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Investment Cycles and Startup Innovation

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Abstract

We find that VC-backed firms 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 IPO, 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 the flood of capital in hot markets also 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.

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JEL Classification: G24, G32, O31

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Only those who dare to fail greatly can ever achieve greatly. - Robert Kennedy

I. Introduction

Venture capital 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 semi-conductors, biotechnology and the internet, as well as the introduction of several innovations across a spectrum of sectors in healthcare, IT and new materials have 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, since 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]

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

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 et al. (2008)) may 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)). Indeed, 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 note that 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 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 (Schumpeter (1942); Aghion and Howitt (1992)). 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 in 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, the results are equally strong. 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 rather by investors who seem to change their investments across the cycle. Furthermore, we find that even the most experienced venture capital investors who consistently invest across the cycles 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 point in 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 a view where worse projects are funded and one where 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 may be observed if investment opportunities are systematically different in hot and cold periods. Or, time varying risk preferences may alter the willingness of investors to experiment. Alternatively, investors may change the type of investments they make in hot markets, independent of the investment opportunities available to them. For example, Nanda and Rhodes-Kropf (2011) argue that hot markets may lower financing risk faced by investors, and hence make investors more willing to finance experimentation.

In order to shed light on this question, we use an instrumental variables estimation strategy. We instrument the venture capital activity in a given quarter with fund-raising by *leveraged buyout* funds that closed in the 5-8 quarters before that quarter. Leveraged buyout funds focus their investments on existing companies with significant 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 investing in early-stage ventures invest in startup 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, since it suggests that increased capital in the venture industry seems to alter how venture capitalists invest.

Our work 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 et al. (2011), Sorensen (2007), Tian and Wang (2011), Manso (2011), Hellmann and Puri (2000), Mollica and Zingales (2007), 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 may impact real outcomes. Rather than just reducing frictions in the availability of capital for new ventures, investment cycles may play a much more central role in the diffusion and commercialization of technologies in the economy. Financial market investment cycles may 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 et al. (2009), Li (2012)). 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 relationship 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)). We note that our results are completely consistent with this finding. In fact, we document 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 may therefore be a critical aspect of the process through which new technologies are commercialized. Our results are therefore also relevant for policymakers who may be concerned 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 relationship 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.

II. 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 may be associated with systematically lower discount rates, so that investors make *ex ante* worse investments during hot times. On the other hand, others have argued that better startups may 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, non-existent markets and unproven teams (Hall and Woodward (2010)). 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" (page 2). Sequoia's $12.5 million investment was worth $4 billion when they sold their stake in the firm in 2005, returning 320 times their 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 may 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.

The examples above point to the fact that while VCs may not be able to easily distinguish good and bad investment opportunities *ex ante*, they may have a better sense of how risky a potential investment might be. An investment that is more risky *ex ante* will be 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. However, on average the successes in risky investments will be bigger than less risky ones, while worse investments will do badly regardless. Figure 1 highlights how the *ex post* distribution of risky investments differs from the *ex post* distribution of worse investments. That is, rather than 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 a few, bigger successes.

Nanda and Rhodes-Kropf (2011) propose that investors may fund riskier investments in hot markets as these times allow investors to experiment more effectively. If this is the case, then we should expect to see more failures for firms funded in hot markets. However, conditional on a successful outcome such as an IPO or big acquisition, we would expect firms funded in hot markets to do even better.

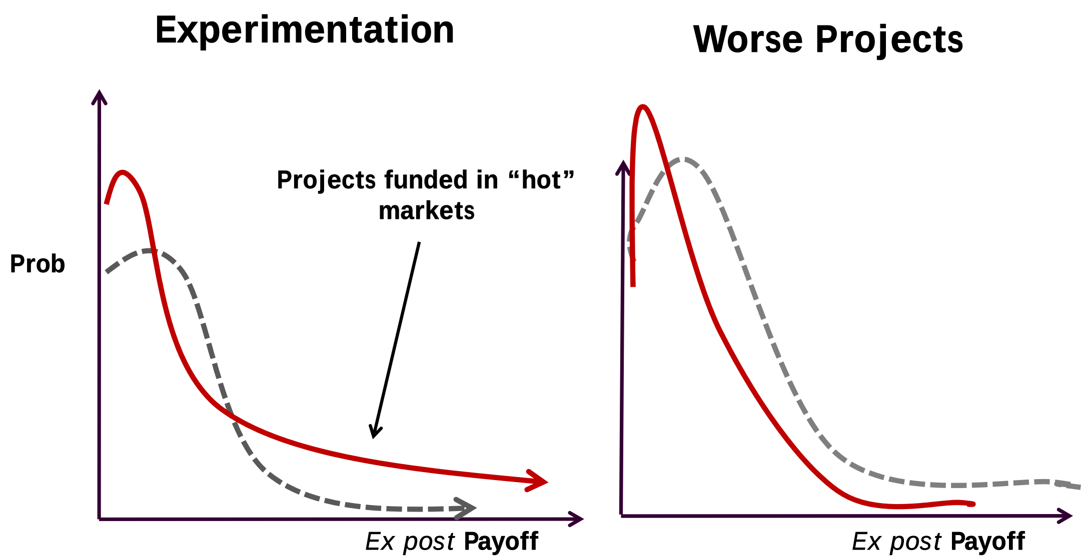

Figure 1: Distinguishing Risky Investments from Worse Investments by looking at the ex post distribution of outcomes

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. Our analysis has two main elements. First, we document a robust correlation between firms funded in boom times being simultaneously 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.

The second element of our analysis entails an initial look at 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, there is also a possibility that in addition to this, the flood of money during boom times allows VCs to experiment more effectively, and thereby change the type of investments they choose to make towards more novel, innovative startups. We examine the extent to which this second mechanism of "money *changing* deals" may also be at play, by using instrumental variables to untangle the endogeneity in the analysis.

Before proceeding with the results, we first outline the data used in our analysis in Section III below.

III. Data

Our analysis is based on data from Dow Jones Venture Source.[2] This dataset, along with Thompson Venture Economics, forms the basis of most academic papers on venture capital. Kaplan et al. (2002) compare the two databases and note

[2] This dataset was formerly known as Venture One.

that Venture Source is less likely to omit deals, a fact that will be important when looking at firm bankruptcies. The Venture Source data also provides a more comprehensive view of exits, including more accurate data on the pre-money valuations of firms at IPO and acquisition, both of which are critical to our analysis of firm outcomes.[3]

We focus our analysis on US based startups, since data for these firms is most 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 onwards. 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 twenty year period from 1985 to 2004, but follow these firm's eventual outcomes until the end of 2010.

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

As can be seen from Table 1, there are 12,285 firms that meet our criteria of US-based startups that received their first early stage financing between 1985 and 2004. The probability that the firm goes bankrupt in our sample is 27%, but varies from 20% for biotechnology and healthcare startups to 36% for business and financial services.[4]

As noted in Section II 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 will lead to fewer successes (and hence a higher probability of failure in the context of our sample), risky investments would imply that conditional on an IPO, firms funded in active investment markets will have a higher economic return than those funded in less active markets. On the other hand, worse investments would imply that even conditional on an IPO, firms funded in hot markets had lower value than those funded in cold markets. In order 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 M. However, this varied from over $300 M for

[4]This number is consistent with Hall and Woodward (2010) who find 22% of their investments are "confirmed zero-value outcomes." Following Hall and Woodward (2010) 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%, completely in line with Hall and Woodward's estimation of 50%.

communications and networking startups to just \$84 M 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 quite consistent.

We also report the outcome of exits that include information on acquisitions, where available. Data on acquisitions is more likely to be available for larger exits, but this bias does not substantively impact our analysis. Since, by definition, 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 "firesales." Consistent with this notion, we report the valuation for all exits above \$50M (including IPOs above \$50M) that we have information on in our data set. The numbers are extremely similar to the valuations obtained when looking only at 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) in order to look at their innovation prior to when they went public. We look at two different measures of firm innovation. First, we look at the raw count of patents granted to the firm that were filed in the years *following* its first funding. The second measure is 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.[5] Matching firms in our sample to the patent database therefore allows us to calculate their patenting in the three years immediately following their first funding and the subsequent citations those patents received in the three years following their grant. This facilitates the study of the innovations by the startups while they were still private.

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 the following section. 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

[5]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 2-3 year delay in the granting of patents from the time they are filed).

money prior to their IPO, and interestingly, took almost the same time from first funding to 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.

IV. Regression Results

A. Riskier Investments or Worse Investments?

In Tables 3 and 4, we turn to firm-level regressions to examine the relationship 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:

$$Y_i = \alpha_1 OTHFIN_t + \alpha_2 X_i + \phi_j + \tau_T + \varepsilon_i \qquad (1)$$

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 1 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. Since 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, corresponding to 1985-1990, 1991-1997 and 1998-2004. Period fixed effects refer to dummy variables for these three periods.[6]

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 therefore captures the level of financing activity in the quarter that the focal firm was first funded, and proxies for the extent to which a given quarter was "active" in that period. 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 may 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

[6]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 rather than a steady trend, we prefer the non-parametric approach of controlling for distinct periods of activity in venture capital.

interest is measured at the quarterly-level.

Table 3 reports estimates from OLS regressions.[7] As can be seen from Table 3, firms that were first financed in quarters with a lot of financing activity are much more likely to fail. However, this could be due to the fact that active investment periods are associated with younger firms being financed. Indeed, columns (2)-(5) show that firms that were older at the time of first funding and those that raised more money at the time of first funding were less likely to fail. Controlling for these and other covariates, including industry fixed effects and period fixed effects, the results still continue to be robust. In addition, 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 while the failure rate is a level, 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 1.37 percentage 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. Since the variation across quarters

[7] We have reported the results from OLS regressions, in order to facilitate comparisons with the IV regressions in following tables. The results are robust to running the regressions as probit models.

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. As noted above, however, these results do not necessarily imply that VCs fund lower quality firms in hot markets. In order 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 $OTHFIN_t$, that 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 may 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 M) is associated with a 4% increase in the value at IPO (that is, ˜$12

M). This implies that the marginal dollar invested by VCs will return a 3X return for firms that are successful.[8].

An important concern with our results thus far is that firms funded in hot markets may go public in very different environments than those funded in less active periods. We therefore 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. In column 5 we again drop quarters with the extreme spike in activity and show 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 a lot 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

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

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 M increase in the value at IPO. Our results suggest that VCs fund riskier firms in quarters with more financing activity. Although these firms have a lower probability of going public, *conditional* on an IPO, they are significantly more valuable.

B. Investor Fixed Effects

Our results so far have documented 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: they were both more likely to fail completely and more likely to be extremely successful. While this is consistent with more risky firms being funded in hot times, that is not necessarily the case. If the most experienced investors take advantage of the better investment opportunities in hot times while simultaneously "fools rush in", this could explain our result entirely. That is, our results could be due to differences in the types of VCs investing in hot vs. cold times, as opposed to the same VCs changing their investments across the cycle.

In order to examine the driver of these results, 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 syndicate. In these instances, each observation corresponds to the specific investor-firm 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 where 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.[9] 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 firms in the sample for whom we have a unique identifier and who had multiple investments. In columns 2 and 5 we also reduce the set of investors to the most experienced firms which includes only the firms that made at least 5 investments in the two years prior to the focal

[9] Note that 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 investor per firm as with Tables 3 and 4 and add investor fixed effects.

investment.[10] In columns 3 and 6 we look at the performance of less experienced investors.

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 vs. less active times. Column 2 of Table 5 shows that the pattern continues to hold for the more experienced investors. Startups funded by less experienced investors may have a marginally higher change of failing, but this difference is not statistically significant.

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 the same investors also change the types of investments

[10]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 twenty investments over the period 1985-2004. The point estimates are extremely similar.

they make in hot markets. Columns 5 and 6 of Table 5 highlight this further. They show that less experienced investors have successes that are not as large and that the relationship documented 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 relationship 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 may 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.

C. Money Changing Deals?

Thus far, we have documented 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 will be 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 upwards, as the omitted variable would be responsible for driving both the variance in outcomes and attracting venture capital investment.

In addition to this explanation, however, Nanda and Rhodes-Kropf (2011) provide a theoretical model linking financial market activity to more novel investments. In their model, the increase in financing activity also lowers financing risk, which makes investors more willing to experiment, and hence take on more innovative investments. According to this view, the flood of money associated with the presence of heated investment activity may actually cause VCs to change the type of investments they are willing to make – towards more risky, innovative startups in the market. If this factor was important in driving our results, we expect our OLS coefficient may be biased towards zero. This is because our proxy for the willingness of investors to experiment is the number of investments per quarter. To the extent that there is measurement error in our proxy, this will tend to bias the OLS coefficients towards zero.

In order to examine the extent to which these mechanisms may 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. Second, and more importantly, limited partners tend to allocate capital to "private equity" as an asset class even though there are significant differences 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 may 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, where 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 5-8 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 for re-balancing or return chasing reasons, leads fund raising by venture and buyout funds to be associated.[11] We note here that a similar IV strategy was used by Gompers and Lerner (2000). While the IV strategy is similar, 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 fund-raising is used as an instrument to account for the fact that venture funds take 1-3 years to fully invest the capital in their funds and has the added advantage of further distancing the instrument from current VC opportunities.[12]

We therefore run two-stage-least-squares regressions, where the variable $OTHFIN_t$ in equations (1) and (2) is treated as endogenous and a variable that calculates the total dollars raised by buyout funds that closed 5-8 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

[11] As a robustness test we also use the count of buyout funds that closed in the 5-8 quarters prior to the investments.

[12] To account for the concern that time trends may 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.

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 column 1 to column 2 in Table 6 and in particular, column 3 with column 4, we see that the coefficients on the IV are larger than the OLS coefficients. The IV coefficients therefore suggest that the 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. Referring to our discussion above, these findings are consistent with a model where an abundance of capital may in fact lead 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 (2011)).

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

These are fascinating results because they suggest a much larger role for financial markets in the commercialization of new technologies. Rather than 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 actually change the type of the projects that get funded. The question then is, is this just a shift to riskier projects or actually to more innovative ones?

D. "Risky" vs "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 et al. (1993)), we use firm-level patenting as our measure of innovation. While patenting is only one 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 et al. (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.[13] 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 prior tables suggesting that the supply of capital may have 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, there is a concern that firms funded in hot times may 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

[13]The distribution of patent counts tends to be highlight 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, in order to be consistent in our comparisons with the IV regressions, we run OLS specifications with logged values of patent counts and patent citations.

patent data therefore reinforce the patterns observed using financial outcomes.

V. Robustness Checks

We run several analyses to check the robustness of our results. Two sets of analysis are worth particular note. First, as was noted above, 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 $50M. This measure is patchy by definition, as it may not include all acquisitions that met the threshold, but it is nonetheless a useful robustness check to 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 Table 4 and Table 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. Since more firms are funded in active quarters, it is possible that 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 therefore 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 M valuation. These results are reported in Table A2, where we document 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 documented in the main results.[14]

A. Ex Ante Differences

Thus far, all the differences we have documented are based on *ex-post outcomes*. If in fact the differences we document 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 vs. 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, where 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

[14]Furthermore, we show in Table A3 that firms funded in active investment quarters are less likely to IPO. Since IPOs are tail outcomes in themselves (Hall and Woodward (2010)), this suggests our results are due to a substantive difference in the types of firms being funded across periods rather than a mechanical relationship due to order statistics.

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 to 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 first investment.

Our second measure examines the size of the syndicate at the time of first funding. Nanda and Rhodes-Kropf (2011) provide a rationale for why syndicates may be systematically smaller when financing risk is low compared to when it is high. They highlight that times of abundant funding are ones where 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 indeed the changes we show are driven by changes in financing risk as outlined by Nanda and Rhodes-Kropf (2011), 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.

VI. Conclusions

New firms that create and commercialize new technologies have the potential to have profound effects on the economy (Aghion and Howitt (1992), Foster et al. (2008)). The founding of these new firms and their financing is highly cyclical (Gompers et al. (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. Since 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. Our results suggest 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 experiment, and thereby fund more novel, risky investments. This finding is consistent with Nanda and Rhodes-Kropf (2011), 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 may not only be a response to the arrival of new technologies, but may in fact play a critical role in driving their creation and commercialization. That is, the abundance of capital may change the type of firm investors are willing to finance in these times. Financial market investment cycles may therefore create innovation cycles.

Our findings suggest many avenues for future research which 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 et al. (2007)), or the question of whether investors pick the jockey or the horse (Kaplan et al. (2009)), may all vary based on the where investors are in the investment cycle.

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Table 1

Descriptive Statistics

This table reports descriptive statistics on US based startups who received Seed or Early Stage financing from an investor in the Venture Source database between 1985 and 2004. For these firms, we repo
the first financing event and the ultimate outcome of the startup as of December 2010.

	Number of Firms	Share failed outright	Share with IPO	Median Pre-Money Valuation if had an IPO ($, MM)	Average Pre-Money Valuation if had an IPO ($, MM)	Median Pre-Money Valuation for all exits > $ 50 M	Average Valuation $
Full Dataset	12,285	27%	11%	151	311	165	[illegible]
Biotechnology and Healthcare	2,490	20%	19%	104	154	135	[illegible]
Business and Financial Services	1,738	36%	5%	273	380	230	[illegible]
Communications and Networking	1,315	32%	13%	320	665	240	[illegible]
Computer Hardware and Electronics	1,232	27%	11%	149	283	157	[illegible]
Computer Software and Internet	4,157	26%	7%	195	372	165	[illegible]
Consumer Goods and Services	1,044	33%	11%	146	266	156	[illegible]
Industrial Goods and Materials	309	20%	13%	84	204	186	[illegible]

Table 2

Characteristics of Startups Funded in Active vs. Less Active investment periods

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

	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 ($, MM)	$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 M	$319	$353	$239
Number of patents in 3 years following first funding	6.1	6.5	5.4
Citations per patents in 3 years following first funding	7.3	8.4	5.5

Table 3

Probability of failure based on market when the startup received first funding

This table reports the probability of a startup being coded as having failed based on the characteristics of the VC funding environment when it first received funding. All regressions are OLS regressions where the dependent variable takes a value of 1 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 the IV regressions i later tables. Industry Fixed Effects control for the 7 industries outlined in Table 1. Period Fixed effects control for the startup being funded in the period 1985-90, 1991-1997 or 1998-2004. Standard errors are clustered by quarter.

	1985-2004				Drop if Funding Yea 1998-2000
	(1)	(2)	(3)	(4)	(5)
Log of number firms financed in that quarter	0.094***	0.102***	0.097***	0.137***	0.057***
	(0.008)	(0.007)	(0.007)	(0.010)	(0.020)
Log $ 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

Pre-money valuation at IPO based on when startup received first funding

This table reports the results from regressions looking at the pre-money value of firms that had an IPO, based on the quarter in which they received their first funding. Coefficients in the table are from OLS specifications where the dependent variable is the log of the pre-money valuation on the day the firm IPO'ed. Industry Fixed Effects control for the 7 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.

	1985-2004				Drop if Funding Year = 1998-2000
	(1)	(2)	(3)	(4)	(5)
Log of number firms financed in that 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 $ 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

Table 5

Funding environment and startup outcomes - Investor fixed effects

This table reports the results from OLS regressions where the data used to estimate these regressions is at the investor-startup level. We report three sets of specific
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 measure as hav
invested in at least 5 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 co
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 exac
same as in Tables 3 and 4. In addition, all regressions include investor fixed effects. Standard errors are clustered by quarter.

	Probability of Failure			Pre-Money Value conditional on I		
	All Investors	Investors >= 5 Investments in Prior 2 Years	Investors < 5 Investments in Prior 2 Years	All Investors	Investors >= 5 Investments in Prior 2 Years	Inves Invest Prior
	(1)	(2)	(3)	(4)	(5)	
Log of number firms financed in that quarter	0.134***	0.130***	0.139***	0.158**	0.233***	0
	(0.011)	(0.014)	(0.012)	(0.069)	(0.082)	(0
Control Variables	Yes	Yes	Yes	Yes	Yes	
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	
Investor Fixed Effects	Yes	Yes	Yes	Yes	Yes	
Number of observations	22,011	8,663	13,348	2,959	1,407	1
R-squared	0.22	0.15	0.19	0.77	0.72	

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 other firms financed in a given quarter is instrumented with a variable that measures the total dollars raised by leveraged buyout funds that closed in the 5-8 quarters before the firm was funded. 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.

	Probability of Failure		Pre-Money Value conditional on IPO	
	OLS (Reg (4) in Table 3)	IV	OLS (Reg (4) in Table 4)	IV
	(1)	(2)	(3)	(4)
Log of number firms financed in that 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		0.473***		0.360***
		(0.119)		(0.077)
Partial R-squared		0.171		0.197
F-Statistic		15.67		21.09

Table 7

The effect of increased capital at time of funding on startup outcomes - Investor fixed effects

This table reports the results of two stage least squares regressions using data at the investor-startup level. As with Table 6, the number of other firn financed in the quarter the firm received its first funding is instrumented with the dollars raised by buyout funds in the 5-8 quarters before the focal investment. 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 variabl and fixed effects are as reported in Table 6. In addition, all regressions include investor fixed effects. Standard errors are clustered by quarter.

	Probability of Failure		Pre-Money Value conditional o IPO	
	OLS (Reg (1) in Table 5)	IV	OLS (Reg (4) in Table 5)	IV
	(1)	(2)	(3)	(4)
Log of number firms financed in that 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		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 coefficients from OLS and IV regressions looking at the patenting activity of firms that IPO'ed while they were still private. The number of other firms financed in the quarter the firm received its first funding is instrumented with the dollars raised by buyout funds in 5-8 quarters before the focal investment. In order to remain consistent with the 1985-2004 funding period, we examine patents filed in the three year following first funding and the citations per patent upto 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 1985-90, 1991-1997 or 1998-2004. Standard errors are clustered by quarter.

	Log Patents filed in 3 years following first funding		Log citation per patent	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Log of number firms financed in that 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		0.519***		0.519***
		(0.094)		(0.094)
Partial R-squared		0.359		0.359
F-Statistic		30.45		30.45

Table 9

Funding environment and startup innovation - 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.

	Log Patents filed in 3 years following first funding		Log citation per patent	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Log of number firms financed in that quarter	0.182**	0.239***	0.161**	0.202**
	(0.069)	(0.097)	(0.076)	(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		0.467***		0.467***
		(0.091)		(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 - Instrumental Variables

This table reports the results of OLS and IV regressions using data at the investor-startup level. Columns (1) and (2) estimate regressions where the dependent variable is the log of the startups age at first funding. Columns (3) and (4) report results from regressions where the dependent variable is the number of venture capital investors in the first funding syndicate. The number of other firms financed in the quarter the firm received its first funding is instrumented with the dollars raised by buyout funds in the 5-8 quarters before the focal investment. Control variables and fixed effects are as reported in Table 7. Standard errors are clustered by quarter.

	Startup's age at first funding		Syndicate size at first funding	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Log of number firms financed in that quarter	-0.148***	-0.295***	-0.030***	-0.108***
	(0.030)	(0.077)	(0.009)	(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		0.416***		0.425***
		(0.107)		(0.112)
Partial R-squared		0.150		0.150
F-Statistic		15.12		14.47

Table A1

Valuation Conditional on all exits above $50M

This Table reports the results from regresions used in columns (3) and (4) of Table 6. However, as a robustness check, the sample includes all firms with an exit above $50M instead of using only firms that had an IPO. This includes acquisitions above $50M and also excludes IPOs with a premoney value below $50M.

	Pre-Money Value on all Exits > $50M	
	OLS	IV
	(1)	(2)
Log of number firms financed in that 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		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 regressions looking at the value of firms that had an IPO or were acquired for more than $50M, based on the quarter in which they received their first funding. Coeffieints in the table are from quantile regressions (estimated at the median) where 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.

	Pre-Money Value conditional on IPO	Pre-Money Value on all Exits > $50M
	(1)	(2)
Log of number firms financed in that quarter	0.184***	0.063*
	(0.054)	(0.034)
Firm's Age at IPO	-0.016**	-0.007
	(0.007)	(0.004)
Log total $ raised prior to IPO	0.403***	0.335***
	(0.028)	(0.019)
Log value of NASDAQ on day of IPO	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

Probability of an IPO based on market when the startup received first funding

This table reports the probability of a startup being coded as having an IPO based on the characteristics of the VC funding environment when it first received funding. All regressions are OLS regressions where the dependent variable takes a value of 1 if the startup had an IPO and zero otherwise. Industry Fixed Effects control for the 7 industries outlined in Table 1. Period Fixed effects control for the startup being funded in the period 1985-90, 1991-1997 or 1998-2004. Standard errors are clustered by quarter.

	1985-2004				Drop 98-'00
	(1)	(2)	(3)	(4)	(5)
Log of number firms financed in that quarter	-0.100***	-0.105***	-0.100***	-0.025***	-0.084***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.014)
$ raised in 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