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THE DEREGULATION OF THE PRIVATE EQUITY MARKETS AND THE DECLINE IN IPOS

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ABSTRACT

The deregulation of securities laws|in particular the National Securities Markets Improvement Act (NSMIA) of 1996|has increased the supply of private capital to late-stage private startups, which are now able to grow to a size that few private firms used to reach. NSMIA is one of a number of factors that have changed the going-public versus staying-private trade-off, helping bring about a new equilibrium where fewer startups go public, and those that do are older. This new equilibrium does not reflect an IPO market failure. Rather, founders are using their increased bargaining power vis-a-vis investors to stay private longer.

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The number of initial public offerings (IPOs) in the United States has experienced a sharp decline since peaking in 1996 (Doidge, Karolyi, and Stulz, 2013, 2017; Gao, Ritter, and Zhu, 2013). While this decline has garnered considerable attention in academic and policy circles and in the press,[1] both its causes and its consequences remain unclear. Gao et al. (2013, p. 1663) argue that the drop in IPOs follows from technological changes due to which "the advantages of selling out to a larger organization ... have increased relative to the benefits of operating as an independent firm." By contrast, Doidge et al. (2017) note that the U.S.-centric nature of the IPO decline suggests that global technological shocks cannot completely explain it. At the same time, both Gao et al. (2013) and Doidge et al. (2013, 2017) agree that the Sarbanes-Oxley Act and other early-2000s changes in public firms' regulatory environment did not drive the fall in IPOs. Doidge et al. (2013, p. 571) go on to add that while their "findings are consistent with the view that U.S. financial markets became less hospitable for young, small firms, direct tests of this view, while needed, are beyond the scope of [their] paper."

The going-public decision is likely to be a multi-faceted one that depends on the relative costs of public and private capital. While several prior studies have analyzed changes in the public equity markets during the IPO decline, this paper focuses on changes in the *private* equity markets during the decline. Our analysis shows that, as hypothesized by de Fontenay (2017), the deregulation of securities laws in the 1990s has facilitated the process of raising capital privately and has been a key—but by no means the only—contributor to the changes in the going-public versus staying-private trade-off.

One such notable deregulation event has been the National Securities Markets Improvement Act (NSMIA), passed in October 1996. NSMIA has made it easier for both private startups and the private funds investing in them to raise capital. First, NSMIA exempts private firms selling unregistered securities under Rule 506 of Regulation D from state securities regulations known as blue sky laws (Rule 506 is one of the exemptions firms can use to issue private shares not registered with the SEC). As a result, NSMIA has made it easier for startups to raise private capital from out-of-state investors by exempting private firms from complying with the blue sky laws of every new state where they issue securities (public firms have long been exempt

[1] For recent examples, see Driebusch (2017), Buttonwood (2018), and Chernova (2019).

from blue sky laws). Second, NSMIA has made it easier for private funds such as venture capital (VC) and private equity (PE) funds to raise large amounts of capital by increasing the number of investors in a fund that force the fund to register under the Investment Company Act (ICA).[2] Registered funds have to regularly disclose their investment portfolio and face leverage and other restrictions, and so VC and PE funds tend to avoid having to register.

To investigate NSMIA's effect on startups' access to private capital, we perform several difference-in-differences (diff-in-diff) analyses.[3] Our first diff-in-diff test builds on the notion that the higher capital requirements of late-stage startups imply that they should be more intensely treated by a reduction in the cost of raising out-of-state capital than their early-stage counterparts. Consistent with this prediction, we show that after the passage of NSMIA, late-stage startups are more likely to raise capital from out-of-state investors than early-stage startups. In addition, our second diff-in-diff test shows that after NSMIA, late-stage startups' ability to raise large funding rounds increases more than that of their early-stage counterparts.

NSMIA's passage coincided with the years of the Internet boom, during which VC fundraising and investment grew rapidly. A potential concern is that our diff-in-diff results might be driven by factors other than NSMIA that could have both helped fuel the Internet boom and led VC investors to increase the size of their investments by focusing on out-of-state and late-stage startups even in the absence of NSMIA. The results of a number of identification tests help alleviate this concern.

Most notably, a triple-diff analysis that exploits pre-NSMIA variation in the extent to which different states had voluntarily uniformized their blue sky laws shows that the impact of NSMIA on late-stage startups' ability to raise both out-of-state and large funding rounds is strongest in states with non-uniform blue sky laws. In addition, our diff-in-diff and triple-diff results are unique to NSMIA's actual treatment period and (with only one exception) become insignificant when estimated over adjacent placebo periods that also experienced similar growth in VC fundraising. Furthermore, the findings on the effects of NSMIA are robust to the exclusion of

[2] Strictly speaking, a VC fund is a particular type of private equity fund. Throughout this paper, we distinguish between VC funds and other types of funds making private equity investments, which we refer to as "PE funds."

[3] All empirical analyses in the paper focus on VC-backed startups, which have traditionally been a major player in both the IPO market (Ritter, 2018) and the production of innovation (e.g., Gornall and Strebulaev, 2015), and for which pre-IPO financing data are widely available.

information technology (IT) startups.

Our third diff-in-diff test seeks to identify NSMIA's effect on VC and PE funds' ability to raise large amounts of capital, an effect that should be strongest for funds investing in capital-intensive late-stage startups. Consistent with this prediction, we find that the size of late-stage funds increases more than that of their early-stage counterparts around the passage of NSMIA—a finding that also passes our battery of identification tests.[4]

Our findings thus indicate that by creating a uniform regulatory environment for private security issues across the U.S. and by facilitating VC and PE funds' access to a larger number of investors, NSMIA has increased the supply of private capital available to late-stage VC-backed startups—precisely the kind of firms that have traditionally been IPO candidates.

A series of descriptive results further support this conclusion. First, we show that the decline in U.S. IPOs has been accompanied by an increase in the fraction of startups that stay independent and privately-held long after they first raise capital. In particular, this indicates that IPOs have not been replaced by an increase in the number of firms that rely on capital provided by a publicly listed acquirer to fund their growth. Second, we find that those startups that still go public now are older when they do so: The median numbers of years from first VC financing to IPO has increased from around four years in the 1990s to seven in recent years.

Third, we show that startups that stay private longer are increasingly able to raise large amounts of capital and reach levels of employment and sales that in the early 1990s only their public peers consistently reached. Fourth, and consistent with our diff-in-diff results, we find that the average round size raised by late-stage private startups sharply increased after NSMIA's passage in October 1996: The average late-stage round was between \$5.9 million and \$7.3 million from 1992 to 1995, it increased to \$10.3 million in 1997, and it has stayed above \$10 million since then. Fifth, consistent with our firm-level evidence, the aggregate amount of private capital raised by late-stage startups has increased substantially, from \$1.3 billion in 1995 to \$7.7 billion in 2000—a 498% five-year cumulative increase—and then to \$11.4 billion in 2005,

[4] The pre-NSMIA registration requirements under the federal ICA affected all U.S. funds equally. Thus, our fund size analysis does not lend itself to exploiting cross-state variation in the impact of NSMIA. Instead, we use data on foreign VC and PE funds to show that the increase in the size of late-stage funds around the passage of NSMIA is unique to U.S. funds. An analogous triple-diff analysis also shows that the increase in late-stage startups' ability to raise large funding rounds is unique to U.S. startups.

$14.1 billion in 2010, and $33.0 billion in 2015. Finally, we show that non-traditional startup investors—especially PE funds, which like VC funds benefited from the passage of NSMIA, but also hedge funds and mutual funds (Chernenko, Lerner, and Zeng, 2017; Kwon, Lowry, and Qian, 2019)—have been a key driver of the growth in the supply of late-stage private capital.

Taken together, our findings suggest that late-stage VC-backed startups are now able to raise large sums of capital while remaining private. A natural question then follows: Are these late-stage startups raising capital privately because they cannot go public—or is an IPO still an option for successful startups, but many choose to remain private instead?

To shed light on this question, consider why a startup may choose to stay private. In their survey of CFOs, Brau and Fawcett (2006) find that the main reason leading successful firms to stay private is their managers' desire to preserve decision-making control and ownership—a reason also emphasized by Boot, Gopalan, and Thakor (2006) and Helwege and Packer (2009), among others. However, founders' desire to stay private (and independent) often conflicts with VCs' desire to go public, as IPOs ensure a timely liquidation of their investment (as do acquisitions) and carry considerable reputational benefits for VCs (Gompers, 1996).[5]

Consistent with founders and their investors having conflicting exit preferences, we find that those founders that are able to retain a larger degree of control of their startup are less likely to eventually go public or be acquired.[6] Of course, founder control is likely endogenous. Our instrumental variable identification strategy is based on the assumption that founders who raise their first financing round in state-years with higher VC supply benefit from a "money chasing deals" environment (Gompers and Lerner, 2000) that allows them to extract better terms—and, in particular, retain a higher equity stake. Our instrument interacts two sources of plausibly exogenous variation in the supply of venture capital at the state-year level: (1) variation in the assets of state and local pension funds (Gonzalez-Uribe, 2014; Bernstein, Lerner, Sorensen, and Strömberg, 2016), and (2) variation in the funds' propensity to exhibit home-state bias in their investment decisions as captured by the fraction of state officials in their boards of trustees

[5] For a recent example of this conflict, see Dwoskin, Winkler, and Pulliam (2015) and Chernova (2019), which describe the "deepening rift in Silicon Valley between private companies that want to stay that way and investors who want to unlock at least some of the profits from their most successful investments."

[6] We measure founder control using the founders' equity stake one year after the startup's first financing round. We show that this measure is strongly and persistently correlated with the fraction of seats in a startup's board of directors that the founders control.

(Hochberg and Rauh, 2013; Andonov, Hochberg, and Rauh, 2018).

The exclusion restriction requires our instrument to impact the exit decision of startups only through its effect on the founders' initial equity stake. Two facts help support this assumption. First, the assets of a state's pension funds reflect the funds' past contributions and investment performance as opposed to their investment opportunities at the time of the local startups' exit decision. Second, the board seat allocation of most public pension fund boards is stable and was initially set decades before pension funds were allowed to invest in VC and PE funds (Andonov, Hochberg, and Rauh, 2018).

Importantly, we show that the decline in IPOs has coincided with a gradual increase in founder control over time. The reasons driving this increase are likely several, and a full analysis of these reasons falls beyond the scope of our paper. That said, both the increase in the supply of private capital that we document as well as technological changes decreasing startups' capital requirements early in their lifecycle—when uncertainty is highest and capital is most expensive (Ewens, Nanda, and Rhodes-Kropf, 2018)—have likely played a role.

Our paper makes two contributions. First, the paper enhances our understanding of how the going-public versus staying-private trade-off has changed since the early 1990s. We show that the deregulation of the private equity markets—in particular, the National Securities Markets Improvement Act of 1996—has increased the supply of private capital and reduced the relative cost of being private. At the same time, this increased supply of private capital appears to have helped strengthen founders' bargaining power vis-à-vis investors. The end result is that more founders now have both the board votes and the private capital to realize their preference for control by delaying—or avoiding altogether—an IPO.

To be sure, our findings do not imply that NSMIA caused the IPO decline—although the fact that its passage in 1996 coincided with the U.S. listing peak suggests that it may have been a contributing factor.[7] But our evidence does indicate that the IPO decline has not impeded late-stage startups' ability to finance their growth. Ruling out the possibility that the fall in IPOs has led to an overall reduction in the supply of capital available to startups would require

[7] Doidge, Kahle, Karolyi, and Stulz (2018) discuss several other potential contributing factors, including the increased importance of intangible assets coupled with public markets' disadvantage in supporting young, R&D-intensive firms.

a comparison of startups' ability to fund their investment opportunities today to that of similar startups before 1996—a comparison unlikely to be feasible. However, our finding that both VCs and other less traditional startup investors provide an increasing amount of private capital to late-stage startups, allowing them to grow to a size until recently reached by few private firms, suggests that private markets now supply much (if not all) of the capital IPOs used to supply.

Second, our paper contributes to the legal debate on the costs and benefits associated with NSMIA's federal preemption of blue sky laws. Some legal scholars have expressed concerns that "NSMIA's preemption of state regulation of private placements ... created a regulatory black hole," proposing to "return to the states supervision of private placements by or to nonregulated persons or entities" (Johnson, 2010, p. 155). Our paper highlights the benefits of having a uniform regulatory environment for private equity issues across the U.S. and the potential costs of returning to a system where each state imposes its own regulations. Differences aside, our findings can also be seen as pointing to the benefits of creating a similarly uniform regulatory environment for private equity markets in Europe and other parts of the world.[8]

1 Data and Sample

Our main analysis sample consists of VC-backed startups. We begin by considering all startups in the VC database VentureSource (a division of Dow Jones) that raised their first private round of funding between 1992 and 2016.[9] In order to be in our sample, a startup needs to (1) be headquartered in the U.S. and (2) have raised at least one equity financing round from a traditional VC investor (defined as a standard fixed-life fund that raises capital from limited partners). For these startups, we collect data on all the capital (equity and debt) they raise both from traditional VCs and from other non-VC investors, such as PE funds, corporations, or mutual funds (Section 4.3 provides further details on non-VC investors).

We supplement the VC and private investment data provided by VentureSource with information from Correlation Ventures, a quantitative VC fund. This supplemental information

[8] Section IA.2 in the Internet Appendix discusses the European Union's recent attempts to integrate the European private equity markets and the challenges these attempts have faced.

[9] We choose 1992 as the starting point because the coverage of VC financings and investors is poor before then. We observe investments through the end of 2016, and we adjust our sample period according to the data demands of each analysis.

(described in detail in Ewens, Nanda, and Rhodes-Kropf (2018)) is particularly useful for those analyses that require data on founder equity, exit valuations, and firm failures, as the coverage of these data is poor in commercial databases. In addition, we obtain VC and PE fund-level information from VentureSource and PitchBook. Sections IA.3 and IA.4 in the Internet Appendix provide additional details on the data and variables used in our analyses.

Our focus on VC-backed startups, while admittedly restrictive, offers three key advantages. First, we are able to observe firm- and financing-level outcomes that are typically unavailable for non-VC-backed private firms. As a result, VC-backed startups offer a unique window on the changes in the U.S. private markets that have accompanied the decline in IPOs. Second, although VC-backed firms make up less than 1% of all privately held firms (Puri and Zarutskie, 2012), historically they have accounted for a sizable share of the U.S. IPO market,[10] which makes them particularly relevant to any analysis of the decline in IPOs. Third, VC-backed firms play a prominent role in the production of innovation (Kortum and Lerner, 2000; Gornall and Strebulaev, 2015). Understanding how they are adapting to the ongoing changes in the entrepreneurial finance market is thus critical, as the success of their adaptation is likely to have economy-wide consequences.

2 The Deregulation of the Private Equity Markets and the Supply of Capital to Late-Stage Startups

This section discusses how regulatory changes in the private equity markets can impact the supply of private capital to late-stage startups, focusing on one major deregulation event, NSMIA. We test the empirical predictions derived here in the following section.

2.1 Private capital markets and the IPO decision

Traditionally, a major benefit of going public has been the ability to tap a larger pool of capital than is available in the private markets. In their survey of CFOs, Brau and Fawcett (2006, p. 410) find that the "need for capital to support growth" is one of the main drivers of the IPO

[10] Ritter (2018) shows that, over 1990–2016, VC-backed firms accounted for 42% of all IPOs in the U.S.

decision. Public markets have also been shown to provide private firms and their investors more liquidity (Pagano, Panetta, and Zingales, 1998; Bodnaruk, Kandel, Massa, and Simonov, 2007), currency for acquisitions (Celikyurt, Sevilir, and Shivdasani, 2010), and improved flexibility for employee compensation. Founders considering an IPO must weigh these benefits against the loss of control (Brau and Fawcett, 2006) and other costs associated with going public—including one-time listing costs as well as the ongoing costs of disclosure (Farre-Mensa, 2017; Aghamolla and Thakor, 2019), takeover risk (Zingales, 1995), and short-termist pressures and other agency problems (Asker, Farre-Mensa, and Ljungqvist, 2015; Bernstein, 2015).

The early 2000s saw a number of major regulatory changes in the public-equity markets, including Regulation Fair Disclosure in 2000, the Sarbanes-Oxley Act of 2002, and the 2003 Global Settlement. Several public commentators have argued that these changes increased the cost of being public, particularly for small- and medium-sized public firms, leading to a decline in the number of IPOs (e.g., Zweig, 2010; Weild, 2011). Yet both Gao, Ritter, and Zhu (2013) and Doidge, Karolyi, and Stulz (2013) conclude that such regulatory changes cannot, on their own, explain the reduction in IPOs and why the number of U.S. listed firms has been cut in half since peaking in 1996 (Doidge, Karolyi, and Stulz, 2017). In particular, Doidge et al. (2013, p. 549) write that small-firm IPOs "became abnormally low before these changes took place."

Importantly, regulatory changes that increase the supply of *private* capital—which have received far less attention in the finance literature—should also alter the going-public versus staying-private trade-off. Indeed, an increased ability to raise private capital allows late-stage startups and other traditional IPO candidates to delay—temporarily or indefinitely—their IPO, while still being able to finance their growth opportunities and avoid many of the above-mentioned listing costs. We next analyze one such regulatory shock: NSMIA.[11]

2.2 NSMIA: Deregulating and uniformizing the private capital markets

A few years before the adoption of the early-2000s regulatory changes affecting public firms, one major regulatory change made it easier for both private startups and their investors to

[11] Other notable regulatory changes include the SEC's adoption of Rule 144A in 1990 and the several subsequent amendments to Rule 144, as a result of which "Rule 144 now effectively permits the unlimited and unfettered resale of restricted securities [such as private shares] after a six-month or one-year period" (de Fontenay, 2017, p. 468), as well as the JOBS Act of 2012.

raise capital: NSMIA, signed into law by President Clinton on October 11, 1996.

While NSMIA has received little attention among finance scholars, several legal scholars and practitioner-oriented publications have argued that it has played a first-order role in facilitating private firms' access to capital (e.g., Denos, 1997; Campbell, 1998; Cox, 2013; Badway et al., 2016). Writing of NSMIA and other regulatory changes affecting the private-equity markets, de Fontenay (2017, p. 466) notes:

> The liberalization of the rules for selling and trading private securities is arguably the most significant development in securities regulation of the last thirty years, but the empirical literature on the decline of public equity has largely overlooked it. This is a critical and surprising omission, because the changes to the private side of securities regulation bear directly on a company's decision to go public.

Two distinct provisions of NSMIA have helped increase the supply of private capital available to late-stage startups. First, NSMIA exempts qualified private security issuers from having to comply with the blue sky laws of each state where they issue securities. Second, NSMIA makes it possible for VC and PE funds to raise capital from a larger number of investors without registering under the Investment Company Act of 1940.[12] We describe them in turn.

2.2.1 NSMIA's preemption of state blue sky laws

Consider a hypothetical private startup seeking to raise outside capital to finance its growth. Prior to NSMIA's passage, several regulations applied in this setting. First, the startup needed to qualify for one of the federal exemptions that allow firms to issue securities privately without registering them with the SEC (e.g., a Regulation D exemption).

In addition to complying with federal regulations, the startup also needed to comply with the laws governing the issuance of securities in each state where it sold its securities, commonly known as blue sky laws. Though the specific provisions of the blue sky laws varied across states—a feature we will exploit in Section 3.3.1—they generally required the registration with the appropriate state regulatory agency of private offerings made by firms (or funds) to individuals or institutions in a state in order to protect the public from fraud. Registration

[12] NSMIA's preemption of state blue sky laws went into effect at its signing, on October 11, 1996; NSMIA's amendments to the ICA went into effect on January 1, 1997.

requirements ranged from filing a simple form and paying a filing fee to requiring state regulators to pre-approve the offering after a so-called "merit review" (further discussed below). In addition, most state blue sky laws provided private causes of action for private investors who believed to have been injured by securities fraud, and some also required periodic disclosures.

To illustrate, if a startup headquartered in Seattle issued shares to investors located in Washington, California, New York, and Texas, the startup needed to comply with the blue sky laws of these four states. Importantly, it was the issuer—i.e., in this example, the startup, not its investors—that needed to comply with each state's blue sky law. In a pre-NSMIA legal treatise for entrepreneurs and their lawyers, Sherman and Williams (1995, p. 5.3) noted:

> Failure to observe blue sky laws, even after full compliance with federal laws, can invalidate the [security issuance] transaction and subject the parties to civil and criminal liabilities and can also render it very difficult or even impossible at the time of an under-written public offering for issuer's counsel to give the necessary assurances to the underwriters that there are no contingent liabilities, particularly rescission rights, relating to the issuer's outstanding securities.

Legal practitioners had long recognized the compliance burdens posed by this dual system of federal and state regulations.[13] Former SEC chairman Armstrong (1958, pp. 714-715) wrote:

> The "blue sky" laws had come to have a special meaning—a meaning full of complexities, surprises, unsuspected liabilities for transactions normal and usual—in short, a crazy-quilt of state regulations no longer significant or meaningful in purpose, and usually stultifying in effect, or just plain useless.

Almost 40 years later, in 1996, then-SEC chairman Levitt (1996) testified in the U.S. Senate:

> The current system of dual Federal-State regulation is not the system that Congress—or the [SEC]—would create today if we were designing a new system. While securities markets today are global, issuers and securities firms still must register many securities offerings in 52 separate jurisdictions; satisfy a multitude of separate books and records requirements; and bear the substantial costs of compliance with the overlapping requirements.

[13] The original state blue sky laws were passed between 1911 and 1931, at a time when federal securities regulation in the U.S. was largely nonexistent (Macey and Miller, 1991; Mahoney, 2003). Agrawal (2013) shows that during the pre-SEC years, the laws induced firms to increase dividends, issue equity, and grow in size, and they facilitated improvements in operating performance and market valuations. The dual federal-state system arose with the passage of the Securities Act of 1933 and the Securities Exchange Act of 1934, which created the SEC. Over the following years, most state blue sky laws were amended to largely exempt public firms—but they remained fully in force for private firms.

Perhaps the most onerous provision of the blue sky laws was the merit review that private security issuers in many states had to undergo before being able to issue securities in a state. Reporting on the findings of a subcommittee of the American Bar Association (ABA), Sargent (1986, pp. 805-806) described the blue sky merit review process as follows:

> Many of the statutes that confer merit authority provide very broad grounds for the administrator's decision to deny, revoke, or suspend the effectiveness of a registration statement. A typical provision would authorize the administrator to take such action if he or she finds that the offering is not "fair, just and equitable." There are numerous minor variations in the statutory statements of such general standards. Some statutes direct the administrator simply to determine the fairness of the offering, while others may require consideration of fairness of the "issuer's plan of business." ...
>
> These broadly formulated standards apparently grant the administrator the authority to impose, in the name of fairness, a wide variety of restrictions and standards on all aspects of an offering and on the underlying transactions. The administrators have used this general authority to create a complex network of specific merit requirements.

The report also discussed the potential costs associated with state merit review (p. 846):

> First, there are compliance costs associated with merit regulation. ... Among those costs are the attorneys' fees incurred when the issuer or the underwriter has to negotiate with merit administrators to clear an offering in one or more states. These costs can increase when the offering is controversial and negotiations are prolonged. ...
>
> The burden on the issuer of these direct compliance costs, however, is relatively insignificant, at least in most cases. More significant are the delays sometimes generated by difficulties with merit review. ... These delays are perhaps most likely to occur when a new securities product is about to come to market. ...
>
> Although compliance costs and the costs of delay can be important, they are perhaps not the most significant: there are larger and less quantifiable costs. For example, we do not know whether denying access to merit states' securities markets has seriously inhibited potentially successful enterprises from raising capital. Similarly, we do not know whether merit regulation inhibits venture capital investment.

The costs and uncertainty associated with the pre-NSMIA merit review process highlighted by the ABA report were particularly onerous for the innovative startups that tend to raise venture capital, for two reasons. First, investments in innovative startups tend to involve considerable risk and uncertainty (e.g., Hall and Woodward, 2010). Assessing whether a proposed investment in, say, a four-year-old startup is "fair, just and equitable" from a regulatory perspective is a much more complex and subjective endeavor than when the investment involves a mature company with a tried-and-true business model and dozens of comparable companies that can help price the offering. Second, startups tend to have limited financial slack and pursue investment opportunities that require quick responses (e.g., Kerr and Nanda, 2011, 2015). As a result, delaying an offering for several months—or years—while state regulators determine its fairness can severely undermine a startup's competitive position and even threaten its survival.

NSMIA was Congress's response to the "crazy-quilt of state regulations" described above. Title I, Section 102 of NSMIA "preempts state securities law in certain areas long burdened by duplicative regulation by both federal and state governments" (Denos, 1997, p. 101). Among the "covered securities" that NSMIA exempts from complying with state blue sky laws are those sold under Rule 506 of Regulation D, which allows private issuers to raise unlimited amounts of capital as long as all investors are "accredited investors."[14] As Section 3.1 shows, Rule 506 is the most popular exemption used by VC-backed startups to avoid SEC registration.[15]

2.2.2 NSMIA's changes to the Investment Company Act of 1940

Title II, Section 209 of NSMIA changed some key federal-level regulations affecting VC and PE funds through amendments to the Investment Company Act of 1940 (ICA). The ICA "regulates the organization of companies, including mutual funds, that engage primarily in investing, reinvesting, and trading in securities, and whose own securities are offered to the investing public" (SEC, 2019). The ICA requires investment companies to register with the SEC and imposes extensive regulations on registered entities, including investment and leverage restrictions, restrictions on related party transactions, and ongoing reporting requirements. In particular, all investment companies registered under the ICA are required to disclose their portfolio holdings, including the fair value of each security they hold, on a quarterly basis (semi-annually before May 2004).

Such disclosures are incompatible with the typical business model of VC funds, for at least

[14] Accredited investors include institutions, individuals with annual income in excess of $200,000 ($300,000 for couples), or individuals and couples with net worth in excess of $1,000,000 excluding the primary residence. For an example of how a state securities board describes NSMIA, see Texas State Securities Board (2019).

[15] Rule 506 is also the most common SEC-registration exemption used by private funds (in particular, VC and PE funds) when raising capital (Badway et al., 2016). Most blue sky laws also apply to private funds when they raise capital in a state, not just to private operating companies. Thus, in addition to exempting private firms from complying with blue sky laws, NSMIA also exempted VC and PE funds from the laws when the funds themselves raise capital.

two reasons. First, disclosing the valuation of the funds' portfolio holdings on a quarterly (or semi-annual) basis would harm the competitive position of their portfolio companies.[16] Second, if VC funds had to disclose the valuation of their portfolio holdings every quarter or semester, this would affect the pricing of additional funding rounds as well as exit opportunities. In fact, in a letter to the SEC in 2011, the National Venture Capital Association (NVCA, 2011, p. 6) said that even disclosing the gross and net asset values of a fund—much less detailed information than an ICA-registered fund would have to disclose—would be problematic:

> Information as to the gross and net asset values of a VCF [VC fund] . . . may allow "reverse engineering" regarding valuations associated with particular portfolio companies and should not be publicly available. If so, those private-company valuations could affect pricing of additional rounds of financing and/or exit opportunities in a manner adverse to the VCF, its portfolio companies and its investors.

Thus, as we show below, most VC and PE funds rely on ICA registration exemptions to avoid registering as investment companies.[17] So much so that the SEC (2011, p. 9) adopted "not [being] registered under the Investment Company Act" as one of the five criteria to define a VC fund.

Prior to NSMIA, private investment companies could rely on only one exemption to avoid registration under the ICA: Section 3(c)(1) of the ICA, which exempts from registration funds with up to 100 investors.[18] NSMIA has made it possible for VC and PE funds to raise capital from a larger number of investors while still avoiding ICA registration by adding a new registration exemption: Section 3(c)(7) of the ICA, which allows exempt private funds to surpass the 100-investor limit as long as all the investors are "qualified purchasers" (natural persons

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[16] The case of Google illustrates why. When Google filed for its IPO in 2004, investors were surprised to learn how profitable the company was (e.g., Delaney and Sidel, 2004). If Google's VC investors had had to disclose the value of their investment in Google on a semi-annual basis between 1999 (when it raised its first VC round) and 2004, Google's competitors would have learned about its profitability much earlier than they did, potentially harming the company's competitive position. Google's founders recognized this in the company's S-1 filing: "As a smaller private company, Google kept business information closely held, and we believe this helped us against competitors" (p. iv).

[17] In the case of PE funds, the leverage restrictions associated with being a registered investment company can be even more onerous than the disclosure requirements discussed above.

[18] A private investment company is one that does not make a public offering of securities. Any company making a public offering of securities must register under the ICA. The SEC's "integration doctrine" is designed to ensure that funds cannot get around the 100-investor limit by using parallel (or clone) funds with the same investment purpose, each with up to 100 investors (see, e.g., SEC, 1996, p.35).

who own at least $5 million in investments or institutions that own at least $25 million).[19] In addition, NSMIA has also made it easier for funds relying on the 100-investor limit exemption to avoid surpassing this limit by relaxing the Section 3(c)(1) "look-through" requirement.[20]

Facilitating VC funds' access to capital was a key reason why NSMIA added the Section 3(c)(7) registration exemption to the ICA. Speaking in the U.S. Senate in support of NSMIA, NY Senator D'Amato (1996) noted:

> This legislation will make it easier to raise capital in the securities market. The bill will create a new category of unregistered private investment companies [3(c)7 funds] that will help venture capitalists tap the capital markets to fund business endeavors.

An analysis of VC funds' Form ADV filings from the SEC website provides suggestive evidence that NSMIA has been successful in helping accomplish these goals.[21] As of August 2017, all active VC funds use either the 3(c)(1) or 3(c)(7) exemption to avoid having to register and comply with the disclosure and other requirements of the ICA. Importantly, approximately 5% of all VC funds report having more than 100 investors—a number that, before NSMIA's addition of Section 3(c)(7) to the ICA, would have triggered ICA registration. These 3(c)(7) funds have an average size of $450 million, compared to $62 million for VC funds with up to 100 investors. These findings suggest that by raising the cap on the number of investors that trigger ICA registration, NSMIA made it possible for VC funds to raise larger amounts of capital—a prediction we formally test in Section 3.5 below.

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[19] Until 2012, private funds still needed to stay below 500 investors, which was the universal beneficial ownership trigger for SEC registration. The JOBS Act raised this trigger to 2000 owners in 2012.

[20] Prior to NSMIA, an investing entity that owned 10% or more of a fund counted as only one (legal) person for purposes of the 100-investor limit only if the value of all of the entity's holdings of Section-3(c)(1)-exempt investment companies did not exceed 10% of that entity's assets. Otherwise, the law required to look through the entity to its beneficial owners and count each of them as an investor of the fund. NSMIA allows funds to count all owners as one person for purposes of the 100-investor limit, except in the case of investors owning 10% or more of a fund that are themselves investment companies (including Section 3(c)(1)- and Section 3(c)(7)-exempt investment companies), for which the look-through requirement remains.

[21] VC fund advisers (i.e., managers) need to file annual reports using (an abbreviated version of) Form ADV.

3 Identifying the Effects of NSMIA

3.1 NSMIA and Rule 506 private filings

Until 2016, private firms could use one of three SEC-registration exemptions under Regulation D to raise private capital: Rules 504, 505, or 506 (the SEC repealed Rule 505 in 2016 and integrated its provisions into Rule 504). NSMIA's preemption of state blue sky laws applies only to private issuers raising capital under Rule 506. Thus, if firms value NSMIA's blue sky preemption, they should favor Rule 506 over the other exemptions. Consistent with this prediction, Ivanov and Bauguess (2013) show that Rule 506 offerings account for 99% of the capital raised through Regulation D from 2009 to 2012. They note: "More than two-thirds of non-fund issuers could have claimed a Rule 504 or 505 exemption based on offering size, indicating that issuers value the Blue Sky law preemption allowed under Rule 506" (p. 3).

To more directly test whether NSMIA's passage in October 1996 induced a change in private issuers' behavior, we have obtained the full history of Form D filings from 1992 to 1998 via a FOIA request to the SEC.[22] Figure 1, Panel A shows that the number of Form D filings by private firms claiming a Rule 506 exemption increased by 38% in the four quarters following the passage of NSMIA relative to the four quarters preceding its passage. Importantly, Panel B shows that the number of Rule 504 or 505 filings—to which NSMIA's preemption of blue sky laws did not apply—did not experience a similar increase. This suggestive evidence supports the notion that private issuers value NSMIA's blue sky preemption.

3.2 NSMIA and the supply of out-of-state capital to late-stage startups

Prior to NSMIA, private firms needed to comply with the blue sky laws of each state where at least one of their investors was located. Startups typically raise their initial capital from nearby investors located in their own state—often the founders themselves or their friends and family (Sorenson and Stuart, 2001; Robb and Robinson, 2014). NSMIA has made it possible for startups to raise capital from out-of-state investors without having to comply with those other states' blue sky laws in addition to with their own state's law.

[22] Private issuers must file what is known as a "Form D" with the SEC when relying on a Regulation D exemption to issue securities; these filings are only available on the SEC's EDGAR website beginning in 2002.

While all startups can in principle benefit from having easier access to a larger pool of investors without additional regulatory burden, we expect late-stage startups to be more intensely treated by NSMIA than their early-stage counterparts. The reason is that late-stage startups have higher capital requirements (e.g., Gompers, 1995), and so they should benefit more from being able to raise out-of-state capital than early-stage startups, whose smaller capital needs can be more easily fulfilled by local investors.

This prediction motivates our diff-in-diff analysis of NSMIA's effect on startups' propensity to raise capital from out-of-state investors. Late-stage startups (those raising a Series C or higher) comprise the treatment group and their early-stage counterparts the control group:[23]

$$Y_{it} = \beta_0 + \beta_1 Post_t \times \textit{Late-stage round}_{it} + \beta_2 \textit{Late-stage round}_{it} + \gamma_t + \eta_s + \theta_j + \varepsilon_{it} \qquad (1)$$

Table 1 presents the results of estimating Equation (1) using a linear probability model, where the unit of observation is an equity financing round, i identifies startups, and t identifies year-quarters. The dependent variable (Y) is an indicator set equal to one if the round includes at least one out-of-state investor. The sample of financings ranges from 1994 to 1998, and the *Post* indicator identifies all financings that take place in or after the fourth quarter of 1996. All regressions include financing year-quarter fixed effects (γ, which subsume the non-interacted *Post* indicator) as well as startup state (η) and industry (θ) fixed effects.

The results in column 1 support the hypothesis that NSMIA has facilitated late-stage startups' access to out-of-state investors: The coefficient on the interaction term *Post* × *Late-stage round* is positive and significant, indicating that the relative probability that a late-stage financing includes at least one out-of-state investor increases by 4.1 percentage points after NSMIA's passage ($p = 0.001$). Column 2 shows that our conclusions are robust to using a continuous version of the late-stage indicator, the logarithm of the startup's financing round number ($p < 0.001$); column 3 shows that they are also robust to using as the dependent variable the fraction of investors in the round located outside of the startup's headquarter state ($p = 0.009$).

[23] Consistent with our definition of the treatment and control groups, late-stage rounds raised prior to NSMIA were 26% more likely to include out-of-state investors than early-stage rounds.

3.3 Identification

For our control group to be a valid counterfactual, the evolution of the reliance on out-of-state investors for treated (late-stage) and control (early-stage) startups need to share parallel trends—that is, in the absence of NSMIA, the difference between late-stage and early-stage startups should remain constant over time. While the parallel-trends assumption is ultimately untestable, this section discusses a number of identification tests that support its validity.

To begin with, our review of NSMIA's legislative history in Section IA.1 in the Internet Appendix shows that it would have been all but impossible for startups or their investors to predict NSMIA's passage more than a few weeks in advance and meaningfully alter their behavior in anticipation of this passage. Also, NSMIA does not appear to have been passed in response to a major lobbying effort by startups or their investors, thus alleviating reverse causality concerns.[24] In addition, when we allow the effect of the treatment variable to vary over time by interacting *Late-stage* with semester-year indicators in Equation (1), Figure 2 shows that the pre-NSMIA coefficients exhibit no significant trend.

NSMIA's passage coincided with the years of the Internet boom, during which VC fundraising and investment grew at a fast pace. A potential concern—both in Table 1 and the other diff-in-diff analyses we present later in this section—is that our results might be driven by factors other than NSMIA that helped fuel the Internet boom, such as an increase in technological innovation or behavioral considerations leading investors to chase returns. Regardless of the identity of the non-NSMIA drivers of the VC fundraising increase during the 1990s, this increase could lead VC investors to seek to increase the size of their investments by focusing on out-of-state and late-stage startups even in the absence of NSMIA. Our goal in this section is to show that while NSMIA was by no means the only driver of the changes in the entrepreneurial finance market during the mid and late 1990s, it was a significant driver.

To that end, column 4 in Table 1 shows that the post-NSMIA differential increase in late-stage startups' reliance on out-of-state investors persists—and, if anything, becomes more pronounced—when we exclude startups operating in IT industries ($p = 0.001$). This find-

[24] Instead, the mutual fund industry, which prior to NSMIA also fell under the dual system of federal and state regulation, appears to have been the key driver of the bill.

ing helps alleviate concerns that our diff-in-diff results are driven by changes in the IT sector during the Internet boom. To further isolate the effects of NSMIA, we next exploit cross-state variation in NSMIA's expected impact.

3.3.1 Cross-state differences in the impact of NSMIA

Prior to the passage of NSMIA, some states had tried to alleviate the costs and uncertainty faced by private firms seeking to raise capital in multiple states by voluntarily coordinating their blue sky laws. These coordination efforts were spearheaded by two sets of regulatory frameworks adopted by a number of states in the 1980s and early 1990s: the Uniform Limited Offering Exemption (ULOE) and the Small Corporate Offering Registration (SCOR). The ULOE was proposed in 1983 by the North American Securities Administrators Association (NASAA) to help create some uniformity between the SEC's Regulation D and state blue sky laws (Maynard, 1987);[25] in 1989, the NASAA proposed the SCOR to facilitate the simultaneous registration of Regulation D offerings with the SEC and state regulators (Denos, 1997).[26]

We predict that a late-stage startup from a state that had adopted neither the ULOE nor the SCOR (identified by the indicator *Non-uniform blue sky state*) should experience a larger increase in its ability to access out-of-state investors after the passage of NSMIA than a startup from a ULOE or SCOR state, all else equal. The reason is that, before NSMIA's passage, late-stage startups in states that had adopted the ULOE or the SCOR could raise capital from investors located in other ULOE or SCOR states without triggering much additional blue sky burden and uncertainty. By contrast, prior to NSMIA, late-stage startups in non-uniform blue sky states could not raise capital from any out-of-state investors without complying with a new set of blue sky regulations that were not uniformized with those of their own state.

Importantly, this prediction is unique to NSMIA: If our diff-in-diff results were driven by other confounding factors contemporaneous with the passage of NSMIA, we would not expect the results to be stronger precisely in non-uniform blue sky states—particularly given that

[25] The following states had adopted the ULOE (in some form) by the passage of NSMIA: Alabama, Georgia, Idaho, Iowa, Kansas, Kentucky, Massachusetts, Michigan, Missouri, Montana, Nebraska, North Carolina, Ohio, South Carolina, Tennessee, Utah, Virginia, West Virginia, and Wisconsin.

[26] The states that had adopted the SCOR but not the ULOE by NSMIA's passage include Alaska, Arizona, California, Colorado, Indiana, Louisiana, Maine, Mississippi, North Dakota, New Hampshire, New Jersey, Nevada, Oklahoma, Oregon, Pennsylvania, South Dakota, Texas, Vermont, Washington, and Wyoming.

Figure IA.1 in the Internet Appendix shows that uniform and non-uniform blue sky states were on parallel macroeconomic trends prior to NSMIA's adoption.

The results in Table 2 support the prediction that the effect of NSMIA on the propensity to raise out-of-state capital should be stronger for late-stage startups located in non-uniform blue sky states. In columns 1 and 2, we begin by estimating Equation (1) within the subsamples of uniform and non-uniform blue sky states, respectively. The coefficient on the interaction term $Post \times Late\text{-}stage$ is 3.8 times greater for non-uniform blue sky states (0.114) than for their uniform counterparts (0.030). These sub-sample differences are confirmed in the triple-diff analysis reported in column 3, where the coefficient on the triple interaction $Post \times Late\text{-}stage \times Non\text{-}uniform\ blue\ sky\ state$ is positive and highly significant ($p = 0.001$).

3.3.2 Placebo tests

The dynamic diff-in-diff results in Figure 2 suggest that in the two years prior to NSMIA's passage, when VC fundraising already benefited from the Internet boom, late-stage startups' reliance on out-of-state investors was not already on an increasing trend. Table 3 reports placebo tests that further support the notion that our empirical analyses do not conflate the effects of NSMIA with those of other factors that may have also helped fuel VC fundraising during the 1990s. Specifically, the placebo tests estimate our diff-in-diff and triple-diff analyses during 1992–1996, the years immediately preceding the passage and implementation of NSMIA, and 1997–2001, immediately following NSMIA. Unlike the actual analysis period 1994–1998, there was no major regulatory change affecting the U.S. private capital markets in the middle of the two placebo periods. Therefore, if our diff-in-diff and triple-diff regressions capture the effects of NSMIA and not of other confounds, we should not find similar results when estimating analogous regressions during the placebo periods.

Importantly, the two placebo periods experienced similar growth in the inflow of capital to the VC asset class as the actual analysis period. Specifically, whereas average annual real growth in commitments to VC funds during 1994–1998 was 45.9%, it was 52.1% and 43.2% during 1992–1996 and 1997–2001, respectively.[27] This suggests that if our results in Tables 1

[27] NVCA Yearbook (2013, 2016), Figure 2.02. Dollar figures deflated using the GDP deflator.

and 2 are driven by an increase in the inflow of capital to the VC asset class that is unrelated to NSMIA, we should estimate similar results during the placebo periods.

This is not what we find. Table 3 shows that when we estimate Equation (1) during the 1992–1996 or 1997–2001 placebo periods (columns 1 and 3), the interaction *Post*×*Late-stage round* is insignificant ($p = 0.773$ and $p = 0.364$, respectively).[28] The same is true for the triple interaction *Post* × *Late-stage* × *Non-uniform blue sky state* when we estimate placebo triple-diffs during the 1992–1996 (column 2, $p = 0.838$) or 1997–2001 (column 4, $p = 0.515$) placebo periods.

Taken together, the fact that (1) the post-NSMIA increase in the propensity to raise out-of-state capital is strongest in those states with non-uniform blue sky laws and (2) both our diff-in-diff and triple-diff results are unique to NSMIA's actual treatment period supports a causal interpretation of our estimates in Table 1.

3.4 NSMIA and the size of late-stage startup financing rounds

Having shown that NSMIA has facilitated startups' access to out-of-state investors, we now analyze whether the law has also made it possible for these startups to raise larger financing rounds. To do so, we begin by estimating a diff-in-diff regression analogous to Equation (1) where the dependent variable is now the logarithm of the capital raised in a financing round.[29] Column 1 in Table 4 shows that after NSMIA, late-stage financing rounds increase in size more than early rounds: The coefficient of the interaction term *Post* × *Log round number* implies a 30% ($= log(3) \times exp(0.171) - 1$) post-NSMIA increase in round size for a startup raising its third financing round relative to one raising its first round ($p < 0.001$). The relative post-NSMIA increase when excluding IT startups in column 2 is similar (28%, $p = 0.019$).

Columns 3 through 5 exploit the same cross-state differences in the expected impact of NSMIA described in Section 3.3.1 above. Analogous to our findings in Table 2, a comparison of columns 3 and 4 reveals that the impact of NSMIA on late-stage startups' ability to raise large financing rounds is most pronounced in those states whose blue sky laws had not been

[28] In the 1992–1996 and 1997–2001 placebo analyses, *Post* equals one for quarters greater than or equal to 1994Q4 and 1999Q4, respectively.

[29] We use the continuous version of the late-stage indicator (*Log round number*) to better account for the fact that our measure of financing round size is also continuous.

uniformized with those of other states prior to NSMIA; the triple-diff results in column 4 show that this difference is significant, although marginally so ($p = 0.065$).

Table 5 presents placebo tests for our analysis of financing round size analogous to those reported in Table 3 when analyzing startups' reliance on out-of-state investors. Column 1 shows that the diff-in-diff interaction $Post \times Log\ round\ number$ is insignificant when estimated during the 1992–1996 placebo period ($p = 0.626$). The same is true for the triple-diff interaction $Post \times Log\ round\ number \times Non\text{-}uniform\ blue\ sky\ state$ in column 2 ($p = 0.644$).

However, in column 3, the interaction $Post \times Log\ round\ number$ is highly significant when estimated during the 1997–2001 placebo period ($p = 0.001$). This suggests that our diff-in-diff results in Table 4 may at least be partially driven by factors other than NSMIA that increased the supply of capital to late-stage startups during the Internet boom in the late 1990s. Reassuringly, though, the triple interaction $Post \times Log\ round\ number \times Non\text{-}uniform\ blue\ sky\ state$ remains insignificant during this placebo period (column 4, $p = 0.501$). Taken together, these placebo results support the hypothesis that NSMIA was a significant—but by no means the only—driver of the increase in the supply of private capital during the mid and late 1990s.

Columns 6 and 7 in Table 4 provide additional support for this hypothesis. Column 6 shows that the post-NSMIA increase in late-stage startups' ability to raise large financing rounds is unique to the U.S.: There is no similar increase for startups located in Europe and Canada, which did not deregulate the private equity markets in the middle of the 1994–1998 analysis period ($p = 0.408$).[30] Column 7 shows that the difference between the diff-in-diff results reported in column 1 for U.S. startups and those reported in column 6 for their foreign counterparts, as captured by the triple interaction $Post \times Log\ round\ number \times U.S.\ startup$, is significant ($p = 0.045$).

3.5 NSMIA and the size of VC and PE funds

In addition to facilitating private startups' access to out-of-state investors, NSMIA has also made it possible for VC and PE funds to raise capital from a larger number of investors without facing the regulatory and disclosure requirements the ICA imposes on mutual funds and other

[30] Section IA.2 in the Internet Appendix describes the evolution of the rules governing raising private equity in Europe.

registered investment companies. To test whether NSMIA has helped VC and PE firms raise larger funds, we follow a diff-in-diff strategy analogous to that in the previous sections. Here, the control group consists of funds focused on investing in early-stage startups. The low capital need of these funds' portfolio companies implies that they were unlikely to be constrained by the pre-NSMIA 100-investor limit. By contrast, funds investing in startups seeking larger, late-stage investments are more likely to have benefited from NSMIA making it easier for private funds to raise large amounts of capital without registering under the ICA.[31]

Column 1 in Table 6 shows that after NSMIA, late-stage-focused VC and PE firms are able to raise larger funds: The coefficient of the interaction term *Post* × % *late-stage investments* implies a 47% post-NSMIA increase in fund size for a fund fully focused on late-stage investments relative to one fully focused on early-stage investments ($p = 0.025$).[32] The relative increase is even larger in column 2, where we exclude funds specialized in investing in IT startups ($p = 0.012$).

The fact that the pre-NSMIA registration requirements under the federal ICA affected all U.S. funds equally means that our fund-level analysis in Table 6 does not lend itself to exploiting cross-state variation as in the previous sections. However, mirroring columns 6 and 7 in Table 4, we can use data on foreign VC and PE funds to test the prediction that if NSMIA is the driver of the increase in the size of late-stage funds, this increase should be unique to U.S. funds. As expected, column 3 in Table 6 shows no increase in the size of foreign funds around the passage of NSMIA ($p = 0.279$). Furthermore, the triple-diff results in column 4 indicate that the difference between the diff-in-diff estimates for U.S. and foreign funds is significant ($p = 0.042$ for the triple interaction *Post* × % *late-stage investments* × *U.S. fund*).

In order to further alleviate the concern that the post-NSMIA growth in the size of late-stage funds might be a consequence of Internet boom-related factors independent of NSMIA, Table 7 presents placebo tests for our fund size analysis analogous to those in Tables 3 and 5. Columns 1 and 3 show that the diff-in-diff interaction *Post* × % *late-stage investments* is insignificant

[31] Coordination problems imply that it is easier for late-stage startups to raise large amounts of capital from a few funds than small amounts from many funds (Nanda and Rhodes-Kropf, 2018).

[32] Given that NSMIA's amendments to the ICA went into effect on January 1, 1997, we set the *Post* indicator equal to one for funds that closed in 1997 or later. As in Table 4, we use a continuous version of the late-stage identifier (% *late-stage investments*) that combines fund type information (when available) with information on the fund's initial investments to capture a fund's investment focus.

when estimated during the 1992–1996 and 1997–2001 placebo periods ($p = 0.873$ and $p = 0.896$, respectively). The same is true for the triple-diff interaction $Post \times \%$ *late-stage investments* $\times$ *U.S. fund* in columns 2 and 4 ($p = 0.409$ and $p = 0.992$, respectively).

The passage of NSMIA thus appears to have allowed VC and PE funds investing in late-stage startups—traditional IPO candidates—to raise larger amounts of capital. This and our other results in this section showing that NSMIA has facilitated late-stage startups' access to out-of-state investors and has also allowed them to raise larger funding rounds point to NSMIA as a positive shock to the supply of private capital. To be sure, these findings do not imply that NSMIA has been the one and only driver of the changes in the U.S. entrepreneurial finance market over the last three decades. We next conduct a broader examination of these changes.

4 The IPO Decline and the Financing of Late-Stage Startups

This section examines how the financing of VC-backed startups has evolved over the last three decades. While the results in this section are descriptive in nature, they leave little doubt that since 1996—the year that NSMIA was passed and public listings peaked in the U.S. (Doidge, Karolyi, and Stulz, 2017)—private investors play an increasing role in financing even the largest startups.

4.1 The evolution of startup exit rates

We begin by analyzing how the exit rate of VC-backed startups has evolved over time. Specifically, for startups that raised their first VC financing round between 1992 and 2009, Figure 3 shows the stacked fractions of firms that (1) go public, (2) are acquired, (3) fail, or (4) remain private during the seven years following that first financing round.

Consistent with Doidge, Karolyi, and Stulz (2013) and Gao, Ritter, and Zhu (2013), the figure shows a sharp decline in IPO exits: The IPO rate declined from 26% for startups first financed in 1994 to 2% for those first financed in 2000, and has hovered around 2% since then. If those late-stage startups that used to go public were now being acquired by public firms, they would still have access to public investors—albeit not as independent firms. This does not appear to be the case: The acquisition exit rate has stood mostly flat at around 25%

throughout our sample period.[33] Instead, Figure 3 shows that the IPO decline has been made up by an increase in the fraction of startups that remain private (and independent) for at least seven years after their first financing round.

Figure IA.2 in the Internet Appendix performs an analogous analysis measuring exits during the 10 years following the first financing round. The figure shows that the fraction of startups that remain private 10 years after their first financing round more than doubled from 11% for startups that raised their first round in 1992 to 26% for those that did so in 2006. A comparison of Figures 3 and IA.2 indicates that, as we expand the exit window, the fraction of firms that remain private naturally shrinks and those of firms that go public, are acquired, or fail grow. In particular, this suggests that part of the IPO decline is driven by startups taking longer to go public—a trend confirmed by Figure 4, which shows that, for firms that go public, the median numbers of years from first VC financing to IPO has increased by 75%, from around four years in the 1990s to seven in recent years.[34]

4.2 Are private markets able to fund the growth of large startups?

Historically, only large and successful startups have gone public (e.g., Chemmanur, He, and Nandy, 2009). We now study the extent to which private markets are able to finance the growth of these traditional IPO candidates that are now staying private longer.

4.2.1 Raising large amounts of capital as a private firm

We begin by studying private startups' ability to raise large amounts of capital. For each startup in our sample, we compute the total net capital (equity and debt) raised from both public and private sources in the seven years following the startup's first financing round. For firms that do not go public during these seven years, our measure of capital includes only capital raised from private investors; for firms that go public, we include both pre-IPO private capital as well as net capital raised at the IPO and any follow-on public offerings.

[33] If we condition the sample on firms that do exit, then, consistent with Figure 2 in Gao, Ritter, and Zhu (2013), we observe a sharp decline in the fraction of IPO exits and a symmetric increase in the fraction of exits via acquisition. Our Figure 3 differs from Figure 2 in Gao et al. in that we do not condition on exits.

[34] Mulcahy (2015) and Hesseldahl (2015) discuss the challenges faced by VC funds and their investors because of the longer time VCs' portfolio companies take to exit and how VCs are adjusting their contractual terms—and, in particular, relying on liquidation preferences—to protect their returns, respectively.

The blue bars in Figure 5 show that, of those startups that received their first financing round by 1996 (the year NSMIA was passed) and raised at least \$150 million by age seven, 83% went public at some point during these seven years.[35] The ability to raise large amounts of public capital appears to have been a key driver of these firms' IPO decision: Untabulated results reveal that 84% of the total net capital raised by the median such firm was from public investors at or after the IPO. These findings are consistent with the notion that most startups that raised large sums of capital in the pre-NSMIA years did so by going public.

By contrast, the figure shows that of those startups whose first financing round was after 1996 and that also went on to raise over \$150 million, only 42% raised at least some of this capital after going public. Importantly, the red line in Figure 5 shows that the number of startups raising over \$150 million during the seven years following their first financing, scaled by the average annual number of startups going public during these seven years, is similar among those startups first financed in the mid-1990s and the mid-2000s—although lower than at the peak of the Internet boom in 1999 and 2000. Figure 5 thus suggests that, in recent years, private markets supply large sums of capital to startups that remain private.

This conclusion is reinforced by Figure 6, which examines the evolution over time of the relationship between the capital raised by a startup in the seven years following its first financing round and the likelihood that the startup goes public during these seven years. To do so, we plot the annual coefficient estimates β_t from the following regression:

$$Y_{7it} = \beta_t \text{ Log } Capital_{7it} + \gamma_t + \eta_s + \theta_j + \varepsilon_{it} \tag{2}$$

where i indexes startups and t indexes the year of their first financing round. Y_7 is an indicator equal to one if the startup goes public during the seven years following its first financing round; $Capital_7$ is the net amount of capital (public and private) raised by the startup during these seven years; and γ, η, and θ denote first-financing year, state, and industry fixed effects, respectively.

Figure 6 shows that for startups first financed by 1996, there was a strong partial correlation

[35] Throughout the paper, we measure age since a startup's first financing round. Fewer than 6.5% of our sample firms raise \$150 million by age seven, thus making \$150 million a natural (if necessarily arbitrary) threshold to identify large amounts of capital. Our conclusions are robust to using other thresholds, or to not using a binary threshold at all (see Figure 6).

between the log capital a startup raised in the seven years following its first financing round and the likelihood that it had gone public during these seven years. Since NSMIA's passage in October 1996, this partial correlation has decreased by 75%. Thus, the growing ability of private investors to supply large sums of capital has greatly weakened the correlation between the capital raised by a startup and its listing status.

4.2.2 Achieving scale as a private firm: employment and sales

In addition to being able to raise large amounts of capital, are private startups able to reach a large scale as measured by real outcomes such as employment or sales? Figures 7 and 8 present analogous versions of Figures 5 and 6 focusing on employment instead of capital raised. Figure 7 shows that the post-1996 decline in IPOs has been accompanied by a marked decline in the fraction of startups with over 200 employees by age seven that are public—but not in the total number of startups that reach this size, which rebounded strongly after the 2001 recession. Similarly, Figure 8 shows that the partial correlation between a startup's log number of employees and the likelihood that the startup is public was cut in four for startups first financed in the 2000s relative to those first financed by 1996. Figures IA.3 and IA.4 in the Internet Appendix present similar results for sales.

In sum, while we cannot rule out the possibility that the decline in IPOs has made it harder for some startups to fund their growth, the evidence in this section suggests that VC-backed private startups can now reach a scale historically all but reserved to their public peers.

4.3 The evolution of the financing of late-stage startups

Consistent with our diff-in-diff results in Table 4, Panel A in Figure 9 shows that the average round size raised by late-stage private startups (those at least four years old) sharply increased after NSMIA's passage in 1996. The average late-stage round was between $5.9 million and $7.3 million from 1992 to 1995. It increased to $7.6 million in 1996 and, most notably, to $10.3 million in 1997 (the first full year after NSMIA's passage). The average round size then oscillated between $10 million and $15 million between 1998 and 2013, with the only exception of 2000 (the last year of the Internet boom), when it spiked to $20.3 million. The average round reached again $20.3 million in 2015, before decreasing to $15.9 million in 2016.

Moving to the aggregate level, Panel B in Figure 9 shows a large increase in the log aggregate capital raised by late-stage private startups beginning around the time of NSMIA's passage. Specifically, late-stage private startups raised \$1.282 billion of capital in 1995; in 2000, they raised \$7.664 billion, a 498% cumulative increase over five years. After declining in 2001 with the end of the Internet boom, private capital going to late-stage startups continued to grow, reaching \$11.412 billion in 2005, \$14.129 billion in 2010, and \$33.020 billion in 2015. Panel A's finding that the size of late-stage rounds remained largely flat from 2001 through 2013 suggests that the aggregate increase in late-stage private capital during these years was largely driven by an increase in the number of late-stage startups raising private capital. Figure IA.5 in the Internet Appendix confirms that this was indeed the case, consistent with our finding in Figure 3 that the decline in IPOs since their 1996 peak has been accompanied by an increase in the number of startups that remain private in their late-stage years.

When applying a structural break test without imposing a known break date to the time series of log aggregate capital raised by late-stage private startups from 1992 to 2016, the estimated break closely follows NSMIA's passage in October 1996: The estimated break is 1997Q2 when using quarterly data (Wald statistic = 496.9, $p < 0.001$), and 1997 when using annual data (Wald statistic = 141.0, $p < 0.001$).[36] These findings support our conclusion in Section 3 that NSMIA has been a positive shock to the supply of late-stage private capital—but they of course do *not* imply that NSMIA has been the only driver of the private capital growth shown in Figure 9.

Traditionally, VC investors have been a key player in the entrepreneurial finance market, particularly in funding the kind of high-growth startups that become IPO candidates (Puri and Zarutskie, 2012). However, Panel B in Figure 9 shows that non-VC investors play an increasingly important role in financing late-stage startups, consistently accounting for over 60% of late-stage private capital in recent years.

Panel C breaks these non-VC investors into four categories: PE funds, corporations making minority investments in startups, mutual funds, and a fourth category that combines hedge funds and investment banks. PE funds, which Section 3.5 shows were also positively affected by

[36] Specifically, we apply a supremum Wald test, which constructs a test statistic for a structural break without imposing a known break date by choosing the maximum of the Wald tests computed at each possible break date.

NSMIA, are the largest non-VC investor in late-stage startups, with their aggregate investments increasing from $235 million in 1995 to $8.367 billion in 2015. The post-1996 IPO decline thus appears to have been accompanied by a gradual diversification of PE's traditional focus on leveraged buyouts toward growth equity investments in late-stage startups. PE funds are followed in order of importance by corporations, with mutual funds and the combined hedge fund and investment bank category playing a more modest but increasing role.[37]

Panels B and C in Figure 9 show that non-traditional startup investors such as PE, mutual, and hedge funds have greatly increased their investments in late-stage startups. Are these investors equally likely to invest in early-stage startups? Table IA.1 in the Internet Appendix shows that this is not the case: When a non-VC investor first invests in a startup, the startup tends to be older than when a traditional VC makes its initial investment (columns 1 and 2). In addition, the table shows that non-VC investors are more likely to invest in out-of-state and distant startups than their VC counterparts (columns 3 through 6).[38] These conclusions are all robust to considering only pre-NSMIA financings.

The results in Table IA.1 thus indicate that the increasing role that non-VC investors play in funding late-stage startups is not part of a broader phenomenon whereupon these investors have now become major investors in startups of all ages. Rather, non-VC investors appear to concentrate their investments in the kind of late-stage startups that have traditionally been IPO candidates. In turn, non-VC investors' willingness to invest in out-of-state startups suggests that they are uniquely positioned to take advantage of the fact that by preempting state blue sky laws, NSMIA has made it easier for startups to raise out-of-state capital.

5 Why Do Startups Stay Private Longer?

The evidence presented so far points to the emergence of a new equilibrium in the U.S. entrepreneurial finance market, where late-stage startups are able to raise large sums of private capital—both from traditional and new startup investors—to fund their growth while remain-

[37] The rise of mutual funds as investors in private startups is the focus of recent studies by Chernenko, Lerner, and Zeng (2017) and Kwon, Lowry, and Qian (2019).

[38] This analysis includes round number fixed effects, and so it does not simply reflect the fact that non-VC investors tend to invest in more mature startups that require less close monitoring.

ing private. This section seeks to shed light on the reasons that are driving many successful startups to stay private longer than in the past.

5.1 The effect of founder control on exit decisions

A key reason why founders prefer to keep their firms private is that it allows them to retain control of their firms (e.g., Boot, Gopalan, and Thakor, 2006; Brau and Fawcett, 2006; Helwege and Packer, 2009). By contrast, investors' preferences are often quite different, particularly in the case of VC investors. VC funds have a fixed lifecycle (typically, 10 years) at the end of which the funds must be liquidated and the proceeds paid back to investors—ideally in cash or liquid securities. In addition, VCs enjoy considerable reputational benefits from taking their portfolio firms public, which helps them attract new investors—and fees—to their next fund (Gompers, 1996). As a result, VC investors tend to favor taking their successful portfolio companies public (or, to a lesser extent, selling them to a strategic or financial acquirer).

If founders and VC investors differ in their exit preferences, this conflict should ultimately be resolved in favor of the party with decision-making control at the time of the exit decision. We test this prediction by examining how the founders' initial equity stake affects their startup's future exit probability. To do so, we estimate the following equation:

$$Y_{7it} = \beta_0 + \beta_1 Founders\text{'}\ initial\ stake_{it} + \beta_2 \text{ Log } Capital_{it} + \beta_3 X_{st} + \gamma_t + \eta_s + \theta_j + \varepsilon_{it} \quad (3)$$

where i indexes startups and t indexes the year of their first financing round. Y_7 is an indicator equal to one if the startup has a successful exit (defined below) during the seven years following its first financing; *Capital* is the total capital raised by the startup up to one year after its first financing round;[39] the vector X controls for the number of startups, number of public pension funds (state and local), and population size in each state-year; and γ, η, and θ denote first-financing year, state, and industry fixed effects, respectively.

Two reasons motivate our use of *Founders' initial* (*equity*) *stake* (measured one year after the startup's first financing round) to capture the founders' decision-making control at the time of the exit decision. First, Table IA.2 in the Internet Appendix shows a strong and

[39] All our conclusions are robust to excluding this control.

persistent positive correlation between the founders' initial stake and the fraction of seats in the startup's board of directors that the founders control; conversely, there is a strong and persistent negative correlation between the founders' initial stake and the fraction of seats controlled by the startup's investors.[40] Second, measuring the founders' equity stake early in the startup's life avoids capturing a mechanical correlation between the startup's financing (and exit) decisions and the equity owned by the founders later in the startup's life.

Of course, the founders' equity stake still remains endogenous even when measured years before the exit decision. We address this endogeneity using an instrumental variable (IV) approach. Our identification strategy builds on the assumption that VCs tend to invest in nearby startups (Lerner, 1995). As a result, founders who raise their first financing round in state-years with higher VC supply benefit from a "money chasing deals" environment (Gompers and Lerner, 2000) that allows them to extract better terms—and, in particular, retain a higher equity stake.

Our IV exploits variation in the supply of venture capital stemming from the following two facts: Public pension funds are an important source of capital for VCs, and they exhibit substantial—and varying—home-state bias when investing in private equity (Hochberg and Rauh, 2013). Specifically, the IV interacts two plausibly exogenous sources of variation in VC supply at the state-year level, both measured the year before a startup's first financing round: (1) variation in the assets of state and local pension funds (Gonzalez-Uribe, 2014; Bernstein, Lerner, Sorensen, and Strömberg, 2016), and (2) variation in the fraction of state officials (appointed and ex officio) in the funds' boards of trustees. This fraction "is strongly correlated with a tendency of pension funds to bias the allocation toward in-state investments" (Andonov, Hochberg, and Rauh, 2018, p. 2044).[41] Consistent with the IV *State pension assets* $\times$ *% state officials in board* satisfying the relevance requirement, column 1 in Table 8 shows that it has a positive and strong partial correlation with *Founders' initial equity stake* when

[40] The analysis in Table IA.2 uses board size data from Form D filings, which we only have available for VC-backed startups that raised capital in or after 2002. We find a similar negative correlation between the founders' initial equity stake and the number (instead of the fraction) of board seats that investors control three years later ($\rho = -0.26$, $p < 0.01$), which we observe for all startups in our sample.

[41] The data source for the assets of state and local pension funds is the Census Bureau's Annual Survey of Public Pensions (ASPP); data on the composition of each fund's board of trustees has been kindly provided by Andonov, Hochberg, and Rauh (2018). In the case of state-years with multiple public pension funds, we compute the asset-weighted average of the board fraction of state officials across the different funds.

estimating the first stage of Equation (3) ($F = 27.9$, $p < 0.001$).

The exclusion restriction requires that the IV impacts a startup's exit decision up to seven years later only through its effect on the founders' initial stake. The fact that the assets of a state's public pension funds reflect the funds' past contributions and investment performance as opposed to their investment opportunities at the time of the local startups' exit decision supports the exclusion restriction. The exclusion restriction is further reinforced by the fact that the board seat allocation of most public pension funds is stable and was initially set decades before private equity became an established asset class and pension funds were allowed to invest in it (Andonov, Hochberg, and Rauh, 2018). We thus expect the board fraction of state officials in a state's public pension funds to be uncorrelated with changes in the exit decisions of local startups.

Two additional research design choices strengthen the exclusion restriction. First, our inclusion of three state-year level controls in the vector X (number of startups, number of public pension funds, and population size) helps control for changes in a state's economic conditions that could be correlated with both the value of local pension funds and with the local startups' future exit opportunities. Second, a potential identification concern is that in state-years with higher pension assets and thus higher VC supply, the quality threshold required for a startup to be able to raise venture capital may be lower than in state-years with lower pension assets. The goal of our exit analysis is to capture the exit decisions of high-quality startups for which a successful exit is a realistic option, and so over whose exit decision investors and founders are likely to disagree. We thus exclude from the sample all startups that, within seven years of their first financing round, either (1) fail or (2) are acquired at a low valuation that is less than or equal to twice the total capital they raised in all pre-exit financings or $25 million.[42]

Columns 2 through 7 in Table 8 estimate different versions of Equation (3) to investigate the relationship between founder control and startup exit decisions. The dependent variable in columns 2 through 4 is an indicator set equal to one if the startup goes public or is acquired (at a non-low valuation) within seven years of its first financing round; in columns 5 through 7, it is an indicator set equal to one if the startup goes public within seven years.

[42] Table IA.3 in the Internet Appendix shows that our findings are robust to also excluding from the sample those startups that either fail or have a low-valuation acquisition more than seven years after their first financing.

Columns 2 and 5 show that the OLS partial correlation between the initial equity stake of a startup's founders and the startup's probability of exit is positive, though only significantly so when focusing on IPO exits ($p = 0.011$). This finding is not surprising: The founders' initial stake is likely positively correlated with unobserved startup quality, and higher-quality startups are more likely to have a successful exit (and in particular an IPO), all else equal. We rely on our IV identification strategy to address this endogeneity.

Columns 3 and 6 show the reduced-form relationship between the instrument and the dependent variables. The negative signs go in the hypothesized direction: More pension fund capital available to startups increases founder bargaining power, which founders use to retain control of their firms by delaying exits ($p = 0.018$ and $p = 0.010$ in columns 3 and 6, respectively).

Columns 4 and 7 in Table 8 show the 2SLS results of estimating Equation (3). In contrast to our OLS estimates, the instrumented founders' equity stake has a negative effect on the likelihood of exiting via IPO or acquisition; the same is true when we focus only on IPO exits. Specifically, the 2SLS estimate in column 4 indicates that a 1 percentage point increase in the founders' initial equity stake leads to a 1.8 percentage point decrease in the probability that the startup goes public or is acquired within seven years ($p = 0.045$); the decrease is somewhat larger, 2.1 percentage points, when focusing only on IPO exits in column 7 ($p = 0.032$). These effects are economically meaningful, given that the unconditional seven-year exit probability in the sample is 30.6% when acquisition exits are included and 10.4% when they are not. Our IV results thus support the notion that investors and successful founders often have conflicting exit preferences that are resolved in favor of the party with decision-making control.

5.1.1 The role of California in the IV exit analysis

An important limitation of our IV exit analysis is that it is sensitive to the exclusion of California startups, which account for 46% of the sample. Specifically, our IV's first stage has no power when estimated within the Table 8 sample excluding California startups (henceforth, the non-CA sample).[43]

[43] When we estimate the same first-stage equation we report in Table 8 within the non-CA sample, the estimated coefficient on the IV is positive (0.080) but insignificant ($p = 0.772$). The second-stage estimates of the effect of founder equity on exits are negative but meaningless given the IV's lack of power. The large number of California startups in our sample is common in VC papers; see, e.g., Gompers, Kovner, Lerner, and

What explains the key role that California plays in our IV's first stage? California is by far the state with the largest public pension fund assets, but California founders do not seem to have larger equity stakes than their non-California counterparts.[44] Rather, the lack of power of our IV's first stage in the non-CA sample appears to be largely due to the fact that there is insufficient within-state variation in the initial equity stakes of non-California founders for Equation (3) to include state fixed effects. Consistent with this, Table IA.4 in the Internet Appendix shows that replacing state fixed effects with Census division fixed effects increases the power of our first stage within the non-CA sample, although the IV's F statistic remains below 10 ($F = 5.26$, $p = 0.026$). The 2SLS estimates in Table IA.4 are in line with those in Table 8, but noisier ($p = 0.065$ and $p = 0.230$ in columns 3 and 7, respectively).[45]

In sum, while the effect of founder control on exit decisions does not appear to be fundamentally different for California and non-California startups, our IV does not allow us to cleanly identify this effect when California startups are excluded from the sample.

5.2 Can an increase in founder control help explain the decline in IPOs?

The cross-sectional evidence in Section 5.1 raises the possibility that the decline in public listings since 1996 may have been driven, at least in part, by a concurrent increase in founder control: As founder control increases and so more founders are in a position to influence their startup's exit decision, we should see fewer firms going public—particularly if private markets are able to support the firms' growth.

Figure 10 suggests that founder bargaining power has indeed increased since the early 1990s. The figure reports the annual average fraction of equity held by startup founders one year after their first financing round. Average founder equity increased from 50% to 55% during the 1990s, and then dropped significantly in the post-Internet boom years (likely due to a more

Scharfstein (2010) and Puri and Zarutskie (2012).

[44] If anything, the opposite appears to be the case: The mean and median initial equity stakes owned by California founders are 57.5% and 58.3%, respectively; for non-California founders, they are 59.0% and 60.3%.

[45] The IV's F statistic increases above the critical value of 10 (Stock and Yogo, 2005) within the non-CA sample if, in addition to replacing state with Census division fixed effects, Equation (3) does not control for the number of startups in the state-year or the state-year population; in both cases, the 2SLS estimates of the effect of founder equity are similar to those in Table IA.4, but less noisy. By contrast, the IV's F statistic decreases somewhat if we do not control for the number of public pension funds.

challenging fundraising environment). However, by 2006, average founder equity had returned to the year 2000 level, and it has continued to increase since then, approaching 70% for firms first financed in 2015. Figure IA.6 in the Internet Appendix shows a similar pattern for founder equity three years after the first financing event.

Figure 11 further reinforces the notion that founders' control over exit decisions has increased over time: It shows that the presence of redemption rights in first-round financings has experienced a sharp decline since the early 2000s, standing at just 15% in 2016. Redemption rights allow investors to force startups to repurchase their shares after a specified period of time, often triggering an exit as startups do not have the necessary cash to buy investors out.[46]

The reasons driving the increase in founder control are likely to be multiple, and a full analysis of these reasons falls beyond the scope of our paper. For one, the increase in the supply of private capital we have documented has likely strengthened founders' bargaining power when negotiating with investors, even in early rounds. In addition, technological changes decreasing startups' capital requirements early in their lifecycle—when uncertainty is highest and thus capital is most expensive (Ewens, Nanda, and Rhodes-Kropf, 2018)—have likely allowed founders to minimize the dilution they face in early rounds.

Regardless of its ultimate driver, the increase in founder control shown in Figures 10 and 11, combined with the finding in Table 8 that founders with the most control are the most likely to stay private, suggests that founders are using the increase in both their control and in the supply of private capital to delay—temporarily or permanently—their startup's exit.

6 Conclusion

At the JOBS Act signing, President Obama (2012) said:

> For business owners who want to take their companies to the next level, this bill will make it easier for you to go public. And that's a big deal because going public is a major step towards expanding and hiring more workers. It's a big deal for investors as well, because public companies operate with greater oversight and greater transparency.

[46] Figures IA.7 and IA.8 in the Internet Appendix show that both the fraction of equity owned by startup founders and the presence of first-round redemption rights have followed similar evolutions for California and non-California startups.

We show that the notion that going public is a major step toward expanding and hiring may not be as big a deal as anticipated—at least not anymore. The deregulation of the private equity markets—and in particular the National Securities Markets Improvement Act (NSMIA) of 1996—has made it possible for both VC-backed startups and the funds investing in them to raise large sums of private capital. This, together with the growing role of non-traditional investors such as PE or mutual funds in the entrepreneurial finance market, allows private startups to reach levels of employment and sales that few private firms used to reach.

We emphasize that our results should not be interpreted as implying that NSMIA has been the one and only driver of the increase in the supply of private capital and the decline in IPOs in the U.S. The IPO decision is a multi-faceted one that is impacted by a number of supply and demand forces in the public and private equity markets. Other factors—such as the increased importance of intangible assets as well as technological changes that decrease early-stage startups' capital requirements or make it easier for firms and investors to find each other outside of centralized exchanges—are sure to have also helped fuel the fall in IPOs.

But our results do suggest that by increasing the supply of private equity capital, NSMIA has played a significant role in changing the going-public versus staying-private trade-off, helping bring about a new equilibrium where fewer startups go public, and those that go public are older. Importantly, this new equilibrium does not appear to be the result of successful startups attempting to but being unable to go public. Rather, we show that the bargaining power of startup founders vis-à-vis investors has increased and founders are using their increased control over exit decisions to stay private longer. How this new equilibrium is affecting the incentives and returns of startup investors remains an open question.

The second "big deal" highlighted by President Obama—public companies operate with greater oversight and greater transparency—undoubtedly remains a big deal. The new equilibrium in the entrepreneurial finance market implies that an increasing number of the largest and most successful firms in the U.S. economy are private and so avoid much of the scrutiny and governance regulation imposed on their public peers (e.g., Gurley, 2015). It also implies that ordinary stock-market investors—particularly those that invest via index funds—do not hold in their portfolios an increasing number of the fastest growing firms (e.g., Partnoy, 2018). We leave the investigation of these implications for future research.

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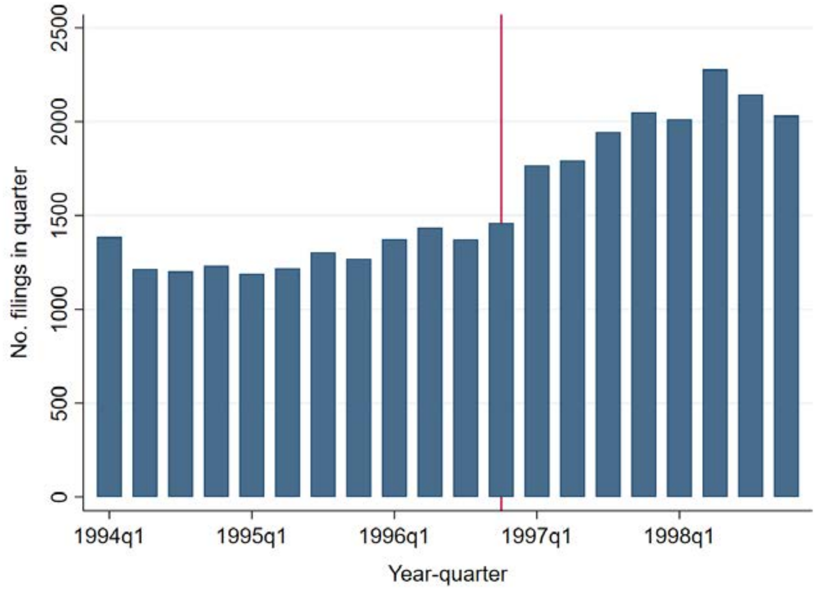
Figure 1: Form D filings around NSMIA.

The figure shows the number of Rule 506 (Panel A) and Rules 504 and 505 (Panel B) Form D filings by private firms for each quarter from 1994 to 1998. The data come from a FOIA request to the SEC. Firms operating in the natural resource industry are excluded. The vertical line represents the last quarter of 1996, the quarter that NSMIA was passed.

(A) Form D filings using 506 exemption



(B) Form D filings using 504 or 505 exemption

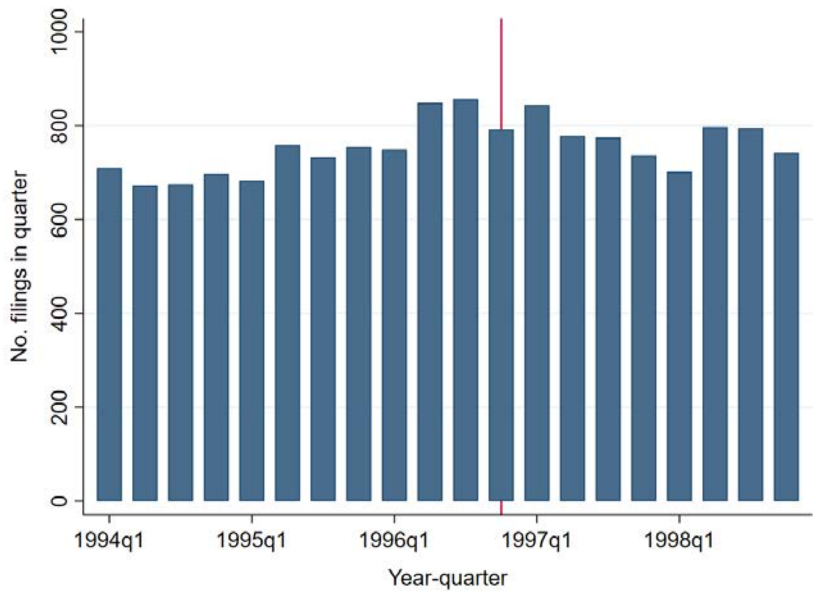


Figure 2: Propensity to raise capital from out-of-state investors around NSMIA – Dynamics.

The figure shows the results of estimating a dynamic version of our diff-in-diff analysis examining NSMIA's effect on startups' propensity to raise capital from out-of-state investors. Specifically, we estimate a dynamic version of Equation (1), where we interact the treatment variable *Late-stage round* with indicators identifying the semester of each financing round. Observations belonging to the semester prior to NSMIA's passage (the first semester of 1996) are excluded. The plot presents the point estimates and 95% confidence intervals of the differences between the coefficients on the interaction terms *Semester* × *Late-stage round* and the pre-NSMIA average of these coefficients, where robust standard errors are clustered at the state level.

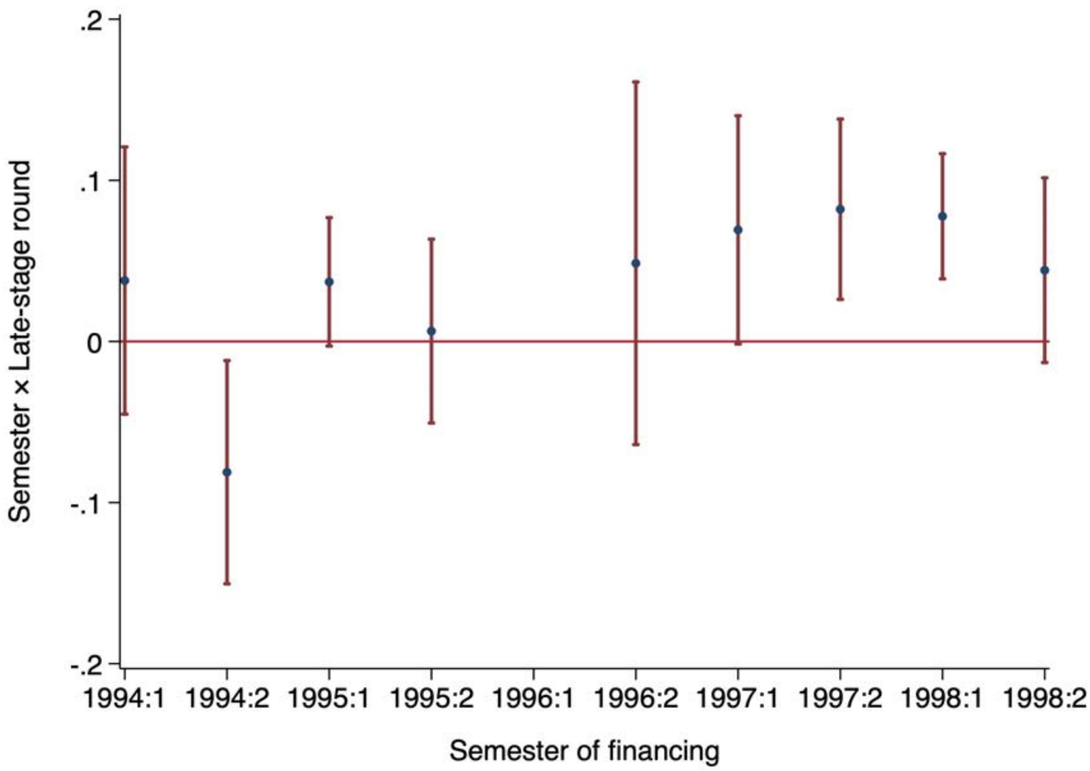


Figure 3: Exit status by year of first VC financing.

For startups that raised their first financing round between 1992 and 2009, the figure shows the (stacked) fractions of startups that (1) go public, (2) are acquired, (3) fail, or (4) remain private during the seven years following that first financing round. For instance, for firms that raised their first financing round in 2000, we measure exits as of 2007. We observe exits through 2016, so ending the sample of first financing rounds in 2009 allows us to observe seven full years of exits for all firms. Section IA.3 in the Internet Appendix describes how we define and identify exits, failures, and firms that are still private.

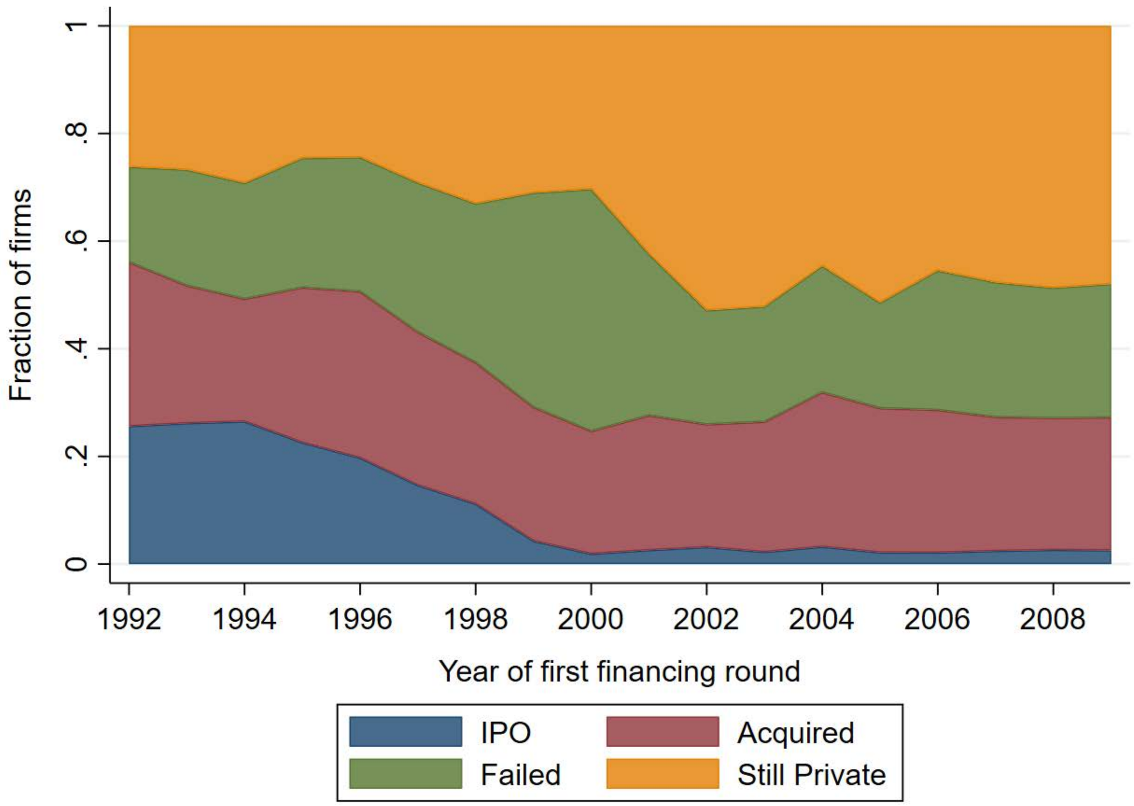


Figure 4: Median age at IPO.

For each IPO year, the figure reports the median age of VC-backed startups at the time of their IPO, where age is defined as the number of years since the startup's first financing round.

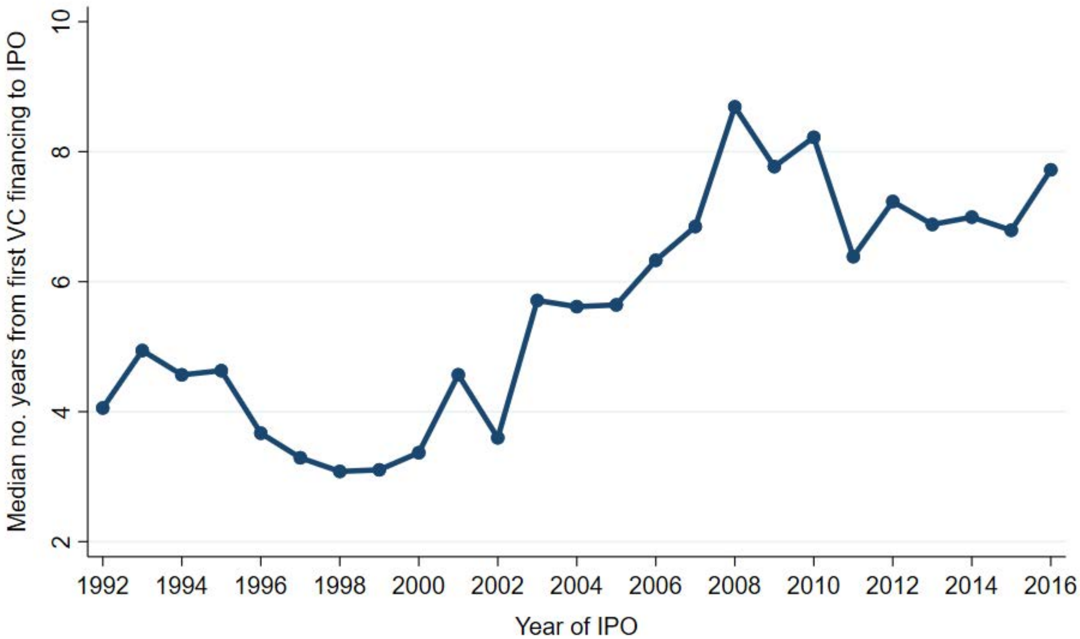


Figure 5: Raising large amounts of capital as a private firm.

For each first financing round year, the figure reports the number of VC-backed startups that raise at least \$150 million (in real 2009 U.S. dollars) during the seven years following their first financing round scaled by the average annual number of VC-backed startups going public during these seven years (red line), as well as the fraction of startups raising at least \$150 million that go public during the same seven years (blue bars). Capital includes both equity and debt. For firms that do not go public during the seven years following their first financing round, our measure of capital includes only capital raised from private investors; for firms that go public, we include both pre-IPO private capital as well as net capital raised at the IPO and any follow-on public offerings within seven years of the first financing round. Both here and in Figure 6, data on capital raised by private firms come from VentureSource; data on capital raised by public firms come from Compustat.

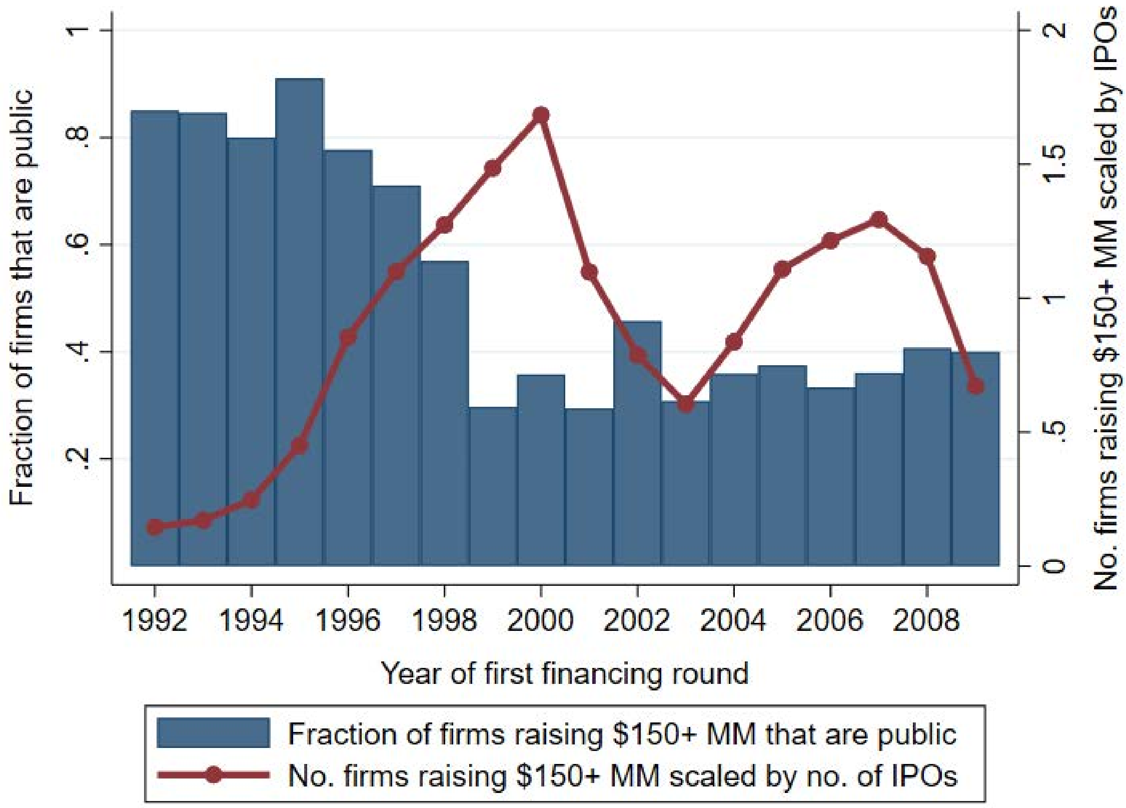


Figure 6: Evolution of the relationship between the capital raised by a startup and the likelihood that the startup is public.

The figure shows the evolution over time of the relationship between the capital raised by a startup in the seven years following its first financing round and the likelihood that the startup goes public during these seven years. Specifically, the figure plots the annual coefficient estimates β_t (alongside their 95% confidence intervals) from the following regression:

$$Y_{7it} = \beta_t \text{ Log } Capital_{7it} + \gamma_t + \eta_s + \theta_j + \varepsilon_{it}$$

where i indexes startups and t indexes the year of their first financing round. Y_7 is an indicator equal to one if the startup goes public during the seven years following its first financing round; $Capital_7$ is the net amount of capital (public and private) raised by the startup during these seven years (in real 2009 U.S. dollars); and γ, η, and θ denote first-financing year, state, and industry fixed effects, respectively. Robust standard errors are used to construct the 95% confidence intervals.

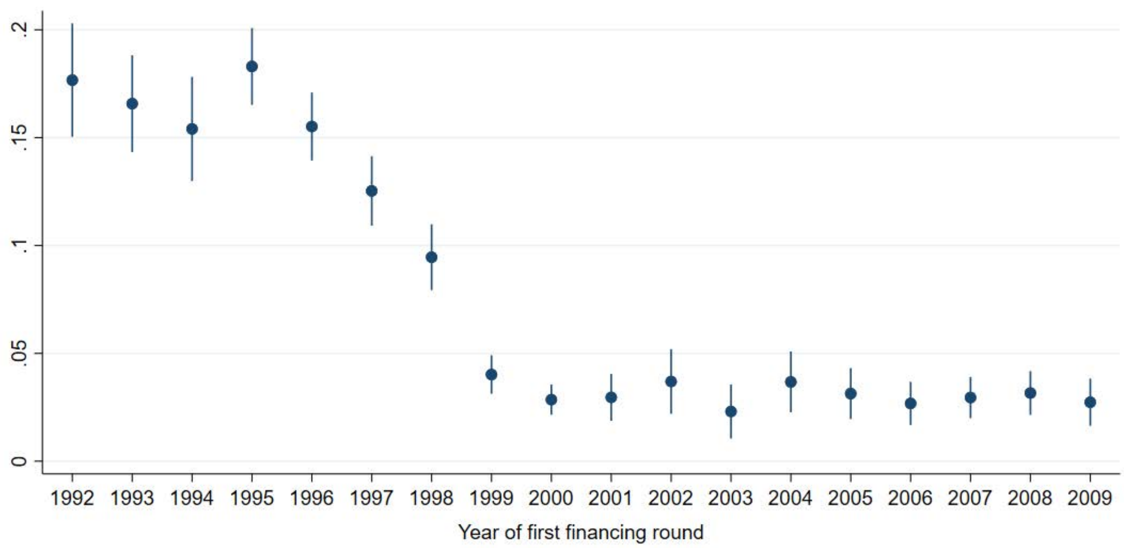


Figure 7: Reaching scale as a private firm – Employment.

For each first financing round year, the figure reports the number of VC-backed startups that have at least 200 employees at some point during the seven years following their first financing round (red line), as well as the fraction of these startups that go public during these seven years (blue bars). Both here and in Figure 8, data on the number of employees of private firms come from VentureSource and the National Establishment Time Series (NETS) database; data on the number of employees of public firms come from Compustat.

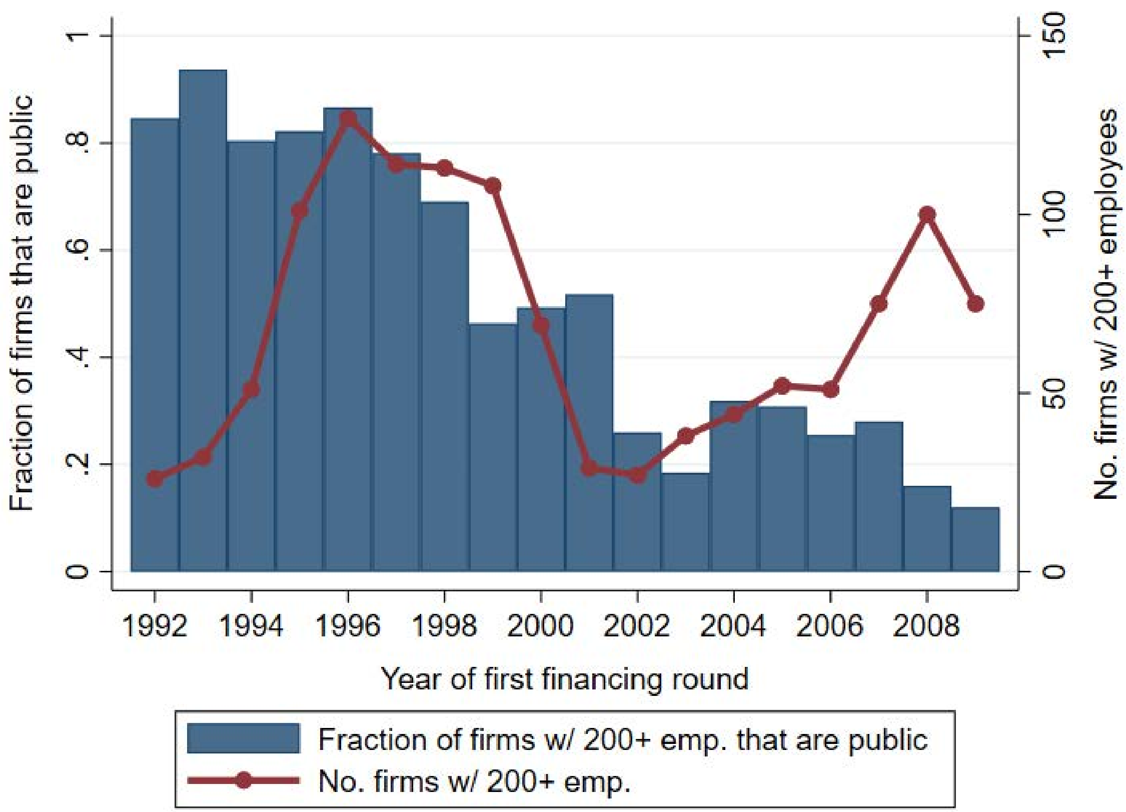


Figure 8: Evolution of the relationship between a startup's employment and the likelihood that the startup is public.

The figure shows the evolution over time of the relationship between a startup's maximum number of employees during the seven years following its first financing round and the likelihood that the startup goes public during these seven years. Specifically, the figure plots the annual coefficient estimates β_t (alongside their 95% confidence intervals) from the following regression:

$$Y_{7it} = \beta_t \text{ Log } Employment_{7it} + \gamma_t + \eta_s + \theta_j + \varepsilon_{it}$$

where i indexes startups and t indexes the year of their first financing round. Y_7 is an indicator equal to one if the startup goes public during the seven years following its first financing round; $Employment_7$ is the startup's maximum number of employees during these seven years; and γ, η, and θ denote first-financing year, state, and industry fixed effects, respectively. Robust standard errors are used to construct the 95% confidence intervals.

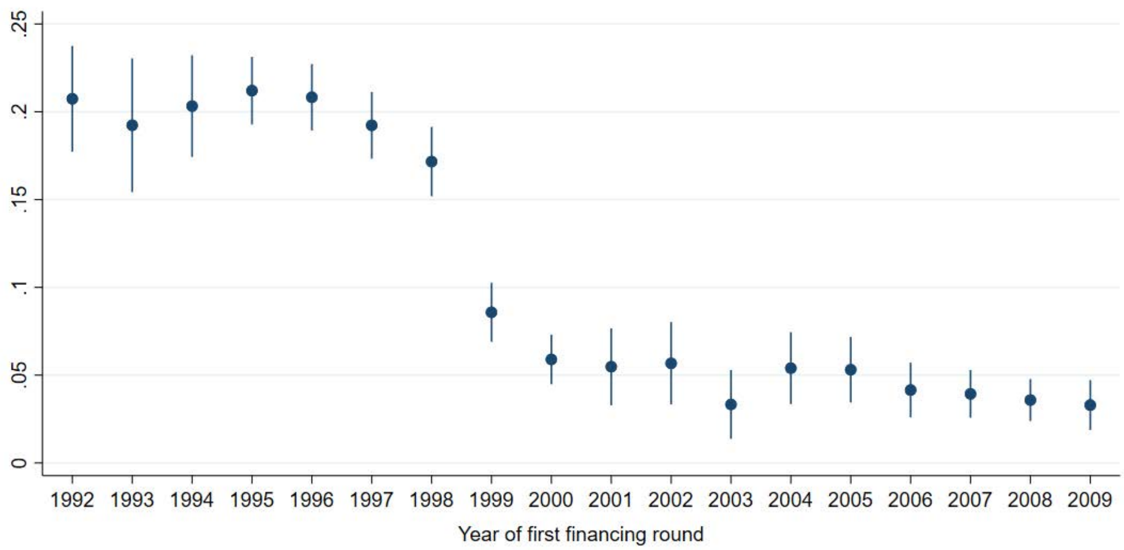
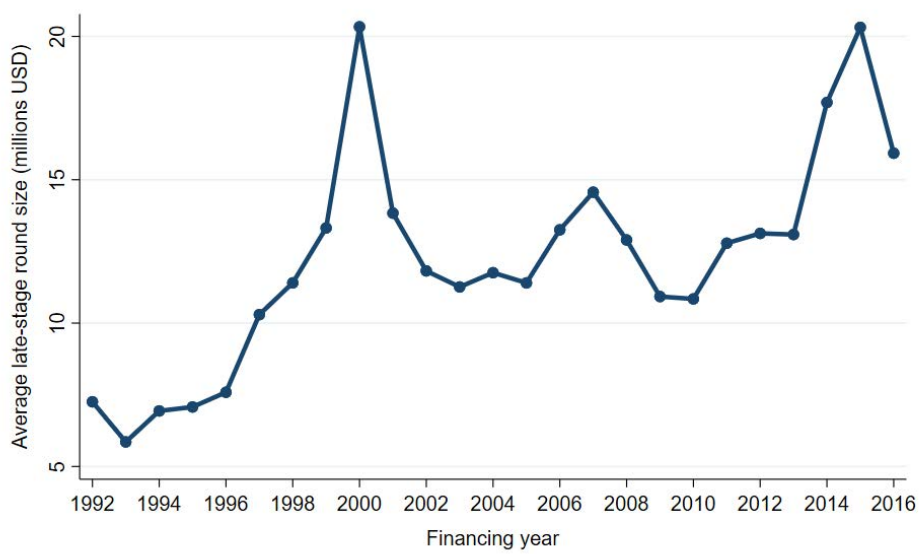


Figure 9: Evolution of the capital raised by late-stage startups – Amount and sources.

For each financing year, Panel A shows the average size (in real 2009 U.S. dollars) of financing rounds raised by VC-backed private startups at least four years old; round sizes are winsorized at the 1% level, and age is defined as years since the first financing round. In Panel B, the red line shows the logarithm of the aggregate amount of capital raised by VC-backed private startups at least four years old; the blue bars report the fraction of this capital provided by investors that are not VCs. Panel C breaks down the capital provided by non-VC investors into four categories: PE funds, corporations making minority investments in startups (either directly or via their venture capital arms), mutual funds, and a fourth category that combines hedge funds and investment banks. Panel C omits investors that VentureSource identifies as "Other," a catch-all category that includes individuals, family offices, and sovereign wealth funds, among others. In the case of financing rounds with multiple investors for which VentureSource does not break their individual investment amounts, we assume that the lead investor provides half of the capital and the rest is split equally among the other syndicate members.

(A) Average late-stage round size



(B) Aggregate capital raised from VC and non-VC investors

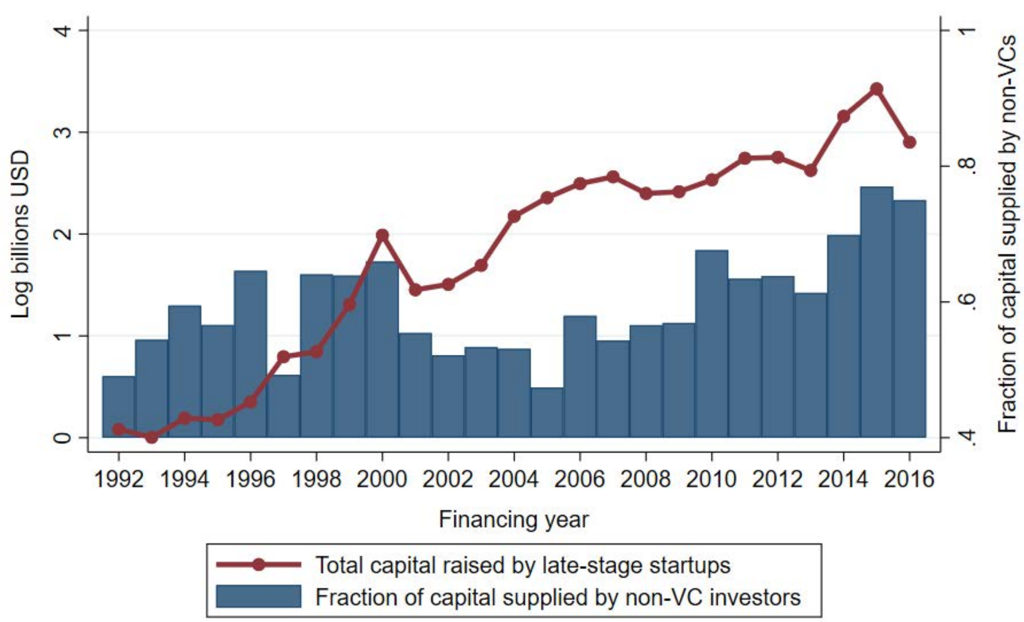


Figure 9: Evolution of the capital raised by late-stage startups – Amount and sources (continued).

(C) Breaking down non-VC investors

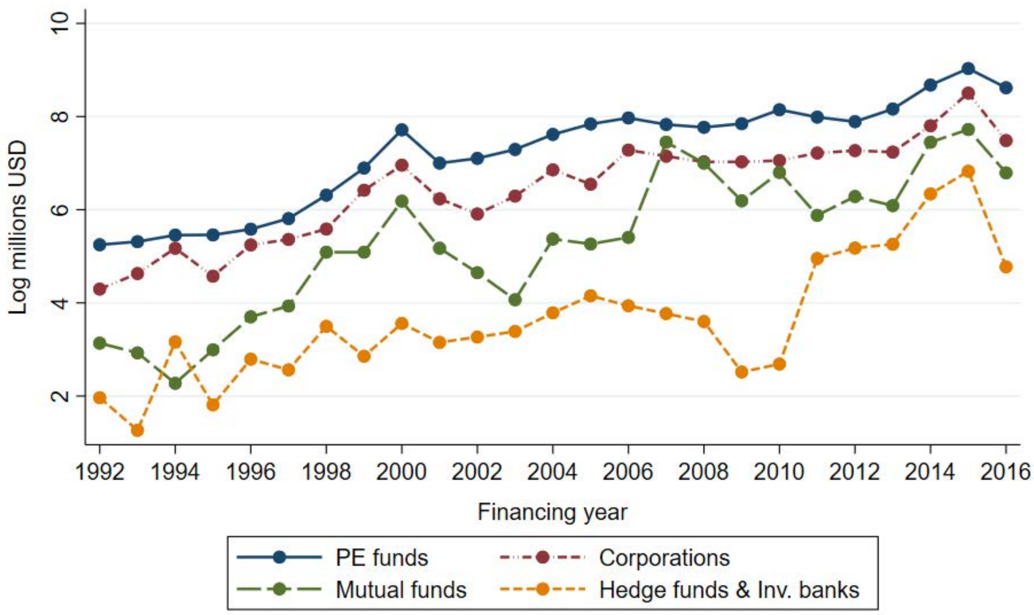


Figure 10: Evolution of startup founders' initial equity stake.

The figure reports the annual average fraction of equity held by startup founders one year after their first financing round (which is why the figure ends in 2015, one year before the end of our sample period). Section IA.4 in the Internet Appendix describes how we compute the founders' initial equity stake.

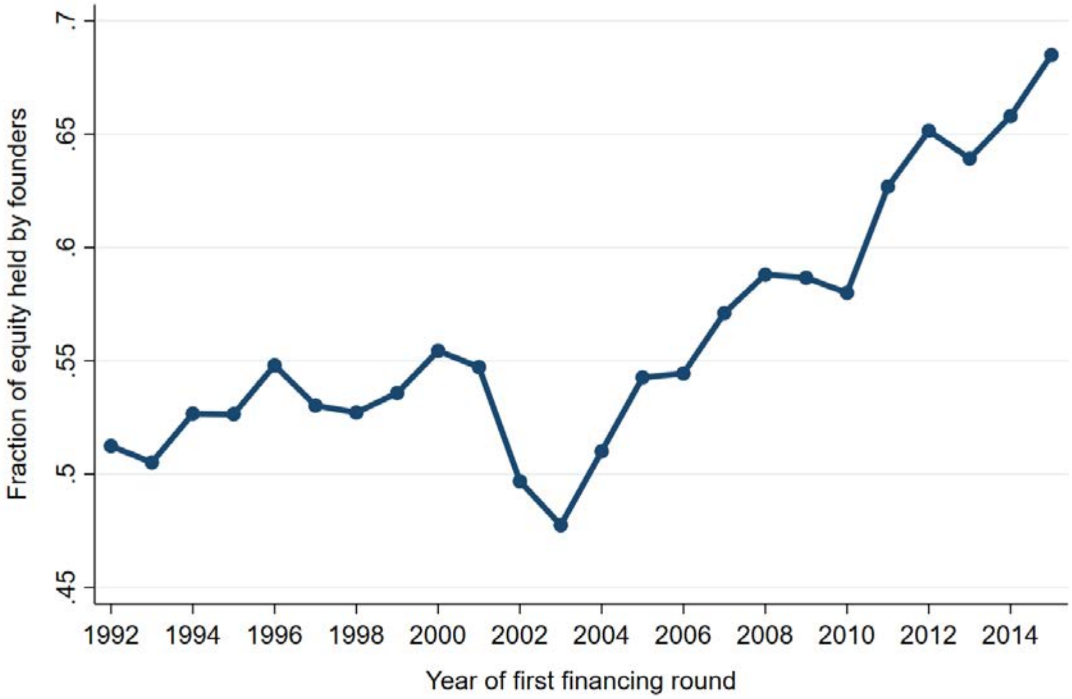


Figure 11: Evolution of the presence of redemption rights in first-round financings.

The figure reports the annual fraction of first-round VC financing contracts that have redemption rights. Redemption rights allow investors to force startups to repurchase their shares after a specified period of time, often triggering an exit as startups do not have the necessary cash to buy investors out. The figure begins in 1995 because this is the first year for which we observe contract features such as redemption rights.

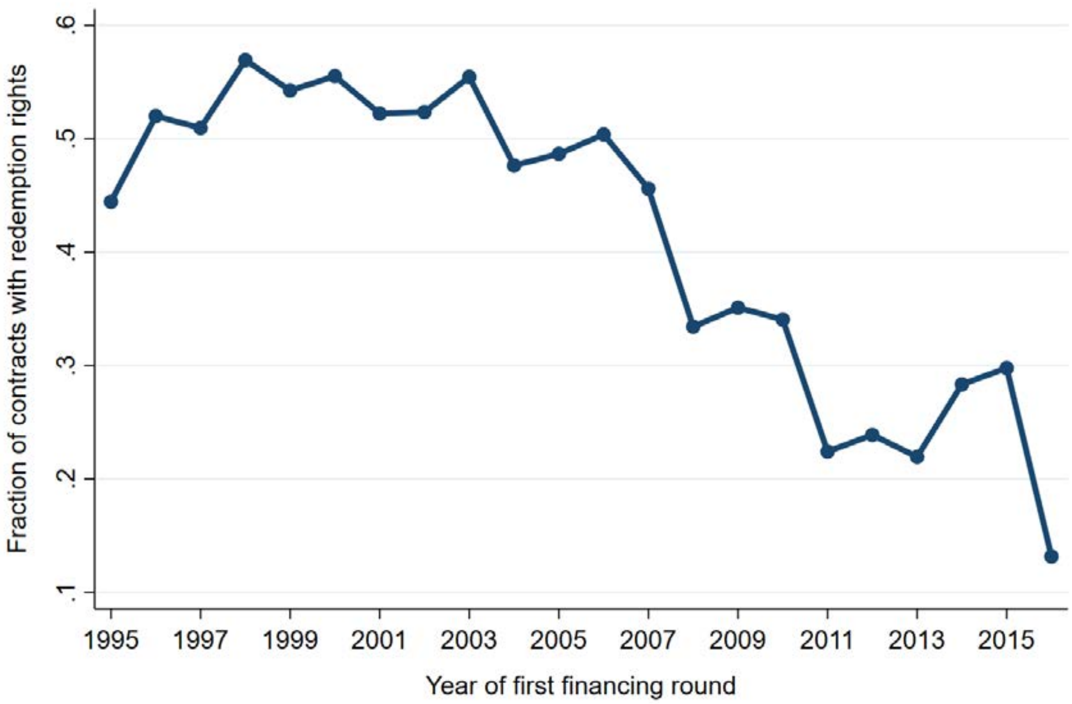


Table 1: Propensity to raise capital from out-of-state investors around NSMIA.

The table presents diff-in-diff analyses examining NSMIA's effect on startups' propensity to raise capital from out-of-state investors. Specifically, we report the results of estimating Equation (1) using a linear probability model, where the unit of observation is an equity financing round and the sample of financings ranges from 1994 to 1998. The dependent variable in columns 1, 2 and 4 is an indicator set equal to one if at least one of the investors in the financing round has offices outside the startup's state; in column 3, the dependent variable is the fraction of investors in the financing round with offices outside the startup's state. Column 4 excludes startups in the information technology (IT) industry. The treatment variable in columns 1, 3, and 4, *Late-stage round*, is and indicator set equal to one if the financing event is a Series C or higher; in column 2, we use a continuous version of the late-stage indicator, the logarithm of the startup's financing round number, capped at five. The *Post* indicator identifies all financings that take place in or after the fourth quarter of 1996. All regressions include financing year-quarter fixed effects (which subsume the non-interacted *Post* indicator) as well as startup state and industry fixed effects. Robust standard errors clustered at the state level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Has out-of-state investors?***		***Fraction of out-of-state investors***	***Has out-of-state investors?***
	All	All	All	Exclude IT
	(1)	(2)	(3)	(4)
Post × Late-stage round	0.041***		0.048***	0.070***
	(0.012)		(0.018)	(0.020)
Post × Log round no.		0.049***		
		(0.011)		
Late-stage round	0.173***		0.080***	0.138***
	(0.028)		(0.010)	(0.022)
Log round no.		0.176***		
		(0.026)		
Constant	0.377***	0.332***	0.415***	0.109
	(0.119)	(0.115)	(0.123)	(0.113)
Observations	8244	8244	8244	3038
R^2	0.109	0.136	0.201	0.125

Table 2: Cross-state variation in the propensity to raise capital from out-of-state investors around NSMIA.

The table investigates cross-state differences in the impact of NSMIA on startups' propensity to raise capital from out-of-state investors. Columns 1 and 2 estimate the same model as column 1 in Table 1 within the subsamples of uniform and non-uniform blue sky states, respectively; uniform blue sky states are those that had voluntarily coordinated their blue sky laws before the passage of NSMIA by adopting the Uniform Limited Offering Exemption (ULOE) or the Small Corporate Offering Registration (SCOR). Column 3 investigates whether the subsample differences in columns 1 and 2 are significant by estimating a triple-diff model. All regressions include financing year-quarter fixed effects (which subsume the non-interacted *Post* indicator), startup state fixed effects (which in column 3 subsume the non-interacted *Non-uniform blue sky state* indicator), and industry fixed effects. Robust standard errors clustered at the state level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Has out-of-state investors?***		
	Uniform blue sky states (1)	Non-uniform blue sky states (2)	Triple-diff (3)
Post × Late-stage round	0.030**	0.114***	0.027**
	(0.011)	(0.028)	(0.012)
Late-stage round	0.186***	0.082*	0.190***
	(0.027)	(0.042)	(0.026)
Post × Late-stage × Non-uniform blue sky			0.099***
			(0.027)
Post × Non-uniform blue sky state			-0.043*
			(0.023)
Late-stage × Non-uniform blue sky state			-0.114**
			(0.046)
Constant	0.363**	0.713***	0.376***
	(0.153)	(0.077)	(0.120)
Observations	7121	1123	8244
R^2	0.111	0.130	0.109

Table 3: Propensity to raise capital from out-of-state investors around NSMIA – Placebo tests.

The table presents placebo versions of our diff-in-diff and triple-diff analyses of NSMIA's impact on startups' propensity to raise capital from out-of-state investors. Specifically, in columns 1 and 2, the placebo tests estimate our diff-in-diff and triple-diff analyses during 1992–1996, the years immediately preceding the passage and implementation of NSMIA; in columns 3 and 4, we estimate analogous placebo tests during 1997–2001, the years immediately following NSMIA's passage. The diff-in-diff and triple-diff placebo analyses in columns 1 and 2 are analogous to their baseline counterparts in Table 1 (column 1) and Table 2 (column 3), respectively, but with the *Post* indicator set equal to one for quarters greater than or equal to 1994Q4; the same is true in columns 3 and 4, but in this case the *Post* indicator is set equal to one for quarters greater than or equal to 1999Q4. When the placebo period is 1992–1996 in columns 1 and 2, the last quarter for which the placebo indicator *Post* is equal to 1 includes the true NSMIA regulatory change; this choice is conservative, as it biases us against estimating no placebo treatment effect. All regressions include financing year-quarter fixed effects (which subsume the non-interacted *Post* indicator), startup state fixed effects (which in columns 2 and 4 subsume the non-interacted *Non-uniform blue sky state* indicator), and industry fixed effects. Robust standard errors clustered at the state level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Has out-of-state investors?***			
	Sample: 1992–1996		Sample: 1997–2001	
	Diff-in-diff (1)	Triple-diff (2)	Diff-in-diff (3)	Triple-diff (4)
Post × Late-stage round	-0.007	-0.004	-0.017	-0.013
	(0.025)	(0.029)	(0.018)	(0.022)
Post × Late-stage × Non-unif. blue sky		-0.009		-0.023
		(0.046)		(0.035)
Late-stage round	0.191***	0.202***	0.242***	0.247***
	(0.037)	(0.037)	(0.033)	(0.036)
Late-stage × Non-uniform blue sky state		-0.094		-0.035
		(0.063)		(0.045)
Post × Non-uniform blue sky state		0.023		-0.004
		(0.053)		(0.037)
Constant	0.660***	0.656***	0.255**	0.254**
	(0.108)	(0.107)	(0.116)	(0.119)
Observations	5516	5516	17871	17871
R^2	0.126	0.127	0.082	0.082

Table 4: Size of startup financing rounds around NSMIA.

The table reports the results of diff-in-diff and triple-diff analyses examining NSMIA's effect on the size of startup financing rounds raised from 1994 to 1998. The dependent variable in all columns is the logarithm of the dollar amount raised by the startup in the financing round (in real 2009 U.S. dollars). We use the continuous version of the late-stage indicator (*Log round no.*) to better account for the fact that our measure of financing round size is also continuous. Otherwise, the analyses in columns 1 through 5 are analogous to their counterparts in Tables 1 and 2. Column 6 estimates the same diff-in-diff model estimated in column 1 using data for startups located in Europe and Canada, while column 7 estimates a triple-diff model combining the data for U.S. and foreign startups. All regressions include financing year-quarter fixed effects (which subsume the non-interacted *Post* indicator) and industry fixed effects. In addition, columns 1 through 5 include state fixed effects (which in column 5 subsume the non-interacted *Non-uniform blue sky state* indicator), column 6 includes country fixed effects, and column 7 includes state-country fixed effects (i.e., state fixed effects for U.S. startups and country fixed effects for foreign ones; these fixed effects subsume the non-interacted *U.S. startup* indicator). Robust standard errors clustered at the state (in columns 1 through 5), country (in column 6), and state-country (in column 7) level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Log capital raised in round***						
	U.S. startups					Non-U.S.	U.S. & Non-U.S.
	Baseline (1)	Exclude IT (2)	Uniform blue sky (3)	Non-uniform blue sky (4)	Triple-diff (5)	Placebo (6)	Triple-diff (7)
Post × Log round no.	0.171***	0.151**	0.159***	0.218***	0.154***	-0.207	-0.346
	(0.032)	(0.062)	(0.037)	(0.059)	(0.039)	(0.245)	(0.246)
Post × Log round no. × Non-uniform blue sky					0.119*		
					(0.063)		
Post × Log round no. × U.S. startup							0.509**
							(0.249)
Log round no.	0.427***	0.433***	0.444***	0.317***	0.450***	1.174***	1.325***
	(0.079)	(0.070)	(0.085)	(0.058)	(0.083)	(0.234)	(0.209)
Post × Non-uniform blue sky state					-0.063		
					(0.078)		
Log round no. × Non-uniform blue sky state					-0.173		
					(0.121)		
Post × U.S. startup							-0.067
							(0.151)
Log round no. × U.S. startup							-0.898***
							(0.226)
Constant	1.169**	0.584	1.734***	-0.761***	1.160**	-1.063***	0.683***
	(0.523)	(0.558)	(0.161)	(0.156)	(0.524)	(0.103)	(0.128)
Observations	7963	2943	6887	1076	7963	818	8783
R^2	0.132	0.139	0.132	0.186	0.133	0.253	0.169

Table 5: Size of startup financing rounds around NSMIA – Placebo tests.

The table presents placebo versions of our diff-in-diff and triple-diff analyses of NSMIA's effect on the size of startup financing rounds. Specifically, in columns 1 and 2, the placebo tests estimate our diff-in-diff and triple-diff analyses during 1992–1996, the years immediately preceding the passage and implementation of NSMIA; in columns 3 and 4, we estimate analogous placebo tests during 1997–2001, the years immediately following NSMIA's passage. The diff-in-diff and triple-diff placebo analyses in columns 1 and 2 are analogous to their baseline counterparts in Table 4, columns 1 and 5, respectively, but with the *Post* indicator set equal to one for quarters greater than or equal to 1994Q4; the same is true in columns 3 and 4, but in this case the *Post* indicator is set equal to one for quarters greater than or equal to 1999Q4. When the placebo period is 1992–1996 in columns 1 and 2, the last quarter for which the placebo indicator *Post* is equal to 1 includes the true NSMIA regulatory change; this choice is conservative, as it biases us against estimating no placebo treatment effect. All regressions include financing year-quarter fixed effects (which subsume the non-interacted *Post* indicator), startup state fixed effects (which in columns 2 and 4 subsume the non-interacted *Non-uniform blue sky state* indicator), and industry fixed effects. Robust standard errors clustered at the state level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Log capital raised in round***			
	Sample: 1992–1996		Sample: 1997–2001	
	Diff-in-diff (1)	Triple-diff (2)	Diff-in-diff (3)	Triple-diff (4)
Post × Log round no.	0.026	0.013	0.198***	0.208***
	(0.053)	(0.050)	(0.058)	(0.069)
Post × Log round no. × Non-unif. blue sky		0.102		-0.082
		(0.220)		(0.121)
Log round no.	0.428***	0.458***	0.723***	0.735***
	(0.090)	(0.086)	(0.086)	(0.093)
Log round no. × Non-uniform blue sky		-0.235		-0.075
		(0.249)		(0.116)
Post × Non-uniform blue sky state		-0.003		-0.100
		(0.109)		(0.113)
Constant	1.599***	1.588***	0.556	0.562
	(0.343)	(0.343)	(0.646)	(0.645)
Observations	5423	5423	17086	17086
R^2	0.118	0.119	0.237	0.237

Table 6: Size of VC and PE funds around NSMIA.

The table reports the results of diff-in-diff and triple-diff analyses examining NSMIA's effect on the size of VC and PE funds that closed between 1994 and 1998. The dependent variable in all columns is the logarithm of the dollar amount committed to the fund (in real 2009 U.S. dollars). As in Table 4, we use a continuous version of the late-stage identifier (% *late-stage investments*) that combines fund type information (when available) with information on the fund's initial investments to capture a fund's investment focus. Given that NSMIA's amendments to the Investment Company Act went into effect on January 1, 1997, we set the *Post* indicator equal to one for funds that closed in 1997 or later. Column 2 excludes funds specialized in investing in information technology (IT) startups. Column 3 estimates the same diff-in-diff model estimated in column 1 using data for funds located in Europe and Canada, while column 4 estimates a triple-diff model combining the data for U.S. and foreign funds. We exclude from the analysis boutique funds with less than $30 million in assets under management. All regressions include fund closing year fixed effects (which subsume the non-interacted *Post* indicator) and industry fixed effects. Robust standard errors clustered at the VC firm level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Log capital committed to fund***			
	U.S. funds		Non-U.S.	U.S. & Non-U.S.
	Baseline (1)	Exclude IT (2)	Placebo (3)	Triple diff (4)
Post × % late-stage investments	0.468**	0.694**	-0.491	-0.481
	(0.208)	(0.273)	(0.452)	(0.413)
Post × % late-stage × U.S. fund				0.946**
				(0.464)
% late-stage investments	-0.015	-0.150	0.894**	0.831**
	(0.154)	(0.202)	(0.441)	(0.385)
% late-stage × U.S. fund				-0.835**
				(0.415)
U.S. fund				0.480***
				(0.178)
Post × U.S. fund				-0.265
				(0.228)
Constant	4.510***	4.513***	4.434***	4.219***
	(0.088)	(0.098)	(0.171)	(0.116)
Observations	518	284	107	625
R^2	0.068	0.089	0.119	0.078

Table 7: Size of VC and PE funds around NSMIA – Placebo tests.

The table presents placebo versions of our diff-in-diff and triple-diff analyses of NSMIA's effect on the size of VC and PE funds. Specifically, in columns 1 and 2, the placebo tests estimate our diff-in-diff and triple-diff analyses during 1992–1996, the years immediately preceding the passage and implementation of NSMIA; in columns 3 and 4, we estimate analogous placebo tests during 1997–2001, the years immediately following NSMIA's passage. The diff-in-diff and triple-diff placebo analyses in columns 1 and 2 are analogous to their baseline counterparts in Table 6, columns 1 and 4, respectively, but with the *Post* indicator set equal to one for fund closing years greater than or equal to 1995; the same is true in columns 3 and 4, but in this case the *Post* indicator is set equal to one for closing years greater than or equal to 2000. We exclude from the analysis boutique funds with less than $30 million in assets under management. All regressions include fund closing year fixed effects (which subsume the non-interacted *Post* indicator) and industry fixed effects. Robust standard errors clustered at the VC firm level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Log capital committed to fund***			
	Sample: 1992–1996		Sample: 1997–2001	
	U.S. funds	U.S. & non-U.S.	U.S. funds	U.S. & non-U.S.
	Diff-in-diff (1)	Triple-diff (2)	Diff-in-diff (3)	Triple-diff (4)
Post × % late-stage investments	0.039	0.592	-0.025	-0.003
	(0.242)	(0.622)	(0.193)	(0.337)
Post × % late-stage × U.S. fund		-0.551		0.001
		(0.666)		(0.390)
% late-stage investments	-0.010	0.474	0.493***	0.256
	(0.171)	(0.364)	(0.135)	(0.233)
% late-stage × U.S. fund		-0.495		0.235
		(0.399)		(0.265)
U.S. fund		0.335***		0.327***
		(0.126)		(0.122)
Post × U.S. fund		0.171		-0.019
		(0.245)		(0.170)
Constant	4.495***	4.067***	4.729***	4.497***
	(0.096)	(0.127)	(0.085)	(0.093)
Observations	307	346	939	1212
R^2	0.042	0.075	0.065	0.085

Table 8: Instrumented effect of founder control on startup exits.

The table examines how the initial equity stake owned by a startup's founders affects the startup's future exit probability by estimating Equation (3) using an instrumental variable (IV) model. The IV, *State pension assets × % state officials in board*, interacts two plausibly exogenous sources of variation in VC supply at the state-year level, both measured the year before a startup's first financing round: (1) variation in the assets of state and local pension funds, and (2) variation in the fraction of state officials in the funds' boards of trustees (Andonov, Hochberg, and Rauh, 2018). Column 1 estimates the first stage of Equation (3), where the dependent variable is the founders' equity stake one year after the startup's first financing round. In columns 2 through 4, the dependent variable is an indicator set equal to one if the startup goes public or is acquired at a non-low valuation (defined below) within seven years of its first financing round; in columns 5 through 7, it is an indicator set equal to one if the startup goes public within seven years. Columns 2 and 5 show the results of estimating Equation (3) by OLS; columns 3 and 6 show the reduced-form relationship between the instrument and the dependent variables; and columns 4 and 7 show the results of estimating Equation (3) by 2SLS. We exclude from the sample all startups that, within seven years of their first financing round, either (1) fail or (2) are acquired at a low valuation that is less than or equal to twice the total capital they raised in all pre-exit financings or $25 million. All variables are defined in Sections IA.3 and IA.4 in the Internet Appendix. All regressions include first-financing year, state, and industry fixed effects. Robust standard errors clustered at the state level are reported in parentheses. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.:	***Founders' initial equity stake***	***IPO or successful acquisition in 7 years***			***IPO in 7 years***		
	First stage	OLS	Reduced form	2SLS	OLS	Reduced form	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Founders' initial equity stake		0.035		-1.831**	0.043**		-2.056**
		(0.037)		(0.914)	(0.016)		(0.961)
State pension assets × % state officials in board	0.365***		-0.669**			-0.751**	
	(0.069)		(0.272)			(0.282)	
Log capital raised	-0.097***	0.063***	0.060***	-0.118	0.059***	0.055***	-0.145
	(0.005)	(0.011)	(0.009)	(0.089)	(0.006)	(0.005)	(0.094)
Log no. startups in state-year	-0.109***	-0.079	0.064	-0.136	-0.198***	-0.037	-0.262**
	(0.032)	(0.067)	(0.119)	(0.127)	(0.057)	(0.105)	(0.124)
Log no. pension funds in state-year	0.008*	-0.013	-0.016	-0.002	0.018	0.015	0.030*
	(0.004)	(0.017)	(0.013)	(0.016)	(0.017)	(0.013)	(0.016)
Log state-year population	0.125	0.094	0.067	0.296	-0.009	-0.040	0.217
	(0.084)	(0.148)	(0.198)	(0.313)	(0.143)	(0.197)	(0.352)
Constant	-0.146	-0.949	-1.862	-1.148	1.978	0.950	1.131
	(1.253)	(2.207)	(2.550)	(3.194)	(2.516)	(2.736)	(3.534)
Observations	7806	7806	7806	7806	7806	7806	7806
R^2	0.333	0.229	0.230	.	0.278	0.279	.
First stage F statistic	27.86						