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## NON-COMPETE COVENANTS: INCENTIVES TO INNOVATE OR IMPEDIMENTS TO GROWTH

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JEL - codes: G24, K31, O49

# Non-compete covenants: Incentives to innovate or impediments to growth\*

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**Keywords:** Venture capital, financial intermediaries, legal institutions, entry, employment, innovation, wages

**JEL Classification:** G24, K31, L26, O43, R11

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# 1 Introduction

Non-compete covenants are clauses in contracts that expressly prohibit employees from competing with their current employer should they leave the firm. They have become a common contractual feature, particularly for technical employees and upper level management. In the United States, for example, surveys have found that nearly 90% of these key employees have signed non-compete agreements (Leonard 2001; Kaplan and Stromberg 2003).

Though a nearly ubiquitous feature of contracts, their enforcement nevertheless varies from state to state. Some states, such as California, disregard these agreements except in rare cases, while other states, such as Massachusetts, generally enforce them (Gilson 1999). Although these differences almost invariably stem from state-level statutes or case precedents that long precede the usage of non-compete covenants in employment contracts, they may nonetheless influence the economic climate and vitality of regions into the present day. Gilson (1999), for example, has implicated this difference in legal infrastructure as a crucial factor behind Silicon Valley's surpassing of the Boston area as the capital of high technology.

Despite the potential importance of this issue to public policy, however, little research has considered systematically how the enforcement or non-enforcement of non-compete agreements may affect regional economies as a whole. Evidence does exist that the non-enforcement of non-competes increases mobility. Fallick et al. (2006) and Marx et al. (2009), for example, report higher levels of mobility among technical workers under regimes of non-enforcement. Similarly, Stuart and Sorenson (2003) find that states that do not enforce these agreements appear to have higher rates of spin-outs following acquisitions and IPOs. Given that non-compete covenants limit employees' outside options, these findings are unsurprising. But one cannot conclude from them that states should not enforce these agreements.

Non-compete covenants also have a positive side. They help companies to protect their investments in human capital, intellectual property and relationships. Companies can often increase their productivity by training their workers, by developing new products and

processes, and by building valuable relationships with customers and suppliers. But these investments carry with them the risk that some employee (or group of employees) might leave the firm, taking these valuable skills and contacts with them. By restricting the mobility of employees, non-compete covenants help to ensure that companies can reap the rewards of their investments. Legal regimes that do not enforce these agreements may unintentionally encourage companies to underinvest in human, intellectual and relational capital. Hence, the overall effect of non-compete clauses on the broader economy remains an open question.

It remains open in part because it has only recently received attention, but also in part because of the empirical difficulties surrounding it. Most notably, the variation in enforcement is almost entirely cross-sectional. With only a couple of exceptions, regions have had stable legal regimes with respect to the enforcement of non-compete covenants for decades (Richey and Malsberger 1996; Gilson 1999). Even states that have shifted their stances have generally made only minor adjustments in their enforcement of them. But any analysis relying entirely on cross-sectional variation in these legal regimes would have great difficulty distinguishing the effects of the enforcement of non-compete covenants from the multitude of unmeasured factors that might confound such an estimate.

We address this issue through an indirect route. In particular, we exploit variation over time in the availability of venture capital to estimate the net effect of these legal regimes.<sup>1</sup> Venture capital stimulates entrepreneurship. Because potential entrepreneurs face fewer constraints in states that restrict or do not enforce non-compete covenants, one would expect venture capital to have even stronger effects on entrepreneurship in these jurisdictions. The more interesting question is: How does this increase in entrepreneurship affect the economy at large, and does that that effect differ across legal regimes?

Using a panel data set covering metropolitan areas in the United States from 1993 to 2002, we estimate the causal effect of venture capital on patenting, entrepreneurship, employment

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<sup>1</sup>Our approach builds on the results of Samila and Sorenson (2008). They find that the supply of venture capital increases entrepreneurship and economic growth. Here, we explore the moderating effect of non-compete covenants.

and economic growth for states that do and do not restrict non-compete covenants. We instrument for the availability of venture capital with a variable related to venture capital fundraising but not (directly) to entrepreneurship: endowment returns. To maintain their optimal asset allocations, institutional investors must adjust their commitments to venture capital in response to the performance of the rest of their portfolios. Our results suggest that non-compete covenants strongly moderate the effect of venture capital on start-up activity, as well as on the economy as a whole. In states that restrict or do not enforce non-compete covenants, increases in the availability of venture capital result in larger increases in the level of entrepreneurship in the region, a result consistent with prior studies that have found a negative relationship between non-compete covenants and mobility (Stuart and Sorenson 2003; Fallick et al. 2006; Marx et al. 2009). In response to an influx of venture capital, patenting also increases more in regions that restrict or do not enforce non-compete agreements. Although we find that increases in the supply of venture capital result in greater employment in states that restrict or do not enforce non-compete covenants, we do not find evidence of variation by legal regime in the effects of venture capital on wage growth.

By providing evidence for the moderating effects of non-compete covenants on the effectiveness of venture capital in fostering entrepreneurship and economic growth, we contribute to a better understanding of the relationships between both venture capital and legal institutions and regional economic dynamics. The fact that these two factors interact has at least three important implications. Most immediately, it suggests that policies aimed at stimulating entrepreneurship in a region through the subsidization of venture capital may have a limited effect if the labor laws in the jurisdiction do not support such investments. If other entrepreneurs behave similarly to those supported by venture capital, these interactions also suggest that the stricter enforcement of non-compete agreements probably comes at a net cost to the regions that have adopted such a stance. More generally, it suggests that legal infrastructures can importantly moderate the effectiveness of financial intermediation and influence the dynamics of regional industrial clusters.

## 2 Non-Compete Covenants

Non-compete covenants in labor contracts stipulate that employees may not work for competing firms, including start-ups, in the event that they leave their jobs. These agreements have become near-standard features of contracts among certain sorts of employees, including executives, research and development staff, and salespeople.

Though non-compete clauses have become a common feature of labor contracts across the United States, their enforcement varies considerably from state to state.<sup>2</sup> The majority of states enforce non-compete agreements by the “rule of reason” (Gilson 1999). Courts in these jurisdictions generally evaluate an agreement as reasonable, and therefore valid, if it does not prevent the individual from being gainfully employed and if the terms of the agreement do not appear longer in duration or broader in geographic scope than necessary to protect the prior employer. But many states also restrict non-compete agreements. At the extreme, several states have statutes or precedents that essentially preclude the enforcement of non-compete clauses. For example, California’s Business and Professions Code §16600 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 rendering null and void not just clauses that would prevent, but even those that would merely penalize, post-employment competition (Gilson 1999).

These state-level differences in enforcement regimes generally have long historical roots.<sup>3</sup> In Massachusetts, for example, the relevant case law stems from English common law dating

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<sup>2</sup>In the United States, labor law falls under the jurisdiction of state-level courts. For summaries of the enforcement of non-compete covenants by state, see Stuart and Sorenson (2003, p. 190) and Garmaise (2008, p. 44). For more detailed descriptions of the case law in each state, see Richey and Malsberger (1996).

<sup>3</sup>Most states have had stable enforcement regimes for decades. Three states have nevertheless seen meaningful changes in enforcement over the last 30 years. In 1985, Michigan’s legislature unintentionally eliminated the statute that made non-compete agreements unenforceable in that state (Marx et al. 2009). For a period of roughly two years before the Louisiana Legislature amended its laws, a ruling from the Louisiana Supreme Court made non-compete covenants unenforceable in Louisiana, except as a barrier to the founding of a competing firm (Long 2005). Though Florida has always enforced non-compete clauses, in 1996, the state legislature modestly strengthened this enforcement (Richey and Malsberger 1996).

to the time of the guilds and even its more proximate roots reach to the mid-to-late nineteenth century (Gilson 1999). The California Business and Professions Code, meanwhile, emerged in the middle of the nineteenth century from the newly established state's urgent 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 into the present day. The enforcement of covenants not to compete has been shown to restrict the mobility of employees (Fallick et al. 2006; Marx et al. 2009) and to reduce the rate of entrepreneurship (Stuart and Sorenson 2003). Gilson (1999) speculates that this friction in the labor market may even hinder economic growth. Policymakers might therefore wish to emulate California in precluding the enforcement of these clauses. But the effect of this friction for the economy as a whole remains an open question because the enforcement of these agreements also strengthens the incentives for firms to invest in certain kinds of assets that they would otherwise find difficult to protect.

## **2.1 Incentives to invest**

To the extent that non-compete covenants restrict the mobility of employees, they encourage firms to invest in at least three kinds of assets: intellectual property, human capital and inter-firm 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 limited alternative mechanisms for protecting their investments in these assets. Companies must therefore worry that employees might appropriate the value of their investments either by leaving or threatening to leave their jobs. At the regional level, these incentives to invest may stimulate growth as companies focus their investments in regions that provide greater protection over capturing the returns from them.

**Intellectual property:** The most discussed justification in the literature for enforcing non-compete agreements stems from their role in protecting intellectual property rights. Although companies can often protect inventions – discrete, codifiable entities – with patents, much of the innovation in which firms engage produces tacit knowledge: routines and practices that are not easily codifiable. These innovations can contribute crucially to the efficiency of firms and may even serve as a source of competitive advantage. Yet, the tacit nature of these innovations means that the firm cannot easily separate them from the individuals in which they reside. Not only does this fact create the possibility that these innovations may spill over to other firms should employees with the relevant knowledge defect to them, but also it means that a firm could even lose the ability to access these assets themselves (if all of those with the knowledge left). Where enforced, non-compete clauses give the employer effective property rights over this tacit knowledge by preventing employees from using it in competing settings for a period of time (Gilson 1999).

Even when companies have available alternative mechanisms for protecting their intellectual property, the enforcement of non-compete covenants might still strengthen these protections, and therefore augment the incentives that they provide for innovation. Consider patents, for example. The property rights to an invention made by an employee generally belong to the employer. An invention, however, is considered to have been discovered at the time when the inventor first conceives of the complete invention, supported by objective evidence (Gilson 1999). Hence, an employee who chooses to leave his or her employer before fully developing an invention – or before creating the evidence to support it – can retain the property rights to it. If the labor contract included an enforceable non-compete clause, however, the inability of the employee to take that invention with them to a startup or to a competing firm might compel the employee to pursue commercialization through his or her current employer.

**Human capital:** Long (2005) draws attention to another form of incentive. Non-compete

covenants also have the ability to protect more general investments in human capital. Given the rapidly changing nature of work in many fields, firms can often improve their performance by updating and upgrading the human capital of their labor forces. Companies can develop the human capital of their employees in many ways. Concomitant to doing their jobs, employees often improve their ability to perform routine tasks through learning-by-doing. Companies can also accelerate this acquisition of skills by assigning more experienced employees to mentor those learning new tasks. They may even sponsor classes or compensate their workers for attending courses outside of the workplace. Capelli (1999), for example, notes that companies now cover or subsidize a large share of part-time college education.

Regardless of the source of this human capital, individual employees retain the rights to it (Long 2005). 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 from their gains in human capital if they change firms (Becker 1964). But more general skills pose a problem. In the absence of a means of tying the employee to the firm, once they have received the training, employees might market their newly-gained skills to other firms, seeking higher salaries. Rational employers, recognizing this problem, will therefore refuse to invest in these more general skills – despite their value to the firm and to society – unless employees accept lower wages in exchange for this training (Becker 1964).<sup>4</sup> Enforceable non-compete covenants therefore can encourage employers to invest more heavily in human capital, particularly in more general skills.

**Business relationships:** To intellectual property and human capital, we would add a third kind of asset in which enforceable non-compete agreements provide incentives to invest: social capital. Though business relationships have received little attention in the academic

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<sup>4</sup>A literature on the design of incentives has sought solutions to this problem. To some extent, companies can protect their investments by adopting pay structures that either increase over time or that contain a large contingent component (Pakes and Nitzan 1983; Møen 2005). But these compensation strategies still increase the cost of capturing the gains from these investments and therefore may still discourage them.

literature on non-compete covenants, it appears in practice that companies recognize the value of non-compete clauses as a means of protecting their business relationships. These clauses, for example, are common among salespeople, a class of employees whose value to the company resides almost entirely in their social connections. One also often sees them in professional services – such as accounting, consulting, law and medicine – where relationships with clients play a particularly important role (Maister 1993).

A large literature in management and sociology has trumpeted the value of having 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 those goods (Kollock 1994). They can also improve the efficiency of supply chains by allowing for the exchange of fine-grained information and by facilitating the coordination of joint problem-solving across stages of production (Uzzi 1996). A company's partners can even overestimate the value of these relationships and, as a consequence, may effectively transfer value to the company by paying higher prices to it or by investing more heavily in joint projects with it (Sorenson and Waguespack 2006). Because of their value, companies invest valuable resources in building and maintaining these relationships.

Despite the evidence of their value, however, the ownership of these relationships remains somewhat ambiguous. Although the literature conceptualizes inter-firm relationships as belonging to the partner 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 often see these ties transfer to their competitors when employees walk out the door. Salespeople, accountants, consultants, doctors and lawyers frequently bring clients with them when they move to a new firm or set up their own practice. Where enforced, non-compete clauses effectively allocate property rights over these relationships to the employer, and therefore may encourage companies to invest more into developing them.

## 2.2 Impediments to growth

Though the enforcement of non-compete covenants may encourage companies to invest in an array of assets, by limiting the mobility of individuals, it may nonetheless also impede 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 non-compete agreements has pointed most prominently to spillovers as a reason why regions might not want to enforce them. As noted above, much of the valuable knowledge that firms create is tacit and embodied in individuals. The diffusion of these innovations across firms therefore occurs through the movement of employees. To the extent that many firms might benefit from the innovations initially developed at (and paid for by) one firm, this pooling of knowledge can improve the competitiveness of many firms in a region. But, of course, the sharing of knowledge across firms faces a collective action problem: Each company wants to prevent its own employees from leaving but wants to enjoy spillovers by hiring the former employees of other firms (Combes and Duranton 2006). By refusing to enforce non-compete clauses, jurisdictions can solve this collective action problem and promote spillovers (Gilson 1999).

The enforcement of non-compete covenants also limits entrepreneurship. Though some simply see this effect as another form of spillover (from incumbent firms to startups), non-compete agreements could also stymie innovation. Even though a large share of entrepreneurs spin out of existing companies in the industry (Sorenson and Audia 2000; Klepper 2002; Franco and Filson 2006), research suggests that many of these new ventures nevertheless pursue novel lines of business (Klepper 2007). In other words, these businesses do not compete directly with the founders' prior employers. In regions that enforce non-compete agreements, one might nonetheless find lower rates of entrepreneurship for at least two

reasons.<sup>5</sup> First, the entrepreneur, even if pursuing an idea distinct from that of the prior employer and developed on the entrepreneur's own time, could face hold-up by their former employers. Second, even if non-compete clauses do not prevent them personally from starting their ventures, entrepreneurs might find it far more difficult to get their organizations off the ground if they cannot hire employees with experience in the industry because those potential employees are bound by non-compete agreements (Stuart and Sorenson 2003).

Finally, by limiting mobility, non-compete clauses, when enforced, might reduce the average quality of the matches between employees and employers. If one assumes that employees are heterogenous and that firms vary in the abilities and attributes that they require from employees, then the quality of the match between employees and 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 potential employees. 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.

### 3 Empirical Evidence

Although studies have examined how the enforcement of non-compete covenants influences the mobility of employees and entrepreneurship rates, whether this enforcement influences the economy as a whole more strongly through the incentives that it gives firms to innovate or through the ways in which it impedes growth remains an open question. Part of the difficulty

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<sup>5</sup>On the other hand, the prospect of losing one's employees to competitors might also lead entrepreneurs to shun states that do not enforce non-compete clauses in favor of locations that offer greater protection for their investments, particularly in the early stages of cluster development (Franco and Mitchell 2008).

in examining this issue comes from the fact that few states have witnessed meaningful changes in their legal regimes (see footnote 3). A purely cross-sectional analysis, therefore, cannot easily disentangle the effects of the enforcement of non-compete agreements from a host of other factors that vary from one region to the next.<sup>6</sup>

By combining this cross-sectional variation with within-region variation on another important factor that influences entrepreneurship and innovation rates, however, one can gain greater purchase on the effect of these legal regimes. Here, we exploit cross-sectional variation in the response of regions' economies to exogenous shocks in the supply of venture capital. Venture capital involves the funding of young, high-potential firms through equity investments. By allocating capital to companies that might otherwise not receive funding from traditional sources, such as bank loans, venture capital firms stimulate entrepreneurship and innovation. Indeed, recent research suggests that the supply of venture capital in a region has a strong positive effect on entrepreneurship in that region, even beyond the firms that actually receive funding (Samila and Sorenson 2008). But does the effect of this supply vary according to the legal infrastructure? If the incentives to innovate outweigh the impediments to growth then one would expect venture capital to have stronger positive effects on the economy in states that enforce non-compete agreements. On the other hand, if the impediments to growth exceed the incentives to innovate, then one would expect the opposite relationship.

Our empirical analysis uses an unbalanced panel data set of all 328 Metropolitan Statistical Areas (MSAs) in the contiguous United States from 1993 to 2002.<sup>7</sup> Each MSA consists of an urban core and a tightly integrated surrounding area. If more than 25% of a county's

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<sup>6</sup>Although Marx et al. (2009) could use a change in Michigan's legal stance to identify the effects of the enforcement of non-compete clauses on mobility, its effect on the economy as a whole is a region-level outcome and hence this experiment does not offer enough points of observation to infer anything about the more macro-level implications of this policy change.

<sup>7</sup>MSAs are defined by the Office of Management and Budget (OMB) roughly three years after each decennial census. The revised definitions from the 1990 census came into use in 1993 and remained in effect until 2002. We limited our study to this ten-year window because consistent definitions of the areal units over time are essential for our analyses.

labor force, or of a township’s labor force in the case of New England, commutes to the urban core, the OMB includes the county or township in the MSA. MSAs therefore offer the smallest geographic regions that one might consider independent in terms of economic activity.

For each MSA, we gathered data from multiple sources, both public and private. The economic data comes from the Small Business Administration, which collects it annually from the Census Bureau. 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 non-compete clauses we collected data from Richey and Malsberger (1996). Information on endowment returns comes from *The Chronicle of Higher Education*. Finally, we obtained data on patents from the National Bureau of Economic Research.

### 3.1 Dependent variables

To assess the net effects of the enforcement of non-compete covenants, we collected measures that capture the creation of new businesses, innovation, and the overall health of the regional economy.

**Patents:** Our measure of innovation comes from patent data. 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 a patent to an MSA based on the address of the inventor 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 we divided the patent equally across the inventors’ addresses.<sup>8</sup> We counted the total number of patents in each MSA-year and transformed this count using the natural logarithm.

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<sup>8</sup>Alternatively, we could have done the assignment using the first inventor, but our approach recognizes the contributions of all those deemed important enough to appear on the application.

**Establishment births:** Our primary measure of entrepreneurship is the total number of new business establishments. The Census Bureau defines business establishments as single physical locations in which business takes place 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 at the same time the following year. A firm may have multiple establishments, but each firm has at least one.

One possible drawback 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 new 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 new firms to these categories based on their size at the end of the year. Since few startups have more than 19 employees by the end of their first year, we focused on establishment births in the 0-19 employees category.<sup>9</sup> In our analyses, we used the natural logarithm, to reduce skewness, of the total number of establishments opened by firms with 0-19 employees at the beginning of the year.

**Employment and payroll:** To assess the response of the economy as a whole 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, employee contributions to pension plans, and the value of taxable fringe benefits during the calendar year. We transformed both variables using the natural logarithm to reduce their skewness.<sup>10</sup>

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<sup>9</sup>This category will nevertheless include some relocations and expansions of very small businesses, adding measurement error to our variable.

<sup>10</sup>The Census Bureau effectively 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-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

## 3.2 Independent variables

**Non-compete enforcement:** We created two measures to assess state-level differences in how statutes and case law restrict or preclude the enforcement of non-compete covenants. The first of these, *ANC* (for “absence of non-compete enforcement”), follows Stuart and Sorenson (2003). In particular, we created a state-level indicator variable that takes a value of one if the state generally precludes, through statutes or case law, the enforcement of non-compete covenants (or zero otherwise). The second, *WNC* (for “weakness of non-compete enforcement”), uses an approach proposed by Garmaise (2008). At the beginning of the section for each state, Richey and Malsberger (1996) report a series of twelve summary questions, such as whether the state imposes geographic or time limits when enforcing non-compete agreements and whether the employer or employee has presumption. Garmaise proposes a threshold value on each question that implies stricter enforcement. This index simply counts the number of questions above these thresholds. In order to ease comparisons across the measures, we rescaled this second measure to run from 0 to 1 (by dividing it by 9, the maximum value obtained by any state) and subtracted the resulting number from one (so that larger values would indicate weaker enforcement).

For each MSA contained entirely within one state, we used the state-level measure. For MSAs that straddle two or more states, we weighted the state-level measures according to the number of zip codes inside that MSA in each state. Thus, for example, an MSA with 60% of its zip codes within a state that precludes the enforcement of non-compete covenants (i.e.  $ANC = 1$ ), and 40% within a state that does not preclude the enforcement of non-compete covenants (i.e.  $ANC = 0$ ), would get a value of 0.60. Out of the 328 MSAs, 14 MSAs have mixed values for *ANC*.<sup>11</sup>

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predict changes in income (limiting us to using only nine years of wage data). We did not have month-level information on our instrumental variables and control variables, they therefore follow the normal calendar.

<sup>11</sup>These were Worcester, MA-CT; Washington, DC-MD-VA-WV; Portland-Vancouver, OR-WA; Fort Smith, AR-OK; Cumberland, MD-WV; Wheeling, WV-OH; Steubenville-Weirton, OH-WV; La Crosse, WI-MN; Parkersburg-Marietta, WV-OH; Huntington-Ashland, WV-KY-OH; Las Vegas, NV-AZ; Duluth-Superior, MN-WI; New London-Norwich, CT-RI; and Minneapolis-St. Paul, MN-WI.

**VC Investment Count:** We measured venture capital activity by counting the number of investments made by venture capital firms that year in the region. We only included investments by venture capital firms organized as limited partnerships with outside investors to exclude angel investors, corporate venture capital, direct investments by university endowments, and other investors who did not need to raise outside funding. Although these investors might have an effect on the regional economy as well, the nature of our instrumental variable constrains us to venture capitalists who need to raise outside funding.

We only counted each target firm the first time it received an investment. If the target received funds from more than one source in this first round (i.e. it received a syndicated investment), then we counted each of these investments. Since few first round investments involve syndication, this coding decision nevertheless has little effect on our results. To focus on the venture capital available in the region, we assigned each investment to a region based on the location of the venture capital firm, even if the recipient of the investment, the target company, resided outside of the region.<sup>12</sup> For example, if a venture capital firm based in New York City made an investment, we would increment the count for New York City by one, even if the target company fell outside of the New York City metropolitan area. For our analyses, we reduced the skewness of the measure by taking its natural logarithm, after adding one to avoid zeros in the logarithm. Table 1 presents summary statistics for these variables.

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<sup>12</sup>We dismissed the alternative of allocating the investment based on the location of the target company because it does not fit with the logic of our instrumental variable, which relates to the location of the supply of funds for venture capital investments rather than the location of investment opportunities. Since venture capital firms generally invest in geographically proximate target companies (Sorenson and Stuart 2001), this choice has little bearing on our results.

### 3.3 Fixed Effects Estimates

We began by estimating models using ordinary least squares regressions. We estimated a logged form of a standard production function:

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

where  $i$  indexes the MSA and  $t$  indexes the year,  $Y_{it}$  is the dependent variable (patent applications, births of new establishments, employment level or total payroll),  $P_{it}$  measures the population level,  $VC_{it}$  represents venture capital activity,  $NC_{it}$  denotes the strength of non-compete enforcement (*ANC* or *WNC*),  $\phi_t$  indicates a series of year fixed effects,  $\eta_i$  denotes the MSA fixed effects (partialled out), and  $\epsilon_{it}$  is the residual error.<sup>13</sup> A statistically significant value for  $\beta_3$  indicates that non-compete enforcement moderates the impact of venture capital.

In all models, we included region-specific fixed effects to control for all time-invariant aspects of each region, such as local institutions, the presence of colleges and universities, geographic factors, and the composition of the labor force. Using fixed effects effectively removes them and their impact from the models.

We also introduced year fixed effects to control for, and remove from the regressions, 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, and also the rate of venture capital investment and fund raising. The year effects effectively remove these national economic factors 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 we also only need

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<sup>13</sup>We did not assume an *iid* error structure. Repeated observations of the same geographic units could lead to correlated errors over time within regions. In ordinary least squares estimation, this correlation will not bias the estimates themselves, but it might affect the estimated standard errors, possibly overstating the significance of coefficients. We therefore estimated all of our models using standard errors robust to repeated observations of the same regions.

to worry about other MSA-specific within-MSA factors as possible confounds. 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.

Our fixed effects strategy does, nonetheless, have one drawback. Both of our measures of enforcement are essentially invariant during our sample period. Though we can test the effect of these regimes indirectly through their interaction with venture capital activity, the fixed effects do not allow us to estimate the effect of non-compete enforcement directly (the “main” effect is absorbed by the MSA fixed effects). This limitation nevertheless seems the lesser of two evils to us since the alternative – not using fixed effects – would leave us with no ability to distinguish the effects of legal regimes from a large number of alternative explanations.

Let us now turn to the estimation results, starting with Table 2, which reports the results with counts of patents and of new establishments as the dependent variables. 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 .03%. Or, for the interactions, a 1% increase in the number of firms funded by venture capital firms in a region that does not enforce non-compete agreements raises the number of patents in the region by .08% ( $= .0597 + .0205$ ).

As expected, venture capital has positive effects on both outcomes (models 1 and 4). When we examine the degree to which this varies across states as a function of their legal regimes, however, we see slightly different patterns. Venture capital clearly appears to have stronger effects on patenting in regimes that restrict or do not enforce non-compete covenants. At least two factors might account for this effect. First, the greater mobility of personnel in these jurisdictions may stimulate innovation by better enabling the recombination of existing technologies into new inventions (Fleming 2001). Second, firms might attempt to substitute patents for the intellectual property protection offered by non-compete covenants. We therefore cannot say for certain whether this measure reflects increased inno-

vation (though it seems improbable that the positive coefficient would appear if innovation actually declined). In terms of entrepreneurship, venture capital in regions with weak enforcement regimes appears somewhat more productive in terms of creating new firms, but the size of the effect is too small for us to reject the possibility that venture capital has equivalent effects across regimes.

Table 3 reports the results of our fixed effects estimates of the effect of venture capital on employment and aggregate payroll (i.e. the regional wage bill). Consistent with past research, we find positive effects of venture capital on both outcomes. We also find strong evidence that venture capital investments result in larger gains in employment and in aggregate income in regions that restrict or do not enforce non-compete agreements than those with stronger enforcement regimes. Though these estimates have relatively wide confidence intervals, the larger size of the coefficient estimates for the interactions in predicting payroll relative to employment suggests that the average wages in the regions that restrict non-compete covenants also increase somewhat in response to venture capital investments.

### 3.4 IV Fixed Effects Estimates

The OLS results may nonetheless be biased for at least two reasons: reverse causality and unobserved heterogeneity. 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. On the other hand, some unobserved MSA-specific within-MSA factor might confound our results. We address both of these issues through the use of an instrumental variable: limited partner (LP) returns.<sup>14</sup>

**LP Returns:** Most institutional investors diversify their investments using a (relatively)

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<sup>14</sup>We also estimated a set of models using the instrument suggested by Gompers and Lerner (2000): investments in LBO funds. Though that instrument produces a statistically equivalent set of results, we prefer the LP returns instrument for two reasons. First, it is more plausibly exogenous to regional economic activity. Institutions might invest in LBO funds in response to the attractiveness of the economy. Second, LP returns has a stronger relationship to venture capital investments in the first stage, and therefore produces more efficient second-stage estimates.

fixed proportional allocation across different asset classes – for example, 40% equities, 40% bonds, and 20% alternative assets – adjusting their investments towards this target allocation at regular intervals. Given the limited maturity of venture capital investments, an increase in returns to the total portfolio would lead to a greater flow of funds into new venture capital investments. Combined with a general preference for geographically proximate private equity funds (Samila and Sorenson 2008), the investment gains of local limited partners should strongly influence the availability of venture capital in a region.

Specifically, we construct our measure by first taking the national average returns to college and university endowments, an important class of institutional investors, and allocating it to different regions based on the number of limited partners in the region which had invested in any private equity funds, including venture capital but also leveraged-buyout and other funds, in the past ten years. Equation 2 provides the definition of this measure for MSA  $i$  in year  $t$ :

$$LP\ Returns_{it} = \sum_{s=t-1}^{t-3} ER_s \ln(1 + LP_{is}), \quad (2)$$

where  $ER_s$  denotes the returns to college endowments in year  $s$ ,  $\ln(1 + LP_{is})$  denotes the logged count of limited partners located in the MSA  $i$  who had invested in any private equity fund in the last ten years (plus 1 to avoid zeros). We summed three years of inflows to create our instrument because venture capital firms typically invest the funds that they raise over the first several years of the partnership (for further justification of this instrument, see Samila and Sorenson 2008). We lagged the time variable by one so that one year’s investments depend on the previous year’s returns.

Although the details of the institution suggest that this measure should offer a valid instrument – predicting venture capital activity but without a direct relation to our dependent variables of interest – we need not rely entirely on intuition. Table 4 reports the first stage estimates for the instrumental variables and our measures of venture capital activity. As expected, LP returns strongly predict venture capital activity. The Kleibergen-Paap  $rk$

Wald  $F$ -statistic (Kleibergen and Paap 2006), reported as “KP Wald  $F$ -statistic”, tests directly whether our instrument predicts a sufficient amount of the variance in the endogenous variables 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 OLS estimates. Our observed value of 103.0 clearly indicates that we need not worry about a weak instrument.<sup>15</sup>

To incorporate the interaction terms, we estimated the IV results in two stages.<sup>16</sup> 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 non-compete covenants in the second stage regressions. Because OLS does not properly estimate the standard errors of the estimates on the predicted values, we obtained the standard errors through bootstrapping the regression 10,000 times.

Like the OLS estimates, the IV models also include year and MSA fixed effects. The results of these models appear in Tables 5 and 6. Beginning with the effects on patenting and firm founding, we see a fairly consistent set of results. Venture capital has a positive effect on patenting in all states, but it has even stronger effects in those states that restrict or do not enforce non-compete agreements. The magnitude of this difference is large; states with weak enforcement regimes experience two to three times stronger effects than those that those with strict regimes. We see an even stronger differential in establishment births. In fact, in states with strict enforcement regimes, the small coefficient estimates suggest that venture capital may have no stimulative effect on entrepreneurship. The average relationship

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<sup>15</sup>We should note that, though the Kleibergen-Paap Wald statistic is robust to within-cluster correlation in the errors, Stock and Yogo (2005) only tabulated critical values for the case of uncorrelated errors.

<sup>16</sup>An alternative 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 statistically equivalent results to the ones reported here.

between venture capital and firm foundings appears to stem entirely from the states that either restrict or do not enforce non-compete agreements.

Turning to the broader regional economy, the results clearly support the earlier tentative conclusions from the ordinary least-squares regressions: not enforcing non-compete covenants significantly improves the impact of venture capital on the regional economy. In fact, we see a roughly three times larger positive relationship between venture capital and employment when non-compete covenants are not enforced. We see a similar relationship between venture capital and the aggregate income in an MSA. In those models, however, our error margins are sufficiently large that we cannot reject the possibility the effect of venture capital on wages does not vary across regimes. If we had greater confidence in these coefficients, we could compare them to the magnitude of the increase in employment, to determine whether venture capital had an effect on the average wage earned in a region (versus simply increasing overall wages through the employment of more people).

## 4 Discussion

We find evidence that the absence or restriction of non-compete covenant enforcement strongly moderates the impact that venture capital funding has on both innovation and the overall regional economy. More specifically, our results suggest that not only does the enforcement of non-compete agreements limit entrepreneurship, consistent with the earlier findings of Stuart and Sorenson (2003), but also it appears to *impede* innovation. Although we cannot rule out the possibility that patenting increases in these regions as firms attempt to substitute patents for non-compete agreements as a means of protecting their intellectual property, it seems quite plausible that the value of the recombination of knowledge facilitated by the elevated mobility of individuals across firms might outweigh the greater incentives to innovate afforded by the enforcement of these non-compete covenants. Some scholars, including Wood (2000), have questioned the importance of non-compete covenants, suggest-

ing that different regions might have developed alternative mechanisms for ensuring labor mobility and the associated knowledge spillovers. Our results strongly suggest otherwise.

We also find that the region as a whole benefits from the non-enforcement or restriction of these agreements 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 42 more firms if the region did not enforce non-compete agreements. That same doubling in investments predicts roughly 5,478 more jobs if the region does not enforce these agreements. If all of these jobs came from the startups, then the average startup would need to employ on the order of 130 people. Since that number dramatically exceeds the actual size of these firms, it suggests that a substantial portion of the job growth in the regions that do not enforce non-compete agreements comes, not from the startups themselves, but from spillovers in the economy to established firms. Both incumbents and entrants 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 mimicing 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 simultaneously changing their labor laws may have little hope of seeing benefits from these programs (even if they succeed in increasing the level of venture capital activity).

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's home to high-tech industry leaders, such as ATI, Nortel and Research in Motion. And its government has done much to try to stimulate a local venture capital community. Yet, the region appears to have yet to develop the dynamics

of a successful high-tech cluster. Part of the answer may lie in the way common law in Canada effectively bars management-level employees from leaving to competing firms, even in the absence of actual non-compete clauses. This broad interpretation of management's fiduciary duty may 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 have 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 while 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.

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Table 1: Summary Statistics

<b>Variable</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>N</b>
Patents	242.92	550.98	2935
Births	1411.65	2507.39	2935
Population (thousands)	655.96	1097.15	2935
Employment (thousands)	273.88	475.95	2935
Payroll (millions)	8508.95	17872.22	2935
VC Count	4.34	28.49	2935
Absence of NC	0.18	0.38	2935
Weakness of NC	0.52	0.23	2935

Table 2: FE Estimates for Innovation and Entrepreneurship

	(1)	(2)	(3)	(4)	(5)	(6)
	Patents	Patents	Patents	Births	Births	Births
Population	1.474*** (4.57)	1.485*** (4.59)	1.484*** (4.59)	0.823*** (10.17)	0.824*** (10.18)	0.826*** (10.17)
VC Cnt	0.0343*** (2.63)	0.0205 (1.53)	0.0312** (2.50)	0.0126*** (2.98)	0.0108** (2.25)	0.0118*** (2.78)
VC Cnt x ANC		0.0597** (2.00)			0.00783 (0.96)	
VC Cnt x WNC			0.0881** (2.01)			0.0216 (1.63)
$R^2$	0.18	0.18	0.18	0.24	0.24	0.24
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

*t* statistics in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors robust to heteroskedasticity and clustered on MSAs.

Table 3: FE Estimates for Regional Economy

	(1)	(2)	(3)	(4)	(5)	(6)
	Emplmnt	Emplmnt	Emplmnt	Payroll	Payroll	Payroll
Population	0.786*** (21.09)	0.789*** (21.19)	0.789*** (21.19)	1.141*** (15.54)	1.145*** (15.51)	1.146*** (15.61)
VC Cnt	0.00833*** (4.03)	0.00489** (2.27)	0.00752*** (3.93)	0.0254*** (5.56)	0.0211*** (4.50)	0.0241*** (5.87)
VC Cnt x ANC		0.0149*** (3.55)			0.0187 (1.44)	
VC Cnt x WNC			0.0225*** (3.16)			0.0369* (1.93)
$R^2$	0.77	0.78	0.78	0.93	0.93	0.93
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

*t* statistics in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors robust to heteroskedasticity and clustered on MSAs.

Table 4: First-Stage of IV Estimation

	(1)
	VC Cnt
Population	1.028** (2.54)
LP Returns	0.00965*** (10.15)
$R^2$	0.27
KP Wald $F$ -statistic	103.0
Clusters	328
Observations	2935

$t$  statistics in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors robust to heteroskedasticity and clustered on MSAs.

Table 5: IV Estimates for Innovation and Entrepreneurship

	(1)	(2)	(3)	(4)	(5)	(6)
	Patents	Patents	Patents	Births	Births	Births
Population	1.337*** (4.15)	1.326*** (4.07)	1.362*** (4.31)	0.808*** (9.54)	0.806*** (9.41)	0.816*** (9.44)
VC Cnt (p)	0.125*** (2.95)	0.0812* (1.96)	0.119*** (2.84)	0.0227* (1.80)	0.0135 (0.95)	0.0207 (1.55)
VC Cnt (p) x ANC		0.198*** (2.89)			0.0413** (2.01)	
VC Cnt (p) x WNC			0.187 (1.39)			0.0596* (1.75)
$R^2$	0.18	0.18	0.18	0.24	0.24	0.24
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

$t$  statistics in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors robust to heteroskedasticity and clustered on MSAs.

Table 6: IV Estimates for Regional Economy with Absence of NC Binary

	(1)	(2)	(3)	(4)	(5)	(6)
	Emplmnt	Emplmnt	Emplmnt	Payroll	Payroll	Payroll
Population	0.776*** (16.24)	0.774*** (15.88)	0.783*** (16.04)	1.051*** (13.13)	1.049*** (13.00)	1.060*** (13.08)
VC Cnt (p)	0.0151** (2.43)	0.00902 (1.32)	0.0134** (2.18)	0.0855*** (6.04)	0.0780*** (5.64)	0.0831*** (6.33)
VC Cnt (p) x ANC		0.0274** (2.31)			0.0339 (1.10)	
VC Cnt (p) x WNC			0.0529** (2.52)			0.0709 (1.49)
$R^2$	0.77	0.77	0.78	0.93	0.93	0.93
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

*t* statistics in parentheses

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors robust to heteroskedasticity and clustered on MSAs.