Journal of Financial Economics 110 (2013) 403–418

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/jfec

Investment cycles and startup innovation ☆

Ramana Nanda [a,*], Matthew Rhodes-Kropf [a,b]

[a] Harvard Business School, Harvard University, United States
[b] NBER, United States

ARTICLE INFO

Article history:
Received 10 May 2012
Received in revised form
19 November 2012
Accepted 10 December 2012
Available online 6 July 2013

JEL classification:
G24
G32
O31

Keywords:
Venture capital
Innovation
Market cycles
Financing risk

ABSTRACT

We find that venture capital-backed startups receiving their initial investment in hot markets are more likely to go bankrupt, but conditional on going public, are valued higher on the day of their initial public offering, have more patents, and have more citations to their patents. Our results suggest that VCs invest in riskier and more innovative startups in hot markets (rather than just worse firms). This is particularly true for the most experienced VCs. Furthermore, our results suggest that increased capital in hot times plays a causal role in shifting investments to more novel startups by lowering the cost of experimentation for early stage investors and allowing them to make riskier, more novel, investments.

1. Introduction

Venture capital (VC) has been a central source of finance for commercializing radical innovations in the US economy over the past several decades (Kortum and Lerner, 2000; Samila and Sorenson, 2011). The emergence of new industries such as semiconductors, biotechnology, and the internet, as well as the introduction of several innovations across a spectrum of sectors in health-care, information technology, and new materials, has been driven in large part by the availability of venture capital for new startups.

Financing radical innovations, however, requires more than just capital. It requires a mindset of experimentation and a willingness to fail. The modal outcome of a venture capital investment is complete failure. Hall and Woodward (2010) report that about 50% of the venture capital-backed startups in their sample had zero-value exits. Sahlman (2010) finds that 85% of returns come from just 10% of investments. In fact, failure is central to the venture capital investment model, as extreme success and greater failure may go hand-in-hand in a world where the outcome of novel technologies or business models is impossible to know ex ante. As one venture capital investor put it: ‘Our willingness to fail gives us the ability and opportunity to succeed where others may fear to tread.[1]

☆ We are grateful to Bo Becker, Shai Bernstein, Ronnie Chatterji, Chuck Eesley, Michael Ewens, Lee Fleming, Paul Gompers, Robin Greenwood, Thomas Hellmann, Bill Kerr, Josh Lerner, Matt Marx, David Mowery, David Robinson, David Scharfstein, Antoinette Schoar, Scott Stern, and Rick Townsend for fruitful discussions and comments and to the seminar participants at MIT, UT Austin, NBER, Tuck School of Business, Carnegie Mellon University, London Business School, Harvard University, Houston University, HEC Paris workshop on Entrepreneurship, UC Berkeley, Northeastern University, University of Lausanne, London School of Economics, World Finance Conference, Queens University Economics of Innovation and Entrepreneurship Conference, University of Notre Dame, Hong Kong University, National University of Singapore and INSEAD. We thank Chris Allen, Laurel McMechan, Oliver Heimes and Sarah Wolverton for research assistance, and the Division of Faculty Research and Development at Harvard Business School and the Kauffman Foundation for financial support. All errors are our own.

* Corresponding author.
E-mail address: rnanda@hbs.edu (R. Nanda).

[1] Quoted by Vinod Khosla, as the reason behind his venture firm's success.

0304-405X/$ - see front matter
http://dx.doi.org/10.1016/j.jfineco.2013.07.001

In this paper, we examine whether there are certain times when venture capital investors are more willing to experiment than others. In particular, we examine whether the peaks in venture capital investment cycles (Gompers and Lerner, 2004; Gompers, Kovner, Lerner, and Scharfstein, 2008) could be times when investors are willing to fund even riskier, more novel companies than at other times, and whether this fundamentally affects the nature of radical innovations that are commercialized in the economy. Conventional wisdom and much of the popular literature tend to associate hot periods in the investment cycle with lower quality firms being financed (Gupta, 2000). Moreover, theories about herding among investors (Scharfstein and Stein, 1990), a fall in investor discipline, or the possibility of lower discount rates in hot markets are all consistent with the notion that projects funded in hot markets might be systematically worse than those funded in less active periods. But increased experimentation would also be associated with increased failure, and what looks like a poor investment ex post may have been very experimental ex ante.

Understanding the links between investment cycles and the commercialization of new technologies is a central issue for both academics and policy makers, given the importance of new technologies in driving the process of creative destruction and productivity growth in the economy (Aghion and Howitt, 1992; Schumpeter, 1942). We shed more light on this issue by examining both the financial outcomes and the innovation outcomes of firms that received early stage venture capital financing between 1985 and 2004. In particular, we aim to study whether there is systematic variation in experimentation across the venture capital investment cycle.

We find that startups receiving their initial funding in more active investment periods were significantly more likely to go bankrupt than those founded in periods when fewer startup firms were funded. However, conditional on being successful, and controlling for the year they exit, startups funded in more active periods were valued higher at IPO or acquisition, filed more patents in the years subsequent to their funding (controlling for capital received), and had more highly cited patents than startups funded in less active investment periods. That is, startups funded in hot markets were more likely to be in the tails of the distribution of outcomes than startups funded in cold markets. They were both more likely to fail completely and more likely to be extremely successful and innovative.

One explanation of these findings is that the most experienced investors take advantage of the better investment opportunities in hot times while, simultaneously, 'fools rush in', so that the mix of investors across the investment cycle leads us to find both more failures and more extreme success at certain times. Another (not mutually exclusive) explanation is that the same investors are investing in more experimental projects in hot markets. When we investigate this view by including investor fixed effects in our estimations, we find evidence for both mechanisms. This highlights that our findings are not being driven only by the ebbs and flows of investors that might only be active in certain times, but also by investors who seem to change their investments across the cycle. We find this is particularly true for the most experienced venture capital investors, who seem to systematically make more experimental investments in hot markets.

Our results, therefore, document a robust association between periods of financial market activity and more experimental investments being made by venture capital investors. That is, rather than a left shift (worse investments) or a right shift (better investments) in the distribution of projects that are funded in such times, they suggest more variance in the outcomes of the investments. They also point to the fact that observing a large number of failures among startups that were funded at a certain time does not necessarily imply that ex ante lower quality firms were funded in those times. Looking at the degree of success of startups is key to distinguishing between one view where worse projects are funded and another in which riskier firms are financed by investors.

We next turn to the question of why investments made in hot markets might be systematically more variable than those made at other times. Our correlation could be observed if investment opportunities are systematically different in hot and cold periods. Or, time varying risk preferences could alter the willingness of investors to experiment. Alternatively, investors could change the type of investments they make in hot markets, independent of the investment opportunities available to them. For example, Nanda and Rhodes-Kropf (2012) argue that hot markets can lower financing risk faced by investors and, hence, make investors more willing to finance experimentation.

To shed light on this question, we use an instrumental variables (IV) estimation strategy. We instrument the venture capital activity in a given quarter with fundraising by leveraged buyout funds that closed in the five to eight quarters before that quarter. Leveraged buyout funds focus their investments on existing companies with a history of revenues and profits, which enables them to raise significant debt to complement their equity investments in portfolio companies. The focus of buyout funds is to generate value for their investors by using a combination of financial engineering and improved operational performance. On the other hand, venture capital funds that make early stage investments in startups focus on pre-revenue firms that are creating and commercializing new technologies. We exploit the fact that the supply of capital into the VC industry is greatly influenced by the asset allocation of limited partners putting money into private equity more broadly and not distinguishing between venture capital and buyout funds. By using buyout fund raising as our instrument, we aim to capture that part of the early stage VC investments that are due to increases in capital unrelated to the investment opportunities available for venture capital funds at the time. Thus, our instrument is useful to the extent that flows into leveraged buyout funds do not systematically forecast changing risk preferences two years later or the variability of early stage innovative discoveries two years later.

Our results are robust to this IV strategy and suggest that, after accounting for the level of investment due to differential opportunities in the cycle, increased capital in the industry seems to change the type of startup that VCs fund, towards firms that are more risky or novel. This finding also holds when we include investor fixed effects, including for the most experienced investors. This is a fascinating result, because it suggests that increased availability of capital in the venture industry seems to alter how venture capitalists invest.

Our paper is related to a growing body of work that considers the role of financial intermediaries in the innovation process (see Kortum and Lerner, 2000; Hellmann, 2002; Lerner, Sorensen, and Stromberg, 2011; Sorensen, 2007; Tian and Wang, forthcoming; Manso, 2011; Hellmann and Puri, 2000; Mollica and Zingales, 2007; Nanda and Rhodes-Kropf, 2013; Samila and Sorenson, 2011; Nanda and Nicholas, 2011). Our results suggest that the experimentation by investors is a key channel through which the financial markets could impact real outcomes. Rather than just reducing frictions in the availability of capital for new ventures, investment cycles can play a much more central role in the diffusion and commercialization of technologies in the economy. Financial market investment cycles can create innovation cycles.

Our findings are also complementary to recent work examining how R&D by publicly traded firms responds to relaxed financing constraints (Brown, Fazzari, and Petersen, 2009; Li, 2011). While this work is focused on the intensive margin of R&D, our work examines how shifts in the supply of capital impacts the choice of firms that investors might choose to fund, thereby having a bearing on the extensive margin of innovation by young firms in the economy.

Our results are also related to a growing body of work examining the relation between the financing environment for firms and startup outcomes. Recent work has noted the fact that many Fortune 500 firms were founded in recessions as a means of showing how cold markets lead to the funding of great companies (Stangler, 2009). Our results are completely consistent with this finding. In fact, we find that firms founded in cold markets are less likely to go bankrupt and more likely to go public. However, we also show that these firms are less likely to be in the tails of the distribution of outcomes. Thus, while many solid but less risky investments are made in less active times, we propose that hot markets seem to facilitate the experimentation that is important for the commercialization and diffusion of radical new technologies. Hot markets allow investors to take on more risky investments, and could therefore be a critical aspect of the process through which new technologies are commercialized. Our results are therefore also relevant for policymakers thinking about regulating the flood of capital during such investment cycles.

The rest of the paper is structured as follows. In Section 2, we develop our hypothesis around the relation between financing environment and startup outcomes. In Section 3, we provide an overview of the data that we use to test the hypothesis. We outline our empirical strategy and discuss our main results in Section 4. Section 5 outlines our robustness checks, and Section 6 concludes.

2. Financing environment and startup outcomes

Popular accounts of investment cycles have highlighted the large number of failures that stem from investments made in hot times and noted that many successful firms are founded in recessions. A natural inference is that boom times lower the discipline of external finance or could be associated with systematically lower discount rates, so that investors make ex ante worse investments during hot times. Others have argued that better startups might be funded in hot markets as these are times when investment opportunities are attractive. The underlying assumption behind these statements is that there is a left or a right shift in the distribution of projects that get funded. Looking at any point in the distribution of outcomes (e.g., the probability of failure or success) is, therefore, sufficient to understand how the change in the financing environment for new firms is associated with the type of firm that is funded.

However, understanding the extent to which a firm is weaker or stronger ex ante is often very difficult for venture capital investors, who invest in new technologies, nonexistent markets, and unproven teams (Hall and Woodward, 2010; Kerr, Nanda, and Rhodes-Kropf, 2013). In fact, venture capitalists' successes seem to stem from taking informed bets on startups and effectively terminating investments when negative information is revealed about these firms (Metrick and Yasuda, 2010). For example, Hall and Woodward (2010) report that about 50% of the venture capital-backed startups in their sample had zero-value exits, and only 13% had an IPO. Similarly, Sahlman (2010) notes that as many as 60% of venture capitalists' investments return less that their cost to the VC (either due to bankruptcy or forced sales) and that about 10% of the investments – typically the IPOs – effectively make the vast majority of returns for the funds. Sahlman (2010) points to the example of Sequoia Capital, that in early 1999 "placed a bet on an early-stage startup called Google, that purported to have a better search algorithm". Sequoia's $12.5 million investment was worth $4 billion when it sold its stake in the firm in 2005, returning 320 times the initial cost.

Google was by no means a sure-shot investment for Sequoia Capital in 1999. The search algorithm space was already dominated by other players such as Yahoo! and Altavista, and Google could just have turned out to be a "me, too" investment. In fact, Bessemer Ventures, another renowned venture capital firm, had the opportunity to invest in Google because a friend of partner David Cowan had rented her garage to Google's founders, Larry Page and Sergey Brin. On being asked to meet with the two founders, Cowan is said to have quipped, "Students? A new search engine? How can I get out of this house without going anywhere near your garage?" (http://www.bvp.com/portfolio/antiportfolio.aspx). In fact, Bessemer Ventures had the opportunity to, but chose not to invest in several other incredible successes, including Intel, Apple, Fedex, eBay, and Paypal.

These examples point to the fact that while VCs might not be able to easily distinguish good and bad investment opportunities ex ante, they could have a better sense of how risky a potential investment might be. An investment that is more risky ex ante is more likely to fail. In this sense, an ex post distribution of risky investments can look a lot like an ex post distribution of worse investments.

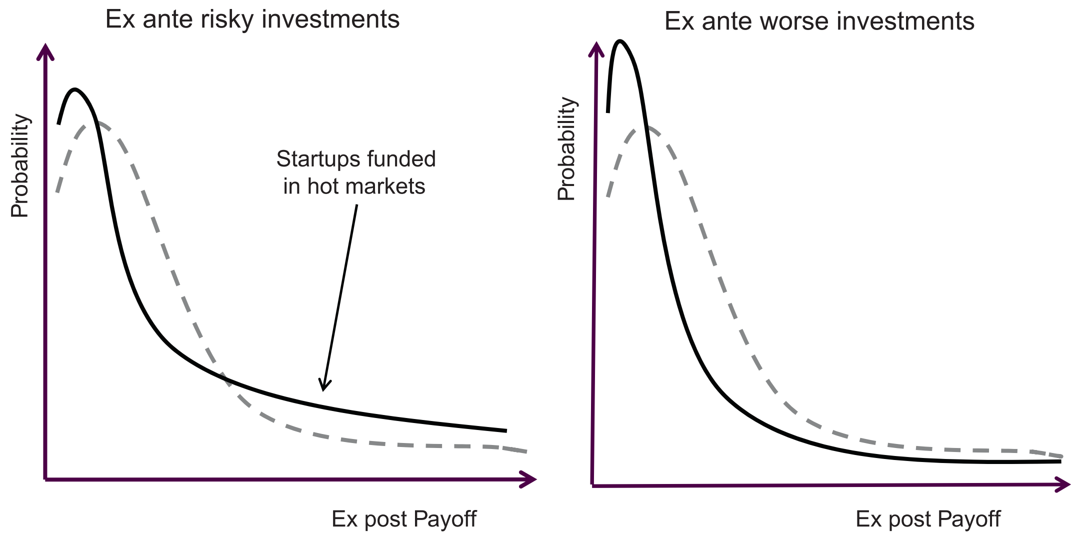

Fig. 1. Distinguishing risky investments from worse investments. The figure depicts the ex post distribution of outcomes for investments that are riskier versus worse ex ante.

However, on average, the successes in risky investments will be bigger than less risky ones, while worse investments will do badly regardless. Fig. 1 highlights how the ex post distribution of risky investments differs from the ex post distribution of worse investments. That is, instead of a shift in the distribution of outcomes to the left (or the right, if investments are consistently better), riskier investments lead to a twist in the distribution of outcomes, with greater failures but bigger successes.

Nanda and Rhodes-Kropf (2012) propose that investors could fund riskier investments in hot markets as these times allow investors to experiment more effectively. If this is the case, then more failures would be expected for firms funded in hot markets. However, conditional on a successful outcome such as an IPO or big acquisition, firms funded in hot markets would be expected to do even better.

The main objective of this paper is, therefore, to examine the extent to which the pattern of VC investments in boom times looks more like the chart on the left, as opposed to the chart on the right in Fig. 1. Our analysis has two main elements. First, we document a robust correlation between firms funded in boom times being more likely to go bankrupt but having bigger successes in the fewer instances when they do have an IPO or get acquired. We also show that the bigger successes are not just limited to a financial measure of valuation, but also extend to real outcomes such as the level of a firm's patenting and the citations to its patents. This suggests that VCs also invest in more innovative firms in boom times.

Second, we investigate the mechanism behind this correlation. VC investments clearly follow investment opportunities, so that investment opportunities associated with new technologies and markets are likely to be riskier and also attract more VC money. However, in addition to this, the flood of money during boom times could allow VCs to experiment more effectively and thereby change the type of investments they choose to make toward more novel, innovative startups. We examine the extent to which this second mechanism of 'money *changing* deals' could also be at play, by using instrumental variables to untangle the endogeneity in the analysis.

3. Data

Our analysis is based on data from Dow Jones Venture Source. This dataset, along with Thompson Venture Economics, forms the basis of most academic papers on venture capital. Kaplan, Sensoy, and Stromberg (2002) compare the two databases and note that Venture Source is less likely to omit deals, a fact that is important when looking at firm bankruptcies. The Venture Source data also provides more accurate information on exits, in particular data on the pre-money valuations of firms at IPO and acquisition, both of which are critical to our analysis of firm outcomes.[2]

We focus our analysis on US-based startups, because data for these firms are comprehensive. The US is also a good setting for our study because the institutionalization of the venture industry in the US implies that startups backed by venture capital firms are likely to comprise the majority of startups that commercialize new technologies. Our sample for the analysis is startups whose first financing event was an early stage (Seed or Series A) investment from 1985 onward. This allows us to focus on the initial investment decision by venture capital investors and to follow the investments to see their eventual outcome. Given that we are interested in following the firms until they exit, we truncate the sample in 2004 to allow ourselves sufficient time for firms that were first financed in 2004 to achieve an exit. We, therefore, focus our analysis on startups receiving their initial early stage investment over the 20-year period from 1985 to 2004

[2] The pre-money valuation is the value of the firm before accounting for the new money coming into the firm at the IPO. Because firms raise different amounts of money in the IPO, the pre-money allows a more clear-cut comparison of value across firms.

Table 1
Descriptive statistics.
This table reports descriptive statistics on US-based startups that received Seed or Series A financing from an investor in the Venture Source database between 1985 and 2004. For these firms, we report data on the first financing event and the ultimate outcome of the startup as of December 2010.

Industry	Number of firms	Share that failed outright (%)	Share with IPO (%)	Median pre-money valuation if had an IPO (in millions of dollars)	Average pre-money valuation if had an IPO (in millions of dollars)	Median pre-money valuation for all exits > $50 million	Average pre-money valuation for all exits > $50 million
Full dataset	12,285	27	11	151	311	165	319
Biotechnology and health-care	2,490	20	19	104	154	135	188
Business and financial services	1,738	36	5	273	380	230	352
Communications and networking	1,315	32	13	320	665	240	590
Computer hardware and electronics	1,232	27	11	149	283	157	274
Computer software and Internet	4,157	26	7	195	372	165	306
Consumer goods and services	1,044	33	11	146	266	156	264
Industrial goods and materials	309	20	13	84	204	186	288

but follow these firms' eventual outcomes until the end of 2010.

There are 12,285 firms that meet our criteria of US-based startups that received their first early stage financing between 1985 and 2004 (see Table 1). The probability that the firm goes bankrupt in our sample is 27%, varying from 20% for biotechnology and health-care startups to 36% for business and financial services.[3]

As noted above, a key way of distinguishing whether worse firms or riskier firms are being funded in hot markets is that their ex post distribution of outcomes is different. That is, although both risky and worse investments lead to a higher probability of failure in the context of our sample, risky investments would imply that, conditional on success, firms funded in active investment markets have a higher economic return than those funded in less active markets. On the other hand, worse investments would imply that, even conditional on success, firms funded in hot markets had lower value than those funded in cold markets. To examine this claim, a key measure we use is the pre-money valuation at IPO for firms that eventually had an IPO. As can be seen from Table 1, the median pre-money valuation for a firm in our sample that had an IPO was $151 million. However, this varied from over $300 million for communications and networking startups to just $84 million for industrial goods and materials startups. Table 1 also documents the skewed distribution of returns for successful outcomes: The average pre-money valuation is double the median. Nevertheless, the pattern across industries when looking at average returns is consistent with the pattern seen with median returns.

We also report the outcome of exits that include information on acquisitions, when available. Data on acquisitions are more likely to be available for larger exits, but this bias does not substantively impact our analysis. Because we are interested in looking at the tails of the distribution, our aim is to capture the high value exits. We are, therefore, less concerned about missing information on acquisitions of firms that may be more likely to be fire-sales. Consistent with this notion, we report the valuation for all exits above $50 million (including IPOs above $50 million) that we have information on in our dataset. The numbers are extremely similar to the valuations obtained when looking at only IPOs.

Part of our aim is to determine whether the differences in outcomes were purely financial or also present in real outcomes. To do so, we also examine firm innovation using patent data. We hand-match firms that had an IPO to data on patent assignees in the US Patent and Trademark Office (USPTO) to look at their innovation prior to when they went public. We look at two different measures of firm innovation: the raw count of patents granted to the firm that were filed in the years following its first funding, and the average number of citations per patent. One challenge with the data on patent filings and citations is that we need to control for the number of years since the patent was granted, so that we do not disproportionately count citations to patents granted in the early years of our sample. Given that we want to look at patents filed after funding and the cumulative citations to those patents, we choose a three year window for each. That is, we look at patents granted to firms that were filed in the three years following the first funding and the three-year cumulative citations to those patents.[4] Matching firms in our sample to the patent database therefore facilitates the study of the innovations by the startups while they were still private.

[3] This number is consistent with Hall and Woodward (2010), who find 22% of their investments are “confirmed zero-value outcomes.” Following Hall and Woodward, we use an alternative measure of failure that also captures firms coded as being private, but are more than five years past their last venture round. Including these firms raises our measure of failure to 55%, in line with Hall and Woodward's estimation of 50%.

[4] While the three year windows are somewhat arbitrary, they are chosen so as to minimize the number of years that would be dropped from the analysis (given about a two-to-three year delay in the granting of patents from the time they are filed).

Table 2
Characteristics of startups funded in active versus less active investment periods.
This table reports differences in the characteristics of firms that receive their first funding in active versus less active periods. Active periods are defined as being above median of quarters in terms of startups being funded in each of the periods 1985–1990, 1991–1997 and 1998–2004.

Variable	All	Funded in active periods	Funded in less active periods
All firms in the sample			
Number of quarters	80	40	40
Number of firms funded per quarter	154	213	95
Age of startup at first funding (years)	2.2	2.1	2.4
Dollars invested in first funding (millions of dollars)	$5.5	$5.9	$4.3
Number of investors in first funding syndicate	2.21	2.20	2.21
Share of startups that failed (%)	27	32	18
Share that had an IPO (%)	11	10	13
Firms that had an IPO			
Firm age at IPO	6.4	6.2	6.7
Total dollars raised prior to IPO	$44	$51	$32
Average pre-money value at IPO	$311	$376	$200
Average pre-money valuation for all exits > 50 million	$319	$353	$239
Number of patents in three years following first funding	6.1	6.5	5.4
Three year cumulative citations per patent	7.3	8.4	5.5

In Table 2, we provide descriptive statistics that show the main patterns in the data. The descriptive statistics highlight the basic pattern we test in Section 4. We find that startups funded in more active investment quarters were slightly younger and significantly more likely to fail, despite raising more money in their first round of funding. Successful firms funded in hot markets raised more money prior to their IPO and, interestingly, took almost the same time from first funding to the IPO. Conditional on having a successful exit, firms funded in active investment markets were valued more on the day of the IPO or when acquired, had more patents and more citations to their patents, suggesting that riskier, more novel startups are funded in the more active investment quarters.

4. Regression results

We next turn to a multi-variate analysis to better-understand whether there are systematic differences in the types of startups funded by VCs across the investment cycle.

4.1. Riskier investments or worse investments?

In Tables 3 and 4, we report results from firm-level regressions that examine the relation between the financing environment in the quarter a firm received its first financing, and the ultimate outcome for that firm. The estimations take the form:

$$Y_i = \alpha_1 OTHFIN_t + \alpha_2 X_i + \phi_j + \tau_T + \varepsilon_i \tag{1}$$

In these regressions, each observation corresponds to an individual entrepreneurial firm and the dependent variable, Y_i refers to the eventual outcome for firm i. It takes the value one if the firm went bankrupt and zero otherwise. ϕ_j, refers to industry-level fixed effects, corresponding to the seven industries outlined in Table 1. τ_T refers to period fixed effects. Because our hypothesis is about the cyclicality of investment over time, we cannot absorb all the inter-temporal variation in our data by including quarter-level or annual fixed effects for the period in which the startup was funded. However, given that our sample spans 20 years, we also want to ensure that we do include some period controls to account for systematic changes in the venture capital industry as it matured. We therefore segment the data into three periods: 1985–1990, 1991–1997, and 1998–2004. Period fixed effects refer to dummy variables for these three periods.[5]

The variable $OTHFIN_t$ is our main variable of interest and refers to the log of the number of other firms in the sample that received their initial early stage financing in the same quarter as firm i. It captures the level of financing activity in the quarter that the focal firm was first funded. The matrix X_i refers to firm-level covariates that we include in the regressions. These include the amount of money the startup raised in the financing event, the startup's age at the time of first financing, and the number of investors in the syndicate that made the investment. California and Massachusetts account for over 50% of all startups in the data, and industry observers note that investors in these regions could have different investment styles. We therefore also include dummy variables to control for whether the startup was based in California or Massachusetts. All standard errors are clustered by quarter to account for the fact that our main outcome of interest is measured at the quarterly-level.

Table 3 reports estimates from OLS regressions where the dependent variable takes a value of one if the firm went bankrupt.[6] As can be seen from the table, firms that

[5] Another approach to control for the time series variation is to include a linear time trend as a control. However, given that the venture capital is associated with bursts of activity instead of a steady trend, we prefer the non-parametric approach of controlling for distinct periods of activity in venture capital.

[6] We report the results from OLS regressions to facilitate comparisons with the IV regressions in following tables. The results are robust to running the regressions as probit models.

Table 3
Funding environment and probability of failure.

This table reports results from OLS regressions looking at the probability of firm failure. The dependent variable takes a value of one if the startup failed and zero otherwise. The results are robust to using probit regressions, but coefficients from OLS specifications are reported to facilitate comparisons with instrumental variable regressions in subsequent tables. Industry fixed effects control for the seven industries outlined in Table 1. Period fixed effects control for the startup being funded in the period 1985–1990, 1991–1997 or 1998–2004. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	1985–2004				Drop if funding year is 1998–2000
Variable	(1)	(2)	(3)	(4)	(5)
Log number of firms financed in the same quarter	0.094***	0.102***	0.097***	0.137***	0.057***
	(0.008)	(0.007)	(0.007)	(0.010)	(0.020)
Log of funds raised by firm in its first financing		−0.028***	−0.032***	−0.026***	−0.039***
		(0.008)	(0.008)	(0.007)	(0.007)
Firm age at first financing		−0.003**	−0.003**	−0.003***	−0.003**
		(0.001)	(0.001)	(0.001)	(0.001)
Number of investors in syndicate		0.009***	0.009***	0.007**	0.005
		(0.003)	(0.003)	(0.003)	(0.004)
Startup based in California		0.020**	0.019**	0.019**	0.005
		(0.008)	(0.008)	(0.008)	(0.009)
Startup based in Massachusetts		−0.034**	−0.028*	−0.029*	−0.021
		(0.016)	(0.016)	(0.016)	(0.015)
Period fixed effects	No	No	No	Yes	Yes
Industry fixed effects	No	No	Yes	Yes	Yes
Number of observations	12,285	11,497	11,497	11,497	6,518
R-squared	0.07	0.08	0.09	0.13	0.08

Table 4
Funding environment and pre-money valuation at IPO.

This table reports the results from OLS regressions looking at the pre-money value of firms that had an IPO. The dependent variable is the log of the firm's pre-money valuation on the day of its IPO. Industry fixed effects control for the seven industries outlined in Table 1. IPO-year fixed effects control for the year in which the startup had its IPO. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	1985–2004				Drop if funding year is 1998–2000
Variable	(1)	(2)	(3)	(4)	(5)
Log number of firms financed in the same quarter	0.792***	0.413***	0.244***	0.214***	0.225***
	(0.082)	(0.065)	(0.045)	(0.051)	(0.067)
Log firm's revenue at IPO		0.161***	0.157***	0.129***	0.125***
		(0.014)	(0.013)	(0.014)	(0.016)
Firm's age at IPO		−0.025***	−0.016***	−0.015***	−0.016***
		(0.007)	(0.006)	(0.005)	(0.006)
Log total funds raised prior to IPO		0.454***	0.382***	0.390***	0.405***
		(0.029)	(0.028)	(0.027)	(0.031)
Startup based in California		0.179***	0.157***	0.115**	0.110**
		(0.050)	(0.044)	(0.045)	(0.049)
Startup based in Massachusetts		0.078	0.121*	0.085	0.055
		(0.075)	(0.064)	(0.066)	(0.062)
Log value of NASDAQ on day of IPO			0.857**	0.888**	0.586
			(0.381)	(0.389)	(0.399)
IPO year fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	No	No	Yes	Yes
Number of observations	1,216	1,197	1,197	1,197	977
R-squared	0.27	0.51	0.63	0.65	0.65

were first financed in quarters with more financing activity are more likely to fail. Columns 2–5 show that firm age at the time of first funding and raising more money at the time of first funding are associated with a lower likelihood of failure. However, even when controlling for these and other covariates, including industry fixed effects and period fixed effects, we continue to find that firms funded in more active quarters are more likely to fail. In Column 5 we drop the quarters associated with the extreme spike in activity during the Internet bubble to ensure that the results were not being driven by these outliers.

The variable $OTHFIN_t$ is measured in logs and failure is measured as a binary variable, so the magnitude of the coefficient in Column 4 (with industry and period fixed effects and all controls) implies that a 10% increase in the number of early stage investments in a given quarter is associated with a 137 basis point increase in the probability of failure. Given the baseline failure probability is 27%, this implies that a 10% increase in the number of firms being funded is associated with the 5% increase in the probability of failure. Because the variation across quarters in the number of firms funded is much larger than 10%, the coefficient on Column 4 of Table 3 implies that the magnitude is economically significant. To put it in perspective, a startup funded in the 75th percentile in the number of firms funded per quarter has a 75% higher chance of failing relative to one funded in a quarter representing the 25th percentile in the number of investments (an increase from 20% chance of failure to a 35% chance of failure). Table 3 therefore highlights the fact that firms are consistently more likely to fail when they are funded in active investment markets. These results do not necessarily imply that VCs fund lower quality firms in hot markets. To make this inference, we also need to examine the degree of success for the firms that do well.

In Table 4, we report estimates from firm-level regressions where the dependent variable is the log of the pre-money value for the firm, conditional on it eventually going public. That is, for the firms in our sample that did eventually go public, we run regressions that take the form

$$\log(PREVAL)_i = \beta_1 OTHFIN_t + \beta_2 X_i + \phi_j + \tau_T + \varepsilon_i \tag{2}$$

As with Table 3, each observation in these regressions corresponds to an individual firm and the dependent variable, $\log(PREVAL)_i$ refers to the pre-money value for the firm on the day it went public. Again, our main variable of interest is $OTHFIN_t$, which measures the log of number of firms in our original sample that were first financed in the same quarter as firm i. The matrix X_i refers to firm-level covariates that we include in the regression. These include the startup firm's age and revenue at the time of the IPO, the total amount of money it raised prior to the IPO, and dummy variables to control for whether the startup was based in California or Massachusetts. As before, standard errors are clustered at the quarter-level.

Columns 1 and 2 of Table 4 report the correlation without any industry or period fixed effects. They show that firms financed in more active quarters are associated with higher valuations on the day of the IPO. Column 2 controls for a numbers of covariates that one might worry would lead to a spurious correlation. For example, if firms funded at different points in the cycle systematically differ in the age or the revenue they have at the time at which they exit, this could lead to a spurious correlation. Relatedly, firms funded in active investment markets raise more money prior to exiting, and this could mechanically lead to the association we find. The results are robust to these controls. Not surprisingly, we find a strong positive association between the firm's revenue at IPO and its valuation. We also find a positive association between the amount of money raised by the firm prior to the IPO and its valuation. The coefficient on log of total dollars raised prior to IPO in Column 4 implies that a 10% increase in the amount of money raised (that is ~4 million) is associated with a 4% increase in the value at IPO (that is, ~12 million). This implies that the marginal dollar invested by VCs will return a threefold return for firms that are successful.[7]

An important concern with our results thus far is that firms funded in hot markets could go public in very different environments than those funded in less active periods. We want to ensure that our results are not simply due to the fact that firms funded in more active times go public at different times and, hence, face a systematically different threshold of going public. To address this concern, we control for the value of the NASDAQ on the day of the IPO in Column 3. In addition, we also include IPO-year fixed effects in our regressions in Columns 3–5. Including IPO year fixed effects implies that our estimations are effectively comparing firms that had an IPO in the same year but that received funding when the market was more or less active. We add industry fixed effects in Column 4 and in Column 5 we again drop quarters with the extreme spike in activity to check that our results are robust to their exclusion.

As can be seen from Table 4, conditional on going public and controlling for the year in which they IPO, firms funded in quarters with greater levels of funding activity have a higher valuation on the day of their IPO. The coefficient on Column 4 (with industry and IPO year fixed effects and all controls) implies that a 10% increase in the funding activity in a given quarter is associated with a 2.1%, or $6.5 million increase in the value of a firm if it goes public. Going from the 25th to the 75th percentile in the number of firms funded is associated with a $54 million increase in the value at IPO. Our results suggest that VCs fund riskier firms in quarters with more financing activity.

4.2. Investor fixed effects

Our results so far have shown that startups funded in hot markets were more likely to be in the tails of the distribution of outcomes than startups funded in cold markets. While this is consistent with more risky firms being funded in hot times, that is not necessarily the case. If some investors take advantage of the better investment opportunities in hot times while others simultaneously fund worse firms, this could explain our result entirely. That is, our results could be due to differences in the types of VCs investing in hot versus cold times, as opposed to the same VCs changing their investments across the cycle.

To probe the results further, we run the same regressions as outlined in Tables 3 and 4, but at the investor-firm level. That is, we change the unit of analysis from the startup level to the investor-startup level. We, therefore, have multiple observations for firms with more than one investor in the first round of financing. In these instances, each observation corresponds to the specific investor-firm

[7] While seemingly large, this needs to be weighted against the extremely large number of complete write-offs faced by VCs.

pair in that round of funding, so that Y_i becomes Y_{ik} and $\log(PREVAL)_i$ becomes $\log(PREVAL)_{ik}$. Expanding the data to the investor-startup level allows us to include investor fixed effects in a panel in which every investor in each deal is included, and thereby allows us to examine whether the same investors change the types of firms they fund in hot and cold markets.[8] Specifically, Table 5 reports results from estimations that take the form

$$Y_{ik} = \gamma_1 OTHFIN_t + \gamma_2 X_i + \phi_j + \psi_k + \tau_T + \varepsilon_{ik} \tag{3}$$

and

$$\log(PREVAL)_{ik} = \delta_1 OTHFIN_t + \delta_2 X_i + \phi_j + \psi_k + \tau_T + \varepsilon_{ik} \tag{4}$$

where ψ_k refers to investor fixed effects and all the other variables are exactly as defined in Tables 3 and 4.

Table 5 reports these estimates for all VC investors in the sample. Column 1 of Table 5 is comparable to Column 4 of Table 3, except that the regressions in Table 5 are run at the investor-startup level and also include investor fixed effects. The fact that the coefficients are extremely similar implies that the increased failure rates in hot times seem to be driven by within-VC variation in the types of firms that are funded, as opposed to across-VC variation in hot versus less active times. Column 4 of Table 5 is comparable to Column 4 of Table 4, except that the regressions in Table 5 are run at the investor-startup level and also include investor fixed effects. Comparing the tables highlights that including investor fixed effects reduces the coefficient somewhat. That is, part of the effect shown on the coefficient in Column 4 of Table 4 seems to be driven by different VCs investing across the cycle. However, the within-VC effect still remains economically and statistically significant, showing that on average, the same investors also change the types of investments they make in hot markets.

We next examine whether there are any differences between more and less experienced investors that are driving the pattern observed above. In Columns 2 and 5 we look at experienced VCs, by including only those VCs that made at least five investments in the two years prior to the focal investment.[9] In Columns 3 and 6 we look at the performance of less experienced investors. Columns 2 and 3 of Table 5 therefore compare failure rates for more versus less experienced investors. Startups funded by less experienced investors have a marginally higher chance of failing, but this difference is not statistically significant. The difference is more stark when comparing Columns 5 and 6 of Table 5. They show that less experienced investors have successes that are not as large and that the relation found in Column 4 seems to be driven by the more experienced investors. In fact, we cannot reject the hypothesis that the successful outcomes for less experienced investors are no different based on whether they were funded in hot or cold markets.

These findings are important as they highlight elements of both the mechanisms we outlined above. The observed relation between active investment markets and more experimental firms seems to come from the most experienced VCs changing the type of investments they make across the cycle. Less experienced investors show a similar pattern, but their returns from the successes seem much lower, suggesting that the benefits they accrue from the more risky investments do not outweigh the costs. While we do not have the data to accurately calculate this, our results suggest that only the more experienced VCs are able to make money from their more novel investments in hot markets.

4.3. Money changing deals?

Thus far, we have shown a pattern of more risky investments being undertaken by investors in hot markets, in particular the most experienced venture capital investors. One explanation for our results is that venture capital investments are particularly high at times when risky technologies, ideas and startups are available to be financed. That is, the same new technologies that attract investment from venture capitalists could also be riskier opportunities. In this explanation, the change in the projects that VCs invest in is driven by the investment opportunities. If this was the main factor driving our results, our OLS results would be biased upward, as the omitted variable would be responsible for driving the variance in outcomes and attracting venture capital investment.

In addition to this explanation, however, it is possible that hot markets may make investors more willing to experiment, and hence change the type of investments they are willing to make, toward more risky, innovative startups in the market (Fleming, Schwienbacher, and Cumming, 2005; Nanda and Rhodes-Kropf, 2012). Alternatively, these times may lead investors and entrepreneurs to manage the startup using strategies that are more risky (Arora and Nandkumar, 2011). If these factors were important in driving our results, our OLS coefficient could be biased downward. This is because our proxy for the willingness of investors to experiment is the number of investments per quarter. To the extent that measurement error exists in our proxy, this will tend to bias the OLS coefficients toward zero.

To examine the extent to which these mechanisms could be at play, we turn to an instrumental variables strategy. Our IV approach is predicated on two particular features of the venture industry. First, the supply of capital into the VC industry is greatly influenced by the asset allocation decisions of university endowment and pension fund managers, who tend to allocate capital to sectors based on backward-looking (rather than forward-looking) metrics, as well as the need to re-balance their portfolio based on the performance of other asset classes (Samila and Sorenson, 2012). Second, and more important, limited partners tend to allocate capital to private equity as

[8] Investor fixed effects would still be identified when running specifications at the startup level as with Tables 3 and 4. However, this would lead to us estimate investor fixed effects using only about half the investor-startup deals, given the average of about two investors per startup. Although we cluster our standard errors at the quarterly level, we also check to see that our results in the tables using investor fixed effects are not arising purely as an artifact of the larger sample size. The results are extremely similar if we just include one (randomly chosen) investor per firm as with Tables 3 and 4, and add investor fixed effects.

[9] Our results are robust to alternative ways to measuring whether an investor is experienced. For example, we have looked at another measure that codes investors as experienced if they made more than 20 investments over the period 1985–2004. The point estimates are extremely similar.

Table 5
Funding environment and startup outcomes, with investor fixed effects.

This table reports the results from OLS regressions in which the data used to estimate the regressions are at the investor-startup level. We report three sets of specifications. The first includes all firms for which we have data on the identity of the investors. The second reports only the more experienced investors, who we define as having invested in at least five other startups in the two years prior to the investment. The third includes those with less than five investments in the prior two years. Time controls refer to period fixed effects in Columns 1–3 and to IPO-year fixed effects in Columns 4–6. Control variables, industry, period and IPO-year fixed effects are the same as in Tables 3 and 4. In addition, all regressions include investor fixed effects. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Probability of failure			Pre-money value conditional on IPO		
Variable	All investors (1)	VCs with ≥5 investments in prior two years (2)	VCs with <5 investments in prior two years (3)	All investors (4)	VCs with ≥5 investments in prior two years (5)	VCs with <5 investments in prior two years (6)
Log number of firms financed in the same quarter	0.134***	0.130***	0.139***	0.158**	0.233***	0.049
	(0.011)	(0.014)	(0.012)	(0.069)	(0.082)	(0.090)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Investor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,011	8,663	13,348	2,959	1,407	1,552
R-squared	0.22	0.15	0.19	0.77	0.72	0.89

an asset class even though significant differences exist in the types of private equity funds within this broader asset class, and these respond to very different investment opportunities. For example, leveraged buyout funds focus on established companies with significant revenues and profits to support leverage and generate value for their investors from financial engineering and improved operational performance. These are often old economy firms such as those in manufacturing that need assistance in improving operational performance. On the other hand, venture capital firms invest in startup firms that are commercializing new technologies such as a novel biotechnology compound or an idea for an Internet company.

We, therefore, use an instrumental variables estimation strategy, in which the number of startup firms financed by venture capital investors in a given quarter is instrumented with a variable that measures the total dollars raised by leveraged buyout funds that closed in the five to eight quarters before the firm was funded. The assumption is that the limited partners' decision to invest in buyout funds is uncorrelated with the riskiness of future innovations that lead to early stage venture capital funding. However, the fact that limited partners allocate capital to the private equity asset class as a whole leads fund raising by venture and buyout funds to be associated.[10] This IV strategy is similar to Gompers and Lerner (2000). However, our exclusion restriction is somewhat stronger as it requires that the level of buyout fund raising two years before is unrelated to the *variance* in outcomes for venture capital investments in a given period.

Our instrumental variables estimation should capture that part of the VC investments that are due to increases in capital unrelated to the investment opportunities available at the time for venture capital funds. Lagged buyout fundraising is used as an instrument to account for the fact that venture funds take one-to-three years to fully invest the capital in their funds and has the added advantage of further distancing the instrument from current VC opportunities.[11]

We run two stage least squares regressions, where the variable $OTHFIN_t$ in Eqs. (1) and (2) is treated as endogenous and a variable that calculates the total dollars raised by buyout funds that closed five to eight quarters before t is used to instrument for $OTHFIN_t$. These results are reported in Columns 2 and 4 of Table 6. We report the coefficients from comparable OLS regressions in Columns 1 and 3 for easy comparison. As can be seen from the bottom of Table 6, the regressions have a strong first stage, and pass the F-test for possible weak instruments.

Comparing the OLS and IV coefficients in Table 6 shows that the IV coefficients are larger than the OLS coefficients. This pattern suggests that increases in capital that are unrelated to the investment opportunities facing VCs make them more likely to invest in riskier startups. That is, the IV regressions accentuate our finding that risky firms are funded when capital is abundant. These findings are consistent with a model in which an abundance of capital leads investors to experiment more and, hence, invest in riskier, more innovative startups, independent of the investment opportunities available at the time (Nanda and Rhodes-Kropf, 2012).

In Table 7, we report the result of the same regressions but run at the investor-firm level and including investor fixed effects. The results continue to hold, implying that the high level of investment activity leads the same VCs to

[10] As a robustness test we also use the count of buyout funds that closed in the five-to-eight quarters prior to the investments.

[11] To account for the concern that time trends could be driving the IV result, we have also run robustness checks where we control for the level of contemporaneous buyout fund-raising. The results remain equally robust when including this control.

Table 6
The effect of increased capital at time of funding on startup outcomes.
This table reports the results of two stage least squares regressions, where the number of firms financed in a given quarter is instrumented with the total dollars raised by leveraged buyout funds that closed in the five to eight quarters before the firm was funded. For ease of comparison, the coefficients from the respective OLS regressions in Tables 3 and 4 are replicated in Columns 1 and 3. Time fixed effects refer to period fixed effects in Columns 1 and 2 and to IPO-year fixed effects for Columns 3 and 4. Control variables and fixed effects are as reported in Tables 3 and 4. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Probability of failure		Pre-money value conditional on IPO	
Variable	OLS (Column 4 of Table 3) (1)	IV (2)	OLS (Column 4 of Table 4) (3)	IV (4)
Log number of firms financed in the same quarter	0.137***	0.151***	0.214***	0.461***
	(0.010)	(0.030)	(0.051)	(0.107)
Control variables	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Number of observations	11,497	11,497	1,197	1,197
R-squared	0.13	0.12	0.62	0.61
Coefficient on instrument and first stage statistics				
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.473***		0.360***
		(0.119)		(0.077)
Partial *R*-squared		0.171		0.197
F-statistic		15.67		21.09

change the type of investments that they make, toward risky startups that have a higher probability of failure, but also have bigger successes.

These results suggest a much larger role for financial markets in the commercialization of new technologies. Instead of just responding to the need for good ideas to be funded, the results in Tables 6 and 7 suggest that a flood of money into the venture community could change the type of the projects that get funded.

4.4. Risky versus novel investments

Thus far, the results we have reported in Tables 3–7 are based on financial measures of success. That is, firms funded in hot markets are more likely to fail, but are valued higher on the day of their IPO. In Tables 8 and 9, we extend the estimation framework we used to study valuation to real outcomes associated with firm-level innovation. That is, we ask whether these are purely more risky investments in financial terms or whether the investments VCs make in hot markets are associated with more novel technologies or innovative firms.

Following a long literature in economics (for example Jaffe, Trajtenberg, and Henderson, 1993), we use firm-level patenting as our measure of innovation. Although patenting is an incomplete measure of firm innovation, it is a very relevant measure of innovation in our sample of high-tech firms. Sixty percent of the firms in our sample that had an IPO filed at least one patent in the three years following their first investment. Moreover, patent citations have been shown to correlate closely with both the quality of inventions as well as their economic effects (Hall, Jaffe, and Trajtenberg, 2005).

In Tables 8 and 9, we re-run the estimations reported in Tables 6 and 7, but with the log of the number of patents and log of the average citation per patent as the dependent variable.[12] Columns 1 and 2 of Table 8 show that among firms that had an IPO, those funded in hot markets got more patents in the first three years following the first funding than those funded in less active periods. Moreover, the IV specifications show that this is still robust, again consistent with the results in Tables 6 and 7, suggesting that the supply of capital pushed investors to invest in more novel opportunities. Although we do control for the amount of money raised by the firm in its first funding, a concern arises that firms funded in hot times could be systematically more prone to patenting than those funded in less active periods, for reasons unrelated to how novel they are. We therefore also look at the average citations to the patents as a way to measure the impact of the innovations. Columns 3 and 4 show that the patent citations show a similar pattern, suggesting that difference is not only due to any increase in patenting propensity by startups in more active investing periods.

In Table 9, we include investor fixed effects and again report the estimates from patent and citation regressions run at the investor-startup level. The results of these regressions continue to document the same pattern, suggesting that even the most experienced investors are likely to change their investments towards more novel, innovative startups in periods of high financing activity. Our results using patent data therefore reinforce the patterns observed using financial outcomes.

[12] The distribution of patent counts tends to be highly skewed. One estimation approach is to use count models. We have checked that our patent regressions are robust to Negative Binomial specifications that are often used in patent research. However, to be consistent in our comparisons with the IV regressions, we run OLS specifications with logged values of patent counts and patent citations.

Table 7
The effect of increased capital at time of funding on startup outcomes, with investor fixed effects.

This table reports the results of two stage least squares regressions using data at the investor-startup level. The number of other firms financed in the same quarter that the firm received its first funding is instrumented with the total dollars raised by buyout funds in the five to eight quarters before the focal investment. For ease of comparison, the coefficients from the respective OLS regressions in Table 5 are replicated in Columns 1 and 3. Time fixed effects refer to period fixed effects in Columns 1 and 2 and to IPO-year fixed effects in Columns 3 and 4. Control variables and fixed effects are as reported in Table 6. In addition, all regressions include investor fixed effects. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Probability of failure		Pre-money value conditional on IPO	
Variable	OLS (Column 1 of Table 5) (1)	IV (2)	OLS (Column 4 of Table 5) (3)	IV (4)
Log number of firms financed in the same quarter	0.134***	0.123***	0.158**	0.311***
	(0.011)	(0.033)	(0.069)	(0.118)
Control variables	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Investor fixed effects	Yes	Yes	Yes	Yes
Number of observations	22,011	22,011	2,959	2,959
R-squared	0.22	0.21	0.77	0.77
Coefficient on instrument and first stage statistics				
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.401***		0.312***
		(0.106)		(0.067)
Partial *R*-squared		0.145		0.167
F-statistic		14.41		21.75

Table 8
Funding environment and startup innovation.

This table reports the results from OLS and two stage least squares regressions looking at innovation by firms that had an IPO. The level of patenting measures the log of cumulative patents filed in the three years following first funding and citation per patent measures the log of the cumulative citations per patent up to three from the date the patent was granted. All regressions control for the number of investors in the syndicate, the firm's age at first funding, the amount of money raised in the first round of funding and whether the firm was based in Massachusetts or California. Industry fixed effects control for the 7 industries outlined in Table 1. Period Fixed effects control for the startup being funded in the period 1985–1990, 1991–1997 or 1998–2004. The instrumental variables strategy is the same as that reported in Tables 6 and 7. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Level of patenting		Citations to patents	
Variable	OLS (1)	IV (2)	OLS (3)	IV (4)
Log number of firms financed in the same quarter	0.219***	0.228***	0.156***	0.172**
	(0.055)	(0.088)	(0.054)	(0.086)
Control variables	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Number of observations	1,197	1,197	1,197	1,197
R-squared	0.18	0.17	0.10	0.11
Coefficient on instrument and first stage statistics				
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.519***		0.519***
		(0.094)		(0.094)
Partial *R*-squared		0.359		0.359
F-statistic		30.45		30.45

5. Robustness checks

We run several analyses to check the robustness of our results. Two sets of analyses are worth particular note. First, a number of successful exits for firms are not necessarily through IPOs but can be through acquisitions of the startups. We therefore check to see whether our results on firm outcomes are robust to a different measure of success, namely all exits in our database that are coded as above $50 million. This measure is patchy by definition, as it might not include all acquisitions that met the threshold. Nonetheless, it is a useful robustness check to

Table 9
Funding environment and startup innovation, with investor fixed effects.

This table reports the same regressions as in Table 8 but using data at the investor-startup level. Control variables, fixed effects and instrumental variables are the same as in Table 8 with the additional inclusion of investor fixed effects. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Level of patenting		Citations to patents	
Variable	OLS (1)	IV (2)	OLS (3)	IV (4)
Log number of firms financed in the same quarter	0.182** (0.069)	0.239*** (0.097)	0.161** (0.076)	0.202** (0.098)
Control variables	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Investor fixed effects	Yes	Yes	Yes	Yes
Number of observations	2,959	2,959	2,959	2,959
R-squared	0.29	0.28	0.32	0.23
Coefficient on instrument and first stage statistics				
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.467*** (0.091)		0.467*** (0.091)
Partial *R*-squared		0.324		0.324
F-statistic		26.51		26.51

Table 10
Ex ante differences at time of first funding.

This table reports the results of OLS and two stage least squares regressions using data at the investor-startup level. Columns 1 and 2 report results from regressions where the dependent variable is the log of the startup's age at first funding. Columns 3 and 4 report results from regressions where the dependent variable is the log of the number of venture capital investors in the first funding syndicate. Control variables, fixed effects and the instrumental variables strategy are as reported in Table 7. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Startup's age at first funding		Syndicate size at first funding	
Variable	OLS (1)	IV (2)	OLS (3)	IV (4)
Log number of firms financed in the same quarter	−0.148*** (0.030)	−0.295*** (0.077)	−0.030*** (0.009)	−0.108*** (0.025)
Control variables	Yes	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Investor fixed effects	Yes	Yes	Yes	Yes
Number of observations	22,011	22,011	22,011	22,011
R-squared	0.28	0.27	0.46	0.46
Coefficient on instrument and first stage statistics				
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.416*** (0.107)		0.425*** (0.112)
Partial *R*-squared		0.150		0.150
F-statistic		15.12		14.47

ensure that our results are not driven by the particular set of firms that had an IPO. We report the results of these analyses in Table A1. Consistent with the findings reported in Tables 4 and 6, we find that firms funded in more active quarters have higher exit values and that these results are robust to our IV specification. Our finding, that firms funded in active investment quarters have higher exits is not restricted to the sample of firms that have an IPO.

Second, we check to see that our results are not driven by outliers. Because more firms are funded in active quarters, there is a higher likelihood of having extreme outcomes purely as a result of order statistics. We explicitly check to see that our regressions on the values at exit are not driven by any outliers. We report the results from quantile regressions, estimated at the median exit value for firms that had an IPO and for firms that exited with at least a $50 million valuation. These results are reported in Table A2, which shows that our results are not driven by this statistical artifact. As can be seen from the results in Table A2, median regressions exhibit the same pattern shown in the main results.[13]

[13] Furthermore, we show in Table A3 that firms funded in active investment quarters are less likely to have an IPO. Because IPOs are tail outcomes in themselves (Hall and Woodward, 2010), this suggests that our results are due to a substantive difference in the types of firms being

5.1. Ex ante differences

Thus far, all the differences we have shown are based on ex post outcomes. If, in fact, the differences we find stem from variations in the willingness to experiment at the time of the investment, we should also expect to see some differences exist ex ante. We therefore look at two other measures that could shed light on whether the same investors invest differently in more versus less active times.

Our first measure is the startup's age at the time of first funding. Columns 1–2 of Table 10 report the results from both OLS and IV specifications, in which the dependent variable is the log of the startup's age at first funding. As with Table 7, the regressions are run on data at the investor-startup level, and all regressions include investor fixed effects in addition to controlling for startup-level covariates, industry and period controls. As can be seen from Column 1, startups funded in more active quarters tend to be systematically younger than those funded in less active times. Although not the only explanation, it is certainly consistent with a view that investors are willing to invest in ex ante riskier startups in more hot times. In Column 2 we examine the IV coefficients. Our results continue to be robust and again are stronger in the IV regressions compared with OLS, suggesting that investors are likely to change their investments in active times in ways that are observable at the time of the investor's initial investment.

Our second measure examines the size of the syndicate at the time of first funding. Nanda and Rhodes-Kropf (2012) provide a rationale for why syndicates could be systematically smaller when financing risk is low compared with when it is high. They highlight that times of abundant funding are ones when investors are less concerned about the difficulty in receiving follow-on funding for their investment in subsequent rounds. This makes them more willing to have smaller syndicates, as the insurance provided by having a larger syndicate is less critical at those times. If the changes we show are driven by changes in financing risk as outlined by Nanda and Rhodes-Kropf (2012), we may also expect to see these differences in the size of the syndicates in more active investment markets relative to less active times. Columns 3 and 4 of Table 10 report the results from both OLS and IV specifications, where the dependent variable is the log of the number of syndicate members that round of funding. As with Columns 1 and 2, the regressions are run on data at the investor-startup level, and all regressions include investor fixed effects in addition to controlling for startup-level covariates, industry and period controls. Columns 3 and 4 document a consistent pattern that syndicates tend to be smaller in hot times (controlling for the amount of capital raised in the round of funding) and, furthermore, that investors change their syndicates in active times. Although this is not the only possible reason for smaller syndicates in more active investment climates, these results again provide evidence that is consistent with the fact that venture capital investors change their investment strategies across the investment cycle.

(*footnote continued*)
funded across periods rather than a mechanical relationship due to order statistics.

6. Conclusions

New firms that create and commercialize new technologies can have profound effects on the economy (Aghion and Howitt, 1992; Foster, Haltiwanger, and Syverson, 2008). The founding of these new firms and their financing is highly cyclical (Gompers, Kovner, Lerner, and Scharfstein, 2008). Conventional wisdom associates periods with active investment either with worse firms being funded (a left shift in the distribution of projects) or with better investment opportunities (a right shift in the distribution of projects).

However, the evidence in our paper suggests another, possibly simultaneous, phenomenon. We find that firms that are funded in hot times are more likely to fail but simultaneously create more value if they succeed. This pattern could arise if more risky and novel firms are funded in hot times. Our results provide a new but intuitive way to think about the differences in project choice across the investment cycle. We show that the same investors invest in more risky, innovative startups in hot times. Because the financial results we present cannot distinguish between more innovative versus simply riskier investments, we also present direct evidence on the level of patenting by firms funded at different times in the cycle. We find that in addition to being valued higher on the day of their IPO, successful firms that are funded in hot markets had more patents and received more citations in the initial years following their first funding than firms funded in less heady times.

Our IV results also highlight that changes in capital availability that are unrelated to the investment opportunities seem to exacerbate our results, suggesting that one mechanism through which hot markets could lead to riskier investments is that it makes investors more willing to experiment, and thereby fund more novel, risky investments. This finding is consistent with Nanda and Rhodes-Kropf (2012), who demonstrate how increased funding in the venture capital market can rationally alter the type of investments investors are willing to fund toward a more experimental, innovative project. According to this view, the abundance of capital associated with investment cycles might not only be a response to the arrival of new technologies, but could also play a critical role in driving their creation and commercialization. That is, the abundance of capital can change the type of firm investors are willing to finance in these times. Financial market investment cycles can therefore create innovation cycles.

Our findings suggest many avenues for future research that consider the impact of the cycle on innovation, venture capital, and the development of new companies. Many of the classic findings in venture capital could be extended to examine how they are impacted by the investment cycle. For example, the interaction of product markets and financing strategy (Hellmann and Puri, 2000), the effect of networks (Hochberg, Ljungqvist, and Lu, 2007), or the question of whether investors pick the jockey or the horse (Kaplan, Sensoy, and Stromberg, 2009), could all vary based on the where investors are in the investment cycle.

Appendix A

See Tables A1 - A3.

Table A1
Funding environment and pre-money valuation for exits above $50M.

This table reports the results from regressions used in Columns 3 and 4 of Table 6. However, as a robustness check, the sample includes all firms with an exit above $50 million instead of using only firms that had an IPO. This includes acquisitions above $50 million for which we have data and excludes IPOs with a pre-money value below $50 million. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Pre-money value on exits > $50million	
	OLS	IV
Variable	(1)	(2)
Log number of firms financed in the same quarter	0.066**	0.171***
	(0.033)	(0.062)
Control variables	Yes	Yes
Exit-year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Number of observations	1,779	1,779
R-squared	0.36	0.36
Coefficient on instrument and first stage statistics		
Log dollars raised by buyout funds 5–8 quarters before firm funded		0.624***
		(0.099)
Partial *R*-squared		0.324
F-statistic		50.63

Table A2
Median valuation of successful firms.

This table reports the results from quantile regressions (estimated at the median) in which the dependent variable is the log of the pre-money valuation on the day the firm had an exit. Industry fixed effects control for the 7 industries outlined in Table 1. Exit-year fixed effects control for the year in which the startup had its IPO or was acquired. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

	Pre-money value conditional on IPO	Pre-money value on all exits above $ 50 million
	(1)	(2)
Log number of firms financed in the same quarter	0.184***	0.063*
	(0.054)	(0.034)
Firm's age at IPO	−0.016**	−0.007
	(0.007)	(0.004)
Log total funds raised prior to exit	0.403***	0.335***
	(0.028)	(0.019)
Log value of NASDAQ on day of exit	0.880*	1.026***
	(0.476)	(0.307)
Startup based in California	0.118**	0.026
	(0.050)	(0.038)
Startup based in Massachusetts	0.079	−0.064
	(0.074)	(0.055)
Exit year fixed effects	Yes	Yes
Industry fixed effects	Yes	Yes
Number of observations	1,197	1,779

Table A3
Funding environment and probability of an IPO.

This table reports results from OLS regressions looking at the probability that the firm had an IPO. The dependent variable takes a value of one if the startup had an IPO and zero otherwise. Industry fixed effects control for the seven industries outlined in Table 1. Period fixed effects control for the startup being funded in the period 1985–1990, 1991–1997 or 1998–2004. Standard errors are clustered by quarter. *, ** and *** refer to significance at 0.1, 0.05 and 0.01 respectively.

Variable	1985–2004 (1)	(2)	(3)	(4)	Drop if funding year is 1998–2000 (5)
Log number of firms financed in the same quarter	−0.100***	−0.105***	−0.100***	−0.025***	−0.084***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.014)
Log of funds raised by firm in its first financing		0.014**	0.013**	0.022***	0.029***
		(0.005)	(0.005)	(0.005)	(0.007)
Number of investors in syndicate		0.009***	0.009***	0.006**	0.010**
		(0.003)	(0.003)	(0.003)	(0.004)
Startup based in California		0.020***	0.025***	0.026***	0.039***
		(0.007)	(0.007)	(0.007)	(0.011)
Startup based in Massachusetts		0.016	0.018*	0.016	0.025*
		(0.010)	(0.010)	(0.010)	(0.014)
Period fixed effects	No	No	Yes	Yes	Yes
Industry fixed effects	No	No	No	Yes	Yes
Number of observations	12,285	11,497	11,497	11,497	6,518
R-squared	0.03	0.04	0.04	0.05	0.04

References

Aghion, P., Howitt, P., 1992. A model of growth through creative destruction. Econometrica 60, 323–351.

Arora, A., Nandkumar, A., 2011. Cash-out of flameout! Opportunity cost and entrepreneurial strategy: theory, and evidence from the information security industry. Management Science 57, 1844–1860.

Brown, J.R., Fazzari, S.M., Petersen, B.C., 2009. Financing innovation and growth: cash flow, external equity, and the 1990s R& D boom. Journal of Finance 66, 151–185.

Fleming, G., Schwienbacher, A., Cumming, D., 2005. Liquidity risk and venture capital finance. Financial Management 34, 77–105.

Foster, L., Haltiwanger, J., Syverson, C., 2008. Reallocation, firm turnover, and efficiency: selection on productivity or profitability. American Economic Review 98, 394–425.

Gompers, P., Kovner, A., Lerner, J., Scharfstein, D., 2008. Venture capital investment cycles: the impact of public markets. Journal of Financial Economics 87, 1–23.

Gompers, P., Lerner, J., 2000. Money chasing deals? The impact of fund inflows on private equity valuations. Journal of Financial Economics 55, 281–325.

Gompers, P., Lerner, J., 2004. The Venture Capital Cycle. MIT Press, Cambridge, MA.

Gupta, U., 2000. Done Deals: Venture Capitalists Tell their Stories. Harvard Business School Press, Boston, MA.

Hall, B., Jaffe, A., Trajtenberg, M., 2005. Market value and patent citations. RAND Journal of Economics 36, 16–38.

Hall, R.E., Woodward, S.E., 2010. The burden of the nondiversifiable risk of entrepreneurship. American Economic Review 100, 1163–1194.

Hellmann, T., 2002. A theory of strategic venture investing. Journal of Financial Economics 64, 285–314.

Hellmann, T., Puri, M., 2000. The interaction between product market and financing strategy: the role of venture capital. Review of Financial Studies 13, 959–984.

Hochberg, Y.V., Ljungqvist, A., Lu, Y., 2007. Whom you know matters: venture capital networks and investment performance. Journal of Finance 62, 251–301.

Jaffe, A.B., Trajtenberg, M., Henderson, R., 1993. Geographic localization of knowledge spillovers as evidenced by patent citations. Quarterly Journal of Economics 108, 577–598.

Kaplan, S., Sensoy, B., Stromberg, P., 2002. How well do venture capital databases reflect actual investments? Unpublished working paper. University of Chicago, Chicago, IL.

Kaplan, S., Sensoy, B., Stromberg, P., 2009. Should investors bet on the jockey or the horse? Evidence from the evolution of firms from early business plans to public companies. Journal of Finance 64, 75–115.

Kerr, W.R., Nanda, R., Rhodes-Kropf, M., 2013. Entrepreneurship as experimentation. Unpublished working paper. Harvard University, Cambridge, MA.

Kortum, S., Lerner, J., 2000. Assessing the impact of venture capital on innovation. RAND Journal of Economics 31, 674–692.

Lerner, J., Sorensen, M., Stromberg, P., 2011. Private equity and long-run investment: the case of innovation. Journal of Finance 66, 445–477.

Li, D., 2011. Financial constraints, R& D investment, and stock returns. Review of Financial Studies 24, 2974–3007.

Manso, G., 2011. Motivating innovation. Journal of Finance 66, 1823–1860.

Metrick, A., Yasuda, A., 2010. Venture Capital and the Finance of Innovation. John Wiley and Sons, Inc., Hoboken, NJ.

Mollica, M., Zingales, L., 2007. The impact of venture capital on innovation and the creation of new businesses. Unpublished working paper. University of Chicago, Chicago, IL.

Nanda, R., Nicholas, T., 2011. Did bank distress stifle innovation during the great depression? Unpublished working paper. Harvard University, Cambridge, MA.

Nanda, R., Rhodes-Kropf, M., 2012. Financing risk and innovation. Unpublished working paper. Harvard University, Cambridge, MA.

Nanda, R., Rhodes-Kropf, M., 2013. Innovation and the financial guillotine. Unpublished working paper. Harvard University, Cambridge, MA.

Sahlman, W., 2010. Risk and reward in venture capital. Harvard Business School Note 811-036, pp. 1–37.

Samila, S., Sorenson, O., 2011. Venture capital, entrepreneurship, and economic growth. Review of Economics and Statistics 93, 338–349.

Samila, S., Sorenson, O., 2012. Institutional investors and the supply of private equity. Unpublished working paper. Yale University, New Haven, CT.

Scharfstein, D., Stein, J., 1990. Herd behavior and investment. American Economic Review 80, 465–479.

Schumpeter, J., 1942. Capitalism, Socialism, and Democracy. Harper, New York, NY.

Sorensen, M., 2007. How smart is smart money? An empirical two-sided matching model of venture capital. Journal of Finance 62, 2725–2762.

Stangler, D., 2009. The economic future just happened. Unpublished working paper. Ewing Marion Kauffman Foundation, Kansans City, MO.

Tian, X., Wang, T.Y. Tolerance for failure and corporate innovation. Review of Financial Studies, forthcoming.