VENTURE CAPITAL, ENTREPRENEURSHIP, AND ECONOMIC GROWTH

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Abstract—Using a panel of U.S. metropolitan areas, we find that increases in the supply of venture capital positively affect firm starts, employment, and aggregate income. Our results remain robust to a variety of specifications, including ones that address endogeneity. The estimated magnitudes imply that venture capital stimulates the creation of more firms than it funds, which appears consistent with two mechanisms: First, would-be entrepreneurs anticipating financing needs more likely start firms when the supply of capital expands. Second, funded companies may transfer know-how to their employees, thereby enabling spin-offs, and may encourage others to become entrepreneurs through demonstration effects.

I. Introduction

ANALYSTS, bureaucrats, business leaders, politicians, and pundits have widely pointed to venture capital (VC) as an important factor underlying the economic growth of certain regions within the United States, such as Silicon Valley, as well as of the country as a whole (Bottazzi & Rin, 2002). These commentators have similarly attributed slow growth to the relative scarcity of venture capital in states from Alaska to Florida, and in nearly every country aside from the United States. Several governments, including those of Canada, Chile, Germany, and Israel, in the interest of stimulating their economies, have even sought to expand their local supplies of venture capital by way of public policy (Gilson, 2003; Cumming & MacIntosh, 2007).

Despite the widespread interest in venture capital as a stimulus for economic growth, little empirical research has examined the validity of these claims (for an exception, see Hasan & Wang, 2006). At first blush, positive relationships among venture capital, entrepreneurship, and economic growth might appear a forgone conclusion, but these relationships in fact rest on two (potentially inaccurate) assumptions: a presumption that VC-funded firms would not have come into being without venture capital and a belief that those employed at these VC-funded firms generate substantially more value for the economy than they would have in other firms. Although firm-level studies have found that VC-funded companies enjoy higher employment and sales growth rates than the average start-up (Jain & Kini, 1995; Engel & Keilbach, 2007), one cannot easily extrapolate from these firm-level relationships to the implications of venture capital for the economy as a whole. It is quite possible, for example, that VC firms simply select the more promising start-ups and substitute for other forms of financing that those ventures would have used had venture capital been unavailable. The macrolevel relationships among the supply of venture capital, entrepreneurship, and economic growth therefore remain open questions.

To determine whether the availability of venture capital stimulates the formation of new firms, and in turn contributes positively to economic growth, we exploited both cross-sectional and longitudinal variation in the supply of venture capital across and within Metropolitan Statistical Areas (MSAs). We estimated the local effects of venture capital activity—measured in terms of the number of companies funded, the number of investments made, and the aggregate dollars invested—on the number of firms established and on employment and aggregate income. Since the supply of venture capital itself may depend in part on the demand for it—that is, on the availability of high-potential businesses in which to invest—we also used endowment returns as an instrument to identify the supply of venture capital. Our results remained robust to these specifications.

Our findings imply that venture capital stimulates start-up activity. A doubling in the number of firms funded by venture capitalists in a region results in the establishment of 0.48% to 2.21% more new establishments on average (depending on the estimation approach). For the average MSA, a doubling means moving from having four firms funded per year to having eight firms funded per year. Our estimates therefore imply that investing in an additional firm would stimulate the entry of two to twelve establishments—in other words, more new firms than actually funded. A doubling in the number of firms funded by venture capital also results in a 0.22% to 1.24% expansion in the number of jobs and a 0.48% to 3.78% increase in aggregate income. These results appear consistent with either of two potential mechanisms. First, nascent entrepreneurs may recognize the need for capital in the future and establish firms only when they perceive reasonable odds of obtaining that funding. Second, VC-funded firms may encourage others to engage in entrepreneurship through a demonstration effect or by training future firm founders.

Consistent with the theoretical literature, an expansion in the availability of financial intermediaries—in this case, venture capitalists—stimulates economic development (Greenwood & Jovanovic, 1990; Keuschnigg, 2004). Our findings therefore contribute to the literature that has been attempting to explain cross-regional differences in economic outcomes (Glaeser et al., 1992; Rosenthal & Strange, 2003). Variation in the availability of venture capital nevertheless appears to account for at best a small amount of this cross-regional heterogeneity. Despite having effects beyond the firms it finances, venture capital operates at a small scale. Even in Silicon Valley, it funds fewer than 4% of new firms and invests at a level of less than 1% of GDP. As a result, even our largest estimates suggest that the gap between the top decile and bottom decile regions would decline by no more than 13% in the

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absence of venture capital. Although venture capital plays an important role in the economy, it is far from a panacea.

II. Venture Capital and Economic Growth

Venture capital, the funding of high-potential companies through equity investments by professional financial intermediaries, has existed in the United States for more than sixty years. Despite some prominent early success stories, these intermediaries nonetheless played only a minor role prior to the 1980s (Gompers & Lerner, 1998). Since then, however, their prominence has been rising rapidly; according to the National Venture Capital Association (NCVA), from 1978 to 2007, the total funds raised by venture capital firms in the United States grew from $549 million (in 2007 dollars) to $35.9 billion.

In the United States, venture capital firms have evolved toward a common organizational form. Each firm consists of one or more limited partnerships, called funds, with life spans of ten to twelve years. The capital in these funds comes from passive limited partners, primarily wealthy individuals and institutional investors, such as college endowments, insurance companies, and pension funds. The general partners, often referred to as venture capitalists, actively manage this capital—identifying attractive investments and then monitoring and advising the companies in which they invest to maximize their returns. In exchange for their services, venture capitalists receive both some fixed compensation and a potentially sizable portion of the capital gains earned on these investments. They therefore have strong incentives to choose their portfolio companies wisely and nurture them as effectively as possible.

Evidence from firm-level studies generally suggests that venture capitalists produce value through the selection and advising of portfolio companies.[1] Jain & Kini (1995), for example, found that firms financed by venture capital grew faster in both sales and employment. But the interest in venture capital reflects not only its value to those investing in it, but also its potential to contribute to the economy as a whole by promoting the development of high-growth companies that create jobs and generate wealth.

A. *Selection and Substitution*

Although firm-level studies find evidence consistent with the idea that venture capital firms create value for their investors, at least two important issues arise in attempting to move from these studies to the potential benefits of venture capital to the economy as a whole: Would these companies have received funding from other sources in the absence of venture capital? and How much of the value of venture capital at the firm-level stems from preinvestment activities (selection)? If these companies had found other sources of funding, then venture capitalists may do little more than help their limited partners to find these investments. Even if venture capital firms do alleviate entrepreneurs' capital constraints, firm-level studies could overestimate the benefits of this capital to the economy as a whole if venture capitalists cherry-pick the best investments.

Although research has not directly investigated the first issue, the literature on wealth and entrepreneurship suggests that insufficient financial resources may prevent many from starting their own businesses. Evans and Jovanovic (1989) and Blanchflower and Oswald (1998), for example, have found that the odds of becoming an entrepreneur rise with household wealth. To the extent that access to financial resources forms a binding constraint on the ability of individuals to engage in entrepreneurship, one might then expect venture capital—as well as other institutions that alleviate these constraints—to stimulate growth by ensuring that good ideas receive funding (Keuschnigg, 2004).

With respect to the second issue—the degree to which venture capitalists add value through their preinvestment activities—at least two recent studies suggest that selection accounts for a substantial portion of the returns to venture capital investing. In a sample of German companies, Engel & Keilbach (2007) found that companies receiving venture capital had more patents at the time of funding than the average start-up. But once they controlled for this difference (through matching), these companies proved no more innovative after receiving VC funding. Similarly, Sorensen (2007), using a structural model to identify pre- versus postinvestment processes, has estimated that roughly two-thirds of the variation across venture capital firms in the probability that their portfolio companies would go public stems from preinvestment sorting processes (selection). Hence, even if venture capital does alleviate capital constraints, selection could still lead extrapolations from firm-level studies to overestimate the benefits of venture capital to the economy.

B. *Expectations and Spin-Offs*

Two other factors, expectations and spin-offs, however, suggest that venture capital may encourage the founding of even more companies than it funds directly. Consider expectations first. If potential entrants assess their odds of success before attempting entry, then the availability of venture capital should have a positive effect on the evaluations of a number of capital-constrained would-be entrepreneurs. Though one might expect entrepreneurs to secure this funding prior to entry, thereby limiting the effects of venture capital to the companies it actually funds, entrepreneurs often enter first and pursue financing later for two reasons.[2] Beginning operations first allows the founder to retain a larger share of the equity, thereby giving him a financial incentive to found the firm—if possible—without outside funding. Also,

[1] Analyses of the financial returns to venture capital investments paint a similar picture (Chen, Baierl, & Kaplan, 2002; Cochrane, 2005).

[2] Consistent with this expectation, the median company in our data does not receive its first round of venture capital investment until 1.6 years after its establishment.

because of the information asymmetries inherent between entrepreneurs and investors, many venture capitalists avoid investing in companies that have not already achieved rudimentary milestones—perhaps filing for a patent or creating a prototype of a product.

A second mechanism through which venture capital may engender entrepreneurship is through spin-offs—that is, through employees in incumbent firms leaving to start their own companies. Venture capital can encourage spin-offs in at least two ways. The first is a demonstration effect. When interviewed, entrepreneurs often say that they first thought of starting a company when they saw someone else do it, potentially even in a different industry (Sorenson & Audia, 2000). Seeing others engage in entrepreneurship can encourage would-be entrepreneurs to start firms. The second is a training effect. Small, entrepreneurial firms operate differently from larger, more bureaucratic ones. Prior experience in small (VC-backed) companies allows would-be entrepreneurs to absorb tacit knowledge on how to design and manage effective entrepreneurial ventures.

Because both the expectations and spin-off mechanisms imply effects external to the companies that actually receive venture capital funding, their influence would not appear in firm- or investment-level studies. We must move to a more macro level of analysis. Research at that level has been scarce. Some evidence exists for a positive relationship between venture capital and patenting (Kortum & Lerner, 2000; Hasan & Wang, 2006), though those results may reflect compositional differences in which industries attract venture capital (Gans & Stern, 2003). Hasan and Wang (2006) have also found positive correlations between regional per capita venture capital activity and firm foundings per capita and GDP growth, though those correlations may reflect an attraction of venture capital to high-growth regions. Our understanding of these relationships remains limited. We therefore investigated the degree to which venture capital stimulates the production of firms, jobs, and aggregate income.

III. Empirical Evidence

To assess these issues, we constructed an unbalanced panel covering all 329 Metropolitan Statistical Areas (MSAs) in the United States from 1993 to 2002. Our data comprise information from a variety of publicly available and proprietary sources. The data on regional economic activity came from the Office of Advocacy of the Small Business Administration (SBA), which reports information collected by the Census Bureau. Our information on venture capital has been derived from Thomson Reuters' VentureXpert database, and our measures of endowment returns came from the *Chronicle of Higher Education*.

We chose MSAs as our geographic unit of analysis because they offered the most finely-grained regions that one might reasonably consider independent with respect to economic activity. The U.S. Office of Management and Budget (OMB) defines each MSA in terms of a core urban area of at least 50,000 inhabitants. It also includes in each MSA any surrounding counties with a high degree of social and economic integration with the urban core.[3] In practice, the OMB assesses social and economic integration by observing commuting patterns. If more than 25% of a county's residents commute to the urban core for work, the OMB includes the county in the MSA.

We limited our analyses to a ten-year window, from 1993 to 2002, because the construction of the panel requires consistent definitions of the regional units of analysis across years. Roughly three years after each decennial census, the OMB redefines the statistical areas for the next ten years on the basis of the decennial data. Developing consistent regions across these redefinitions would require a host of assumptions regarding the distribution of activity within each MSA. The 1993 redefinition governed the reporting of most government statistics from 1993 to 2002. Because a few regions became classified as MSAs only after 1993, our panel consists of a total of 3,270 MSA-years.

A. *Cross-Sectional Estimates*

We began our analyses by estimating effects from cross-sectional variation across MSAs. In particular, we examined the effects of venture capital on three different outcomes five years in the future: the number of business establishments in the region, overall employment in the region, and aggregate income for the region.[4] The Census Bureau defines a business establishment as a single physical location where business occurs and for which a firm maintains payroll and employment records. All firms have at least one establishment, but many have more than one. Our measure of employment includes both full-time employees and (the full-time equivalent of) part-time employees. Aggregate income, labeled "payroll" in the tables, includes all forms of compensation: wages, salary, bonuses, and benefits. For each of these outcomes, we estimated the effects using the following partial linear adjustment model:

$$\ln Y_{i,2000} = \beta_1 \ln F_{i,1995} + \beta_2 \ln E_{i,1995} + \beta_3 \ln P_{i,1995} + \beta_4 \ln \sum_{s=1990}^{1994} I_{i,s} + \beta_5 \ln \sum_{s=1990}^{1994} VC_{i,s} + \epsilon_i, \quad (1)$$

[3] In contrast to the rest of the country, in New England, the Census Bureau uses townships instead of counties to determine the boundaries of MSAs.

[4] Alternatively, one might focus only on establishments started and employment in the industries most relevant to venture capital. Restricting the range of industries included would nevertheless have two disadvantages. From a practical point of view, venture capital firms have invested in nearly every two-digit SIC code, so we would have little ability to discriminate across industries in their potential for receiving venture capital. But also, from a theoretical perspective, to the extent that venture capital stimulates the founding and growth of companies not directly funded by it, excluding some industries from the analyses would result in downward bias in the effects of venture capital on the economy.

where i indexes the MSA observed at two points in time: 1995 and 2000.[5] $Y_{i,2000}$ denotes the dependent variable (establishments, employment, and payroll). $F_{i,1995}$, $E_{i,1995}$, and $P_{i,1995}$ are the number of establishments, employees, and the payroll in the region, respectively. In each model, one of these measures serves as a lagged dependent variable. $I_{i,s}$ controls for innovation in the region (through patent counts), $VC_{i,s}$ measures the supply of venture capital, and ϵ_i represents a normally distributed error term.

Both the opportunities to create firms and invest in venture capital might depend on the arrival of technological opportunities ($I_{i,t1}$). To control for these opportunities, we used the count of patent applications (eventually approved) made by inventors located in an MSA between 1990 and 1994. If a patent application had multiple inventors listed (n), we assigned $1/n$ patents to the MSA of each inventor.

We considered three measures of the supply of venture capital. In computing all three of these measures, we restricted the VentureXpert data to limited partnerships with a stated focus on seed-stage, early-stage, later-stage, expansion, development, or balanced-stage investing.[6] The VentureXpert database includes information on many private equity partnerships that do not invest in early-stage companies, such as LBO, real estate and distressed debt funds, and funds of funds. The fund focus information allowed us to remove these non-VC private equity investors from the data. Restricting the analyses to limited partnerships also effectively eliminated investments by angel investors and corporate venture capital arms, as well as direct investments by university endowments and other institutional investors. Although these other forms of financing early-stage companies may also have important effects on entrepreneurship and economic growth, the validity of the instrument we introduce depends on limited partners' demand for private equity investments. We therefore restricted our analyses to funds with limited partners.

Our first measure of venture capital activity counted the number of firms funded between 1990 and 1994 by VC firms located in a particular MSA.[7] To focus on the number of companies funded, we counted only initial investments in target companies.[8] We included all target companies in this count regardless of whether those companies resided in the same MSA as the VC firm.[9] So, for example, if a San Diego target company received three rounds of capital infusions from a venture capital firm located in Orange Country, we would increment this measure by 1 in Orange County in the year of the first investment. Because some regions have no activity, we added 1 to this count before logging it.

Our second measure paralleled the first but counted all investments made between 1990 and 1994 by VC firms located in the MSA (again regardless of the location of the investment target). This measure should also capture the effects of continuing support for companies that have already received venture capital. We again added 1 to this count before logging because some regions have no activity.

Our third measure summed the total amount of money invested each year by VC firms located in the MSA.[10] In essence, this measure weights the investments in the second measure by their size in dollars to determine whether larger investments have larger effects. As with the other two measures, we added 1 to the sum before logging it. Descriptive statistics for the variables used appear in table 1.

Table 2 reports the results of the cross-sectional analyses. Beginning with the first three columns, venture capital appears to have a small effect, at most, on the number of

TABLE 1.—SUMMARY STATISTICS

Variable	Mean	S.d.	N
Cross-section			
Firms 2000	15,781.5	26,082.8	323
Firms 1995	14,692.8	24,242.1	323
Employment 2000 (1,000s)	299.9	517.5	323
Employment 1995 (1,000s)	262.9	453.4	323
Payroll 2000 (millions)	\$10,723.1	22,361.7	323
Payroll 1995 (millions)	\$7,290.1	14,679.1	323
Patents 1990–1994	882.7	1,767.5	323
VC count first 1990–1994	7.1	48.6	323
VC count, all 1990–1994	34.4	238.0	323
VC amount, 1990–1994 (millions)	28.9	205.2	323
Panel			
Firm births	1,415.2	2,523.6	3270
Employment (1,000s)	275.5	478.6	3264
Payroll (millions)	\$8,725.4	18,339.1	3264
Population (1,000s)	659.4	1,104.9	3270
Patents	235.4	534.1	3270
VC count first	4.2	27.5	3270
VC count all	15.9	106.4	3270
VC amount (millions)	\$39.8	340.1	3270

Areal unit is the Metropolitan Statistical Area (MSA). Panel data covers period 1993–2002. See the text for details on variable construction.

[5] In unreported models, we experimented with longer lags between the predictors and the outcomes, up to a maximum of nine years, using 1993 values to predict 2002 outcomes. These longer lags produced nearly identical coefficient estimates, suggesting that most of the effects of venture capital at the regional level occur in the first five years following funding.

[6] We also restricted the individual investments used in constructing our count and amount variables to those that fell into these stages.

[7] The Census Bureau polls the country each March. The firm birth and employment data therefore count the number of new firms from the beginning of April of a given year to the end of March in the next year and employment at the end of March. Payroll meanwhile aggregates income in each region from January to December. We have used the exact dates of investments from the venture capital data to align the timing of investments to the relevant calendar for each dependent variable.

[8] In cases that involved multiple VC firms investing in a single target, we counted each VC firm as having made one investment.

[9] Although VC firms tend to invest in close proximity to their offices and therefore in firms located in the same MSA (Sorenson & Stuart, 2001), they sometimes do invest farther away. Alternatively, therefore, one might locate investments according to the headquarters of the companies funded (rather than by the location of the VC firms). We dismissed this alternative because the logic of our instrumental variable allows us to identify the local supply of venture capital rather than its deployment. To the extent that VC firms invest outside their MSAs, however, our estimates should err on the side of being conservative.

[10] Although VentureXpert includes relatively complete information on the size of investment rounds (95% of our cases), it does not contain information on the proportion of the funding in these rounds contributed by each investor. In the absence of more detailed information, we allocated the total funds invested in a round equally across all investors in that round.

TABLE 2.—CROSS-SECTIONAL ANALYSIS OF THE IMPACT OF VENTURE CAPITAL INVESTMENTS

	Dependent Variables from Year 2000								
	Ln Firms (1)	Ln Firms (2)	Ln Firms (3)	Ln Empl (4)	Ln Empl (5)	Ln Empl (6)	Ln Pay (7)	Ln Pay (8)	Ln Pay (9)
Ln Firms 1995	1.019***	1.017***	1.020***	0.0727***	0.0693***	0.0753***	0.125***	0.118***	0.139***
	(.01696)	(.01696)	(.01686)	(.02251)	(.02248)	(.02243)	(.03562)	(.03544)	(.03623)
Ln Employment 1995	0.0635*	0.0701**	0.0606*	1.037***	1.047***	1.028***	−0.0204	0.0000337	−0.0654
	(.03362)	(.03376)	(.03314)	(.04465)	(.04476)	(.0441)	(.07064)	(.07056)	(.07122)
Ln Payroll 1995	−0.0897***	−0.0950***	−0.0876***	−0.108***	−0.116***	−0.101***	0.881***	0.867***	0.917***
	(.02566)	(.02575)	(.02525)	(.03407)	(.03414)	(.03359)	(.05391)	(.05382)	(.05425)
Ln Patents 1990–1994	0.0173***	0.0171***	0.0166***	0.0144***	0.0139***	0.0132**	0.0347***	0.0331***	0.0303***
	(.00406)	(.00405)	(.00408)	(.00539)	(.00536)	(.00543)	(.00853)	(.00846)	(.00876)
Ln VC Cnt First 1990–1994	0.00451			0.00873**			0.0389***		
	(.00312)			(.00415)			(.00656)		
Ln VC Cnt All 1990–1994		0.00455**			0.00816***			0.0300***	
		(.00224)			(.00297)			(.00468)	
Ln VC Amount 1990–1994			0.000810			0.00129*			0.00509***
			(.00053)			(.00071)			(.00114)
R^2	1.00	1.00	1.00	1.00	1.00	1.00	0.99	0.99	0.99
Observations	323	323	323	323	323	323	323	323	323

OLS regression results; $^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. The unit of observation is the MSA, and the data include all MSAs in the 48 contiguous United States. The dependent variables reflect year 2000 values: the explanatory variables reflect either 1995 values or, for venture capital and patents, sums over 1990–1994. See the text for details on variable construction.

establishments. Only the estimates for the second measure, the count of all investments, is sufficiently precise for us to reject the null hypothesis of no effect. That fact does not imply that venture capital does not increase entrepreneurship, but it does suggest either that these start-ups displace incumbents in the region or do not survive long on average. The local supply of venture capital does, however, appear to increase both employment and aggregate income in the region. The magnitudes of these effects are substantial. Depending on the measure used, a doubling in venture capital investments from 1990 to 1994 implies 0.09% to 0.61% higher employment and 0.35% to 2.73% greater aggregate income in the region in 2000. The larger marginal effect of venture capital on income relative to employment suggests either that venture capital produces particularly high-paying jobs or that the greater availability of entrepreneurship and small-firm employment as an outside option places upward pressure on the wages paid by existing employers.

Although the long lags suggest a causal relation, these cross-sectional estimates may nonetheless confound the effects of venture capital with a wide range of other factors that vary from one region to the next. To investigate these issues further, we turned to analyses that used longitudinal variation within regions to identify the effects of venture capital on the economy.

B. Panel Estimates

Our panel estimates began with specifications that included region fixed effects to control for time-invariant characteristics of MSAs that might both attract venture capital and influence entrepreneurship and economic growth. They also incorporated indicator variables for calendar years to control for macroeconomic factors that might commonly influence the outcomes and venture capital. The tables that follow report estimates with and without region-specific trends (to capture regional differences in the average growth rates in entrepreneurship, employment, and aggregate income over time). In particular, we estimated a logged form of a standard production function:

$$\ln Y_{i,t} = \beta_1 \ln I_{i,t-1} + \beta_2 \ln Pop_{i,t-1} + \beta_3 \ln VC_{i,t} + \phi_t + \eta_i + \nu_i t + \epsilon_{i,t}, \quad (2)$$

where i and t index the MSA and the year, respectively. As above, $Y_{i,t}$ denotes the various outcome measures, $I_{i,t-1}$ indicates innovation (patent applications), and $VC_{i,t}$ represents venture capital activity. We again considered three measures of the supply of venture capital: first investments made, all investments made, and the total amount invested each year by VC firms located in the MSA.[11] The panel models also included a control for MSA population ($Pop_{i,t}$), a series of indicator variables for each year (ϕ_t), MSA fixed effects (η_i, partialed out), and (in some models) an MSA-specific growth trend ($\nu_i t$). We clustered the standard errors on MSAs to allow correlation in the errors within regions across years.

Although the employment and aggregate income outcomes parallel those used in the cross-sectional analysis, in the panel models, we used a count of establishment births in the category of zero to nineteen employees—rather than the count of all establishments—to focus on entrepreneurial activity.[12]

[11] Because all three measures include some regions with no venture capital activity in a year, we added 1 to each measure before logging it.

[12] The Census Bureau's count of new establishments captures both relocations by existing businesses across MSAs and the opening of new plants and places of business by existing firms. To distinguish these activities from entrepreneurship, we used information on the size of the firm at the time of entry. Based on the total employment of organizations (across all business locations), the SBA splits establishment births into three categories: 0–19 employees, 20–499 employees, and over 500 employees. A large firm establishing a small local branch would appear in the 500+ category regardless of the number of individuals employed at the local branch. New firms should appear only in the 0–19 employee category (though this category might still include some relocations or expansions of small existing firms).

TABLE 3.—IMPACT OF VC ON REGIONAL ECONOMY: OLS FIXED EFFECTS

	(1) Ln Births	(2) Ln Births	(3) Ln Births	(4) Ln Empl	(5) Ln Empl	(6) Ln Empl	(7) Ln Pay	(8) Ln Pay	(9) Ln Pay
Ln Patents $(t-1)$	0.0157**	0.0155**	0.0161**	0.0115***	0.0117***	0.0123***	0.0246***	0.0242***	0.0262***
	(.00697)	(.00697)	(.00696)	(.00318)	(.00321)	(.00325)	(.00546)	(.00553)	(.00581)
Ln Population $(t-1)$	0.798***	0.796***	0.800***	0.807***	0.809***	0.814***	1.084***	1.075***	1.096***
	(.07836)	(.07856)	(.07821)	(.03823)	(.03837)	(.03886)	(.06213)	(.06233)	(.06591)
Ln VC Cnt First	0.0111***			0.00868***			0.0256***		
	(.00402)			(.00219)			(.00461)		
Ln VC Cnt All		0.00940***			0.00618***			0.0224***	
		(.00336)			(.00215)			(.00422)	
Ln VC Amount			0.000967**			0.000323			0.000933*
			(.00039)			(.00031)			(.00048)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA trend	No	No	No	No	No	No	No	No	No
R^2	0.22	0.22	0.22	0.81	0.81	0.81	0.93	0.93	0.93
Clusters	328	328	328	328	328	328	328	328	328
Observations	3,270	3,270	3,270	3,264	3,264	3,264	3,264	3,264	3,264

OLS regression results; $^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The unit of observation is the MSA year, and the data cover the 48 contiguous United States from 1993 to 2002. In models 1–3, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In models 4–6 the dependent variable is the total employment in the MSA. In models 7–9 the dependent variable is the total payroll in the MSA. See the text for details on variable construction.

TABLE 4.—IMPACT OF VC ON REGIONAL ECONOMY: OLS FIXED EFFECTS WITH MSA TRENDS

	(1) Ln Births	(2) Ln Births	(3) Ln Births	(4) Ln Empl	(5) Ln Empl	(6) Ln Empl	(7) Ln Pay	(8) Ln Pay	(9) Ln Pay
Ln Patents $(t-1)$	0.00743	0.00751	0.00755	0.000659	0.000670	0.000694	0.00357	0.00354	0.00359
	(.00689)	(.0069)	(.0069)	(.00259)	(.00259)	(.0026)	(.00277)	(.00277)	(.00278)
Ln Population $(t-1)$	−0.143	−0.162	−0.167	0.0903	0.0862	0.0834	0.341***	0.333***	0.317***
	(.16208)	(.16295)	(.16193)	(.1046)	(.10481)	(.10445)	(.12105)	(.12084)	(.12093)
Ln VC Cnt First	0.00784**			0.00325*			0.0103***		
	(.00396)			(.0017)			(.00298)		
Ln VC Cnt All		0.00150			0.00191			0.00944***	
		(.00447)			(.00176)			(.00274)	
Ln VC Amount			−0.000181			−0.00000297			0.000520**
			(.00041)			(.00021)			(.00025)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.50	0.50	0.50	0.91	0.91	0.91	0.97	0.97	0.97
Clusters	328	328	328	328	328	328	328	328	328
Observations	3,270	3,270	3,270	3,264	3,264	3,264	3,264	3,264	3,264

OLS regression results; $^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The unit of observation is the MSA year, and the data cover the 48 contiguous United States from 1993 to 2002. In models 1–3, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In models 4–6, the dependent variable is the total employment in the MSA. In models 7–9, the dependent variable is the total payroll in the MSA. See the text for details on variable construction.

Fixed effects. Table 3 reports the results of these fixed-effects regressions without region-specific trends. All of the measures of venture capital have positive effects on entrepreneurship. As in the cross-sectional models, the results nonetheless suggest that first investments have a larger stimulative effect on entrepreneurship than later investments. A doubling in the number of firms funded implies a 0.78% increase in the number of new establishments.[13] By comparison, a doubling in the number of overall investments corresponds to a 17% smaller effect. Larger investments appear even less effective in promoting entrepreneurship: a doubling in the dollars invested implies an increase of only 0.07% in the number of new companies.

[13] Although one could interpret the log-log coefficients directly as elasticities, we chose not to do so because one cannot meaningfully consider small percentage changes in the number of companies funded (only 11 of the 329 regions had more than 100 companies funded in any single year).

Two of the three measures of venture capital similarly relate positively and significantly to overall employment, and all three measures have positive effects on the payroll. A doubling in the number of firms funded in a region, for example, corresponds to a 0.60% increase in total employment and 1.79% higher income in the region. Follow-on investments, and especially larger investments, again had weaker effects on employment and aggregate income.

Fixed effects with trends. Table 4 presents a parallel set of analyses with region-specific trends. The inclusion of these trends reduces the magnitudes of all of the estimated relationships. For example, the magnitude of the effects of the number of firms funded on entrepreneurship, employment, and aggregate income declines by 29%, 62%, and 60% respectively. The effects associated with all investments and with the total amount of these investments fall to levels indistinguishable from zero with 90% confidence, except in the

TABLE 5.—HIGH-VC-ACTIVITY STATES AND ESTABLISHMENT BIRTHS

	(1) All	(2) No California	(3) No Massachusetts	(4) No Texas	(5) No California, Massachusetts, Texas
Ln Patents ($t-1$)	0.00743	0.00694	0.00779	0.00695	0.00669
	(.00689)	(.00709)	(.00688)	(.00747)	(.00769)
Ln Population ($t-1$)	−0.143	−0.181	−0.124	−0.0548	−0.0635
	(.16208)	(.17195)	(.16092)	(.18255)	(.19532)
Ln VC Cnt First	0.00784**	0.00702*	0.0103***	0.00797*	0.00998**
	(.00396)	(.0041)	(.00385)	(.00417)	(.00417)
Year dummies	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes
MSA trend	Yes	Yes	Yes	Yes	Yes
R^2	0.50	0.50	0.53	0.50	0.54
Clusters	328	303	317	301	265
Observations	3,270	3,020	3,160	3,000	2,640

OLS regression results; $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The dependent variable in all models is the logged births of new establishments for firms with zero to nineteen employees at the beginning of the year. Model 1 includes all MSAs in the 48 contiguous United States. Model 2 excludes MSAs in California. Model 3 excludes MSAs in Massachusetts. Model 4 excludes MSAs in Texas. Model 5 excludes MSAs in California, Massachusetts, and Texas. See the text for details on variable construction.

models predicting payroll. Because of this fact, the robustness checks below focus on the number of firms funded as the measure of venture capital activity.

In terms of translating these effects into absolute numbers, since the average MSA year had four VC-funded companies (and 1,415 establishment births), the funding of one additional company appeared to generate roughly two new establishments. That same additional funded company would correspond to roughly 148 more full-time-equivalent jobs in the typical region. If all of these jobs appeared in the firms stimulated by the increased supply of venture capital, the typical new venture would need to employ more than seventy people. Since that number far exceeds the average size of these ventures, it suggests that at least some of the gains in employment must accrue to existing firms in the region.

The funding of an additional firm also corresponds to an increase of $14.9 million in the wage bill for the region. Since the average venture capital investment deploys only $2.5 million in capital, this income effect suggests that venture capital investments produce a high social return. If all of this income growth stemmed from the incomes associated with the 148 jobs created, either directly or indirectly, by that additional investment, then it would imply that the average job produced by venture capital had a total compensation of about $100,000.

Although most MSAs have at least one local venture capital firm, and therefore contribute to our estimates, one might worry that outliers contribute heavily to these results. Notably, California, Massachusetts, and Texas together accounted for a little over half of all venture capital during the period being analyzed. Table 5 therefore reports the effect of the number of firms funded on establishment births for a series of models that systematically remove MSAs in each of these three states—and in the final column, all of them—from the analysis. The magnitude of the entrepreneurship effect appears insensitive to the exclusion of those regions with the highest volumes of venture capital activity. Similar analyses using employment and aggregate income as dependent variables also revealed little sensitivity in the results to the removal of specific regions.

Temporal structure. Table 6 explores the temporal structure of the number of firms funded by venture capital and the various outcomes. As one would expect, the sizes of the estimated coefficients decline steadily as the lags grow longer, always falling to a level indistinguishable from zero with 90% confidence with more than a one-year lag. At least in terms of the temporal structure, the forward lags suggest that reverse causality does not appear to be a concern.

First differences. Although the region-specific fixed effects and trends remove consistent variation over time within MSAs, one might nonetheless worry about persistence in levels—a common shock simultaneously influencing the supply of venture capital, entrepreneurship, and economic growth. We therefore reestimated the models using first

TABLE 6.—TEMPORAL STRUCTURE OF IMPACT OF VC ON REGIONAL ECONOMY

	(1) Ln Births	(2) Ln Empl	(3) Ln Pay
Ln Patents ($t-1$)	0.00748	0.000644	0.00367
	(.0069)	(.00259)	(.00277)
Ln Population ($t-1$)	−0.141	0.0881	0.344***
	(.16166)	(.10481)	(.12008)
Ln VC Cnt First ($t+1$)	0.000493	−0.000368	−0.00112
	(.00354)	(.00146)	(.00206)
Ln VC Cnt First	0.00781**	0.00302*	0.00919***
	(.00356)	(.00159)	(.00256)
Ln VC Cnt First ($t-1$)	0.00562	0.000687	0.00942***
	(.00439)	(.00199)	(.00311)
Ln VC Cnt First ($t-2$)	0.000715	−0.00239	−0.00273
	(.00407)	(.00174)	(.00281)
Ln VC Cnt First ($t-3$)	0.00628	0.00174	0.00101
	(.00536)	(.0019)	(.00251)
Year dummies	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes
MSA trend	Yes	Yes	Yes
R^2	0.50	0.91	0.97
Clusters	328	328	328
Observations	3,270	3,264	3,264

OLS regression results; $^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year, and the data cover MSAs in the 48 contiguous United States from 1993 to 2002. In model 1, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In model 2, the dependent variable is the total employment in the MSA. In model 3, the dependent variable is the total payroll in the MSA. See the text for more details on variable construction.

TABLE 7.—IMPACT OF VC ON REGIONAL ECONOMY: OLS FIRST DIFFERENCES

	(1) Ln Births	(2) Ln Births	(3) Ln Empl	(4) Ln Empl	(5) Ln Pay	(6) Ln Pay
Ln Patents ($t-1$)	0.00329	0.00229	−0.000576	−0.00178	0.00336*	0.00167
	(.00723)	(.00747)	(.00175)	(.00176)	(.00184)	(.00187)
Ln Population ($t-1$)	0.310***	−0.282*	0.427***	−0.0350	0.696***	0.211*
	(.09809)	(.1633)	(.04604)	(.08447)	(.06237)	(.11052)
Ln VC Cnt First	0.00815**	0.00696**	0.00416***	0.00339***	0.00812***	0.00689***
	(.00335)	(.00349)	(.00119)	(.00115)	(.00181)	(.00172)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA trend	No	Yes	No	Yes	No	Yes
R^2	0.11	0.12	0.26	0.26	0.29	0.27
Clusters	328	328	328	328	328	328
Observations	2,942	2,942	2,935	2,935	2,935	2,935

OLS regression results; $^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. Models estimated on the first differences of the all variables. Models 2, 4, and 6 include an MSA-specific growth trend estimated by applying a fixed-effects estimator to the differenced equation. The unit of observation is the MSA-year and the data cover MSAs in the 48 contiguous United States from 1993 to 2002. In models 1 and 2, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In models 3 and 4, the dependent variable is the total employment in the MSA. In models 5 and 6, the dependent variable is the total payroll in the MSA. See the text for more details on variable construction.

TABLE 8.—DYNAMIC PANEL ESTIMATES

	(1) Ln Births	(2) Ln Births	(3) Ln Empl	(4) Ln Empl	(5) Ln Pay	(6) Ln Pay
Estimator	OLS FE	Arellano-Bond	OLS FE	Arellano-Bond	OLS FE	Arellano-Bond
Ln Births ($t-1$)	0.263***	0.280***				
	(.02997)	(.09566)				
Ln Births ($t-2$)	0.110***	0.190***				
	(.02602)	(.05433)				
Ln Empl ($t-1$)			0.675***	0.586***		
			(.01946)	(.07836)		
Payroll ($t-1$)					0.743***	0.726***
					(.01919)	(.05005)
Ln Population ($t-1$)	0.505***	0.300**	0.201***	0.0943	0.222***	0.0773
	(.07051)	(.1243)	(.02784)	(.06657)	(.03549)	(.06811)
Ln Patents ($t-1$)	0.00819	0.00279	0.00213	−0.000444	0.00396	0.000379
	(.00678)	(.00865)	(.00199)	(.0023)	(.0029)	(.00297)
Ln VC Cnt F	0.00636*	0.0256***	0.00563***	0.0135***	0.0123***	0.0264***
	(.00353)	(.00614)	(.00131)	(.00268)	(.00232)	(.00485)
Ln VC Cnt F ($t-1$)					0.00364*	0.00369
					(.00187)	(.00297)
Hansen's J (p-value)		0.705		0.750		0.769
AR(2) (p-value)		0.555		0.201		0.549
Clusters	328	328	328	328	328	328
Observations	2,614	2,286	2,935	2,606	2,935	2,606

$^*p < 0.10$. $^{**}p < 0.05$. $^{***}p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The number of DV lags chosen to remove autoregression of order 2. The number of VC lags chosen to match the number of significant lagged coefficients in table 6. In Arellano-Bond regressions, the lagged differences of the DV and of VC Cnt First were instrumented by lagged levels of two or more years. The unit of observation is the MSA year, and the data cover MSAs in the 48 contiguous United States from 1993 to 2002. In models 1 and 2, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In models 3 and 4, the dependent variable is the total employment in the MSA. In models 5 and 6, the dependent variable is the total payroll in the MSA. See the text for more details on variable construction.

differences, both with and without region-specific trends.[14] Table 7 reports the results of these models. The effects estimated from first differences are of the same approximate magnitude as those estimated in the fixed-effects models with region-specific trends.

Dynamic panels. An additional concern is that the changes in the dependent variables might depend on past realizations of those variables. In table 8, we reestimated the first-differenced models with the technique proposed by Holtz-Eakin, Newey, and Rosen (1988) and Arellano and Bond (1991) and implemented in `xtabond2` (Roodman, 2003), including lagged dependent variables as explanatory variables and using the lagged levels as instruments for the first-differenced variables. The first column for each dependent variable presents a model with lagged dependent variables estimated through OLS fixed effects. Because the lagged dependent variable and venture capital are potentially endogenous, however, these OLS results may be biased. The second column for each dependent variable therefore presents a set of estimates using lagged levels of the lagged dependent variables and of venture capital as instruments for their first-differenced counterparts. The p-values for AR(2) and Hansen's J suggest that the models are well specified. The estimated effects of venture capital in these models remain significant and actually increase in magnitude.

[14] In the differenced models, MSA-specific intercepts correspond to region-specific trends.

Instrumental variable. Although the fixed effects and first-differences estimates further support the findings of the cross-sectional estimates that venture capital promotes entrepreneurship, employment, and income growth, their validity nonetheless rests on an assumption that the supply of venture capital does not itself depend on entrepreneurship. As noted above, however, that seems unlikely. Venture capitalists choosing a region in which to locate their offices would presumably find places with more entrepreneurial activity more attractive, and even if venture capitalists tend to emerge out of local communities, their interest in raising—and ability to raise—funds may well depend on perceptions of the degree to which the region offers attractive investment opportunities.

To address these potential endogeneity problems, we also identified the effect using an instrumental variable (IV): returns to the portfolios of limited partners (LPs).[15] Our specification of the IV models follows that described in equation (2), except that we instrumented the supply of venture capital. We used the LIML estimator for these models because of its greater robustness over 2SLS in terms of removing the OLS bias from the estimates (Stock & Yogo, 2005).[16]

Our instrument, LP Returns, relies on the demand for alternative assets by limited partners. Institutional investors generally have an investing strategy that depends in part on an optimal allocation of assets across classes: 60% equity, 30% fixed income, and 10% alternative assets. The managers of these funds regularly rebalance their portfolios to maintain allocations close to these optimal mixes. When the endowments they manage earn high returns, they must shift assets to venture capital to maintain their asset allocations.[17] Investments in VC funds should therefore correlate highly with lagged endowment returns.[18] But these institutional investors rarely invest directly in start-ups, so their returns should not have a direct effect on entrepreneurship.

Although this relationship should hold at the national level, the usefulness of the instrument as a source of exogenous variation in the regional supply of venture capital also depends on an assumption that institutional investors have a tendency to invest in locally headquartered venture capital funds. Such a home bias might exist for a variety of reasons: Institutional investors might feel more comfortable investing near home, and they probably have had prior interactions with the managers of local funds. Regardless of the reasons, this assumption holds. In our sample, the probability of an LP investing in a fund located in the same MSA is roughly double the probability that it will invest in one located in an adjacent area, and more than six times that it will invest in one farther away.

To estimate the effect that rebalancing might have on the local availability of venture capital, we obtained average annual returns data for college and university endowments, an important class of limited partners, from the *Chronicle of Higher Education*. We then weighted that measure for each region by multiplying these average national returns by the logged count of institutional investors in the region that had invested in venture capital at least ten years prior to the focal year (adding 1 to avoid 0s). The ten-year lag should remove endogeneity that might result from institutional investors' initiating investment in venture capital in response to a change in local economic conditions. We then used this information to construct our instrument:

$$LPR_{it} = \sum_{j} \sum_{s=t-1}^{t-3} \frac{ER_s \ln LP_{js}}{1 + dist_{ij}}, \tag{3}$$

where LPR_{it} is our instrument for MSA i in year t, ER_s is the average return across all college endowments in year s, LP_{js} counts the limited partners in MSA j that had made venture capital investments at least ten years prior to year s (with 1 added to avoid 0s), and $dist_{ij}$ denotes the distance in miles between the centroid of MSA i and the centroid of MSA j. Distance weighting accounts for the fact that limited partners have a higher propensity to invest in funds headquartered near them. We cumulated three years of lagged returns because venture capitalists do not immediately deploy the funds committed to them.[19] The venture capital available in a region therefore depends not just on the funds that VC firms raised in the prior year, but also on their fundraising activities in several earlier years.

Table 9 reports our fixed-effects IV estimates with region-specific trends. In the first stages of all three models, the instrument strongly predicts the number of firms funded. The Kleibergen-Paap *rk* Wald statistic (Kleibergen & Paap, 2006)—reported as KP Wald with the first-stage estimates in the IV tables—tests directly whether our instrument predicts a sufficient amount of the variance in the endogenous variables to identify our equations. For LIML estimation with one instrument and one endogenous variable, Stock & Yogo (2005) report a critical value of 16.38 for the IV estimates to have no more than 10% of the bias of the OLS estimates.[20] For

[15] Although not reported here, we also estimated a set of models using the instrument suggested by Gompers and Lerner (2000): inflows into LBO funds in an MSA. Estimates with that instrument yielded substantively equivalent results. We nonetheless prefer LP Returns because it predicts more of the variation in the supply of venture capital and because it is more plausibly exogenous to regional economic activity.

[16] We estimated all IV models using the `xtivreg2` module in Stata 10 (Schaffer, 2005).

[17] Due to the finite maturity of VC investments, the flow of assets to venture capital increases with portfolio returns even when VC investments outperform other assets in the portfolio.

[18] Though one might worry that attractive venture capital investments could drive these returns, because venture capital accounts for only a very small portion of institutional investors' portfolios—less than 1% on average (Blumenstyk, 2008)—reverse causality is not an issue here.

[19] On average, VC firms begin investing in companies roughly one year after closing a fund (Sorenson & Stuart, 2001). Over the first three years of the life of a partnership, the average fund invests roughly 80% of its committed capital. In unreported models, we also entered five years of lags individually as instruments. Three of these lags would always have significant coefficients in the first-stage equations, and the second-stage estimates did not differ substantively from those reported here.

[20] Although we do not report it, the LM version of the Kleibergen-Paap test yielded equivalent results. We should also note that though the Kleibergen-Paap Wald statistic is robust to within-cluster correlation in the errors, Stock & Yogo (2005) tabulated critical values only for the case of uncorrelated errors.

TABLE 9.—IMPACT OF VC USING LP RETURNS AS AN INSTRUMENT

	(1)		(2)		(3)	
	1st Stage	Ln Births	1st Stage	Ln Empl	1st Stage	Ln Pay
Ln Patents $(t-1)$	0.00460	0.00715	0.00823	0.000499	0.000615	0.00322
	(.01787)	(.00652)	(.01501)	(.00244)	(.01907)	(.00272)
Ln Population $(t-1)$	−1.972**	−0.0790	−1.930**	0.121	−1.794**	0.454***
	(.77139)	(.16087)	(.89367)	(.10327)	(.78995)	(.11863)
LP Returns	0.00830***		0.00652***		0.00750***	
	(.00079)		(.00143)		(.00079)	
Ln VC Cnt First		0.0301**		0.0178		0.0535***
		(.01374)		(.0114)		(.01061)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
MSA trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.45		0.53		0.47	
KP Wald F-stat	110.2		20.84		91.38	
Clusters	328	328	328	328	328	328
Observations	3,270	3,270	3,264	3,264	3,264	3,264

*$p < 0.10$. **$p < 0.05$. ***$p < 0.01$. Robust standard errors in parentheses; disturbances clustered by MSA. The unit of observation is the MSA year, and the data cover MSAs in the 48 contiguous United States from 1993 to 2002. In model 1, the dependent variable is births of new establishments for firms with zero to nineteen employees at the beginning of the year. In model 2, the dependent variable is the total employment in the MSA. In model 3, the dependent variable is the total payroll in the MSA. The instrumental variable measures the inflow of new funds to VC as a result of portfolio rebalancing by limited partners. Separate first stages reflect slight differences in the samples due to the differing calendars of the census variables. See the text for more details on variable construction.

all three models, the KP Wald statistic exceeds this critical value.

When the results are assessed, the supply of venture capital has a positive effect on the number of new establishments, employment, and aggregate income. The point estimates of these effects, moreover, have increased in the IV estimation (to levels similar to the Arellano-Bond estimates). Because of the larger standard errors, the OLS estimates nevertheless are within the 95% confidence intervals of the IV estimates. The IV estimates suggest that a doubling in the number of firms funded would result in a 2.11% increase in the number of new establishments (roughly seven new establishments per company funded in an MSA with an average supply of venture capital and the mean number of new establishments), a 1.24% rise in employment, and a 3.78% higher payroll in the region. If all of the increase in aggregate income resulted from the jobs created by venture capital, then the average employee in one of these jobs would need to receive compensation on the order of $96,400 per year. As with the OLS estimates, that number seems reasonable in magnitude; thus, the employment and aggregate income results appear consistent in the magnitude of the effects that they imply.

The models that we have presented are admittedly sparse; hence, one might worry about the consequences of unobserved heterogeneity. Although the MSA fixed effects, first differences, and year dummies should capture most of the factors that influence the provision of venture capital and entrepreneurship (both across regions and over time), our analysis nonetheless remains vulnerable to omitted variable bias.[21] Since we instrument the supply of venture capital, however, such a factor would bias only our estimates if it also explained our instrument.

[21] We did experiment with the inclusion of other time-varying covariates, such as government expenditures and unemployment rates. None of those tried, however, influenced either the size or significance of our estimates of the effects of venture capital or had stable, significant effects themselves on entrepreneurship or economic growth.

IV. Discussion

We find that increases in the supply of venture capital in an MSA stimulate the production of new firms in the region. This effect appears consistent with either of two mechanisms. First, would-be entrepreneurs in need of capital may incorporate the availability of such capital into their calculations when trying to decide whether to start their firms. Second, the firms that VC firms finance may serve as inspiration and training grounds for future entrepreneurs. We further find that an expanded supply of venture capital raises employment and aggregate income in a region. At least some of these employment and income effects probably stem from venture capital allowing entrepreneurs to create value by pursuing ideas that they otherwise could not have. Table 10 summarizes the magnitudes of these estimated effects across our various specifications.

TABLE 10.—EFFECT OF DOUBLING VC SUPPLY

	Ln Births	Ln Empl	Ln Pay
Count of first investments			
Cross-section	0.31%	0.61%	2.73%
Panel FE	0.77	0.60	1.79
Panel FE with trend	0.54	0.22	0.72
Panel FD	0.57	0.29	0.56
Panel FD with trend	0.48	0.24	0.48
Arellano-Bond FD	1.79	0.94	1.12
IV (LP Returns) with trend	2.11	1.24	3.78
Count of all investments			
Cross-section	0.32	0.57	2.10
Panel FE	0.65	0.43	1.56
Panel FE with trend	0.10	0.13	0.66
Total invested dollars			
Cross-section	0.06	0.09	0.35
Panel FE	0.07	0.02	0.06
Panel FE with trend	0.00	0.00	0.04

Estimated effects of a doubling in the supply of VC in an MSA on establishment births (number of establishments for the cross-sectional estimates), employment, and payroll in the region. The top panel refers to estimates using the number of firms funded as the measure of venture capital. The middle panel provides the corresponding estimates using the total number of investing rounds as the measure of venture capital, while the bottom panel calculates effect sizes using the total dollars invested across all rounds as the measure of venture capital.

These effects, moreover, almost certainly underestimate the total economic value of venture capital. Our approach focuses on local returns, but much of the value created by the most successful firms supported by venture capital spills over to other regions. Consider a technology company such as Google. Although Google employs a large number of people in, and brings a great deal of wealth to, Silicon Valley, it also employs people elsewhere, and much of its value stems from the productivity improvements it affords users around the world in the form of cheaper and more effective search. These longer-term, extraregional gains do not contribute to our estimates. We therefore consider our results more of a lower bound than a precise estimate of the value of venture capital.

From a theoretical perspective, our findings are quite consistent with the notion that an expansion in financial intermediation improves the allocation of capital and therefore can stimulate growth (Greenwood & Jovanovic, 1990). Venture capital firms fill a niche that allows the necessary capital to reach some of the least developed and most uncertain ideas. Hence, alternative forms of financing, such as banks, cannot easily substitute for venture capital in its absence. Individual investors ("angels"), moreover, may lack the requisite skills and experience to choose and incubate these young companies effectively (though the relative efficacy of angel investments versus venture capital falls outside the scope of this analysis and deserves greater research attention).

Given that venture capital accelerates growth and that the richest regions have the largest supplies of it, one might wonder whether it acts as a countervailing force to the convergence found by Barro and Sala-i-Martin (1992). One could imagine a virtuous cycle in which venture capital stimulates growth, which in turn leads to a greater supply of venture capital, and so on. Our results nevertheless suggest that such a dynamic would do little more than slightly slow the rate the convergence. Venture capital operates at a small scale. Even in the regions with the largest supplies, it funds no more than roughly 4 out of every 100 start-ups. Because of this small scale, our estimates suggest that the difference in per capita income between the richest and poorest regions would have been only between 2% and 13% smaller in the absence of venture capital.

But the results also do not suggest that regions would benefit from vast expansions in their supplies of venture capital. The estimated elasticities fall far short of 1, implying decreasing returns to the availability of venture capital. We can, moreover, use the estimated effects from venture capital dollars invested on aggregate income to place a minimum on the optimal supply of venture capital. Although the payroll does not include all capital gains, the point at which an additional dollar of venture capital would yield less than an additional dollar in income at least provides a lower bound on the optimal supply of venture capital. In other words, we calculated the point at which the change in predicted payroll equals the change in investment ($\Delta Payroll = \Delta VCAmount$). For the average MSA, this calculation yields a sum of roughly \$4.5 million to \$44 million (depending on which estimates one uses), or between \$16.50 and \$161.00 per employee.

When one adjusts for the fact that smaller regions require less venture capital and larger regions more, our estimates suggest that in only 32% of years do MSAs exceed the lowest estimate of this minimum bound and in only 10% of years do they surpass the highest estimate. Only five regions—Boulder-Longmont, Oakland, San Diego, San Francisco, and San Jose—exceed the highest estimate of the minimal optimum supply of venture capital in every year in our window. Many regions could therefore benefit from an expansion in the supply of venture capital. Given this inadequate supply, several questions naturally arise. First, why do some regions have an undersupply of venture capital? Does it stem from a paucity of local institutional investors or a shortage of experienced venture capitalists? Second, can public policy correct this situation, and if so, how? Obviously, the answers to this second question depend in large part on those to the first.

Another interesting line of inquiry would consider whether the efficiency of venture capital depends on other factors. For example, to the extent that our results depend on entrepreneurship, legal regimes that restrict the mobility of labor, such as by enforcing noncompete agreements, might stunt the effects of venture capital. This question also has clear policy relevance. Government programs to expand the supply of venture capital have met with mixed success. At one extreme, the Israeli model has almost uniformly been praised and offered as an example of best practices. At the other, both the Canadian and German models appear to have had limited success and may have even stunted the development of (private) venture capital in those regions. Although the focus to date has primarily been on the internal design of these programs, variation across countries in the success of these programs may also stem from differences in complementary factors across these jurisdictions.

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