	1.30%	4.90%
Peer Turnover	3,773	0.80%	0.70%	0.50%	0.40%	1.00%
Peer AIM	3,773	1.83	1.47	1.60	0.36	2.89
Top Underwriter	3,775	60.7%	1	48.8%	0	1
Integer Price	3,775	82.1%	1	38.3%	1	1
New Shares Ratio	3,775	32.2%	28.9%	17.0%	21.6%	38.0%
Sentiment	3,766	0.32	0.31	0.46	-0.06	0.63
Assets	3,775	183.65	29.70	577.74	11.00	90.10
Age	3,775	14.83	8.00	19.75	4.00	16.00
VC-backed	3,775	44.7%	0	49.7%	0	1

Panel B: Correlation matrix

	IPO Underpricing	Peer Spread	Peer Turnover	Peer AIM	Top Underwriter	Integer Price	New Shares Ratio	Sentiment	Log (Assets)	Log (1+Age)	VC-backed
IPO Underpricing	1.00	-0.22	0.31	-0.32	0.12	0.13	-0.21	-0.09	-0.01	-0.14	0.15
Peer Spread	-0.23	1.00	-0.60	0.89	-0.46	-0.18	0.41	0.40	-0.54	-0.08	-0.24
Peer Turnover	0.47	-0.53	1.00	-0.59	0.29	0.20	-0.39	-0.20	0.15	-0.12	0.34
Peer AIM	-0.30	0.90	-0.55	1.00	-0.53	-0.18	0.48	0.25	-0.58	-0.06	-0.20
Top Underwriter	0.16	-0.47	0.27	-0.54	1.00	0.12	-0.28	-0.14	0.43	0.05	0.25
Integer Price	0.15	-0.15	0.20	-0.17	0.12	1.00	-0.10	-0.08	0.05	-0.04	0.12
New Shares Ratio	-0.21	0.29	-0.32	0.33	-0.16	-0.08	1.00	0.06	-0.14	0.16	-0.25
Sentiment	-0.09	0.28	-0.14	0.18	-0.12	-0.07	0.01	1.00	-0.16	-0.03	-0.11
Log(Assets)	-0.01	-0.49	0.16	-0.55	0.44	0.05	0.01	-0.14	1.00	0.34	-0.10
Log(1+Age)	-0.17	-0.08	-0.12	-0.07	0.08	-0.03	0.16	-0.04	0.40	1.00	-0.23
VC-backed	0.19	-0.25	0.32	-0.24	0.25	0.12	-0.24	-0.09	-0.12	-0.23	1.00

Table 3

Baseline: IPO underpricing and expected liquidity

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of underpricing on the issuer's expected liquidity and control variables, specified by regression Equation (6). Expected liquidity is measured by *Peer Turnover* in Columns (1) and (2), by *Peer Spread* in Columns (3) and (4), and by *Peer AIM* in Columns (5) and (6). *Spread (Turnover)* is the peer public firms' average daily spread (turnover) in the 12-month period prior to the issuer's IPO time. *Peer AIM* is the natural log of one plus peer public firms' average daily AIM in the 12-month period prior to the issuer's IPO time. Variable construction is described in Appendix. The industry-year fixed effects control for time-varying industry effects. Industries are defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

	Expected Liquidity Measures					
	Peer Spread		Peer Turnover		Peer AIM	
	(1)	(2)	(3)	(4)	(5)	(6)
Expected Liquidity	**-2.422*** **	**-2.152*** **	**35.204*** **	**26.584*** **	**-0.083*** **	**-0.060*** **
	(-3.96)	**(-5.52)**	**(4.23)**	**(3.02)**	**(-4.91)**	**(-8.14)**
Top Underwriter	0.062*	0.036**	0.057***	0.044***	0.020	0.015
	(1.88)	(2.59)	(2.64)	(3.26)	(0.83)	(1.19)
New Shares Ratio	-0.248***	-0.141***	-0.117***	-0.105***	-0.142**	-0.086***
	(-2.85)	(-3.91)	(-3.40)	(-2.97)	(-2.17)	(-2.66)
Integer Price	0.101***	0.055***	0.057***	0.047***	0.087***	0.050***
	(4.34)	(5.28)	(6.61)	(5.55)	(4.50)	(5.05)
Sentiment	-0.034	-0.164*	-0.028	-0.156**	-0.043	-0.169**
	(-0.60)	(-1.96)	(-1.03)	(-2.03)	(-0.87)	(-2.10)
Log(Assets)	-0.020*	-0.017**	-0.020**	-0.012**	-0.040***	-0.030***
	(-1.89)	(-2.44)	(-2.49)	(-2.27)	(-3.36)	(-4.44)
Log(1+Age)	-0.050***	-0.035***	-0.032***	-0.029***	-0.044***	-0.032***
	(-4.07)	(-4.75)	(-5.75)	(-5.08)	(-4.06)	(-4.43)
VC-backed	0.041	0.041*	-0.005	0.032*	0.033	0.039*
	(1.43)	(1.80)	(-0.29)	(1.80)	(1.18)	(1.79)
Constant	0.439***	0.213***	0.058	0.142	0.568***	0.463***
	(4.82)	(2.62)	(1.42)	(1.45)	(5.39)	(9.08)
Industry-Year FE	NO	YES	NO	YES	NO	YES
Observations	3,649	3,649	3,764	3,764	3,764	3,764
Adjusted R-squared	0.124	0.268	0.241	0.298	0.160	0.283

Table 4
Cross-sectional analysis of the baseline: VC-backed, top-underwriter, and new shares ratio

The table reports coefficients and t-statistics in the parenthesis of OLS regressions, which are cross-sectional analysis of the baseline, by adding interaction terms of the dummy variable *VC-backed*, *Top Underwriter*, or *New Shares Ratio* with expected liquidity to Equation (6). Expected liquidity is *Peer Spread*, *Peer Turnover*, or *Peer AIM*. Variable construction is described in Appendix. Industries are defined by the Fama-French 10-industry classification. The industry-year fixed effects control for time-varying industry effects. For the sake of brevity, estimation results for control variables including *Integer Price, Sentiment, Log(Assets), Log(1+Age)*, and the intercept are not presented. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

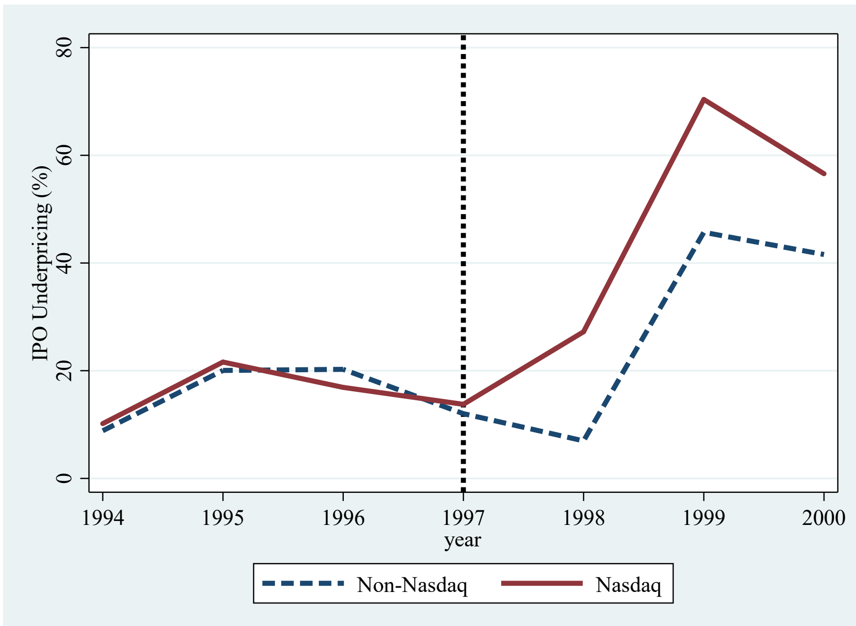
	Expected Liquidity Measures								
	Peer Spread			Peer Turnover			Peer AIM		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Expected Liquidity	-1.324***	-1.354***	-5.272***	13.830**	10.261	45.650***	-0.034***	-0.029***	-0.124***
	(-4.03)	(-4.39)	(-4.28)	(2.53)	(1.52)	(4.08)	(-5.39)	(-5.71)	(-5.35)
VC-backed	0.200***	0.045*	0.039*	-0.170***	0.034*	0.028	0.197***	0.051**	0.038*
	(3.77)	(1.96)	(1.78)	(-2.70)	(1.77)	(1.57)	(4.11)	(2.22)	(1.81)
Expected liquidity	**-4.689***			**25.820***			**-0.087***		
× VC-backed	**(-3.85)**			**(3.51)**			**(-4.32)**		
Top Underwriter	0.028**	0.215***	0.028**	0.045***	-0.109***	0.042***	0.012	0.174***	0.007
	(2.08)	(4.79)	(2.10)	(3.25)	(-3.39)	(2.91)	(0.97)	(4.59)	(0.62)
Expected liquidity		**-5.458***			**21.790***			**-0.086***	
× Top Underwriter		**(-4.23)**			**(5.13)**			**(-4.47)**	
New Shares Ratio	-0.116***	-0.073**	-0.405***	-0.096**	-0.095***	0.353***	-0.064**	-0.040	-0.400***
	(-3.84)	(-2.44)	(-3.81)	(-2.50)	(-2.61)	(2.64)	(-2.40)	(-1.44)	(-3.73)
Expected liquidity			**7.466***			**-69.112***			**0.168***
× New Shares Ratio			**(3.24)**			**(-4.11)**			**(3.58)**
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,649	3,649	3,649	3,764	3,764	3,764	3,764	3,764	3,764
Adjusted R-squared	0.287	0.292	0.277	0.316	0.310	0.315	0.306	0.302	0.296

Figure 2

IPO underpricing at Nasdaq and non-Nasdaq exchanges

The figure shows the average IPO underpricing for deals listed on Nasdaq and non-Nasdaq exchanges in each year from 1994 to 2000. 1997 is the year when the SEC enacted major changes to Order Handling Rules of Nasdaq. Panel A includes all issuers during the period, while Panel B excludes tech stocks, as defined in Loughran and Ritter (2004).

Panel A: The whole sample



Panel B: Excluding tech stocks

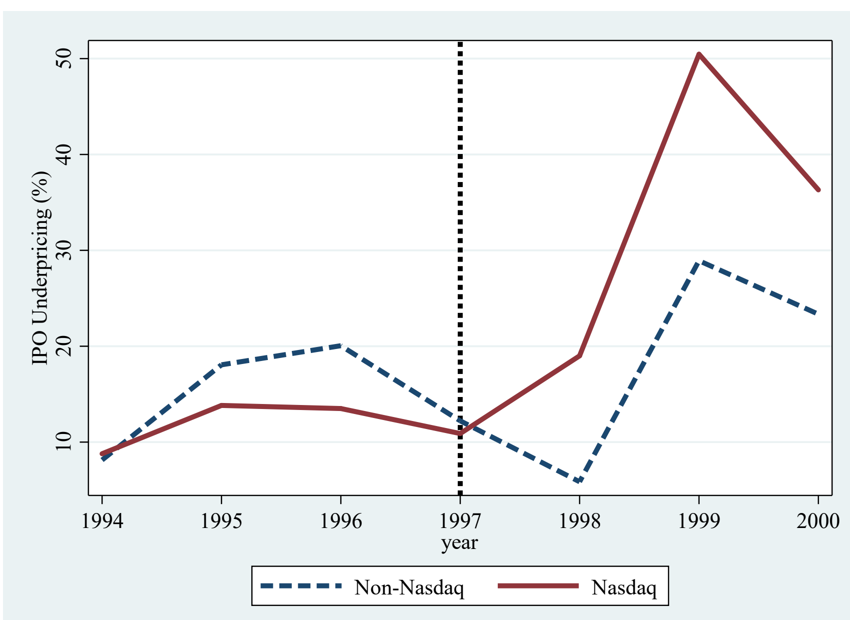


Table 5

IPO underpricing and changes to Order Handling Rules at Nasdaq in 1997

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of underpricing on the enactment of OHR at Nasdaq in 1997, and control variables, as shown in regression Equation (8).The sample period is 1994-2000 excluding the year of 1997, when changes of Order Handling Rules at Nasdaq were enacted. We run the regression in two periods: three years before and after the law passage (1994-2000), and two years before and after the law passage (1995-1999). In Columns (1) and (2), the regression is based on the full sample with all Nasdaq and non-Nasdaq IPOs. In Columns (3) and (4), the regression is based on a matched sample, where each Nasdaq IPO is matched with a non-Nasdaq IPO in the same industry (SIC two-digit code), with a similar size (market capitalization), and in the same IPO year. Each year we select all IPO deals and divide them into five quintiles according to their market capitalization in the first year, and conduct size matching by choosing firms in the same quintile. If there are multiple matches, we select the one with the smallest size difference. Only Nasdaq deals with a matched control deal are included in the sample. One non-Nasdaq deal can be shared by multiple Nasdaq deals as a matched control, so one non-Nasdaq deal can appear several times in the data and is counted each time as a separate observation. *Nasdaq* is a dummy variable that is equal to one if the issuer is listed on Nasdaq and zero otherwise. *Post* is a dummy variable that is equal to one if the issuer goes public after 1997 and zero otherwise. Variable construction is described in Appendix. For the sake of brevity, estimation results for control variables including *Top Underwriter, New Shares Ratio, Integer Price, Sentiment, Log(Assets), Log(1+Age)*, and the intercept are not presented. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

	Full Sample		Matched Sample	
	1994-2000	1995-1999	1994-2000	1995-1999
	(1)	(2)	(3)	(4)
Nasdaq × Post	**0.206*** **	**0.238*** **	**0.177*** **	**0.254*** **
	(4.93)	**(6.04)**	**(2.85)**	**(5.50)**
Nasdaq	-0.056**	-0.066*	-0.032	-0.037
	(-2.17)	(-2.01)	(-1.62)	(-1.65)
Post	0.049	-0.053	0.167**	0.101
	(0.86)	(-0.69)	(2.48)	(1.25)
Industry FE	YES	YES	YES	YES
Observations	1,610	1,148	1,366	1,026
# of Nasdaq deals	1,273	897	683	513
# of non-Nasdaq deals	337	251	683	513
Adjusted R-squared	0.236	0.242	0.214	0.237

Table 6

Placebo tests in the pre-OHR period

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of underpricing on a pseudo event that is assumed to only affect the liquidity of Nasdaq stocks in a particular year between 1981 and 1996, before OHR is enacted in 1997. The regression variables are shown in Equation (8). For brevity, only coefficients of key explanatory variables are shown. We take the pre-treatment period of the sample from 1981 to 1996 and select moving windows of seven years with a pseudo event occurring in the 4th year, The pseudo event years are the years between 1984 and 1993. *Nasdaq* is a dummy variable that is equal to one if the issuer is listed on Nasdaq and zero otherwise. *Post* is a dummy variable that is equal to one if the issuer goes public after the pseudo event year and zero otherwise. Variable construction is described in Appendix. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

Event Year	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993
Event Window	1981-1987	1982-1988	1983-1989	1984-1990	1985-1991	1986-1992	1987-1993	1988-1994	1989-1995	1990-1996
Nasdaq × Post	**-0.026**	**0.030**	**-0.110**	**0.024**	**-0.002**	**0.011**	**0.012**	**-0.021**	**-0.015**	**-0.036**
	(-0.42)	**(0.46)**	**(-1.02)**	**(0.46)**	**(-0.05)**	**(0.40)**	**(0.50)**	**(-0.47)**	**(-0.40)**	**(-1.08)**
Nasdaq	0.036	-0.031	0.098	0.007	0.009	-0.001	-0.015	0.027	0.015	0.011
	(0.60)	(-0.50)	(0.90)	(0.29)	(0.64)	(-0.04)	(-0.87)	(0.61)	(0.52)	(0.47)
Post	0.060	0.047	0.020	-0.061	0.028	0.023	0.049**	0.053	0.098**	0.083***
	(1.43)	(0.87)	(0.51)	(-0.90)	(0.46)	(1.07)	(2.43)	(1.49)	(2.52)	(2.76)
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	358	395	269	352	581	774	843	826	934	1,225
Adjusted R-squared	0.027	0.026	0.064	0.051	0.060	0.068	0.081	0.070	0.114	0.107

Table 7

Individual peer liquidity and changes to Order Handling Rules at Nasdaq in 1997

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of individual peer liquidity on the enactment of OHR at Nasdaq in 1997, and control variables, as shown in regression Equation (9). The sample period is 1994-2000 excluding the year of 1997, when changes of Order Handling Rules at Nasdaq were enacted. We run the regression in two periods: three years before and after the law passage (1994-2000), and two years before and after the law passage (1995-1999). *Nasdaq* is a dummy variable that is equal to one if the issuer is listed on Nasdaq and zero otherwise. *Post* is a dummy variable that is equal to one if the issuer goes public after 1997 and zero otherwise. Individual peer spread (turnover, or AIM) are the monthly spread (turnover, or AIM) calculated as averages of daily data for each peer firm of the issuer. Peer firms are publicly traded companies in the same industry (SIC two-digit code), with a similar size (belonging to the same quintile in the year of IPO as the issuer when the COMPUSTAT-CRSP universe is sorted by market capitalization), and listed on the same exchange as the issuer. *Log (1+ Peer Age), Log (Sales), Log (Market cap)*, and *Number of shareholders* are annual characteristics of the peer firm. *Market return* is the value-weighted monthly market return (NYSE/AMEX/NASDAQ/ARCA) reported in CRSP. *Variance of market returns* is the variance of daily market returns in a given month. *Sentiment* is the monthly market sentiment index downloaded from Prof. Jeff Wurgler's website. *Interest rate* is the monthly three-month T-bill rate downloaded from the Federal Reserve Bank's website. Additional variable construction is described in Appendix. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and firm-level clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

	Dependent Variable					
	Individual Peer Spread		Individual Peer Turnover		Individual Peer AIM	
	1994-2000	1995-1999	1994-2000	1995-1999	1994-2000	1995-1999
	(1)	(2)	(3)	(4)	(5)	(6)
Nasdaq × Post	**-0.033*** **	**-0.029*** **	**0.001*** **	**0.001*** **	**-0.237*** **	**-0.190*** **
	(-32.75)	**(-27.46)**	**(5.92)**	**(3.71)**	**(-8.78)**	**(-7.24)**
Nasdaq	0.003***	-0.001	0.003***	0.004***	0.182***	0.130***
	(2.60)	(-1.10)	(20.22)	(19.24)	(5.70)	(3.93)
Post	0.014***	0.010***	0.000	-0.000***	0.114***	0.077***
	(15.70)	(11.33)	(0.43)	(-3.28)	(5.96)	(4.10)
Log(1+ Peer Age)	0.001***	0.001***	-0.001***	-0.001***	0.124***	0.129***
	(3.51)	(4.20)	(-15.29)	(-16.52)	(10.88)	(10.56)
Log(Sales)	0.000	0.001**	-0.001***	-0.001***	0.032***	0.038***
	(1.59)	(2.07)	(-12.24)	(-10.27)	(3.81)	(4.06)
Log(Market cap)	-0.018***	-0.018***	0.002***	0.002***	-0.769***	-0.777***
	(-55.15)	(-49.91)	(33.56)	(28.70)	(-72.74)	(-67.07)
Number of shareholders	0.429***	0.430***	-0.035***	-0.034***	19.335***	20.102***
	(24.98)	(23.16)	(-7.88)	(-7.13)	(25.57)	(25.04)
Market return	0.014***	0.018***	0.006***	0.004***	-0.033	0.325***
	(15.52)	(15.48)	(23.35)	(12.27)	(-1.16)	(9.00)
Lagged market return	-0.004***	-0.002***	0.003***	0.004***	-0.576***	-0.307***
	(-4.90)	(-2.68)	(9.88)	(12.29)	(-18.01)	(-9.83)
Variance of market returns	19.732***	24.579***	2.381***	-2.582***	315.015***	814.810***
	(34.15)	(24.86)	(13.53)	(-10.82)	(18.00)	(26.54)
Sentiment	0.002***	0.001	-0.001***	-0.002***	0.101***	-0.009
	(7.80)	(1.51)	(-8.97)	(-15.00)	(13.04)	(-0.72)
Interest rate	-0.007	-0.103***	0.038***	-0.047***	-2.421***	5.706***
	(-0.41)	(-2.69)	(10.38)	(-5.91)	(-4.61)	(5.26)
Constant	0.125***	0.134***	-0.004***	0.001**	4.857***	4.447***
	(53.42)	(40.28)	(-10.00)	(1.97)	(61.10)	(44.52)
Industry FE	YES	YES	YES	YES	YES	YES
Observations	232,513	162,457	234,051	163,429	233,911	163,319
Adjusted R-squared	0.508	0.503	0.217	0.203	0.621	0.619

Table 8

Distribution of VC and PE firms across states, 1993-2000

The table reports the number and percentage of VC and PE firms, in each of the 50 states and the District of Columbia in 1993-2000, ranked from the largest to the smallest. States (AK, ND) with zero VC and PE firms are not shown. This period covers three years before and after the passage of the National Securities Market Improvement Act (NSMIA), which is enacted in October 1996. Data source: Thomson Reuters Eikon.

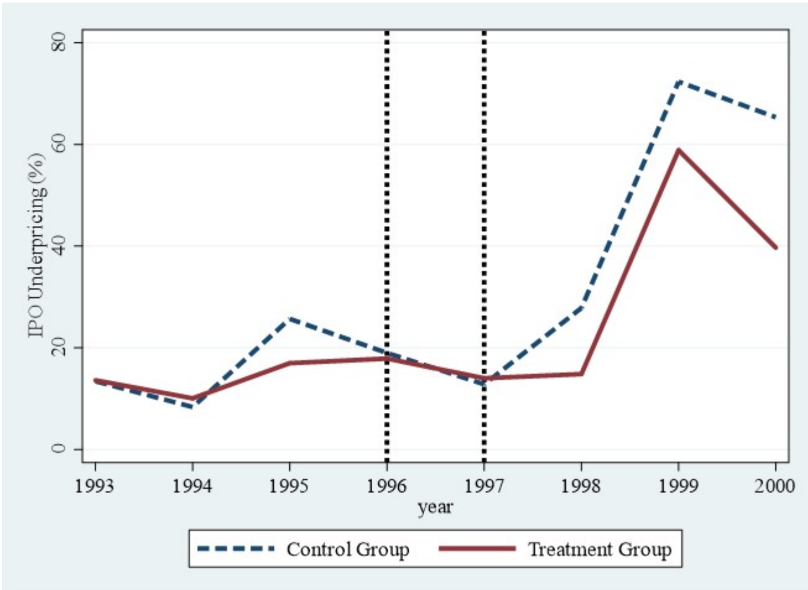
Rank	State	Freq.	Percent	Rank	State	Freq.	Percent
1	CA	727	25.14	26	AZ	14	0.48
2	NY	519	17.95	27	AL	13	0.45
3	MA	246	8.51	28	RI	11	0.38
4	TX	182	6.29	29	LA	10	0.35
5	IL	136	4.70	30	NH	9	0.31
6	CT	128	4.43	31	DE	7	0.24
7	PA	100	3.46	31	NV	7	0.24
8	NJ	77	2.66	33	KS	6	0.21
9	WA	60	2.07	33	SC	6	0.21
10	MN	59	2.04	33	NM	6	0.21
11	CO	54	1.87	33	ME	6	0.21
12	GA	53	1.83	33	IA	6	0.21
12	MD	53	1.83	38	OK	5	0.17
14	FL	52	1.80	38	KY	5	0.17
15	OH	49	1.69	40	AR	3	0.10
16	NC	42	1.45	40	WY	3	0.10
17	VA	40	1.38	40	MT	3	0.10
18	DC	39	1.35	40	NE	3	0.10
19	MI	29	1.00	40	VT	3	0.10
20	TN	25	0.86	45	MS	2	0.07
21	MO	23	0.80	45	WV	2	0.07
22	IN	17	0.59	47	SD	1	0.03
22	WI	17	0.59	47	HI	1	0.03
24	OR	16	0.55	47	ID	1	0.03
24	UT	16	0.55				
					Total	2,892	100

Figure 3

IPO underpricing in states with and without large numbers of VC and PE firms

The figure shows the average IPO underpricing for issuers headquartered in the top four states (CA, NY, MA, and TX) with largest number of VC and PE firms (the control sample) and issuers headquartered outside these states (the treatment sample). The National Security Market Improvement Act (NSMIA) was enacted in October 1996, and the two black dashed lines mark the event time. We plot average underpricing in each year in 1993-2000 of the two groups. Panel A includes all issuers, while Panel B excludes tech stocks, as defined in Loughran and Ritter (2004).

Panel A: The whole sample



Panel B: Excluding tech stocks

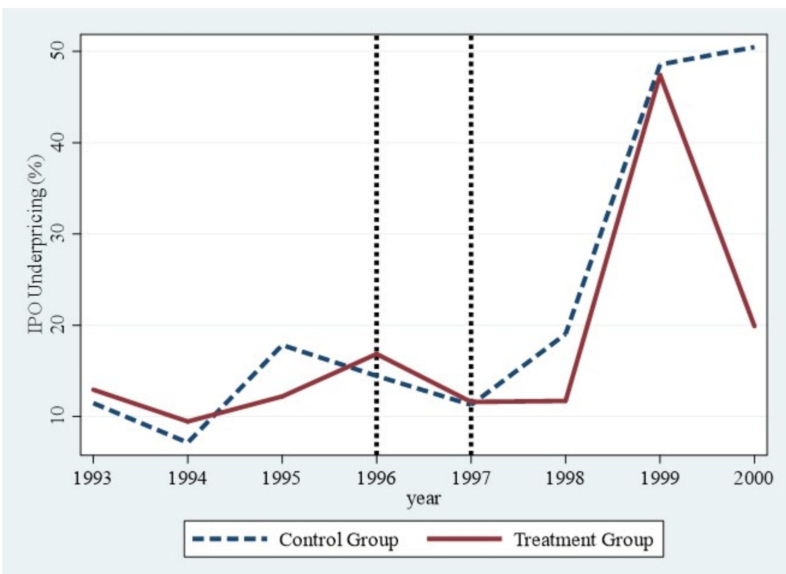


Table 9

IPO underpricing and the National Security Market Improvement Act in October 1996: Difference-in-difference

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of IPO underpricing on the passage of the National Security Market Improvement Act (NSMIA) in October 1996, as shown in regression Equation (10). The sample period is 1993-2000 excluding the years of 1996 and 1997. We run the regression in two times periods: three years before and after the law passage (1993-2000) and two years before and after the law passage (1994-1999). The control sample includes issuers headquartered in states with the largest numbers of VC and PE firms (top eight, four, or two), and the treatment sample includes issuers headquartered outside of those states. Expected liquidity is measured by *Peer Spread* in Panel A, by *Peer Turnover* in Panel B, and by *Peer AIM* in Panel C. The three panels have otherwise identical columns. In Columns (1) and (2), the control sample includes issuers headquartered in CA, NY, MA, TX, IL, CT, PA, and NJ. In Columns (3) and (4), the control sample includes issuers headquartered in CA, NY, MA, and TX. In Columns (5) and (6), the control sample includes issuers headquartered in CA and NY. *Treated* is a dummy variable that is equal to one if the issuer is in the treatment sample and zero if the issuer is in the control sample. *Post* is a dummy variable that is equal to one if the issuer goes public after 1997 and zero if before 1996. Additional variable construction is in Appendix. For the sake of brevity, estimation results for control variables including *Top Underwriter, New Shares Ratio, Integer Price, Sentiment, Log(Assets), Log(1+Age)*, and the intercept are not presented. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

Panel A: Controlling for peer spread

	Control Sample					
	CA, NY, MA, TX, IL, CT, PA, NJ		CA, NY, MA, TX		CA, NY	
	1993-2000	1994-1999	1993-2000	1994-1999	1993-2000	1994-1999
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	**-0.151*** **	**-0.107** **	**-0.178*** **	**-0.134** **	**-0.101* **	**-0.124*** **
	(-3.71)	**(-2.22)**	**(-4.69)**	**(-2.27)**	**(-1.80)**	**(-2.77)**
Treated	0.038**	0.025	0.041***	0.034*	0.024	0.014
	(2.29)	(1.30)	(2.85)	(1.97)	(1.03)	(0.54)
Post	0.191***	0.103	0.222***	0.129	0.201***	0.142*
	(3.92)	(1.48)	(4.28)	(1.63)	(3.34)	(1.76)
Peer Spread	-3.459***	-3.847***	-3.491***	-3.862***	-3.523***	-3.923***
	(-3.83)	(-2.89)	(-3.84)	(-2.87)	(-3.85)	(-2.86)
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,343	889	1,343	889	1,343	889
Adjusted R-squared	0.256	0.276	0.260	0.278	0.253	0.279

Panel B: Controlling for peer turnover

	Control Sample					
	CA, NY, MA, TX, IL, CT, PA, NJ		CA, NY, MA, TX		CA, NY	
	1993-2000	1994-1999	1993-2000	1994-1999	1993-2000	1994-1999
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	**-0.121*** **	**-0.071**	**-0.149*** **	**-0.104** **	**-0.078**	**-0.099** **
	(-2.98)	**(-1.65)**	**(-4.39)**	**(-2.34)**	**(-1.60)**	**(-2.67)**
Treated	0.035**	0.024	0.036**	0.026	0.024	0.017
	(2.32)	(1.15)	(2.65)	(1.45)	(1.22)	(0.66)
Post	0.055	0.031	0.084*	0.056	0.061	0.069
	(1.10)	(0.93)	(1.76)	(1.49)	(0.98)	(1.53)
Peer Turnover	44.903***	45.022***	44.794***	44.947***	45.231***	44.975***
	(4.22)	(3.02)	(4.25)	(3.03)	(4.26)	(3.00)
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,343	889	1,343	889	1,343	889
Adjusted R-squared	0.327	0.345	0.330	0.347	0.325	0.348

Panel C: Controlling for peer AIM

	Control Sample					
	CA, NY, MA, TX, IL, CT, PA, NJ		CA, NY, MA, TX		CA, NY	
	1993-2000	1994-1999	1993-2000	1994-1999	1993-2000	1994-1999
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	**-0.146*** **	**-0.100** **	**-0.172*** **	**-0.126** **	**-0.102* **	**-0.124*** **
	(-3.65)	**(-2.08)**	**(-4.59)**	**(-2.17)**	**(-1.88)**	**(-2.85)**
Treated	0.037**	0.020	0.039**	0.028	0.027	0.015
	(2.19)	(1.14)	(2.54)	(1.62)	(1.14)	(0.59)
Post	0.166***	0.074	0.196***	0.099	0.179***	0.116
	(3.29)	(1.04)	(3.74)	(1.26)	(2.95)	(1.43)
Peer AIM	-0.090***	-0.101***	-0.091***	-0.100***	-0.092***	-0.102***
	(-4.53)	(-3.45)	(-4.54)	(-3.46)	(-4.57)	(-3.44)
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,343	889	1,343	889	1,343	889
Adjusted R-squared	0.268	0.292	0.272	0.294	0.266	0.295

Table 10

Placebo tests in the pre-NSMIA period

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of underpricing on a pseudo event that is assumed to only affect the liquidity of private firms in treated states in a particular two-year period between 1981 and 1996, before NSMIA is enacted in October, 1996. The regression variables are shown in Equation (10). For brevity, only coefficients of key explanatory variables are shown. The control states are the top eight states with the highest number of PE and VC firms, i.e. CA, NY, MA, TX, IL, CT, PA, and NJ. The treated states include all other states. We take the pre-treatment period of the sample from 1981 to 1996 and select moving windows of eight years with a pseudo event occurring in the 4th and 5th year, The pseudo event years are the two consecutive years between 1984 and 1993. The event window includes three years before and after the pseudo event years. *Treated* is a dummy variable that is equal to one if the issuer located in the treated states. *Post* is a dummy variable that is equal to one if the issuer goes public after the pseudo event years and zero otherwise. Expected liquidity is proxied by *Peer AIM*. Variable construction is described in Appendix. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

Event Years	1984&1985	1985&1986	1986&1987	1987&1988	1988&1989	1989&1990	1990&1991	1991&1992	1992&1993
Event window	1981-1988	1982-1989	1983-1990	1984-1991	1985-1992	1986-1993	1987-1994	1988-1995	1989-1996
Treated × Post	**-0.070**	**-0.077**	**0.085**	**0.013**	**-0.005**	**-0.023**	**0.004**	**-0.083**	**-0.042**
	(-1.13)	**(-1.67)**	**(0.93)**	**(0.32)**	**(-0.19)**	**(-1.45)**	**(0.21)**	**(-1.56)**	**(-0.98)**
Treated	0.095	0.082*	0.000	0.021	0.012	0.019*	-0.002	0.083	0.034
	(1.58)	(1.86)	(0.00)	(1.14)	(0.96)	(1.77)	(-0.14)	(1.63)	(0.86)
Post	0.167**	0.173***	0.010	-0.029	0.011	0.054***	0.036**	0.127***	0.111***
	(2.56)	(2.95)	(0.12)	(-0.51)	(0.51)	(3.25)	(2.02)	(3.67)	(2.91)
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	323	217	144	366	633	846	829	753	963
Adjusted R-squared	0.074	0.164	0.066	0.080	0.105	0.142	0.130	0.193	0.154

Table 11

IPO underpricing and the National Security Market Improvement Act in October 1996: Cross-sectional

The table reports coefficients and t-statistics in the parenthesis of OLS regressions of IPO underpricing on the passage of the National Security Market Improvement Act (NSMIA) in October 1996, as shown in regression Equation (11). The sample period is 1993-2000 excluding the years of 1996 and 1997. Expected liquidity is measured by *Peer Spread* in Panel A, by *Peer Turnover* in Panel B, and by *Peer AIM* in Panel C. We run the regression in two times periods: in Columns (1) and (2), the sample is from three years before and after the law passage (1993-2000); and in Columns (3) and (4), the sample is from two years before and after the law passage (1994-1999). We interact the rank (*Rank*) or the percentage (*Percentage*) of VC and PE firms of each state shown in Table 7 with the dummy variable *Post,* which is equal to one if the issuer goes public after 1997, and zero if before 1996. Variable construction is described in Appendix. For the sake of brevity, estimation results for control variables including *Top Underwriter, New Shares Ratio, Integer Price, Sentiment, Log(Assets), Log(1+Age)*, and the intercept are not presented. The industry fixed effects control for the issuer's industry, defined by the Fama-French 10-industry classification. The t-statistics are computed based on standard errors adjusted for heteroskedasticity and industry-year clustering. Asterisks denote statistical significance at 1% (***), 5% (**), or 10% (*) level.

Panel A: Controlling for peer spread

	1993-2000		1994-1999	
	(1)	(2)	(3)	(4)
Rank × Post	**-0.009*** **		**-0.006*** **	
	(-4.97)		**(-3.09)**	
Rank	0.002**		0.002**	
	(2.50)		(2.22)	
Percentage × Post		**0.007*** **		**0.006** **
		(2.80)		**(2.62)**
Percentage		-0.002		-0.001
		(-1.46)		(-0.75)
Post	0.206***	0.066	0.114	-0.005
	(4.21)	(1.31)	(1.65)	(-0.08)
Peer Spread	-3.515***	-3.520***	-3.892***	-3.923***
	(-3.85)	(-3.84)	(-2.87)	(-2.86)
Industry FE	YES	YES	YES	YES
Observations	1,343	1,343	889	889
Adjusted R-squared	0.256	0.256	0.276	0.279

Panel B: Controlling for peer turnover

	1993-2000		1994-1999	
	(1)	(2)	(3)	(4)
Rank × Post	**-0.007*****		**-0.004***	
	(-3.25)		**(-1.97)**	
Rank	0.002***		0.002**	
	(2.81)		(2.18)	
Percentage × Post		**0.005****		**0.005****
		(2.51)		**(2.51)**
Percentage		-0.001		-0.001
		(-1.47)		(-0.62)
Post	0.066	-0.042	0.039	-0.043
	(1.25)	(-0.98)	(1.10)	(-1.16)
Peer Turnover	44.996***	44.985***	45.139***	44.866***
	(4.22)	(4.25)	(3.00)	(3.01)
Industry FE	YES	YES	YES	YES
Observations	1,343	1,343	889	889
Adjusted R-squared	0.327	0.327	0.345	0.347

Panel C: Controlling for peer AIM

	1993-2000		1994-1999	
	(1)	(2)	(3)	(4)
Rank × Post	**-0.009*****		**-0.006*****	
	(-5.21)		**(-3.17)**	
Rank	0.002**		0.002**	
	(2.40)		(2.06)	
Percentage × Post		**0.007*****		**0.006****
		(2.84)		**(2.64)**
Percentage		-0.002		-0.001
		(-1.43)		(-0.63)
Post	0.183***	0.043	0.089	-0.029
	(3.61)	(0.85)	(1.25)	(-0.51)
Peer AIM	-0.092***	-0.092***	-0.102***	-0.102***
	(-4.52)	(-4.55)	(-3.42)	(-3.44)
Industry FE	YES	YES	YES	YES
Observations	1,343	1,343	889	889
Adjusted R-squared	0.269	0.268	0.292	0.295

Table 12

Changes to OHR on Nasdaq and the passage of NSMIA

The table presents the distribution of the two dummy variables *Nasdaq* and *Treated (NSMIA)* among observations used in the two regressions of Equations (9) and (10), after 1997 (*Post = 1*), in the years of 1998, 1999, and 2000. We construct a 2 × 2 matrix, showing the number of observations in four groups: *Nasdaq=1* and *Treated (NSMIA)=1, Nasdaq=1* and *Treated (NSMIA)=0, Nasdaq=0* and *Treated (NSMIA)=1,* and *Nasdaq=0* and *Treated (NSMIA)=0*. Panel A defines *Treated (NSMIA)* in Equation (10) with the control group consisting of deals in CA, NY, MA, TX, IL, CT, PA, and NJ. Panel B defines *Treated (NSMIA)* in Equation (10) with the control group consisting of deals in CA, NY, MA, and TX. Panel C defines *Treated (NSMIA)* in Equation (10) with the control group consisting of deals in CA and NY.

Panel A: Control group of NSMIA in top eight states with largest numbers of VC and PE firms

	Nasdaq=1	*Nasdaq=0*
Treated (NSMIA)=1	177	43
Treated (NSMIA)=0	412	85

Panel B: Control group of NSMIA in top four states with largest numbers of VC and PE firms

	Nasdaq=1	*Nasdaq=0*
Treated (NSMIA)=1	247	56
Treated (NSMIA)=0	342	72

Panel C: Control group of NSMIA in top two states with largest numbers of VC and PE firms

	Nasdaq=1	*Nasdaq=0*
Treated (NSMIA)=1	338	86
Treated (NSMIA)=0	251	42