MANAGEMENT SCIENCE
Vol. 57, No. 3, March 2011, pp. 425–438
ISSN 0025-1909 | EISSN 1526-5501 | 11 | 5703 | 0425

informs

DOI 10.1287/mnsc.1100.1280

Noncompete Covenants: Incentives to Innovate or Impediments to Growth

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We find that the enforcement of noncompete clauses significantly impedes entrepreneurship and employment growth. Based on a panel of metropolitan areas in the United States from 1993 to 2002, our results indicate that, relative to states that enforce noncompete covenants, an increase in the local supply of venture capital in states that restrict the scope of these agreements has significantly stronger positive effects on (i) the number of patents, (ii) the number of firm starts, and (iii) employment. We address potential endogeneity in the supply of venture capital by using endowment returns as an instrumental variable. Our results point to a strong interaction between financial intermediation and the legal regime in promoting entrepreneurship and economic growth.

Key words: venture capital; financial intermediaries; legal institutions; entrepreneurship; employment; innovation; wages
History: Received May 13, 2010; accepted October 8, 2010, by Gérard P. Cachon, entrepreneurship and innovation. Published online in *Articles in Advance* January 20, 2011.

1. Introduction

Noncompete covenants are clauses in contracts that expressly forbid individuals from competing with their former employers. They have become a common term of employment, particularly for technical workers and upper-level management. In the United States, for example, surveys report that nearly 90% of these employees have signed noncompete agreements (Leonard 2001, Kaplan and Strömberg 2003).

Though nearly ubiquitous, their enforcement nevertheless varies from state to state. Some states, such as California, disregard covenants not to compete except in rare cases, whereas others, such as Massachusetts, routinely enforce them (Gilson 1999). Though these differences almost invariably stem from state-level statutes or precedents that long precede the modern usage of noncompete covenants, they may nonetheless influence the economic climate and vitality of regions to the present day. Gilson (1999), for example, has proposed this difference in legal infrastructure as the crucial factor behind Silicon Valley's surpassing of the Boston area as the capital of high technology.

Despite the potential importance of this issue, however, little research has considered how the enforcement of noncompete agreements affects regional economies. Evidence does exist that the nonenforcement of noncompetes, which we refer to as an "employee-friendly" regime, increases mobility. Fallick et al. (2006), Garmaise (2009), and Marx et al. (2009), for example, report higher levels of mobility among executives and technical workers in states with employee-friendly regimes. Similarly, Stuart and Sorenson (2003) find that these states have higher rates of biotech entrepreneurship following acquisitions and initial public offerings in the industry. Given that noncompete covenants limit employees' outside options, one would expect their enforcement to impede mobility.

But noncompete covenants also have a positive side, helping companies to protect the human capital, intellectual property, and relationships they have developed. Companies can increase their productivity by training workers, developing new products and processes, and building relationships with customers and suppliers. These activities nevertheless leave firms vulnerable. An employee (or group of employees) might leave, taking this knowledge and these relationships with them. By restricting mobility, noncompete covenants ensure that companies can reap the rewards of these assets, thereby promoting these productivity-enhancing activities (Franco and Mitchell 2008). The overall effect of noncompete clauses on outcomes other than mobility therefore remains an open question.

It remains open in part because it has only recently received attention, but also because of the empirical difficulties surrounding it. Most notably, the variation

in enforcement exists almost entirely in the cross section. With few exceptions, states have had stable legal regimes with respect to the enforcement of noncompete covenants for decades (Richey and Malsberger 1996, Gilson 1999). Even those that have shifted their stances have generally made only minor modifications in enforcement. But any analysis relying entirely on cross-sectional variation in these regimes could not distinguish the effects of this enforcement from a multitude of unmeasured factors.

We address this issue by focusing on a piece of the puzzle. Rather than examining the average differences across regions, we estimate how regions respond to shocks in the supply of one form of financial capital—venture capital (VC)—and examine whether the effects of these shocks depend on the enforcement regime. On average, venture capital stimulates entrepreneurship, creates jobs, and raises incomes (Samila and Sorenson 2011). But do these effects vary across legal regimes? Because potential entrepreneurs face fewer constraints in states with employee-friendly regimes, venture capital should lead to higher levels of entrepreneurship in these jurisdictions. The expected effects on innovation and economic growth, however, are less clear. They could increase as entrepreneurship leads to the pursuit of new ideas and the creation of new jobs, but if incumbent firms, or even start-ups, invest less intensely in human, intellectual, and relational capital in response to the higher probability of losing employees, then increases in the supply of venture capital might have no—or even a negative—effect on innovation and economic growth in employee-friendly regimes.

Using panel data on metropolitan areas in the United States from 1993 to 2002, we estimated the causal effect of venture capital on patenting, entrepreneurship, employment, and aggregate income for states that do and do not restrict noncompete covenants. To address the potential endogeneity in the supply of venture capital, we instrumented it with a variable related to venture capital fundraising but not (directly) to entrepreneurship: the portfolio returns of institutional investors. To maintain their target asset allocations, institutional investors adjust their commitments to venture capital in response to the performance of the rest of their portfolios. These past portfolio returns, however, should not directly influence regional differences in innovation, entrepreneurship or economic growth.

Our results suggest that noncompete covenants strongly moderate the effect of venture capital on start-up activity, as well as on other outcomes. In states with employee-friendly legal regimes, increases in the availability of venture capital result in larger increases in the level of entrepreneurship in the region, consistent with prior studies that have found a negative relationship between noncompete covenants and mobility (Stuart and Sorenson 2003, Fallick et al. 2006, Garmaise 2009, Marx et al. 2009). An influx of venture capital also leads to higher levels of patenting and of employment in employee-friendly jurisdictions.

By providing evidence for the moderating effects of noncompete covenants on the effectiveness of venture capital in fostering innovation, entrepreneurship, and economic growth, we contribute to a better understanding of the relationships between venture capital and legal institutions and regional economic dynamics. This interaction has at least three important implications. Most immediately, it suggests that policies aimed at stimulating entrepreneurship through increases in the supply of venture capital may not succeed if the labor laws in the jurisdiction do not support such investments. Moreover, if the enforcement of noncompete agreements similarly affects the efficacy of other forms of financial capital, this interaction suggests that states might benefit broadly by relaxing their enforcement of these agreements. Finally, more generally, it suggests that legal infrastructures importantly moderate the effectiveness of financial intermediation and influence the dynamics of regional industrial clusters.

2. Noncompete Covenants

Noncompete covenants stipulate that employees may not work for competing firms, including start-ups, if they leave their jobs. These agreements have become ubiquitous in employment contracts among certain sorts of employees, including executives, research and development (R&D) staff, and salespeople.

Though noncompete clauses have become common, their enforcement varies from state to state.[1] The majority of states enforce these agreements by the "rule of reason"—considering an agreement valid if it does not prevent the individual from being gainfully employed and if it does not appear longer in duration or broader in scope than necessary to protect the prior employer (Gilson 1999). But many states also restrict noncompete agreements. At the extreme, several states have statutes or precedents that essentially preclude their enforcement. For example, California's Business and Professions Code §16,600 states that "every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void." Courts have interpreted this statute as invalidating not just clauses that

[1] In the United States, states have jurisdiction over labor law. For summaries of the enforcement of noncompete covenants by state, see Stuart and Sorenson (2003, p. 190) and Garmaise (2009, p. 44). For more detailed descriptions, see Richey and Malsberger (1996).

would prevent, but even those that would merely penalize postemployment competition (Gilson 1999).

These state-level differences in enforcement regimes generally have deep historical roots.[2] In Massachusetts, for example, its origins reach back to English common law from the time of the guilds (Gilson 1999). The California Business and Professions Code, meanwhile, emerged in the 19th century from the newly established state's need to create a consistent legal code. Though the reasons why these statutes and precedents have been adopted have often been lost to history, they do not appear to have arisen from any concern over their effects on entrepreneurship or innovation (Gilson 1999).

Despite their deep historical roots, the consequences of these enforcement regimes continue to the present day. The enforcement of covenants not to compete has been shown to restrict the mobility of employees (Fallick et al. 2006, Garmaise 2009, Marx et al. 2009) and to reduce the rate of entrepreneurship (Stuart and Sorenson 2003). Gilson (1999) speculates that this labor market friction may even hinder economic growth. Policy makers might therefore be tempted to emulate California's employee-friendly regime. But the enforcement of these agreements could also promote innovation and economic growth by encouraging firms to develop certain sorts of assets (Franco and Mitchell 2008).

2.1. Incentives to Invest

To the extent that noncompete covenants restrict the mobility of employees, they encourage firms to allocate resources to the development of certain sorts of assets, such as intellectual property, human capital, and interfirm relations. These incentives stem from two common features: (1) the control of these assets, to a large extent, resides in individuals within the firm; and (2) firms have few alternative mechanisms for protecting these assets. Companies must therefore worry that employees might appropriate their value by either leaving or threatening to leave their jobs. At the regional level, the enforcement of noncompete covenants could therefore stimulate innovation and growth if companies allocate more to developing productivity-enhancing assets in regions that provide greater protection for them.

[2] Most states have had stable enforcement regimes for decades. Four states have nevertheless experienced meaningful changes over the last 30 years. In 1985, Michigan's legislature unintentionally eliminated the statute that made noncompete agreements unenforceable in that state (Marx et al. 2009). Florida modestly strengthened its enforcement in 1996 (Garmaise 2009). From 2002 to 2003, a ruling from the Louisiana Supreme Court made noncompete covenants unenforceable in Louisiana, except as a barrier to the founding of a firm (Garmaise 2009). Most recently, in 2008, Oregon passed legislation to curtail the enforcement of noncompete agreements in that state (Amir and Lobel 2010).

2.1.1. Intellectual Property. The most commonly noted justification for enforcing noncompete agreements is to protect intellectual property. Although companies can often protect inventions—discrete, codifiable entities—with patents, much innovation comes in the form of tacit knowledge: routines and practices that are not easy to codify. This knowledge can contribute crucially to the efficiency of firms and may serve as a source of competitive advantage. Yet, its tacit nature means that firms cannot easily separate it from the individuals in which it resides. Not only does this fact open the possibility that this knowledge may spill over to other firms (should employees with the knowledge defect to them), but also it means that a firm could even lose the ability to access this asset itself (if all of those with the knowledge left). Where enforced, noncompete clauses effectively give the employer property rights over this tacit knowledge (Gilson 1999).

Even when companies have alternative mechanisms for protecting their intellectual property, the enforcement of noncompete covenants might still strengthen these protections. Consider patents, for example. The rights to an invention generally belong to the inventor's employer. But courts consider an invention to have occurred when the inventor first conceives of the complete invention, supported by objective evidence (Gilson 1999). Hence, an employee who leaves his or her employer before fully developing an invention—or before creating the evidence to support it—can retain the property rights to it. When facing an enforceable noncompete clause, however, the inability of the inventor to bring that invention to a start-up or to a competing firm might limit the inventor's temptation to pursue the idea outside his or her current employer.

2.1.2. Human Capital. Rubin and Shedd (1981) and Garmaise (2009) draw attention to another issue: noncompete covenants also protect investments in human capital. Firms can improve their performance through the updating and upgrading of the human capital of their labor forces. Individual employees nevertheless retain the rights to this human capital (Rubin and Shedd 1981). When these upgrades involve the acquisition of abilities specific to the needs of the employer, the firm can usually reap the rewards of these investments because employees cannot benefit elsewhere from their human capital (Becker 1964). But more general skills pose a problem. In the absence of a means of tying the employee to the firm, once employees have received the training, they might market their newly gained skills to other firms, seeking higher salaries. Rational employers, recognizing this problem, may therefore refuse to develop these more general skills—despite their value to the firm and to society (Becker 1964). Enforceable noncompete

covenants, therefore, could encourage employers to build human capital.[3]

2.1.3. Business Relationships. To intellectual and human capital, we would add a third asset in which enforceable noncompete agreements provide incentives to invest: social capital. Though business relationships have received little attention in the academic literature on noncompete covenants, in practice, companies appear to recognize the importance of protecting business relationships. Noncompete clauses, for example, appear commonly in the contracts of salespeople, employees whose value resides almost entirely in their connections. One also sees them often in professional services—such as accounting, consulting, law and medicine—where relationships with clients play a particularly important role (Maister 1993).

A large literature has trumpeted the value of trusted social relationships. They can solve a sort of market failure in the sale of products and services by connecting customers willing to pay for higher quality with the producers capable of providing it (Kollock 1994). They can also improve the efficiency of supply chains by facilitating coordination across production stages and the exchange of fine-grained information (Uzzi 1996). In the hope of realizing these benefits, companies allocate time and effort to building business relationships.

Despite the evidence of their value, however, the ownership of these relationships remains somewhat ambiguous. Although the literature conceptualizes interfirm relationships as belonging to organizations, these connections, and the trust imbued in them, commonly reside with the individuals anchoring each end of the relationship (Løvås and Rogan 2005). Firms therefore frequently see these ties transfer to their competitors when employees defect. Salespeople, accountants, consultants, doctors, and lawyers, for example, bring clients with them when they change employers or set up their own practices. Where enforced, noncompete clauses effectively allocate property rights over these relationships to the employer, and therefore may encourage companies to develop them.

2.2. Impediments to Growth

Though the enforcement of noncompete covenants can encourage companies to invest in an array of assets, it may also impede innovation and economic growth in at least three ways: (i) through the slowing of spillovers, (ii) through the reduction of entrepreneurship, and (iii) through a loss of efficiency in the matching of employees to employers.

The literature on noncompete agreements has pointed most prominently to spillovers as a reason why regions might not want to enforce them. As noted above, much of the knowledge that firms create is tacit and embodied in individuals. The diffusion of these ideas across firms therefore depends on the movement of employees. To the extent that many firms might benefit from the ideas initially developed at (and paid for by) one firm, this knowledge sharing can improve the competitiveness of a region. By increasing interfirm mobility, employee-friendly jurisdictions promote spillovers (Gilson 1999).

The enforcement of noncompete covenants may also limit entrepreneurship. Some simply see this effect as another form of spillover (from incumbent firms to start-ups); after all, a large share of entrepreneurs enter the industries of their former employers (Franco and Filson 2006). But many entrepreneurs pursue novel lines of business (Klepper 2007), and the enforcement of noncompete agreements could also inhibit these noncompeting start-ups for at least two reasons. First, entrepreneurs, even if pursuing ideas distinct from those of their former employers and developed on their own time, could face holdup (Hellmann 2007). Second, even if noncompete clauses do not prevent founders personally from starting their ventures, they might nonetheless find it difficult to get their organizations off the ground if they cannot hire employees with experience in the industry because noncompete agreements bind those potential hires (Stuart and Sorenson 2003).

Finally, by limiting mobility, noncompete clauses, when enforced, might reduce the average quality of matches between employees and employers. If one assumes that employees have varying abilities and that firms differ in the degree to which they can use these abilities, then the matching of employees to employers can importantly influence the productivity of companies and regions (Roy 1951, Kremer 1993). But finding the right match often requires a bit of experimentation. Individuals may not be aware of their own abilities and particularly of how those abilities fit with potential employers. Employers, similarly, may either fail to understand completely what skills they require or find themselves unable to assess those qualities in job applicants. In the absence of perfect information, anything that adds friction to the movement of employees across firms, therefore, will obstruct the trial-and-error process and increase the odds of a poor match (e.g., Hopenhayn and Rogerson 1993).

3. Empirical Strategy

Empirical research on the effects of institutional differences, such as legal regimes, on economic growth have

[3] Garmaise (2009) points out that the enforcement of noncompete covenants reduces the employee's own incentives in invest in human capital. As a result, the expected *net* effect of noncompete enforcement on human capital investment remains indeterminant.

generally adopted one of two possible approaches. One research tradition examines variation in the levels of investment as a function of some institutional feature (e.g., Mauro 1995). Here, for example, the enforcement of noncompete agreements could lead to higher or lower levels of capital investment and consequently to differing productivity and economic growth across regions. A second research tradition, meanwhile, focuses on variation in the returns to a given level of investment across institutional regimes (e.g., King and Levine 1993, Levine and Zervos 1998). In this case, the enforcement of noncompetes might affect the marginal social returns to the same capital investment.

Clearly, institutions could influence both of these margins. Our empirical approach nevertheless follows the second tradition for a number of reasons. First and foremost, from a theoretical point of view, the arguments above suggest that the enforcement of noncompete agreements would result in changes in the allocations of resources by incumbent firms, start-ups, and employees. Hence, across enforcement regimes, one might see differences in productivity despite similar levels of capital expenditure. Second, and also theoretically, investors may find it difficult to capture fully the returns to their investments (i.e., the social returns may come in the form of externalities). The levels of investments therefore may not adjust in response to differences in the social returns to those investments. Third, from a practical point of view, focusing on variation in the returns to investments allows us to exploit within-region variation over time—in this case, in the supply of venture capital—to estimate these effects. As a result, this approach has the advantage of allowing us to control—through the use of region fixed effects—for a wide range of other features that vary across regions (that otherwise could confound cross-sectional estimates of the effects of noncompete enforcement).

More specifically, we examine the effects, across regimes, of changes in the supply of venture capital on four regional outcomes: the level of entrepreneurship, the rate of patenting, the level of employment, and the total wage bill. Venture capital funds young, high-potential firms through equity investments. By allocating capital to companies that otherwise would not receive funding, venture capital firms stimulate entrepreneurship, employment, and income growth (Samila and Sorenson 2011). But do its effects vary across legal infrastructures?

Given the consistent finding that employee-friendly regimes promote mobility (Fallick et al. 2006, Marx et al. 2009), and given that these regimes should also ease the ability of entrepreneurs to recruit employees (Stuart and Sorenson 2003), one would clearly expect higher rates of entrepreneurship in these places in response to increases in the supply of venture capital. To a large extent, then, this outcome simply serves as a validity check on our approach.

What one would expect in terms of the other outcomes, however, seems far less clear. Consider patents. On the one hand, in employee-friendly regimes, incumbent firms might respond to the increased probability of losing employees—and with them the value of intellectual property—to start-ups by investing less in R&D and therefore producing fewer innovations. On the other hand, the greater mobility of individuals in employee-friendly regimes might allow these start-ups to assemble teams that usefully combine ideas from multiple firms to produce new ideas. Depending of the relative magnitudes of these effects, the enforcement of noncompete agreements could either increase or decrease the degree to which venture capital stimulates innovation.

Similarly, the effect of enforcement on economic growth—employment and aggregate income—remains an empirical question. On the positive side, the enforcement of noncompete agreements encourages both incumbent firms and start-ups to allocate resources to the development of intellectual property, human capital, and business relationships. To the extent that these investments increase the efficiency of production, they may increase employment and earnings. On the negative side, however, enforcement restricts entrepreneurship, spillovers, and innovation from the recombination of ideas developed at different firms. It therefore may limit the ability of regions to benefit from the innovations that arise within them and the ability of entrepreneurs to create high-growth firms that create jobs and generate wealth.

4. Data and Estimation

Our empirical analysis uses an unbalanced panel of all 328 metropolitan statistical areas (MSAs) in the contiguous United States from 1993 to 2002. The Office of Management and Budget defines MSAs roughly three years after each decennial census; the definitions from the 1990 census came into use in 1993 and remained in effect until 2002. We limited our study to this 10-year window because consistent definitions of the areal units over time are essential for our analyses.

MSAs offer the smallest geographic regions that one might consider independent in terms of economic activity. Each MSA consists of an urban core and a tightly integrated surrounding area (any county—or township in the case of New England—in which more than 25% of the labor force commutes to the urban core). For each MSA, we gathered data from several sources, both public and private. The economic data come from the Small Business Administration, which

collects them annually from the Census Bureau. The patent data came from the Patent Network Dataverse (Lai et al. 2009). The VentureXpert database of Thomson-Reuters serves as our source of information on venture capital activity. To assess state-level differences in the enforcement of noncompete clauses, we used data from Stuart and Sorenson (2003) and Garmaise (2009). Finally, information on institutional investors and endowment returns comes, respectively, from the National Center for Charitable Statistics and the *Chronicle of Higher Education*.

4.1. Dependent Variables

Patents. We use patents to assess innovation. Although we recognize that many kinds of innovation do not appear in patenting data, patents nevertheless offer one of the few means of measuring innovation across a broad spectrum of industries and over time. To create our measure, we assigned each patent to an MSA based on the inventor's address and to a year based on the date of application. If a patent had multiple inventors, we assumed that they all participated equally in the invention and hence divided the patent equally across the inventors' addresses.[4] We counted the total number of patents in each MSA-year and transformed this count using the natural logarithm.

Establishment Births. As a measure of entrepreneurship, we counted the number of new business establishments. The Census Bureau defines business establishments as single physical locations in which business occurs and for which employment records are maintained. It records an establishment birth when a location had no employees in the pay period covering March 12 in one year but has employees on that same date in the following year. A firm may have multiple establishments, but every firm has at least one.

One possible shortcoming of this measure is that it captures relocations and expansions in addition to the creation of new firms. To focus on entrepreneurship, we used information on the size of the firm creating the establishment. The Census Bureau reports establishment births by three categories of firm size: 0–19 employees, 20–499 employees, and over 500 employees. It allocates firms to these categories based on their size at the end of the year. Because few start-ups have more than 19 employees by the end of their first year, we focused on establishment births in the 0–19 employees category. Our measure transforms, by the natural logarithm, the total number of establishments opened by firms with 0–19 employees.

[4] Assigning patents to regions using only the addresses of the first inventors produced equivalent results.

Employment and Payroll. To assess the response of the economy to changes in the supply of venture capital, we examined two additional outcomes: the total number of people employed in the region, both full and part time, during the pay period covering March 12 and their aggregate income, including all forms of compensation such as salaries, wages, reported tips, contributions to pension plans, and taxable fringe benefits. We transformed both variables using the natural logarithm.[5]

4.2. Independent Variables

Noncompete Enforcement. We created two measures of state-level differences in the enforcement of noncompete covenants (i.e., in the employee friendliness of legal regimes). The first, *absence of noncompete enforcement* (*ANC*), follows Stuart and Sorenson (2003), creating a state-level indicator variable with a value of 1 if the state generally precludes, through statute or precedents, the enforcement of noncompete covenants (or 0 otherwise). The second, *weakness of noncompete enforcement* (*WNC*), follows Garmaise (2009). For each state, Richey and Malsberger (1996) report 12 summary provisions, such as whether the state imposes geographic or time limits on the enforcement of noncompete agreements. Garmaise proposes a threshold value on each provision that implies a more employer-friendly regime. We reverse coded this index to count the number of employee-friendly provisions.[6] Higher values indicate more employee-friendly legal regimes. To ease comparisons across these measures, we rescaled *WNC* to run from 0 to 1 (by dividing it by 9, the maximum value for any state). Though these two variables correlate at 0.47, each nonetheless captures unique variance in legal regimes.

For MSAs that straddled two or more states, we weighted the state-level measures according to the number of people in the MSA residing in each state. For example, an MSA with 60% of its population in a state that precludes the enforcement of noncompete

[5] The Census Bureau reports establishment births and employment on an April-to-March calendar. We therefore used venture capital investments from April of one year to March of the following year to predict the entrepreneurship during the period and employment at the end of it. We also counted patents on an April-to-March calendar. The payroll data, however, follows a January-to-December calendar. To keep the sample consistent, we nevertheless decided to use the same measure of venture capital activity to predict changes in income (limiting us to using only nine years of wage data). We did not have month-level information on our instrumental and control variables; they therefore follow the normal calendar.

[6] In unreported analyses, we explored whether some of these provisions proved more important than others in promoting entrepreneurship and economic growth. We could not, however, reject the null hypothesis that each provision had equal importance in moderating the effect of venture capital.

Table 1 Summary Statistics

Variable	Mean	Std. dev.	*N*
Patents	244.63	573.12	2,935
Births	1,411.65	2,507.39	2,935
Population (thousands)	655.96	1,097.15	2,935
Employment (thousands)	273.88	475.95	2,935
Payroll (millions)	8,508.95	17,872.22	2,935
VC count	4.34	28.49	2,935
ANC	0.18	0.38	2,935
WNC	0.52	0.23	2,935
LP returns	38.13	40.93	2,935
LP returns (insurance & pensions)	9.13	16.2	2,935

Table 2 Most Active and Fastest-Growing Regions for Venture Capital

Average first investments per year	
San Jose, CA	254
San Francisco, CA	195
Boston, MA–NH	163
New York, NY	91
Oakland, CA	76
Washington, DC–MD–VA–WV	74
San Diego, CA	65
Los Angeles–Long Beach, CA	63
Seattle–Bellevue–Everett, WA	60
Atlanta, GA	48
Compound average growth rate per year, 1993–2001 (%)	
Raleigh-Durham–Chapel Hill, NC	43
Pittsburgh, PA	35
Baltimore, MD	32
New York, NY	29
Austin–San Marcos, TX	29
Santa Barbara–Santa Maria–Lompoc, CA	26
Minneapolis–St. Paul, MN–WI	26
Albuquerque, NM	22
Middlesex–Somerset–Hunterdon, NJ	22
Lawrence, MA–NH	22

covenants ($ANC = 1$) and 40% in a state that does not ($ANC = 0$) would receive an ANC value of 0.60. Our results nevertheless remain robust to the exclusion of the 41 state-spanning MSAs.

VC Investment Count. We measured venture capital activity by counting the number of firms in a region that received venture capital financing in each year. We only counted each firm the first time that it received an investment, and we only included investments from venture capital firms organized as limited partnerships with outside investors.[7] Although angel investors, corporate venture capital, and direct investments by university endowments undoubtedly also influence the regional economy, our instrumental variable constrains us to studying the effects of capital supplied by institutional investors. Table 1 presents summary statistics for this variable and the others used in our analyses, and Table 2 reports the regions with the highest average levels of venture capital and the most rapid growth in venture capital over our observation window.

4.3. Fixed-Effects Estimates

We began by estimating a standard production function:

$$\ln Y_{it} = \alpha + \beta_1 \ln P_{i,t-1} + \beta_2 \ln VC_{it} + \beta_3 NC_i \ln VC_{it} + \phi_t + \eta_i + \epsilon_{it}, \quad (1)$$

where i indexes the MSA and t indexes the year, Y_{it} is the dependent variable (patents, establishment births, employment, or payroll), $P_{i,t-1}$ measures the population, VC_{it} represents venture capital activity, NC_i denotes the strength of noncompete enforcement (ANC or WNC), ϕ_t indicates a series of year fixed effects, η_i denotes the MSA fixed effects (partialed out), and ϵ_{it} represents the residual error.[8] A statistically significant value for β_3 would indicate that noncompete enforcement moderates the effect of venture capital—a positive interaction would suggest that employee-friendly regimes promote more and better start-ups, whereas a negative interaction would suggest that firms in these regimes more than offset these benefits by reducing their investments in human, intellectual, and social capital.

We included region-specific fixed effects (η_i) to control for all time-invariant aspects of each region, such as local institutions and tax laws, the presence of colleges and universities, geographic factors, and the composition of the labor force. Using fixed effects effectively purges them from the models. Because noncompete enforcement does not vary meaningfully within MSAs over time, however, these fixed effects also absorb the "main" effect of noncompete enforcement, and therefore we cannot estimate a coefficient for it.

We also introduced year fixed effects to control for all time-varying factors at the national level, most notably stock market performance, interest rates, and other general economic conditions. These would naturally influence entrepreneurship, economic growth,

[7] Although we only report the results using this count of first investments, we experimented with a number of other specifications, including, for example, counting all investments and summing investment amounts. All of these specifications produced broadly consistent results.

[8] Repeated observations of regions could lead to correlated errors over time within regions. In least squares estimation, these correlations will not bias the point estimates, but they can affect the standard errors. We therefore estimated our models using standard errors robust to repeated observations of the same regions.

Table 3 Fixed-Effects Estimates for Innovation and Entrepreneurship

	(1) Ln *patents*	(2) Ln *patents*	(3) Ln *patents*	(4) Ln *births*	(5) Ln *births*	(6) Ln *births*
Ln *population* $(t-1)$	1.605***	1.611***	1.630***	0.778***	0.779***	0.782***
	(4.74)	(4.74)	(4.83)	(9.06)	(9.04)	(9.05)
Ln *VC count* (t)	0.0317***	0.0208**	0.0315***	0.0103***	0.00755*	0.0102***
	(3.49)	(2.20)	(3.54)	(3.20)	(2.11)	(3.20)
Ln *VC count* × *ANC* (t)		0.0630***			0.0155**	
		(2.70)			(2.39)	
Ln *VC count* × *WNC* (t)			0.108***			0.0167*
			(3.48)			(1.65)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.17	0.17	0.17	0.23	0.23	0.23
Clusters	328	328	328	328	328	328
Observations	2,935	2,935	2,935	2,935	2,935	2,935

Notes. OLS regression results are shown, with robust t-statistics in parentheses. Disturbances are 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 the count of patent applications. In models (4)–(6), the dependent variable is births of new establishments for firms with 0–19 employees at the beginning of the year.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

and also the supply of venture capital. The year effects (ϕ_t) remove these national economic trends from our analyses.

We therefore identify our effects off of MSA-specific, within-MSA changes in venture capital, innovation, entrepreneurship, and economic growth. That means that only other MSA-specific, within-MSA factors could possibly confound our results. We include one such variable explicitly in the analysis, population (the logged count of individuals living in an MSA), and deal with other unobserved factors below through the use of an instrument.

Table 3 reports the results of these models for patents and establishment births. Because of the log-log specification, we can interpret these coefficients as elasticities. Thus, for example, a 1% increase in the number of firms funded by venture capital firms in a region increases the number of patents in that region by 0.03%.

Turning to how these effects differ as a function of legal regimes, both measures of employee friendliness reveal consistent results. States that do not enforce noncompetes or that enforce them less intensely enjoy higher rates of patenting and of entrepreneurship in response to increases in the supply of venture capital. As noted above, the higher levels of entrepreneurship appear in line with the results of prior studies that find that these regimes encourage employee mobility (e.g., Fallick et al. 2006). The higher levels of patenting, meanwhile, are consistent with the idea that start-ups in employee-friendly regimes can better exploit the recombination of existing technologies (Fleming 2001).[9] But patenting might also increase even in the absence of greater innovation if firms attempt to substitute patents for the intellectual property protection offered by noncompete covenants. Though our data cannot adjudicate between these possibilities, a recent study points to the first explanation. Garmaise (2009) finds that firms, on average, invest *less* in R&D under regimes of strong noncompete enforcement; thus one would expect them to produce fewer innovations.

Table 4 reports the results of our estimates of the effects of venture capital on employment and aggregate income. Consistent with past research, we find positive "main" effects of venture capital on both outcomes. Our results also suggest that venture capital investments produce more jobs in employee-friendly regimes. For example, whereas the point estimates suggest that a 1% increase in the number of venture capital investments in a state that does not enforce noncompete covenants would result in 16 jobs (in an average-size MSA), the same increase in venture capital in an employee-friendly regime predicts 56 additional jobs. Employee-friendly regimes similarly see larger increases in aggregate income with each venture capital investment.[10]

[9] Amir and Lobel (2010) also present experimental evidence suggesting that noncompete clauses may reduce effort and output in tasks that require creativity.

[10] One might worry here that Silicon Valley and the Internet boom drive these results, because California does not enforce noncompete agreements. Estimates excluding the MSAs in the Bay Area yielded statistically identical results.

Table 4 Fixed-Effects Estimates for Regional Economy

	(1) Ln *employment*	(2) Ln *employment*	(3) Ln *employment*	(4) Ln *payroll*	(5) Ln *payroll*	(6) Ln *payroll*
Ln *population* $(t-1)$	0.791***	0.793***	0.797***	1.173***	1.174***	1.181***
	(21.75)	(21.82)	(21.88)	(15.71)	(15.64)	(15.82)
Ln *VC count* (t)	0.00897***	0.00562**	0.00892***	0.0247***	0.0215***	0.0246***
	(5.13)	(3.01)	(5.22)	(7.41)	(5.78)	(7.54)
Ln *VC count* × *ANC* (t)		0.0192***			0.0183*	
		(4.90)			(1.90)	
Ln *VC count* × *WNC* (t)			0.0226***			0.0349***
			(3.45)			(2.64)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.77	0.77	0.77	0.93	0.93	0.93
Clusters	328	328	328	328	328	328
Observations	2,935	2,935	2,935	2,935	2,935	2,935

Notes. OLS regression results are shown, with robust t-statistics in parentheses. Disturbances are 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 the total employment in the MSA. In models (4)–(6), the dependent variable is the total payroll in the MSA.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

4.4. Instrumental Variables Estimates

Two factors might bias the above results. On the one hand, venture capital firms may actively search for and locate their offices in the regions with the highest levels of innovation, entrepreneurship, and economic growth—leading to endogeneity in the supply of venture capital. On the other hand, some unobserved MSA-specific, within-MSA factor—that is, an omitted variable—might confound our results. We address both of these issues through the use of an instrumental variable (IV): limited partner (LP) returns.[11]

The institutions that invest in venture capital as limited partners usually use a fixed asset allocation ratio to determine the allocations of their investments across asset classes. Given this fixed ratio, as the total asset pie grows or shrinks with the returns of the portfolio, the slice devoted to venture capital should also grow and shrink by a similar proportion. LP returns should therefore correlate highly with the local availability of venture capital.

The validity of LP returns as an instrument depends on two assumptions: (1) Institutional investor returns predict future venture capital investments within the region. (2) Institutional investor returns do not directly stimulate entrepreneurship or economic growth within the region. We will discuss the second assumption below, but we can readily justify the first assumption by decomposing it into three steps:

1. LP returns are positively related to future investments in venture capital.

2. Institutional investors exhibit a "home bias" when investing in venture capital funds.

3. Venture capital funds have a "home bias" for investing in target companies.

Beginning with the first step, most institutional investors diversify their investments using a (relatively) fixed proportional allocation across asset classes—for example, 40% equities, 40% bonds, and 20% alternative assets—adjusting their investments toward this target allocation at regular intervals. Given the limited maturity of venture capital investments, rebalancing requires that an increase in returns to the total portfolio results in a greater flow of funds into venture capital.

When they invest these funds, institutional investors exhibit a "home bias"—that is, they tend to invest in funds headquartered close to them. This bias probably stems from the constraints facing first-time funds. Because the partners starting these firms lack track records, they find it difficult to raise funds and generally only receive investments from those with whom they have prior business dealings or personal relationships. Even when raising second and subsequent funds, this local bias persists because firms rarely move. As a consequence, limited partners invest in funds in the same MSA at twice the rate at which they invest in funds in adjacent regions and at six times the rate of those further away (Samila and Sorenson 2011).

Finally, it has been well documented that venture capital funds have a strong tendency to invest locally

[11] We also addressed these issues through two other approaches. First, we estimated dynamic panel models that used past levels of the explanatory variables as instruments for the changes in these same variables (Arellano and Bond 1991). Second, we estimated models using the instrument suggested by Gompers and Lerner (2000): investments in leveraged buyout funds. Despite the fact that these approaches rely on different identifying assumptions, they yielded substantively equivalent results to those presented here, lending substantial confidence to the validity of these results.

(Sorenson and Stuart 2001). Venture capitalists rely on local social networks to find investments and then must travel to their portfolio companies regularly to monitor and advise them; they therefore prefer to invest locally. Together, these facts imply that high returns among institutional investors portfolios in one year lead to more venture capital investments in the next few years in the same regions and in neighboring regions to those institutional investors.

LP Returns. We constructed our instrument by interacting the national average returns to college and university endowments, an important class of institutional investors, with the number of limited partners in the region that had invested in private equity prior to 1993. Thus, for MSA i in year t,

$$LP\ returns_{it} = \sum_{j} \sum_{s=t-1}^{t-3} \frac{ER_s \ln(1+LP_j)}{1+dist_{ij}}, \qquad (2)$$

where ER_s denotes the average returns to college endowments in year s, $\ln(1+LP_j)$ denotes the logged count of limited partners located in MSA j who had invested in any private equity fund in 1992 or earlier (plus one to avoid zeros), 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 to them and for the fact the venture capitalists have a higher propensity to invest in target companies located near to their headquarters. We summed three years of inflows because venture capital firms typically spread their initial investments in target companies over the first three to four years of their funds (Samila and Sorenson 2011). The investments produced by rebalancing should therefore have lasting effects on regional economies.

As noted above, the validity of our instrument also depends on a second assumption, that the path described—that is, an increase in the local supply of venture capital—forms the only connection between institutional investor returns and the economic health of the region. We made several choices in the construction of the instrument to ensure that this assumption holds. First, instead of using the actual returns of limited partners, we used the national average returns for a year as a proxy for these returns. If we instead used the actual returns, one might worry about reverse causality—that the institutional investors in the region did well because of the strength of the local economy. By using national average returns, we eliminated this potential threat to the validity of our instrument.[12]

[12] We also tested to see whether colleges and universities in employee-friendly regimes experienced significant differences in the returns on their portfolios. We found no evidence of any such differences.

Second, instead of using a time-varying count of limited partners in the region, we fixed the count at the number that had invested in private equity prior to our observation window. If instead we had used a time-varying count, one might worry that institutions entered endogenously because of the strength of the local economy. By using the count from the beginning of our observation window, we remove this potential threat to the instrument's validity. Moreover, because the models include fixed effects for each region, these intercepts should absorb any variation in the reasons why some regions have more or fewer active institutional investors at the beginning of the observation window.[13]

Model (1) of Table 5 reports the first-stage estimates for the instrumental variable and our measure of venture capital activity. As expected, LP returns strongly predict venture capital activity. The Kleibergen and Paap (2006) (KP) *rk* Wald *F*-statistic tests whether our instrument predicts a sufficient amount of the variance to identify our equations. For two-stage least squares (2SLS) estimation with one instrument and one endogenous variable, Stock and Yogo (2005) report a critical value of 16.38 for the IV estimates to have no more than 10% of the bias of the ordinary least squares (OLS) estimates. Our observed *F*-statistic of 173 indicates that we need not worry about instrument weakness.

To incorporate the interaction terms, we estimated the IV results in two stages.[14] We first regressed venture capital activity on the instrument (*LP returns*), *population*, year dummies, and region fixed effects, exactly as in the first stage of a standard 2SLS estimation. We then predicted the value of the venture capital measure using the estimated coefficients and used that prediction and its interaction with the enforcement of noncompete covenants (*ANC* and *WNC*) in the second-stage regressions. Because least squares estimation does not produce the correct standard errors for predicted values, we estimated the standard errors by bootstrapping the regression 10,000 times.

[13] One might still worry that limited partners could have self-selected into investing in venture capital on the basis of their (accurate) expectations of future economic growth in the region. To address this possibility, we instrumented the supply of venture capital using the count of the entire population of nonprofit insurance companies and pension plans within each region—both those that invested in venture capital pre-1993 and those that did not. Even this more conservative specification produced statistically equivalent results.

[14] An alternate approach for including an interaction with an instrumental variable involves instrumenting both the endogenous variable, VC activity, and the interaction of the endogenous and exogenous variables with the instrument and the interaction of the instrument with the exogenous variable. This approach produced substantively equivalent results to the ones reported here.

Table 5 Instrumental Variables Estimates

	(1) Ln *first stage*	(2) Ln *patents*	(3) Ln *patents*	(4) Ln *births*	(5) Ln *births*	(6) Ln *employment*	(7) Ln *employment*	(8) Ln *payroll*	(9) Ln *payroll*
LP returns	0.0149***								
	(13.23)								
Ln *population* $(t-1)$	1.529***	1.453***	1.498***	0.722***	0.732***	0.774***	0.787***	1.088***	1.103***
	(3.53)	(4.40)	(4.57)	(7.20)	(7.25)	(15.03)	(15.77)	(12.81)	(12.96)
Ln *VC count* (p) (t)		0.0880***	0.111***	0.0376**	0.0428***	0.0114**	0.0145***	0.0722***	0.0753***
		(2.67)	(3.30)	(2.67)	(3.12)	(2.24)	(3.00)	(7.24)	(7.63)
Ln *VC count* $(p) \times$ *ANC* (t)		0.156***		0.0358*		0.0304***		0.0328	
		(2.94)		(1.92)		(3.30)		(1.22)	
Ln *VC count* $(p) \times$ *WNC* (t)			0.116		0.0271		0.0539***		0.0631
			(1.13)		(0.74)		(3.22)		(1.59)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
KP Wald F-stat.	175.10								
R^2	0.22								
Clusters	328	328	328	328	328	328	328	328	328
Observations	2,935	2,935	2,935	2,935	2,935	2,935	2,935	2,935	2,935

Notes. 2SLS regression results are shown, with bootstrapped t-statistics in parentheses. 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 the count of patent applications. In models (4)–(6), the dependent variable is births of new establishments for firms with 0–19 employees at the beginning of the year.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

The results of these IV estimates appear in the remaining columns of Table 5. Beginning with the effects on patenting and firm founding, we see a consistent set of results. As in the OLS estimates, venture capital has a positive effect on patenting in all states, but it has even stronger effects in those states that do not enforce noncompete agreements. The difference is large; states that do not enforce these agreements experience twice the increase in patents of those that do enforce them in response to an influx of venture capital. Establishment births show a similar pattern.

Turning to the broader regional economy, the results also support the conclusions from the OLS regressions: employee-friendly regimes significantly amplify the beneficial effects of venture capital on employment. In fact, employee-friendly regimes enjoy roughly three times the employment growth of firm-friendly regimes in response to an increase in venture capital. A similar relationship appears to hold between venture capital and the aggregate income in an MSA. Here, however, we cannot reject the possibility that the coefficients on the interaction terms do not differ significantly from zero. Though the point estimates exceed those of the fixed-effects models, the inefficiency of the IV estimates has led to large error margins around this estimate.

LP Returns (*Insurance and Pensions*). Any remaining threat to the validity of the IV estimates would need to stem from a process that would produce a positive within-region, region-specific, time-varying relationship between the portfolio returns of institutional investors and the economic health of a region. We could think of only one: perhaps these institutions increase their spending in response to a good year of investment returns. For example, a university might expand its faculty or raise its salaries more in the years following strong endowment returns. To address this issue, we reran our analyses including only insurance companies and pension plans in our count of institutional investors (i.e., in LP_i). These classes of institutional investors face the same asset allocation issues as universities and other institutional investors. Their payouts, however, depend either on random events (in the case of insurance companies) or on the age distribution of their beneficiaries (in the case of pension plans); their payouts therefore do not vary with their investment returns. The results of this analysis appear in Table 6. As one can see, the results remain robust to focusing on this smaller set in which the instrument most clearly identifies exogenous variation in the supply of venture capital.

Although our results point strongly to differing causal relationships, as a function of the enforcement of noncompete agreements, between the local supply of venture capital and entrepreneurship, innovation, and economic growth, at least one threat does remain in the interpretation of these results, namely, the legal regimes, themselves, have not been randomly assigned. The use of region fixed effects allows us to rule out a wide variety of confounding factors that might influence these outcomes. But, if (i) some other factor correlated strongly across regions both with the absence of noncompete enforcement and with the weakness of noncompete enforcement, and

Table 6 Instrumental Variables Estimates for Instrument Limited to Insurance and Pensions

	(1) Ln *first stage*	(2) Ln *patents*	(3) Ln *patents*	(4) Ln *births*	(5) Ln *births*	(6) Ln *employment*	(7) Ln *employment*	(8) Ln *payroll*	(9) Ln *payroll*
LP returns (insurance & pensions)	0.0242*** (6.22)								
Ln *population* $(t-1)$	1.544*** (3.13)	1.466*** (4.32)	1.523*** (4.61)	0.695*** (6.54)	0.720*** (6.57)	0.767*** (15.17)	0.785*** (15.29)	1.086*** (12.26)	1.097*** (12.24)
Ln *VC count* (p) (t)		0.0786** (1.97)	0.0927** (2.23)	0.0488** (2.01)	0.0529** (2.11)	0.0167*** (2.80)	0.0169*** (2.82)	0.0805*** (6.35)	0.0791*** (6.63)
Ln *VC count* $(p) \times$ *ANC* (t)		0.186** (2.46)		0.0710*** (2.97)		0.0347** (2.54)		0.0118 (0.42)	
Ln *VC count* $(p) \times$ *WNC* (t)			0.0986 (0.78)		0.0651 (1.34)		0.0710*** (3.48)		0.0562 (1.43)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
KP Wald F-stat.	38.73								
R^2	0.17								
Clusters	328	328	328	328	328	328	328	328	328
Observations	2,935	2,935	2,935	2,935	2,935	2,935	2,935	2,935	2,935

Notes. 2SLS regression results are shown, with bootstrapped t-statistics in parentheses. 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 the count of patent applications. In models (4)–(6), the dependent variable is births of new establishments for firms with 0–19 employees at the beginning of the year.

$^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

(ii) that factor also improved the efficacy of venture capital in producing entrepreneurship, innovation, and economic growth, then that factor—rather than noncompete enforcement—could account for our results. Though we do not have a likely candidate, this possibility remains.

5. Discussion

Some scholars have questioned the importance of noncompete covenants, suggesting that regions that enforce these agreements could have developed alternate mechanisms for ensuring labor mobility and the associated knowledge spillovers (e.g., Wood 2000). Our results suggest otherwise. We find that the enforcement of noncompete covenants moderates the effects that venture capital has on both innovation and the overall regional economy. More specifically, our results imply that not only does the enforcement of noncompete agreements limit entrepreneurship, consistent with the earlier findings of Stuart and Sorenson (2003), but it also appears to *impede* innovation.[15]

We further find that regions as a whole benefit from an employee-friendly legal regime through greater employment. Here, it is interesting to consider the size of these effects. Our estimates suggest that a doubling in the number of venture capital investments in an average region would result in 17 to 41 more firms if the region did not enforce noncompete agreements (depending on whether one uses the OLS or IV estimates). That same doubling in investments predicts 4,273 to 6,767 more jobs in these employee-friendly jurisdictions. If all of these jobs came from the start-ups, then the average start-up would need to employ more than 150 people. Because that number dramatically exceeds the actual scale of these fledgling firms, it suggests that a substantial portion of the job growth in the regions with employee-friendly regimes comes not from the start-ups themselves, but from spillovers in the economy to established firms. Both incumbents and entrants therefore may well benefit from the greater mobility of employees.

These results may tell us a great deal about why some regions appear to have benefited more from venture capital than others. Several regional and national governments around the world have attempted to grow local venture capital communities in the hope of mimicking the success of dynamic regions such as Silicon Valley (Gilson 2003). The success of these attempts, however, has been varied. Our estimates suggest that communities or states that implement programs to promote venture capital without adopting supportive labor laws may have little hope of seeing benefits from these programs (even if they succeed in increasing the supply of venture capital).

But the results also offer hope for these regions. The fact that we find similar effect sizes in both

[15] We cannot rule out the possibility that patenting increases in these regions as firms substitute patents for noncompete agreements to protect their intellectual property. However, consistent with the idea that innovation declines in these regions, Garmaise (2009) finds that firms invest *less* in R&D under regimes of strong noncompete enforcement.

our discrete and continuous measures of noncompete enforcement suggests that jurisdictions need not adopt California's extreme stance to increase the efficacy of venture capital. Rather, by incrementally adopting more employee-friendly provisions, it would appear that states could gradually improve the regional returns to these investments. In this sense, our results accord well with prior findings, from China, that even relatively minor institutional changes that improve the fluidity of labor markets can have relatively large effects on the efficiency of an economy (Groves et al. 1994).

Heterogeneity in labor laws, moreover, may have even larger effects across countries than it does across states within the United States. Consider the case of Canada, or more specifically Ontario. On the one hand, the province seems well suited to venture capital. Its universities produce cutting-edge research. It is home to high-tech industry leaders, such as ATI and Research in Motion. Its government has done much to develop a local venture capital community. But the region has yet to develop the dynamics of a successful high-tech cluster. Part of the answer may reside in the way common law in Canada effectively bars management-level employees from leaving to competing firms, even in the absence of actual noncompete clauses. This broad interpretation of management's fiduciary duty could have unintended consequences by effectively precluding the emergence of spin-off firms and, concomitantly, of a self-sustaining cluster.

In this respect, our findings also suggest a new research agenda. The literature analyzing the effectiveness of attempts to stimulate venture capital has focused almost entirely on the internal features of these programs, such as the incentives that they offer to the professional investors (e.g., Gilson 2003). But the reasons why some government programs have succeeded whereas others have failed may well reside outside of the programs themselves and reflect instead the broader institutional environments in which these policies have been implemented. Labor law matters. Perhaps the effectiveness of venture capital depends on other features of the environment as well, such as taxes, public support for research and development, or even the degree of connectedness between the academic, business, and financial communities. We therefore see a need for a research program that considers the broader context as a potential catalyst for financial capital.

Acknowledgments

Grants from the Social Science and Humanities Research Council of Canada to S. Samila, through its institutional grant to Brock University, and to O. Sorenson, through Grant 410-2007-0920, provided financial support for this research. The authors thank Mark Garmaise for graciously providing access to his data and Gérard Cachon, Lee Fleming, April Franco, two reviewers, and an associate editor for their insightful comments.

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