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Non-compete Covenants: Incentives to Innovate or Impediments to Growth

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Abstract:
We find that the enforcement of non-compete 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 non-compete 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 issues 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.

Keywords: Venture Capital; Financial Intermediaries; Legal Institutions; Entrepreneurship; Employment; Innovation; Wages

Jel codes: G24; K31; L26; O43; R11

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1 Introduction

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

Though nearly ubiquitous, the enforcement of these agreements nevertheless varies from state to state. Some states, such as California, disregard them except in rare cases, while other states, such as Massachusetts, generally enforce them (Gilson 1999). Though these differences almost invariably stem from state-level statutes or precedents that long precede the modern usage of non-compete covenants, these differences may nonetheless influence the economic climate and vitality of regions to the present day. Gilson (1999), for example, has forwarded 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 systematically how the enforcement of non-compete agreements affects regional economies as a whole. Evidence does exist that the non-enforcement of non-competes, which we refer to as an "employee-friendly" regime, increases mobility. Fallick et al. (2006), Garmaise (2008) 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 IPOs in the industry. Given that non-compete covenants limit employees' outside options, these findings seem unsurprising. But one cannot conclude from them that states should void 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 mobility, non-compete covenants help to ensure that companies can reap the rewards of their investments. Employee-friendly regimes may therefore unintentionally encourage companies to underinvest in human, intellectual and relational capital (Franco and Mitchell 2008). 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 exists almost entirely in the cross-section. With few 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.[1] Venture capital stimulates entrepreneurship. Because potential entrepreneurs face fewer

[1]Our approach builds on the empirical approach of Samila and Sorenson (2010). They find that the supply of venture capital increases entrepreneurship and economic growth. Here, we explore the moderating effect of non-compete covenants.

constraints in states with employee-friendly legal regimes, 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, and does that effect differ across regimes?

Using panel data on metropolitan areas in the United States from 1993 to 2002, we estimate the causal effect of venture capital on patenting, entrepreneurship, employment 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. These past portfolio returns should, however, not directly influence future regional differences in innovation, entrepreneurship or economic growth.

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 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 non-compete covenants and mobility (Stuart and Sorenson 2003; Fallick et al. 2006; Garmaise 2008; 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 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. This interaction also 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. If other entrepreneurs behave similarly to those backed by venture capital, this interaction further suggests that states could benefit broadly by relaxing their enforcement of non-compete agreements. More generally, it suggests that legal infrastructures importantly moderate the effectiveness of financial intermediation and influence the dynamics of regional industrial clusters.

2 Non-Compete Covenants

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

Though non-compete clauses have become common, their enforcement varies from state to state.[2] 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 non-compete agreements. At the extreme, several states have statutes or precedents that essentially preclude their enforcement. 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

[2]In the United States, states have jurisdiction over labor law. 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, see Richey and Malsberger (1996).

invalidating 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 deep historical roots.[3] 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 nineteenth 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 2008; 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. Policymakers might therefore wish to emulate California's employee-friendly regime. 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 sorts of assets (Franco and Mitchell 2008).

[3]Most states have had stable enforcement regimes for decades. Three states have nevertheless experienced meaningful changes in their 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). From 2002 to 2003, 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). And though Florida has always enforced non-compete clauses, in 1996, the state legislature modestly strengthened this enforcement (Garmaise 2008).

2.1 Incentives to invest

To the extent that non-compete covenants restrict the mobility of employees, they encourage firms to invest in certain assets, such as 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 few alternative mechanisms for protecting investments in these assets. Companies must therefore worry that employees might appropriate these assets' value either by leaving or threatening to leave their jobs. At the regional level, the enforcement of non-compete covenants could therefore stimulate growth if companies focus their investments in regions that provide greater protection over these investments.

Intellectual property: The most commonly discussed justification for enforcing non-compete agreements is to protect intellectual property rights. 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, non-compete clauses give the employer effective property rights over this tacit knowledge (Gilson 1999).

Even when companies have alternative mechanisms for protecting their intellectual property, the enforcement of non-compete 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 non-compete clause, however, the inability of the inventor to bring that invention to a startup or to a competing firm would limit the inventor's temptation to pursue the idea outside his or her current employer.

Human capital: Long (2005) and Garmaise (2008) draw attention to another form of incentive. Non-compete 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. Concomitant to doing their jobs, for example, employees enhance their ability to perform routine tasks through learning-by-doing. Employers can even accelerate this acquisition of skills by assigning more experienced employees to mentor those learning new tasks. Companies may even sponsor classes or compensate their workers for attending courses outside of the workplace. Capelli (1999), for example, notes that employers now pay for 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 human capital at other 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 (Becker 1964).[4] Enforceable non-compete covenants, however, may encourage employers to invest more heavily in human capital.[5]

Business relationships: To intellectual and human capital, we would add a third asset in which enforceable non-compete agreements provide incentives to invest: social capital. Though business relationships have received little attention in the academic literature on non-compete covenants, in practice companies appear to recognize their utility as a means of protecting business relationships. These 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 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 it (Kollock 1994). They can also improve the efficiency of supply chains by facilitating the exchange of fine-grained information and the coordination of joint problem-solving across production stages (Uzzi 1996). Because of these benefits, companies invest valuable resources in building and maintaining business relationships.

Despite the evidence of their value, however, the ownership of these relationships remains somewhat ambiguous. Although the literature conceptualizes inter-firm relationships as

[4]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; Franco and Filson 2006; Møen 2005). But these compensation strategies increase the cost of these investments and therefore may still discourage them.

[5]Garmaise (2008) points out that the enforcement of non-compete covenants reduces the employee's own incentives in invest in human capital. As a result, the expected *net* effect of non-compete enforcement on human capital investment remains indeterminant.

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 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 can encourage companies to invest in an array of assets, it may 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 knowledge that firms create is tacit and embodied in individuals. The diffusion of this knowledge 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. But this sharing 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 can also stymie entrepreneurial innovation (Hellmann 2007). Even though a large share of entrepreneurs enter the industries of their former employers (Franco and Filson 2006), many pursue novel lines of business (Klepper 2007). The enforcement of non-compete agreements could nonetheless inhibit such entrepreneurship for at least two reasons. First, entrepreneurs, even if pursuing ideas distinct from that of their former employers and developed on the entrepreneur's own time, could face hold-up (Hellmann 2007). 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 non-compete agreements bind those potential hires (Stuart and Sorenson 2003).

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

Although studies have examined how the enforcement of non-compete covenants influences employee mobility and entrepreneurship, the net effect of this enforcement on the economy as a whole remains an open question. Part of the difficulty in examining this issue comes from the fact that few states have had meaningful changes in their legal regimes (see footnote 3). But a purely cross-sectional analysis cannot disentangle the effects of enforcement from a host of other factors that vary across regions. By combining cross-sectional variation in enforcement with within-region variation on another important factor that influences entrepreneurship and economic growth, however, one can gain greater purchase on the importance of these legal regimes.

Here, we exploit cross-sectional variation in the response of regions' economies to changes in the supply of venture capital. 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 2010). But do its effects vary across legal infrastructures? 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 of all 328 Metropolitan Statistical Areas (MSAs) in the contiguous United States from 1993 to 2002.[6] MSAs offer the smallest

[6]The Office of Management and Budget defines MSAs 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.

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 comes from the Small Business Administration, which collects it annually from the Census Bureau. Our patent variables draw on the information available from the National Bureau of Economic Research (Hall et al. 2001). 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 used data from Stuart and Sorenson (2003) and Garmaise (2008). Finally, information on endowment returns comes from *The Chronicle of Higher Education*.

3.1 Dependent variables

To assess the effects of the enforcement of non-compete covenants, we created measures that capture regional innovation, entrepreneurship, and economic health.

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.[7] We counted the total number of patents in each MSA-year and

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

transformed this count using the natural logarithm.

Establishment births: As a measure of entrepreneurship, we count 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 the same date 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 sizes 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.[8] Our measure transforms, by the natural logarithm, 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

[8]This category will nonetheless include some relocations and expansions of very small businesses, adding measurement error to our variable.

logarithm to reduce their skewness.[9]

3.2 Independent variables

Non-compete enforcement: We created two measures of state-level differences in the enforcement of non-compete covenants (i.e. in the employee-friendliness of legal regimes). The first, ANC (for "absence of non-compete enforcement"), follows Stuart and Sorenson (2003). In particular, we created a state-level indicator variable with a value of one if the state generally precludes, through statute or precendents, the enforcement of non-compete covenants (or zero otherwise). The second, WNC (for "weakness of non-compete enforcement"), follows Garmaise (2008). For each state, Richey and Malsberger (1996) report twelve summary provisions, such as whether the state imposes geographic or time limits on the enforcement of non-compete agreements. Garmaise proposes a threshold value on each provision that implies a more employee-friendly regime. This index simply counts the number of employee-friendly provisions.[10]

To ease comparisons across these measures, we rescaled the second one to run from 0 to 1 (by dividing it by 9, the maximum value for any state). Higher values indicate more employee-friendly legal regimes. Though these two variables correlate at 0.47, they nonetheless each capture some unique variance in legal regimes and therefore we report estimates using both measures.

[9]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-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.

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

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 non-compete covenants (i.e. $ANC = 1$), and 40% in a state that does not (i.e. $ANC = 0$), would receive an ANC value of 0.60. Our results nevertheless remain robust to the exclusion of these mixed MSAs.[11]

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.[12] Although angel investors, corporate venture capital, and direct investments by university endowments and other investors 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 analysis, 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.

3.3 Fixed Effects Estimates

We began by estimating a standard production function:

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

[11] Forty-one MSAs cross state boundaries.

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

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), 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.[13] A statistically significant value for β_3 would indicate that non-compete enforcement moderates the effect of venture capital.

In all models, 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 removes them from the models. Because non-compete enforcement does not vary meaningfully within-MSAs over time, however, these fixed effects also absorb the "main" effect of non-compete 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, and also the rate of venture capital investing and fund raising. The year effects (ϕ_t) effectively remove these national economic factors from our analyses.

We therefore identify our effects from 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

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

MSA), and deal with other unobserved factors below through the use of an instrument.

Let us turn to the estimation results, beginning with Table 3, which reports the results for patents and new establishments. 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 increases the number of patents in the region by .08% ($= .0626 + .0223$).

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 employee-friendly regimes. At least three factors might account for this effect. First, the greater mobility of personnel in these jurisdictions could stimulate innovation by better enabling the recombination of existing technologies into new inventions (Fleming 2001). Second, firms may invest more in patentable R&D if the movement of personnel limits their ability to compete on the basis of people (e.g., Garmaise 2008). Third, firms might attempt to substitute patents for the intellectual property protection offered by non-compete covenants. We therefore cannot say whether this effect reflects increased innovation (though it seems improbable that the positive coefficient would appear if innovation actually declined). Venture capital nevertheless appears more productive, in terms of creating new firms, in employee-friendly regimes.

Table 4 reports the results of our fixed effects estimates of the effect of venture capital on employment and aggregate income. Consistent with past research, we find positive effects of venture capital on both outcomes. We also find strong evidence that venture capital in-

vestments produce larger gains in employment and in aggregate income in employee-friendly jurisdictions. Though these estimates have fairly wide confidence intervals, the larger size of the coefficient estimate for the WNC interactions in predicting payroll relative to employment suggests that the average wages in employee-friendly regimes may increase somewhat in response to venture capital investments.

Tables 5 and 6 explore the temporal structure of the effects of venture capital. Venture capital has the most immediate effects in terms of entrepreneurship. In part, that effect probably stems from the funding of firms in their first year. Though the average firm in our data receives its first round of venture capital in its third year, 29% of firms received venture capital in their first year of operation. But in part, it likely reflects entry by would-be entrepreneurs interpreting these investments as information about the probable future availability of financing (Samila and Sorenson 2010).

Venture capital has longer term effects in patenting and particularly in employment and income. As the ventures funded by venture capital firms grow, they continue to contribute to the economies of the regions in which they reside. The differences in these effects according to the employee-friendliness of the regime, moreover, continue to persist over time. Hence, regions with more employee-friendly labor laws appear to enjoy lasting gains in terms of employment and income.

Though our results are quite robust, one concern might be the extent to which Silicon Valley drives them. California does not enforce non-compete agreements and our observation window covers the period of the dot-com boom, which produced extraordinary short-run growth in the San Francisco Bay Area. To examine whether this outlier might drive our results, in Tables 7 and 8, we replicated Tables 3 and 4 excluding MSAs in the Bay Area—San Francisco, San Jose, Fresno, and Oakland. Though the point estimates from this analysis

suggest slightly smaller effect sizes, the pattern and significance levels of the results remain essentially robust to the exclusion of these MSAs.

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. We also estimated models using the instrument suggested by Gompers and Lerner (2000): investments in LBO funds. Though that instrument produces substantively equivalent results, we prefer the LP returns instrument for two reasons. First, these returns are more plausibly exogenous to regional economic activity (discussed further below). Institutions might invest in LBO funds in a region in response to the attractiveness of the local economy. Second, LP returns has a stronger relationship to venture capital investments in the first stage, and therefore produces more efficient second-stage estimates.

LP Returns: Most institutional investors diversify their investments using a (relatively) 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 leads to a greater flow of funds into venture capital. Because institutional investors exhibit a preference for geographically proximate private equity funds (Samila and Sorenson 2010), the investment returns of local limited partners should at least

partially determine the availability of venture capital in a region.

We construct our instument by interacting the national average returns to college and university endowments, an important class of institutional investor, 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:

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

where ER_s denotes the average returns to college endowments in year s, $\ln(1 + LP_i)$ denotes the logged count of limited partners located in MSA i who had invested in any private equity fund in 1992 or earlier (plus one 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 few years of the partnership.

We made several choices in the construction of the instrument to ensure its validity. First, instead of using the actual returns to limited partners in a region, we used the national average returns for a year as a proxy for these returns. Since institutions often exhibit a home bias in their portfolios, the strength of the local economy could contaminate the actual local returns. Second, instead of using a time-varying count of limited partners in the region, we fixed this count at the number that invested in private equity prior to our observation window. Again, a time-varying count of investors might reflect the strength of the economy if strong economies foster the formation of new institutional investors. Finally, we lagged the time variable so that any year's investments depend on the previous (three) year's returns.

Given these precautions, we see no reason that our instrument would have a direct effect on local innovation, entrepreneurship or economic growth. We also need not worry about reverse causality between the dependent variables and portfolio returns. Since venture capital accounts for but a small portion of the total assets, approximately 1% for college and univer-

sity endowments according to the *Chronicle of Higher Education*, the performance of venture capital investments has little influence on these overall returns. LP returns, therefore, should offer a valid instrument for the local supply of venture capital.

Table 9 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-Paap rk Wald F-statistic (Kleibergen and Paap 2006) 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 OLS estimates. Our observed value of 160.62 clearly 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 non-compete covenants (ANC and WNC) in the second-stage regressions. Because OLS does not produce the correct standard errors for predicted values, we estimated the standard errors by bootstrapping the regression 10,000 times.

The results of these IV estimates appear in Tables 10 and 11. Beginning with the effects on patenting and firm founding, we see a fairly consistent set of results. As in the OLS estimates, venture capital has a positive effect on patenting in all states, but it has even

[14] 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 substantively equivalent results to the ones reported here.

stronger effects in those states that do not enforce non-compete agreements. This difference is large. States that do not enforce them experience twice the increase in patents of those that do enforce them in response to an influx of venture capital. We see the same pattern in establishment births.

Turning to the broader regional economy, the results also support the tentative conclusions from the OLS regressions: employee-friendly legal 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 venture capital. A similar relationship appears to hold between venture capital and the aggregate income in an MSA. In those models, however, the inefficiency of the IV estimates has lead to error margins that do not allow us to reject the null hypothesis that the effect of venture capital on wages does not vary across regimes.

4 Discussion

Some scholars, such as Wood (2000), have questioned the importance of non-compete covenants, suggesting that regions that enforce these agreements might have developed alternative mechanisms for ensuring labor mobility and the associated knowledge spillovers. Our results strongly suggest otherwise. We find that the enforcement of non-compete covenants moderates the effects that venture capital has on both innovation and the overall regional economy. More specifically, our results demonstrate 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.[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 15 to 36 more firms if the region did not enforce non-compete agreements (depending on whether one uses the OLS or IV estimates). That same doubling in investments predicts 3,607 to 5,350 more jobs in these employee-friendly jurisdictions. If all of these jobs came from the startups, then the average startup would need to employ more than 150 people. Since that number dramatically exceeds the actual scale of these firms, it suggests that a substantial portion of the job growth in the regions with employee-friendly regimes comes, not from the startups 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 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 adopting supportive labor laws may have little hope of seeing benefits from these programs (even if they succeed in increasing the level of venture capital activity).

[15]Garmaise (2008), on the other hand, finds some evidence that firms may actually invest *less* in R&D in regimes of strong non-compete enforcement.

But the results also offer hope for these regions. The fact that we find similar effect sizes in both our discrete and continuous measures of non-compete 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, 1995).

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 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 reside 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 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 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

Variable	**Mean**	**Std. Dev.**	**N**
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
LP Returns	23.29	38.29	2935

Table 2: Most active and fastest growing regions for venture capital

Avg first investments per year		Avg CAGR per year, 93-01	
San Jose, CA	254	Raleigh-Durham-Chapel Hill, NC	43%
San Francisco, CA	195	Pittsburgh, PA	35%
Boston, MA-NH	163	Baltimore, MD	32%
New York, NY	91	New York, NY	29%
Oakland, CA	76	Austin-San Marcos, TX	29%
Washington, DC-MD-VA-WV	74	Santa Barbara-Santa Maria-Lompoc, CA	26%
San Diego, CA	65	Minneapolis-St. Paul, MN-WI	26%
Los Angeles-Long Beach, CA	63	Albuquerque, NM	22%
Seattle-Bellevue-Everett, WA	60	Middlesex-Somerset-Hunterdon, NJ	22%
Atlanta, GA	48	Lawrence, MA-NH	22%

Table 3: FE Estimates for Innovation and Entrepreneurship

	(1) Patents	(2) Patents	(3) Patents	(4) Births	(5) Births	(6) Births
Ln Population	1.466***	1.470***	1.484***	0.827***	0.827***	0.829***
	(4.56)	(4.56)	(4.63)	(10.32)	(10.31)	(10.30)
Ln VC Cnt	0.0332***	0.0223**	0.0329***	0.00862***	0.00596*	0.00858***
	(3.67)	(2.37)	(3.73)	(2.75)	(1.70)	(2.74)
Ln VC Cnt x ANC		0.0626***			0.0152**	
		(2.76)			(2.42)	
Ln VC Cnt x WNC			0.104***			0.0156
			(3.40)			(1.58)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
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

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is 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.

Table 4: FE Estimates for Regional Economy

	(1) Emplmnt	(2) Emplmnt	(3) Emplmnt	(4) Payroll	(5) Payroll	(6) Payroll
Ln Population	0.785***	0.786***	0.789***	1.139***	1.140***	1.144***
	(21.10)	(21.24)	(21.35)	(15.23)	(15.19)	(15.39)
Ln VC Cnt	0.00767***	0.00438**	0.00762***	0.0229***	0.0198***	0.0228***
	(4.53)	(2.43)	(4.57)	(7.09)	(5.54)	(7.19)
Ln VC Cnt x ANC		0.0188***			0.0177*	
		(4.93)			(1.84)	
Ln VC Cnt x WNC			0.0211***			0.0324**
			(3.31)			(2.48)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.78	0.78	0.78	0.93	0.93	0.93
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is the total employment in the MSA. In models 4-6 the dependent variable is the total payroll in the MSA.

Table 5: FE Estimates for Innovation and Entrepreneurship with Lag Structure

	(1) Patents	(2) Patents	(3) Patents	(4) Births	(5) Births	(6) Births
Ln Population	1.397***	1.398***	1.443***	0.812***	0.811***	0.816***
	(4.25)	(4.24)	(4.44)	(9.96)	(9.90)	(9.93)
Ln VC Cnt (t)	0.0262***	0.0217**	0.0247***	0.00715**	0.00550*	0.00707**
	(3.38)	(2.51)	(3.27)	(2.55)	(1.74)	(2.54)
Ln VC Cnt (t-1)	0.0159*	0.00633	0.0147*	0.00236	0.00154	0.00222
	(1.88)	(0.71)	(1.71)	(0.95)	(0.56)	(0.90)
Ln VC Cnt (t-2)	0.00714	-0.00154	0.00652	0.00226	0.00108	0.00216
	(0.81)	(-0.16)	(0.75)	(0.79)	(0.35)	(0.76)
Ln VC Cnt (t-3)	0.0185**	0.0137	0.0182**	0.00442	0.00256	0.00443
	(2.12)	(1.49)	(2.19)	(1.42)	(0.75)	(1.43)
Ln VC Cnt x ANC (t)		0.0262			0.00966*	
		(1.55)			(1.73)	
Ln VC Cnt x ANC (t-1)		0.0523***			0.00390	
		(2.91)			(0.72)	
Ln VC Cnt x ANC (t-2)		0.0490***			0.00667	
		(2.90)			(1.00)	
Ln VC Cnt x ANC (t-3)		0.0236			0.0106	
		(1.27)			(1.46)	
Ln VC Cnt x WNC (t)			0.0588**			0.0120
			(2.00)			(1.41)
Ln VC Cnt x WNC (t-1)			0.0563*			-0.000104
			(1.86)			(-0.01)
Ln VC Cnt x WNC (t-2)			0.0656**			0.000402
			(2.00)			(0.05)
Ln VC Cnt x WNC (t-3)			0.0228			0.00876
			(0.62)			(0.79)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.18	0.19	0.19	0.24	0.25	0.25
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is 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.

Table 6: FE Estimates for Regional Economy with Lag Structure

	(1) Emplmnt	(2) Emplmnt	(3) Emplmnt	(4) Payroll	(5) Payroll	(6) Payroll
Ln Population	0.770***	0.770***	0.779***	1.060***	1.059***	1.077***
	(20.60)	(20.69)	(20.86)	(15.34)	(15.25)	(15.56)
Ln VC Cnt (t)	0.00623***	0.00398***	0.00588***	0.0153***	0.0142***	0.0145***
	(4.45)	(2.63)	(4.21)	(7.20)	(6.08)	(6.97)
Ln VC Cnt (t-1)	0.00474***	0.00360**	0.00446***	0.0160***	0.0138***	0.0154***
	(3.58)	(2.47)	(3.29)	(6.45)	(5.37)	(6.29)
Ln VC Cnt (t-2)	0.00194	0.000577	0.00185	0.0162***	0.0139***	0.0161***
	(1.44)	(0.40)	(1.37)	(6.04)	(4.75)	(6.16)
Ln VC Cnt (t-3)	0.00115	-0.000179	0.00114	0.0115***	0.00857***	0.0116***
	(0.80)	(-0.13)	(0.83)	(4.93)	(3.34)	(5.19)
Ln VC Cnt x ANC (t)		0.0131***			0.00651	
		(4.42)			(1.23)	
Ln VC Cnt x ANC (t-1)		0.00565*			0.0117*	
		(1.86)			(1.67)	
Ln VC Cnt x ANC (t-2)		0.00796**			0.0123**	
		(2.53)			(2.05)	
Ln VC Cnt x ANC (t-3)		0.00716*			0.0164***	
		(1.86)			(3.28)	
Ln VC Cnt x WNC (t)			0.0115**			0.00737
			(2.10)			(0.91)
Ln VC Cnt x WNC (t-1)			0.00877**			0.0212**
			(2.06)			(2.11)
Ln VC Cnt x WNC (t-2)			0.0140***			0.0263***
			(2.85)			(3.31)
Ln VC Cnt x WNC (t-3)			0.00853			0.0229***
			(1.35)			(3.21)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.78	0.78	0.78	0.94	0.94	0.94
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is the total employment in the MSA. In models 4-6 the dependent variable is the total payroll in the MSA.

Table 7: FE Estimates for Innovation and Entrepreneurship excl. Silicon Valley

	(1) Patents	(2) Patents	(3) Patents	(4) Births	(5) Births	(6) Births
Ln Population	1.467***	1.469***	1.478***	0.824***	0.825***	0.826***
	(4.55)	(4.55)	(4.61)	(10.26)	(10.25)	(10.25)
Ln VC Cnt	0.0289***	0.0232**	0.0295***	0.00797**	0.00607*	0.00806**
	(3.29)	(2.46)	(3.41)	(2.52)	(1.73)	(2.54)
Ln VC Cnt x ANC		0.0368*			0.0122*	
		(1.86)			(1.81)	
Ln VC Cnt x WNC			0.0736***			0.0111
			(2.67)			(1.06)
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	0.24	0.24	0.24
Clusters	324	324	324	324	324	324
Observations	2899	2899	2899	2899	2899	2899

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is 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. Silicon Valley consists of the San Francisco, San Jose, Oakland, and Fresno MSAs.

Table 8: FE Estimates for Regional Economy w/o Silicon Valley

	(1) Emplmnt	(2) Emplmnt	(3) Emplmnt	(4) Payroll	(5) Payroll	(6) Payroll
Ln Population	0.784***	0.785***	0.787***	1.143***	1.143***	1.146***
	(21.05)	(21.16)	(21.24)	(15.29)	(15.27)	(15.35)
Ln VC Cnt	0.00702***	0.00448**	0.00717***	0.0212***	0.0201***	0.0214***
	(4.16)	(2.47)	(4.28)	(6.74)	(5.62)	(6.79)
Ln VC Cnt x ANC		0.0164***			0.00712	
		(4.18)			(0.88)	
Ln VC Cnt x WNC			0.0172***			0.0204*
			(2.62)			(1.74)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.77	0.78	0.78	0.94	0.94	0.94
Clusters	324	324	324	324	324	324
Observations	2899	2899	2899	2899	2899	2899

Notes: OLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. In models 1-3 the dependent variable is the total employment in the MSA. In models 4-6 the dependent variable is the total payroll in the MSA. Silicon Valley consists of the San Francisco, San Jose, Oakland, and Fresno MSAs.

Table 9: First-Stage of IV Estimation

	(1) Ln VC Cnt
Ln Population	1.444***
	(3.57)
LP Returns	0.0153***
	(12.67)
Year Dummies	Yes
MSA Fixed Effects	Yes
R^2	0.22
KP Wald F-statistic	160.62
Clusters	328
Observations	2935

Notes: First-stage of IV estimation; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust t-statistics in parentheses; disturbances clustered by MSA. The unit of observation is the MSA-year and the data cover the 48 contiguous United States from 1993 to 2002. The dependent variable is the count of first VC investments in the MSA.

Table 10: IV Estimates for Innovation and Entrepreneurship

	(1) Patents	(2) Patents	(3) Patents	(4) Births	(5) Births	(6) Births
Population	1.305***	1.292***	1.325***	0.780***	0.776***	0.786***
	(4.00)	(3.97)	(4.08)	(9.11)	(8.94)	(8.90)
VC Cnt (p)	0.124***	0.0949***	0.118***	0.0352***	0.0274**	0.0331**
	(3.43)	(2.64)	(3.23)	(2.84)	(1.97)	(2.45)
VC Cnt (p) x ANC		0.138**			0.0367**	
		(2.33)			(2.02)	
VC Cnt (p) x WNC			0.116			0.0399
			(1.05)			(1.22)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.18	0.18	0.18	0.24	0.25	0.25
Clusters	328	328	328	328	328	328
Observations	2935	2935	2935	2935	2935	2935

Notes: 2SLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.

Table 11: IV Estimates for Regional Economy with Absence of NC Binary

	(1) Emplmnt	(2) Emplmnt	(3) Emplmnt	(4) Payroll	(5) Payroll	(6) Payroll
Population	0.772***	0.769***	0.780***	1.045***	1.042***	1.054***
	(16.24)	(15.96)	(16.20)	(13.01)	(12.77)	(12.98)
VC Cnt (p)	0.0154***	0.00946*	0.0127**	0.0761***	0.0708***	0.0733***
	(3.08)	(1.74)	(2.51)	(6.55)	(6.51)	(6.78)
VC Cnt (p) x ANC		0.0278***			0.0249	
		(2.69)			(0.95)	
VC Cnt (p) x WNC			0.0524***			0.0544
			(3.11)			(1.41)
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
MSA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
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

Notes: 2SLS regression results; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 total employment in the MSA. In models 4-6 the dependent variable is the total payroll in the MSA.