

Non-competes, Business Dynamism, and Concentration: Evidence from a Florida Case Study*

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May 2019

Abstract

We investigate the impact of non-compete enforcement on the distribution of firm size and employment growth and business concentration by focusing on Florida's 1996 legislative change that eased restrictions on the enforcement of non-competes. We first establish the contrast between legal regimes and that wage trends did not change before and after the passage of the legislation. Difference-in-differences models show that following the change, establishments of large firms were more likely to enter Florida; they also created a greater proportion of jobs and increased their share of employment in the state. Entrepreneurs or establishments of small firms, in contrast, were less likely to enter Florida following the law change; they also created a smaller proportion of new jobs and decreased their share of employment. Consistent with these location and job creation dynamics, a variety of business concentration measures increased following the law change in Florida. Border counties in Florida exhibit stronger effects and nationwide cross-sections demonstrate consistent correlations between state-level non-compete enforcement and the employment, size, and concentration dynamics illustrated in Florida.

Keywords: Business Dynamism; Employee Mobility, Entrepreneurship; Firm Sorting; Non-compete Agreement; Business Concentration

JEL Codes: J61, L22, L26, M13, M51.

* We would like to acknowledge the support of the National Science Foundation (Grant 1735650), The Kauffman Foundation, the Haas School of Business, and the Coleman Fung Institute for Engineering Leadership. We thank conference participants at the 2016 Strategic Management Society Special Conference, the 2017 Academy of Management Meeting, the 2017 Roundtable for Engineering Entrepreneurship Research, the Tenth Annual Conference on Innovation Economics, the 2017 Vienna Conference on Strategy, Organizational Design, and Innovation, and the Fourth International ZEW Conference on the Dynamics of Entrepreneurship. We also thank Sam Arts, Jim Bessen, Sampsa Samila, Olav Sorenson, Evan Starr, and Wes Cohen for their comments and feedback. Errors and omissions remain with the authors.

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1. INTRODUCTION

Much recent research has documented a trend of increasing industry concentration, possibly due to scale and network effects (Shambaugh et. al. 2018), deregulation (De Loecker, Eeckhout, and Unger, 2018), or efficiencies of scale, mergers and acquisitions, innovation, or regulatory barriers (Council of Economic Advisors 2016). Other work has documented a broad decline in business dynamism across many sectors in the U.S, including a flat trend in firm exit and declining trends in firm entry and job reallocation (Hathaway and Litan 2014) and a decrease in entrepreneurship (Haltiwanger, Jarmin, and Miranda 2011; Kauffman, 2016). Hathaway and Litan (2014) comment that, “Whatever the reason, older and larger businesses are doing relatively better to younger and smaller ones.” A policy brief from the White House (2016) documents a decline in competition, new firm formation, and business dynamism - and associates these trends with state level non-compete laws that typically decrease workers’ mobility. Scatter plots at the state level, illustrated in [Figure 1](#), reveal positive relationships between non-competes and the share of large firms, job creation by large firms, and regional business concentration. Such plots, however, are static and bivariate, surely mask omitted variable bias, and like other work that has only documented the trends, “...remain[ed] silent on the causes.” (De Loecker, Eeckhout, and Unger, 2018; p. 32)

To investigate one dynamic that could give rise to increased business concentration, we identify a clear change in one state’s non-compete laws, a subsequent change in establishment entry and employment by firm size, and a consistent effect on business concentration. We begin by documenting recent changes in non-compete laws across all U.S. states and establish that Florida’s 1996 non-compete law provides an unambiguous step change that strengthened enforcement. Other states have also changed their non-compete laws, though not as cleanly for the purposes of isolating the impact of non-competes on business concentration. For example, Michigan’s 1985 change – the Michigan Anti-trust Reform Act – was explicitly intended to

increase competitiveness; the legislators and analysts had no intent to change non-compete law (Marx, Strumsky, and Fleming, 2009). Adding to the attractiveness of the research site, there appears to have been little change in institutional and electoral influences, and Florida's wage trends remained stable over the time period of study. Florida's experience appears internally consistent and provides a plausible pathway from non-compete enforcement to business concentration. We discuss and illustrate possible mechanisms, but hesitate to claim wide applicability and external validity, due to the difficulty of generalizing across the many idiosyncrasies that accompany each state's change in non-compete laws, and the many potential influences on business concentration.

Florida's sharp legislative change in non-compete enforcement enables illustration of how stronger non-compete laws might alter business dynamism and the regional size distribution of firms. The law change appears to have favored and attracted establishments of larger firms, and such firms created more new jobs. Stronger enforcement did not increase the establishment of start-ups, the arrival of small firms to the state, and job creation by such firms. Consistent with these trends, we find a significant increase in business concentration measures following Florida's strengthening of non-competes. These results are robust to analyzing adjacent counties on Florida's borders, synthetic matching, industry matching, and placebo tests, and are consistent with a nationwide cross section of states' noncompete enforcement and shares of establishment entry, employment growth, and business concentration.

2. EMPLOYEE NON-COMPETES

If you are a chief executive of a large company, you very likely have a non-compete clause in your contract, preventing you from jumping ship to a competitor until some period has elapsed. Likewise if you are a top engineer or product designer, holding your company's most valuable intellectual property between your ears.

And you also probably have a non-compete agreement if you assemble sandwiches at Jimmy John's sub sandwich chain for a living (New York Times, Oct 14, 2014).

Covenants not to compete (“non-competes”) are agreements in which an employee agrees not to work for the current employer’s direct competitors in a specified area for a certain amount of time. They are becoming increasingly prevalent in many industries besides high technology (Starr, 2015); 351 of 500 U.S. firms (70.2%) reported that they had non-compete agreements with their top executives from 1992 to 2004 (Garmaise, 2009).¹ Amazon requires their warehouse employees, including part-time laborers, to sign non-competes, under which they will not work at “any company where they directly or indirectly support any good or service that competes with those they helped support at Amazon (The Verge, 2015)”.² Physicians, dentists, accountants, and even lawyers can be subject to non-competes (Tanick and Troubaugh, 2012).

Non-competes have developed in part because employers typically prefer labor contracts with mechanisms that aid in the retention of desirable employees. Such contracts respond to concerns about employee separation and are often intended to mitigate the market failure of under-investment in employee training and research activities (Samila and Sorenson, 2011). With non-competes in place, employers can invest in their employees and provide confidential yet necessary information with less fear of information leakage or potential competition. Employees, likewise, can credibly pledge or commit that they will not use the training and information they receive from the current employer for the benefit of its competitors. High technology firms often invest heavily in research and development, and their technical professionals learn a great deal in performing that work. If an employee moves to another organization, the intellectual assets that he or she developed may leak to competitors, posing a significant threat to the former employer (Conti, 2014; Ganco, Ziedonis, and Agarwal, 2015).

Addressing the under-investment in training and research problem may have other effects,

however, on firms, industries, and the regions they both operate in (Gilson, 1999). The enforcement of non-competes creates complications and, in practice, the optimal degree and nuance of their application remains unclear. It is difficult to monitor observance of the agreement and contract on every possible contingency. Non-competes can distort the labor market and create inefficiency, as prior employees cannot utilize their expertise and experience in the same field for a certain amount of time. Employers can potentially increase their leverage over employees because employees have fewer outside options and less bargaining power under a non-compete. Employees often do not understand the legal nuances of labor law and their chances of prevailing, should they face prosecution by their former employer. This confusion can create a chilling effect on worker mobility, as employees are reluctant to incur potentially debilitating personal expenses for an uncertain legal outcome (Marx, 2011). By restricting mobility, non-competes can make it more difficult for firms to hire the talent they need, slow the optimal matching of human capital and opportunities (Jackson, 2013), and potentially retard the diffusion of knowledge and expertise (Fallick, Fleischman, and Rebitzer 2006; Belenzon and Schankerman, 2013).

Empirical work has established a variety of relationships with non-compete enforcement. Stuart and Sorenson (2003) established that greater entrepreneurship followed IPOs in regions that lacked enforcement. Using multiple times-series and cross-sectional variations of enforceability across U.S. states, Garmaise (2009) found that stronger enforcement correlated with executive stability and reduced executive compensation. The Michigan Antitrust Reform Act (MARA) in 1985 has been used with difference-in-differences models to demonstrate decreased intra-state mobility of inventors (Marx, Strumsky, and Fleming, 2009), career detours (Marx, 2011), and inter-state brain drain of inventors (Marx, Singh, and Fleming, 2015). Using panel regressions and an instrument based on university endowment returns, Samila and Sorenson (2011) found that the number of patents, number of start-ups, and rate of employment are more responsive to the supply

of venture capital in states that restrict the enforceability of non-competes. Conti (2014) illustrated an increase in breakthrough and failed innovations in states that enforced non-competes, arguably due to greater risk-taking by firms that were less afraid of losing their technical personnel. Starr, Balasubramanian, and Sakakibara (2017) used matched employer-employee data and found that non-compete enforceability is negatively correlated with formation of small (0-19 employees) within-industry spinouts, but positively correlated with the survival of such new spinouts. Balasubramanian, et al. (2017) found that non-compete enforceability correlates with longer job spells in technology industries, without an increase in wages.

None of the work to date has considered how non-competes might have different impacts on firms of different sizes and in particular, their location decisions and rates of job creation, and ultimately, on their concentration. [Figure 1](#) introduced above suggests that stronger enforcement might lead to larger firms, greater employment by larger firms, and higher business concentration. Before discussing potential mechanisms, we will first establish why Florida's 1996 law change best enables one investigation of the dynamics that might underlie these relationships.

2.1. Use of the 1996 Florida Change in Non-competes as an instrument

In order to investigate the impact of non-competes, we consider a 1996 change in non-compete law in Florida. This change offers a close to ideal research site, in contrast to law changes in other states (see [Appendix](#) for a list of states that have changed their non-compete laws and a discussion of their suitability and comparability to Florida). Florida provides an ideal site because (1) the legislation was focused purely on restrictive covenants, notably non-competes and (2) it was clearly intended to strengthen non-compete enforcement in the state. Furthermore, it does not appear that wage trends changed in Florida around 1996 (please see robustness check below). Considering the presence of relatively strong non-compete laws in Florida for the preceding four

decades, employers and employees were probably familiar with and accustomed to non-competes.

At least three important features of the 1996 amendment support its use as a quasi-natural experiment. First, the amendment explicitly stated and thereby clarified which rule governed a contract and stipulated a clear break on July 1, 1996. Second, an examination of the 1996 amendment to the statutes, along with legal professionals' accounts, illustrates how the amendment significantly strengthened the employer's position in terms of the enforceability of non-compete covenant. The number of words almost tripled, from 455 words in §542.33A to 1,211 words in §542.33B, in the direction of strengthening employers' enforcement of non-competes. The new law was construed in favor of business protection, and courts could no longer refuse non-compete enforcement on the grounds of employee economic hardship or public policy concerns. Third, the 1996 amendment marked a sharp contrast to the preceding 1990 amendment. The post 1990-amendment statute probably made it more difficult to enforce non-compete covenants; in contrast, the post-1996-amendment statute, §542.335, made it easier to enforce non-compete covenants for employers. A legal professional commented that the 1996 amendment "has once again swung the pendulum representing the enforceability of non-competition agreements more in favor of employers (Findlaw, 2008)." [Table 1](#) highlights and summarizes the most important changes that made the post-1996 legal regime (§542.335) more lenient to employers seeking non-compete enforcement than the previous legal regime (§542.33B).³

Although we provide a summary of recent state changes in non-compete enforceability in the appendix, it is not obvious they can be used as additional research sites. First, and in many cases, legislative amendments include changes that would confound the analysis. For example, the intent of the 1986 change in Michigan law (MARA, 1986) was to increase competition. This is particularly important in a study that examines business concentration as an outcome. Second, some changes were only applicable to a limited set of agents in the economy. For example, Utah

in 2018 modified the law to limit the enforcement of non-competes against employees *in the broadcasting industry who make less than \$47,476 per year*. The 2001 Louisiana change provided an asymmetric incentive for economic agents in a sense that they only regulated job moves between employers; employees bound by non-competes could still start their own businesses. Third, many are weak and marginal changes. Some states merely changed their restrictions on choice-of-law provisions or the timing of notice. In some cases, it is not even clear what the direction of change might have been, for example Florida's 1990 change. Fourth, multiple changes sometimes occurred within a small window of time, making it difficult to compare pre- and post-change outcomes. Examples include Louisiana changes (2001 and 2003) and Idaho changes (2016 and 2018) and Utah changes (2016 and 2018). Finally, data are not available for most recent changes. For all of these reasons, we present Florida as essentially a single case study that illustrates one potential path from non-competes to business concentration; further work remains in order to establish a wider validity of that path.

3. THE DIFFERENTIAL EFFECTS OF NON-COMPETES BY FIRM SIZE

Despite a growing literature on non-competes, little work to date has investigated how non-competes might impact firm location and employment, which might in turn influence business concentration, if there were different effects on small vs. large firms. We consider the differential effects of non-competes by firm size on regional location choice (at birth or in movement of extant establishments), job creation, and business concentration. We discuss how the law change in Florida might cause a 1) shift in the distribution of firm sizes, 2) shift in the sources of new job creation, and 3) change in regional business concentration. We discuss mechanisms, but present no formal theory, and explore the answer empirically.

3.1. Non-competes and location choice, for startups

The recruitment of high quality and experienced employees constitutes one of the greatest challenges in the founding and scaling of a new business (Stuart and Sorenson, 2003). Entrepreneurial companies in particular need to hire already capable and experienced workers because 1) they do not have the resources or time to invest in employee training, and 2) compared to large incumbents, they are less likely to have a systematic training process for novice workers.

Startups might prefer locations with weak non-compete laws, as they would ideally like to hire experienced employees (who will be more experienced if they were recently working in a similar job or for a competitor). Hiring unemployed workers remains unattractive because they are generally less experienced than active employees; furthermore, an unemployed yet experienced worker might still be bound by a non-compete and therefore off limits to competitors because non-competes typically hold even when an employee is laid off or fired. Startups also may not value as highly the legal strategies enabled by non-competes. Since startups by construction cover narrower businesses and geographic boundaries, an employee departing a startup will have a wider range of employment opportunities that do not include competitors. This wider range will make it less likely that an employee is leaving for an obvious competitor, because the non-compete will not cover this situation. Add to this the greater likelihood that a startup will lack the resources to pursue legal action against former employees, and a startup might place lower value on location in a region with strong non-compete enforcement.

Startups may also have reasons to prefer locations with strong non-compete laws. Founders and their immediate teams probably share more complete access to all information within the organization, due to the small size of the firm, shared responsibilities, and weak and yet to be formalized information-sharing protocols. Given that startups often have no reputation and few complementary assets, their ideas and intellectual property are often their only advantages, and

they may be attracted to legal regimes where they can more easily keep an employee from departing, particularly to a better-resourced competitor. Foreseeing growth, startups might also prefer locations with strong non-compete laws, as such laws would help keep their current employees as they seek new employees (Starr, Balasubramanian, and Sakakibara 2017). Empirically, if startups find strong non-competes attractive, we would expect to find an increase in the number of small firms, following a shift to the stronger non-compete enforcement (and the opposite if startups find non-competes unattractive).

3.2. Non-competes and location choice, for existing firms

Existing firms, especially if they are not attempting to hire more than a small proportion of their extant workforce, are more likely to prefer regions with stronger non-compete enforcement, and hence more likely to move there or establish additional franchises. When large firms do need to hire, and in contrast to the challenges faced by smaller firms, non-competes might also magnify the typically superior financial and legal resources of large firms. Such firms are more able to buy out non-compete provisions from new employees' former employers. Potential legal costs also favor large firms, which generally have more experience, financial resources, and economies of scale when utilizing legal services, such as contracting advisory or litigation.

Similar to startups and small firms, the strategic importance of retaining existing employees is also likely to be very important for larger firms. Large firms typically have systematic processes in place to train their workers (which is costly) and have granted them access to strategic assets and information. If these workers move to (emerging) competitors, large incumbents could lose their investment in their trained workforce; furthermore, mobile employees might also unwillingly transfer important strategic assets of former employers, either implicitly or explicitly, to the competing firms. Therefore, firms that are not growing rapidly may feel that they gain more than

they lose from immobilized employees and thus may place a higher value on location in a region with strong non-compete enforcement.

Regions with strong non-compete enforcement may also attract larger firms because they can temporarily allocate newly hired (or explicitly poached) employees to business units or subsidiaries that do not directly compete with their former employer. Such firms can then reallocate employees to the most relevant units after their non-compete term expires. In other words, large firms are more likely diversified and thus run businesses in multiple fields; these diversified business units serve as a “holding tank” (Marx and Fleming, 2012) for new employees who might be bound by non-competes. Small firms, in contrast, are more likely to focus on a specific area and lack diversified business units that could serve as legitimate holding tanks.

Analogous to “voting with feet (Tiebout, 1956),” firms should (re-)locate their establishments in municipalities that offer their preferred business environment, essentially shopping for advantageous policies. For the reasons described above, large firms are more likely to prefer strong non-compete regions and hence may open new establishments in Florida or move extant establishments to Florida, following the amendment. The advantages to entrepreneurial firms, on the other hand, are mixed (and it is very possible that there is no monotonic relationship between firm size and preference for non-compete regions – we leave it as an empirical question). Firms surely vary in their preference to contract on and enforce non-competes. Yet large [small] firms that were at the margin – i.e., that previously saw the benefit of non-compete enforceability and cost of (re)locating to Florida as a break-even opportunity – will prefer Florida more [less] after the 1996 law change, because it will have increased [reduced] the benefits and thus made it more lucrative [unprofitable] to [re]locate in Florida. Empirically, if existing (and typically larger) firms find strong non-competes relatively more attractive, we would expect to find an increase in the number of large firms and large firm establishments, following a

shift to the stronger non-compete enforcement.

3.3. Non-competes and the challenges of hiring and job creation, for small firms

The enforceability of non-competes may also differentially affect the creation of new jobs and employment, depending on a company's size. All other things being equal (for example, assuming that all firms want to hire and grow), if it becomes harder [easier] for larger [smaller] firms to hire new workers, we would expect to observe a shift in the distribution of sources of new jobs, following the 1996 law change. Since it is not possible to observe whether individual firms attempt to grow or employee's preferences or responses to employment offers, we will focus on how non-competes could make it more or less difficult for different types of firms – small or large – to hire.

Regional mobility (of workers) decreases with stronger enforcement (Marx, Strumsky, and Fleming, 2009; Balasubramanian et al., 2017) and this decrease may put startups and small firms at a greater disadvantage in hiring employees and creating new jobs. If workers expect to be bound by a non-compete, they may avoid opportunities at smaller and entrepreneurial firms. When workers are unable to hop between jobs and find a better match by trial and error, they are more likely to choose a large employer that typically offers better benefits packages, job stability, internal job hopping, and other non-pecuniary incentives. This is more so when non-competes remain in force after an employee is laid off; workers who sign non-competes bear additional risks should the business go awry, because they remain bound by commitment (and small businesses and particularly startups are more likely to go awry).

Further adding to small firms' challenges in creating jobs, they are typically less able to offer appealing and competitive incentives to prospective employees. Small firms are generally riskier, pay less, and are focused on less diverse businesses (thus affording fewer internal career transfers). Furthermore, they offer less protection from potential non-compete prosecution by

larger firms with intimidating legal resources. This is in contrast to a location without non-competes, where (marginal) job seekers may be more likely to choose small firms that are riskier, because they can leave the small firm and get another job more easily.

This argument, however, can also be turned on its head. Under a strong non-compete enforceability, potential employees may prefer startups and small firms, if they anticipate that those firms will lack the resources or will to prosecute a non-compete, relative to a larger firm. Furthermore, and consistent with the argument above, a narrow startup probably has fewer market and geographical competitors, thus making it less likely that a new employer would compete with the prior employer. If small firms had less firm-specific knowledge to protect in the first place, they also might be less likely to prosecute a non-compete, making them more attractive to employees, and thus easier for the small firm to hire.

3.4. Non-competes and the challenges of hiring and job creation, for large firms

Larger firms should be less challenged in hiring and creating jobs in strong non-compete locations, due in part to the opposite arguments just made for startups (difficulty in attracting risk-averse talent, inability to offer competitive compensation, weaker legal resources in non-compete litigation). Existing firms will find hiring (and training) new employees more attractive in strong non-compete locales, because non-competes make it more likely they will retain their employee and recoup their investment.

Firms that benefit from non-competes will also accrue additional resources that in turn enable future growth in their work force. The greater enforceability of non-competes reduces an employee's outside alternatives, e.g., under standard non-competes, workers cannot be hired by a new employer that operates in the same field as their former employer. This significantly decreases the possibility that a worker is pursued by other employers and thus weakens the worker's

negotiating power against his or her current employer. To the extent that the best alternative for an employee becomes unavailable due to non-competes, the current employer can appropriate this increased gap between the expected value of the current job versus alternatives (Garmaise, 2009). This mechanism provides additional advantage and resources to a *current* employer that can in turn be invested in the expansion of the firm's work force; furthermore, firms with a larger stock of workers will benefit more from it.

3.5. Regional business concentration

A demographic shift towards small or large firms and a proportional change in job creation and employment by either group implies a restructuring of the local economy and change in business concentration, through entrepreneurship, firm relocation, and endogenous growth. We will not repeat the mechanisms detailed above, and instead focus on the impact of those mechanisms on the distribution of firm sizes and regional business concentration.⁴

With regards location of entrepreneurship, if startups are more attracted to a location due to a strengthening in non-compete enforcement, the density of small firms will increase (and decrease if they are not). With regards relocation decisions, if larger (and assumedly incumbent) firms are attracted, they will move into the region or open more establishments, and increase the density of large firms there (at least on the margin). With regards endogenous growth and observed job creation, any differential impact will be observable in the sources of job creation; if startups and small firms are advantaged, they will exhibit an increase in job creation following the law change, and if large firms are advantaged, they will exhibit an increase. The mechanisms need not be monotonic or asymmetric; if the market is restructured in a way that attracts large firms and crowds out small firms, and large firm employment growth is favored over small firm growth, this should be observable in an overall increase in business concentration.

4. EMPIRICAL DESIGN

4.1. Data and Sample

We use the Business Dynamic Statistics (BDS) provided by the U.S. Census Bureau for our main analysis. This data covers almost the universe of establishments and firms in the U.S. and their characteristics. It provides MSA-Firm Size-Year level data on establishment (including count, entry, and exit), job creation, and employment; for each MSA-year, variables on establishments and their employment are provided for twelve firm size categories.

One limitation is that the data are not available at the MSA-Industry-Firm Size-Year level; in other words, we are not able to run industry-specific analysis. To overcome this restriction, in Section 7.1, we use industry information from a separate data source, the Quarterly Census of Employment and Wages (QCEW). This data is constructed from the unemployment insurance (UI) accounting system for each state in the U.S. and provided by the Bureau of Labor Statistics (BLS). We match treated and controlled MSAs based on their industry composition. We calculated the distance in industry composition as the squared sum of differences in employment share by 5-digit NAICS industries. For each treated MSA, we selected and matched five control MSAs that have the most similar industry composition.

Table 2 provides descriptive statistics and a correlation table. There is little evidence of high correlations across variables in our models.

4.2. Difference-in-Differences Model

To empirically explore our hypothesized relationships, we run a difference-in-differences (DiD) estimation. The basic idea is that, as we do not observe MSAs in Florida in the absence of the 1996 amendment, we use non-Florida MSAs (which did not undergo any changes in the rules governing non-competes) as counterfactuals. An important identifying assumption is:

$$E[Y_{i,post}^{FL}(0) - Y_{i,pre}^{FL}(0)] \approx E[Y_{i,post}^{NonFL}(0) - Y_{i,pre}^{NonFL}(0)]$$

where the 0 in parentheses indicates a lack of treatment (i.e., no amendment). While there is no data on the left-hand side, we can observe the right-hand side of the equation and use it as a counterfactual Florida. In other words, we assume that MSAs in our treatment state (Florida) and control states (non-Florida) exhibit the same trends in outcome variables, in the absence of treatment. To better facilitate this “parallel trend,” we exclude MSAs in Alaska, California, Hawaii, Texas, and Puerto Rico from the control group. It is widely accepted that Alaska, California, Hawaii, and Puerto Rico are quite different from other states in terms of economic and geographic characteristics. California and Texas experienced changes in non-compete enforcement in 1998 and 1994, respectively (results remain robust with the inclusion of MSAs in these states). To further minimize the possibility of unobservable variables, Section 6 provides two robustness checks focusing exclusively on treated (Florida) and control (non-Florida) MSAs (1) that have the same industry composition and (2) that are located near the Florida borderline.

In our difference-in-differences regressions, we consider an indicator variable that adopts a value of unity for years following 1996 (*Post*). We interact this with an indicator variable that equals 1 for the MSAs in Florida (*FL*). To test the heterogeneous effects by firm size, we split the sample into two groups: one for firms with no more than 50 employees (“Small”) and another for firms with more than 1,000 employees (“Large”). We then run separate log-linear regressions in Equation (1) for the split samples for 1993-1999 (\pm three years from the year of the amendment)⁵:

$$\log Y_{it} = \mu + \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + X'_{it} \cdot \beta + \epsilon_{it} \quad (1)$$

where Y_{it} is an outcome of interest, μ constant, α_i MSA fixed effect, δ_t year fixed effect, and X'_{it} matrix of covariates. Note that FL_i and $Post_t$ variables are absorbed by the MSA and year fixed effects. The treatment is the 1996 amendment to the Florida statutes – i.e., stronger enforcement

of non-competes –, and the parameter of interest is τ .

A difference-in-differences estimation in Equation (1) forces estimates to be the same within pre- or post-treatment years. We run a more flexible econometric model with distributed leads and lags (“event study regression techniques”) as in Equation (2). We interact the treatment indicator (FL_i) with year indicators ($\mathbf{1}\{Year = t\}$), rather than uniformly assigning zero and unity for all pre- and post-treatment years. We leave the treatment year, 1996, as a baseline reference.

$$\log Y_{it} = \mu + \alpha_i + \delta_t + \sum_{t \neq 1995} \tau \cdot FL_i \cdot \mathbf{1}\{Year = t\} + X'_{it} \cdot \beta + \epsilon_{it} \quad (2)$$

An alternative, more comprehensive approach, is to compare the effects by firm size in the same model. The BDS data provides twelve firm size categories: 1 for 1-4 employees, 2 for 5-9 employees, 3 for 10-19 employees, 4 for 20-49 employees, 5 for 50-99 employees, 6 for 100-249 employees, 7 for 250-499 employees, 8 for 500-999 employees, 9 for 1,000-2,499 employees, 10 for 2,500-4,999 employees, 11 for 5,000-9,999 employees, and 12 for 10,000 or more employees. We created four dummy variables for firm size by collapsing three categories into one: *Size S* (1-19), *Size M* (20-249), *Size L* (250-2,500), and *Size XL* (2,500+).⁶ We then run the difference-in-differences estimation in Equation (3) for the period ranging from 1993 to 1999 with full sample. *Size S* is used as a baseline:

$$\log Y_{ist} = \pi^M \cdot Post_t \cdot FL_i \cdot SizeM_s + \pi^L \cdot Post_t \cdot FL_i \cdot SizeL_s + \pi^{XL} \cdot Post_t \cdot FL_i \cdot SizeXL_s + \mu + \alpha_i + \delta_t + X'_{it} \cdot \beta + \epsilon_{ist} \quad (3)$$

where X'_{it} includes all relevant two-way interactions ($FL_i \cdot Post_t$, $FL_i \cdot SizeM_s$, $FL_i \cdot SizeL_s$, $FL_i \cdot SizeXL_s$, $Post_t \cdot SizeM_s$, $Post_t \cdot SizeL_s$, and $Post_t \cdot SizeXL_s$) and firm size dummies (*SizeM*, *SizeL*, and *SizeXL*). Note that FL_i and $Post_t$ variables are absorbed by the MSA and year fixed effects. The parameters of interest are π^M , π^L , and π^{XL} .

One concern is that our data are yearly. Since the new law applied to the contracts written

on and after July 1, 1996, the inclusion of 1996 as a post-treatment year might bias the estimates. In addition, since the amendment was introduced by the Florida legislature, it is possible that employers and employees expected the change *ex ante* and adjusted their behavior before the effective date, July 1, 1996 (Barnett and Sichelman, 2016). We therefore exclude 6 months before and after the effective date, July 1, and run the regressions in Equations (1)-(3) for 1993-1999, leaving out the year of amendment, 1996.⁷

5. RESULTS

5.1. Business size and location preferences

Figure 2 illustrates the density change of firm size in Florida between 1995 and 1997. The solid line represents the density in 1995, while the dashed line represents the density in 1997 (left-hand side y-axis). Bars behind the density lines show changes in density between 1995 and 1997 (right-hand side y-axis). In Panel (a) of Figure 2, the entry of establishments (business units or branches) of small firms (including small single-unit firms) significantly decreased in 1997, whereas that of large firms shows an increase. As might be expected due to the large number of establishments that do not move, the density lines are less discernable for the total number of establishments in Panel (b). Changes in density shown in bars, however, are consistent with the entry comparison. The decrease in establishments comes from small firms, and large firms increase the number of establishments in Florida, after the amendment.

Figure 3 splits firms *within* Florida by their size. The solid line and left-hand side y-axis represent “Small” Florida firms that have less than 50 workers (first four categories of firm size in the BDS data). The dashed line and right-hand side y-axis represent “Large” Florida firms with more than 1,000 workers (last four categories of firm size in the BDS data). The idea of this approach is to find a divergent movement for Small vs. Large firms, after the 1996 amendment.

The two subgroups may differ in several characteristics, and there could be an idiosyncratic factor that specifically affects small firms. To check this and facilitate the comparison, we adjusted and aligned pre-treatment years (1991-1995) by rescaling the y-axis ranges. We generally find a parallel trend between the Small and Large firms for pre-amendment years, 1991-1995. We show in Panel (a) that the number of establishments of large firms increased to a greater extent than that of small firms, following the amendment in 1996 in Florida.

Figure 4 turns to inter-state analysis, comparing Florida and a counterfactual *synthetic* Florida. We use the Synthetic Control Method to construct a control unit that approximates the characteristics of the treated unit, Florida. This procedure compares a single treated unit to a weighted average of all the other control units (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2012). For the synthetic Florida (control), the weight of each state is chosen based only on the *pre-treatment* period (1991-1995) trends for all the U.S. states except for Alaska, California, Florida, Texas, and Puerto Rico. More specifically, we calculated the weights based on our outcome of interest in 1991, 1992, 1993, 1994, and 1995 after normalizing values relative to the 1991 value. An important advantage of normalizing the values is that we account for the time-invariant difference between Florida and other states, as in the formal difference-in-differences model. In other words, we take it into account that MSAs have different (absolute) numbers of establishments and employment and rely on (relative) changes over time. In this way, we could construct a parallel trend for Florida and its synthetic control for pre-treatment periods in all four graphs in Figure 4.

Since we study differential effects by firm size, we split the sample and plot the result by Small (where there are less than 50 employees) vs. Large firms (where there are more than 1,000 employees). In Figure 4, the red solid line represents Florida, while the brown dashed line represents the counterfactual synthetic Florida. We find in Panel (a) that the number of

establishments of Small firms in Florida becomes significantly lower than that in synthetic/counterfactual Florida, beginning from 1996. In contrast, the number of establishments of Large firms shows the opposite trend: it becomes higher than counterfactual Florida. It is reassuring that we find the opposite response by Small vs. Large firms.

To test if the Synthetic Control Method captures real and not spurious effects from our treatment, we perform a set of “placebo tests” to our control states. We perform the Synthetic Control analyses *as if* our control states had received the treatment (the 1996 law change), even if they were not. We then compare the distribution of the estimator for Florida and all the other control states, under the null hypothesis that the law change had no effect. If we observe similar trends for Florida and other control states that received placebo treatments, we cannot reject the null hypothesis of no effect. To reject the null hypothesis, we need that Florida exhibit a distinct change after 1996. The results are presented in [Figure 5](#). This Figure illustrates that the trend of Florida (black bold line) is exceptional, compared to the distribution of other control states with false treatment assignments (grey lines).

Table 3 provides the result from formal difference-in-differences models. Equation (1) estimates a split sample model. As hypothesized, for the establishment entry in Column (1), we consistently find opposite signs for $FL \times Post$ between the Small (<50 employees) and Large (>1,000 employees) split-samples. Entry by Small firms decreased by 5.6 percent, whereas large firm entry increased by 8.5 percent. The number of establishments in Column (2) shows a similar pattern though the estimate from Small sample is not precisely estimated.

Table 4 shows the results from alternative models (with full sample) where we interact indicators for four firm size categories with $FL \times Post$. For the establishment entry in Column (1), we consistently find that the estimates are positive and large for large firms. Entry of

establishments of firms with 20-249 workers is 3.7% larger than that of firms with 1-19 workers. Entry of establishments of firms with 250-2,500 and more than 2,500 workers is 15.3% and 12.4% larger than that of firms with 1-19 workers, respectively. Column (2) illustrates consistent results for the total number of establishments. The number of establishments of firms with 250-2,500 and more than 2,500 workers increased by 4% and 11% compared to that of firms with 1-19 workers.

This approach estimates the effects for larger firms *relative* to the smallest firm size category, *Size* (1 – 19). To estimate the effects more generally, we estimate separately for each firm size category in Equation (1). The results for the number of establishments by firm size are summarized in Panel (a) of Figure 6, where each dot represents an estimate for $FL \times Post$ from four separate regressions for each firm size category: *Size S* (1 – 19) , *Size M* (20 – 249) , *Size L* (250 – 2,500) , and *Size XL* (2,500+) . We find that the effects primarily come from responses by large firms, as their results are larger and more precisely estimated. Large firms prefer to locate in regions that enforce non-competes.

Entrepreneurship may also weaken with stronger enforcement of non-competes. Existing studies generally view firms with less than 19 employees as more likely to be entrepreneurial (e.g., Starr, 2015). A 5.6% decrease in the entry of establishments by Small firms (with 1-50 workers) suggests such a chilling effect on entrepreneurship. A more direct estimation is presented in Online Appendix D (Column 1) where we use the more granular firm size category variable and interact it with $Post \times FL$ variable. The entry of firms with 1 to 4 employees decreased by 7.5 percent ($-0.0913 + 0.0167 \times 1$) compared to MSAs in control states, while that of firms with 5 to 9 employees decreased by 5.8%, and that of firms with 10 to 19 employees decreased by 4.1%, relative to control state MSAs. Overall, the change in non-compete law appears to have made Florida a more attractive location for large firms and a less attractive location for small firms.

5.2. Business Size, Job Creation, and Employment

Panel (c) of Figure 2 illustrates job creation by size of firm in Florida between 1995 and 1997. While job creation by the smallest (<50 employees) and largest (> 5,000) categories clearly decreased and increased, respectively, the results in the middle of the distribution are mixed. Employment in Panel (d) of Figure 2 shows a similar pattern. Figure 3 splits the data between Small and Large firms *within* Florida. In Panel (b), employment in Small firms (dashed line) decreased, as opposed to that in Large firms (real line), following the 1996 amendment.⁸ Finally, an inter-state comparison with the Synthetic Control in Panel (b) of Figure 4 shows consistent results. We find decreased employment by Small firms in the left-hand side, relative to a weighted average of other control states, beginning from the amendment year, 1996. In contrast, increased employment by large firms is found in the right-hand side figure. Note that both figures in Panel (b) show a fairly good parallel trend for pre-amendment years, 1991-1995.

Table 3 estimates split sample models and illustrates that small firms decreased their job creation by 1.8%, whereas large firms did the opposite (increased by 7.6%), though the estimate for the small firm sample is imprecisely estimated. The alternative specification with four categories for firm size in Table 4 finds similar and consistent results. Firms that have more than 250 workers increased their job creation and employment by 8-24% and 13-16%, respectively, compared to firms that have 1-19 workers. With regards to job creation and employment by entrepreneurial firms, job creation and employment by entrepreneurs with 1 to 4 employees decreased by 1.25 percent and 3.7%, respectively, in the specification with linear *Size* variable interaction (Online Appendix D).

We then estimate the effects separately for the four firm size categories. The results are summarized in Panel (b) of Figure 6. Each dot represents an estimate for $Post \times FL$, and we again

find that the effects primarily come from hiring expansions by large firms (rather than shrinking employments by small firms).

The change in non-compete law appears to have altered job creation and employment by small and large firms. Even though the total number of jobs in Florida increased after the amendment was instituted, these jobs predominantly came from large firms; small firms created relatively fewer jobs.

5.3. Regional Business Concentration

The first two results imply an increase in business concentration for two reasons. First, large firms appear to prefer a region that enforces non-competes when they launch or relocate establishments; small firms appear to be crowded out. Second, large firms appear to be adding jobs and growing at a faster rate than small firms.

Although we do not have firm-level data that covers both small and large firms (note that Compustat only includes large, publicly traded firms), we can estimate changes in business concentration using the following three measures: 1) share of establishments that belong to large firms (“establishment concentration”), 2) share of workers that belong to large firms (“employment concentration”), and 3) Pseudo Herfindahl-Herschman Index (HHI). Note that this Pseudo-HHI measure also uses the share of employees. It is calculated based on the weighted average of the share of employees in each firm size category in each MSA:

$$PseudoHHI_{it} = \sum_{s=1}^{12} \left[\frac{Min_s + Max_s}{2} \times \left(\frac{Number\ of\ Employees_{ist}}{\sum_j Number\ of\ Employees_{ist}} \right)^2 \right]$$

where $\frac{Min_s + Max_s}{2}$ is the representative firm size in each firm size category s (“weight”), and

$\frac{Number\ of\ Employees_{ijt}}{\sum_j Number\ of\ Employees_{ijt}}$ is the share of employees in size category s in MSA i in year t (“share”).

We then calculate a sum over all twelve categories. This measure mimics the way we calculate the original firm-share based HHI and captures the degree of business concentration at the MSA-year level.

Figure 7 shows the results from the Synthetic Control Method. In both Panel (a) and Panel (b), we consistently find that business concentration increases after the year of law change, 1996. We then run the differences-in-difference regression in Equation (4) with the three different measures of business concentration:

$$\log y_{it} = \mu + \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + \epsilon_{it} \dots (4)$$

In our result in Column (1) in Table 5, we find that the establishment-based share of Large firms that have more than 1,000 employees increased by about 2.4%. Column (2) shows the employment-based share of large firms that have more than 1,000 employees. Consistent with our prediction, the results show an increase by 4.7%. Column (3), Table 5, again illustrates that business concentration measured by the Pseudo-HHI increases after stronger non-compete enforcement, by 17.4%.

Since a difference-in-differences model imposes a uniform effect for pre- and post-treatment years, we run a more flexible model with event study techniques. We interact the treatment indicator with year indicators (instead of the $Post_t$ indicator). The results are illustrated in Figure 8, where the solid line shows the estimates by year and vertical lines represent a 95% confidence interval. In Panel (a), the establishment-based share of Large firms increased after the amendment in 1996, and we observe a parallel trend for pre-amendment periods, 1993-1995. In Panel (c), the employment-based share of Large firms increased after the amendment in 1996.

It is worrisome that there appears to be a pre-trend in Figure 8, especially an increase from 1993 to 1994. To further check if our findings result from pre-existing trends, we interact yearly

outcomes for pre-amendment years with a full set of year dummies. This absorbs all the pre-1996 differences in employment share of large firms in our analyses, and some of the post-1996 variation, but makes our post-1996 comparisons close to *ceteris paribus* (Cantoni, Dittmar, and Yuchtman, 2018). The results are shown graphically in Panel (d). By design, there are no pre-1996 differences in trends between treatment and control groups. We again confirm from this very stringent specification that following the 1996 amendment that large firms expanded their employment and increased their share of employment in Florida. The same technique is applied to the establishment-based share of large firms, presented in Panel (b). In summary, the change in non-compete law appears to have preceded increased business concentration, through different firm (re)location choices by size of firm and relatively faster employment growth by larger firms.

6. POTENTIAL THREATS TO IDENTIFICATION

Since we investigate a single event that happened at the state-level to identify the effects, the results are vulnerable to other simultaneous and confounding events, particularly if there was a change that operated in the same direction as the non-compete amendment (i.e., benefitting large firms and harming small firms or start-ups). While it is not possible to consider every event that happened in 1996, we discuss two specific threats to identification: Enterprise Florida, Inc. and electoral changes. [Appendix Figure C.1\(a\)](#) illustrates how wage trends appeared unchanged before and after 1996, which eases concerns that the law change impacted the economy through wage changes and that the effects of non-competes are all absorbed in wage adjustments.

6.1. Enterprise Florida, Inc.

Enterprise Florida, Inc. (EFI) is a “public-private partnership between Florida’s business and government leaders,” aiming to, “expand and diversify the state’s economy through job creation”.

When describing their history, EFI states, “In 1996, under Governor Lawton Chiles, Florida became the first state in the country to place principal responsibility for economic development, international trade, research and business image marketing in the hands of a public-private partnership.” If EFI began a program in 1996 that (1) could affect Florida businesses and (2) disproportionately favored large established firms, there would be potential confounds. However, we do not find any evidence that EFI actively initiated any programs around 1996 or that its policies favored large firms, at the expense of small firms.

First, according to the EFI’s history statement, it was not until 2011 that the EFI created a, “seamless economic development team,” and began publishing annual reports and assessments. Archival research did not find any evidence of its activities in the 1990s. Furthermore, the EFI states that it focused on reforming the state’s industry structure from tourism and agriculture to a more sophisticated mix. Figure G.1 in Online Appendix reveals, however, no noticeable change in Florida’s industry composition for 1991-2001. Second, even if the EFI had actively operated beginning from 1996, its website stated that EFI “...supports small and minority businesses through its capital programs,” and other entrepreneurial goals.

6.2. Electoral Changes

If electoral outcomes changed sharply around 1996 in preference to pro-big business candidates, the findings might result from policies that favored large firms. We do not, however, see a discontinuous change in Florida party politics at this time. First, incumbent Republican U.S. Senator Connie Mack III won re-election to a second term in 1994. Second, in 1992, President Bill Clinton (Democratic) won over Senator Bob Dole (Republican) by a margin of 5.7%. This represented an improvement over his narrow loss of the state in 1992.⁹ Lastly, in 1996, in the 23 districts in Florida, 20 incumbents were re-elected. The remaining three incumbents retired, and

candidates from the same party kept the districts. In summary, it does not appear that electoral outcomes would disproportionately favor large firms against small firms or start-ups in Florida around 1996.

7. ROBUSTNESS CHECKS

7.1. Matching MSAs on Industry Composition

Although enforcement of non-competes typically applies equally to all industries, adoption and implementation (by employers and employees) could still differ. Starr, Prescott, and Bishara (2016) in fact find in their 2014 survey that the use of non-compete varies across states and industries, for example, they find few incidences of non-competes in agriculture and hunting (9%), compared to information (32%), mining and extraction (31%), and professional and scientific (31%) industries. Here we test if our results remain robust to industry heterogeneity across MSAs.

We are not able to control directly for industry composition because the BDS data lack information by industry. As an alternative, we look at the Quarterly Census of Employment and Wages (QCEW) data that provides information on county, MSA, and state-level industry composition. [Figure G.1 in Appendix](#) shows Florida’s industry composition. The idea is that, using industry information in the QCEW, we can control for conflating effects of industry composition by matching control MSAs that share the same industry composition as Florida MSAs. We then bring this treatment-control MSA pairs to the BDS data and re-run the regressions.

Five-digit North American Industry Classification System (NAICS) code (11111-99999) and its employment in each MSAs are used to calculate the Euclidean distance between industry compositions of any two MSAs:

$$Industry\ Distance_{A,B} = \sum_{NAICS\ (5-digit)} (Emp_{A,NAICS} - Emp_{B,NAICS})^2$$

where $Emp_{A,NAICS}$ and $Emp_{B,NAICS}$ are the number of employment in industry NAICS in MSA A and B, respectively. For each Florida MSA, we identify ten non-Florida MSAs that have the most similar industry structure as the focal Florida MSA. We then run the same difference-in-differences estimation using this paired MSA data. Results provided in [Table 6](#) and [Table 7](#) (odd-numbered columns) and [Table 8](#) are not qualitatively different from our main findings, making it less likely that the results are driven by a discrepancy in industry composition between the treated and control MSAs.

7.2. State-Bordering MSAs

One concern might be that the treatment group (MSAs in Florida) and control group (MSAs in states other than Florida) may differ in terms of unobservable characteristics. To mitigate this concern, we compare the MSAs near the Florida state border. In this case, the treatment group is the MSAs in Florida within n miles of the border, while the control group is the MSAs in Alabama and Georgia within n miles of the Florida border. It is expected that the MSAs near the Florida borderline would share many unobservable characteristics, strengthening the validity of the control group and the parallel trend assumption.

MSAs in Alabama, Florida, and Georgia near the border of Florida are identified in [Figure H.1](#) in [Appendix](#). There are four MSAs in Florida, two in Alabama, and one in Georgia. Thanks to geographic proximity and an arbitrary straight border, these MSAs should share many unobservable or intangible characteristics such as commutable area, culture, weather, etc. The results of the formal regression, Equation (1) and (2), are presented in [Table 6](#) and [Table 7](#) (even-numbered columns). The results are not qualitatively different from those in [Table 3](#) and [Table 4](#) (and matching results in odd-numbered columns in [Table 6](#) and [Table 7](#)), though with a much

smaller number of observations, the estimates become less precise.

The magnitudes are generally larger in the model only with border MSAs. One potential explanation for this result (which could only be tested with establishment-level panel data) is a substitution effect arising in the borderline sample. Given the geographic proximity and cultural similarity between the treated and the control in the borderline, the closer a firm is to Florida, the more likely that this firm moves to Florida, *in response to* the 1996 Florida amendment. For example, it is much more likely that potential new entrants choose between Tallahassee MSA (Florida) vs. Valdosta MSA (Georgia) than Tallahassee MSA (Florida) vs. San Francisco MSA (California). The borderline sample captures this substitution effect to a greater extent than the full sample. A move between state-bordering MSAs will more likely to lead to a *double-counting* of the effect when a large firm moves into Florida and a small firm leaves, because a move of single firm from control MSA to treatment MSA is counted twice when we calculate the difference in the number of firms between the two groups.

This argument implies that our control MSAs in Alabama and Georgia borders are also affected by the 1996 Florida amendment. This magnified border effect provides additional evidence that the 1996 Florida amendment drives the observed changes. We find greater effects even if the two MSAs share most of business environments *other than* legal institutions that govern non-compete enforcement, strengthening the probability that the changes in the enforceability may be the only reason for increased relocation of businesses after the 1996 Florida amendment. This magnified result for state-bordering MSAs increases our confidence that firms move in response to changes in non-compete enforceability.

7.3. Potential conflation with changes in wages

The models estimated above rest on the assumption that non-competes impact small and large firms differently and that this difference cannot be adjusted, most obviously, in wages. “Consideration” – i.e., a benefit an employee receives in response to non-competes – provides an obvious threat to this assumption. For example, if employees fully understand the consequences of non-compete enforcement *and* have strong bargaining power (e.g., they are irreplaceable) or an attractive alternative job option, they can negotiate a wage increase to compensate for their reduced mobility. In this situation, small and large firms may behave similarly, because any benefits and losses from non-compete enforceability would be efficiently reflected in wages (or other forms of employee benefits). In other words, firms would pay for the reduced mobility of their workers, and therefore the benefits and costs that arise from non-compete enforcement would offset each other. For this situation to hold, the employee needs to 1) be fully aware of the consequences of non-competes and 2) have the bargaining power to receive a higher wage. This may be rare; a survey by Starr, Bishara, and Prescott (2016) reports that only 10 percent of workers subject to non-competes try to bargain over their non-compete.

Recent empirical investigation of wage consideration by Balasubramanian et al. (2017) finds that non-compete enforceability is *not* positively associated with wage levels for technology workers; the relationship in fact was found to be negative. The U.S. Department of the Treasury (2016; p. 19) also suggests that “a standard deviation in non-compete enforcement reduces wages by about 1.4%”. Balasubramanian et al. (2017) interpret their results as a wage suppressing effect due to a reduction in bargaining power. Even though a more careful study is required to tease out the exact mechanisms for the lack of increased or reduced wages, their findings demonstrate potential frictions in the labor market (i.e., at least one of the conditions of awareness and bargaining power is not met), such that the employees’ reduced mobility is not offset by wages.¹⁰

To empirically research the concern that changes in wages might conflate the impact of

non-competes upon firm sizes, we investigated wage trends in Florida and real and synthetic control states, using data from the Quarterly Census of Employment and Wages (QCEW). In Appendix, Figure C.1(a) compares wage trends in Florida and other comparison states for 1991-2001 and Figure C.1(b) shows wage trends for Florida and its synthetic control. Both graphs indicate very similar wage trends between Florida and control states around 1996. Differences-in-differences estimations also show that we cannot reject the null hypothesis that Florida's wage change is not different from the control states (estimate: 0.0051, p-value: 0.195). It appears that wage levels in Florida remained relatively unaffected by the 1996 amendment.

Furthermore, we examine the possibility that different sized firms adjust their wages differently when they ask their employees to sign a non-compete. On the one hand, it could be the case that employees have more bargaining power against small firms than against large firms, and therefore the wage impact of the law change acts primarily through small firms. In this case, we would expect that the wage paid by small firms would increase disproportionately relative to that of large firms. On the other hand, if large firms extensively use the covenants, the wages of workers in large firms would increase disproportionately as their workers gained compensation. We would then expect to see a larger wage increase for large firms.

We examine wage changes by establishment size over time in Figure C.2.¹¹ Panels (a) and (b) in Figure C.2. show that the share of wage by establishment size does not change meaningfully around 1996 and that wage growth rates are not systematically different for establishments of different size. These analyses provide evidence that the allowed imposition of non-competes is not fully reflected in worker wages. While these analyses and prior literature are consistent with workers being unable to bargain against non-competes, it is also possible that the law change had little effect on wages.

8. DISCUSSION

This study shares limitations with existing studies on non-competes in that the variation in the legal regime we exploit occurs at the state level (unfortunately, most policy or legislative changes on non-competes occur, at a minimum, at the state level). Nor can we observe individual labor contracts (i.e., whether each employee signed a non-compete or not). The stark change in non-compete enforcement makes Florida a good research site, however, and our additional analyses on the industry-matched MSAs and Florida borderline should lessen these concerns. While we investigated other states' changes in non-compete laws, none offered the sharp and focused change of Florida's 1996 statute, and most experienced only a weak and/or ambiguous change in enforcement or were vulnerable to other confounding factors.

This historical research revealed a great deal of heterogeneity in the details of each law change and local context, heterogeneity which makes it fundamentally difficult to generalize the impact of changes in non-compete laws across different states. The research consistently implied that Florida was strong and unique (in particular, the flip in presumption of injury and burden of proof), indeed, the Garmaise scale took Florida from a moderately enforcing state to the most extreme non-compete regime in the country. In characterizing Florida and other non-compete law changes, it became clear that no state has actually flipped from one extreme to the other – instead, most changes have been subtle differences of degree and types of enforcement mechanisms. This calls into question the assumption that every state's change in non-competes – in both directions - can be used as binary and reversible experiments. Researchers should not bin all the changes in non-compete law into two discrete buckets and then run aggregated regressions.

Following our own recommendations then, we present Florida as a single case study. Florida's experience implies that states that enforce non-competes could experience a decrease in small firm entry and employment, an increase in large firm entry and growth, and eventually an

increase in their business concentration. The best evidence for generalization comes not from Florida but from Figure 1, where we found state-level correlations between non-compete enforceability and the Florida outcomes. The left panels in Figure 1 show that states which strongly enforce non-competes tend to have a smaller proportion of small firm establishments and employment. The right panels in Figure 1 reflect this result for larger firms; stronger non-compete enforceability and the proportion of large firm establishments and employment are positively correlated. The sharp contrast between small vs. large firms' cross-sectional correlations are consistent with the illustrated mechanisms in Florida. Panel (c) in Figure 1 then illustrates a positive relationship between a state's strength of enforcement and its business concentration as measured by a pseudo Herfindahl index. These relationships hold consistently for two indices of enforceability (Garmaise, 2009 and Starr, 2016) and without the outliers of California and North Dakota (See Appendix, Section G, for more detail).

Analogous to the brain drain of talented individuals by non-competes (Marx, Singh, and Fleming, 2015), these results could be labeled as a small – and probably entrepreneurial – firm drain (though Florida obviously benefited from the location choices and increased employment of large firms). If both human and organizational capital leaves states that enforce non-competes for states that do not, it is less surprising that California and other non-enforcing states have become hotbeds of entrepreneurship (Guzman and Stern, 2015). For example, Facebook moved when still small from an enforcing state (Massachusetts) to a non-enforcing state (California). Is such movement an anomaly or characteristic of more promising small firms? Possibly reflecting this effect, Marx and Fleming (2012) illustrated that the proportion of elite inventors – as measured by career prior art citations and number of co-authors – have become increasingly likely to emigrate to states that do not enforce non-competes. Fallick, Fleischman, and Rebitzer (2006) also suggest that weaker enforcement of non-competes is positively correlated with “the reallocation of talent

and resources towards firms with superior innovations.” Weighed against this concern is that large firms tend to do better than smaller ones (Hathaway and Litan, 2014) and our finding demonstrated here that more jobs were created in Florida immediately following the strengthening of non-compete enforcement.

9. CONCLUDING REMARKS

Non-compete covenants provide useful and important tools with which both employers and employees can commit themselves and prevent potential labor market failures. Non-competes also decrease the competition caused by separating employees. However, they may also hamper employers’ competition for employees and employees’ freedom to choose their jobs. It is thus important to understand these trade-offs and their wider consequences.

We examined how the stronger enforcement of non-competes influenced business dynamism in one local economy, exploiting the 1996 amendment to Florida statutes on non-competes. The results contribute to the literature by exploring the heterogeneous effects of non-competes by firm size on firm location choice and employment growth, and business concentration. The enforcement of non-competes appears to have affected not only inter-state competition for attracting businesses, but also the in-state distribution of businesses and jobs. Small and large firms responded to non-compete enforceability in opposing ways: large firms appeared more likely to locate (either launch or move) their establishments in Florida and small firms appeared less likely. Although our data could not isolate specific mechanisms or differentiate between new vs. existing firms, small firms appeared reluctant or less able to create new jobs. In contrast, large firms boosted their rate of new job creation following the law change. Likewise, the level of employment decreased for small firms and increased for large firms. Consistent with these results, we observed an increase in the business concentration in Florida, following strengthened

non-compete enforcement. It does not appear that non-competes influenced wages in Florida, or that business friendly policies or legislators caused the effect. Furthermore, across all U.S. states, we observe a negative cross-sectional correlation between non-compete enforcement and small firms' establishment and employment. In consistent contrast, a positive relationship exists between non-compete enforcement and large firms' establishment and employment. Business concentrations also exhibit positive relationships with non-compete enforcement across all U.S. states.

These differential effects on firm (re-)location and employment by firm size have important yet overlooked managerial and policy implications. Firm strategies on R&D and innovation differ by their size (e.g., Cohen and Klepper, 1996a & 199b), and thus it is important for managers to understand how small and large firms (re)locate and grow differently in response to non-compete enforcement. Managers need to be aware that non-compete enforcement may affect not only the mobility of its own workers but also changes in competition and the broader market environment through the redistribution of firm size and increased concentration. Stronger enforcement may have lowered the "birth" and/or move-in rate of establishments of small firms while simultaneously attracting large firms. This, for instance, could affect a firm's search for alliance partners or acquisition targets, competitive strategy, and ultimately performance.

Furthermore, to the extent that small and large firms provide different values and jobs to local economies (e.g., incremental vs. break-through innovations, the quantity and quality and types of jobs, application of productivity enhancing innovations), the effects of non-competes on a local economy could be varied and large. Asymmetries in firm positioning and employment growth (i.e., the dominance of large firms and the jobs they offer) could have important implications for welfare for consumers and producers. For instance, if new jobs at start-ups create unique value for firms and the economy that cannot be provided by already mature firms (for

example, if startups are more likely to incorporate productivity enhancing innovations), state governments may want to attract entrepreneurs and the jobs they create. Geographic agglomeration and clustering of different sizes of firms also have important implications for entrepreneurship, innovation, intellectual property protection, and regional economic growth (The White House, 2016). In this sense, policies and legal constraints on non-competes should not be considered in isolation. Non-competes are not mere contractual provisions agreed upon by employees and employers; they have wider implications for consumers, social welfare, inter-state competition in attracting businesses, intra-state competition for labor forces, and business dynamism. Policy makers and legislators should take these broader impacts into account.

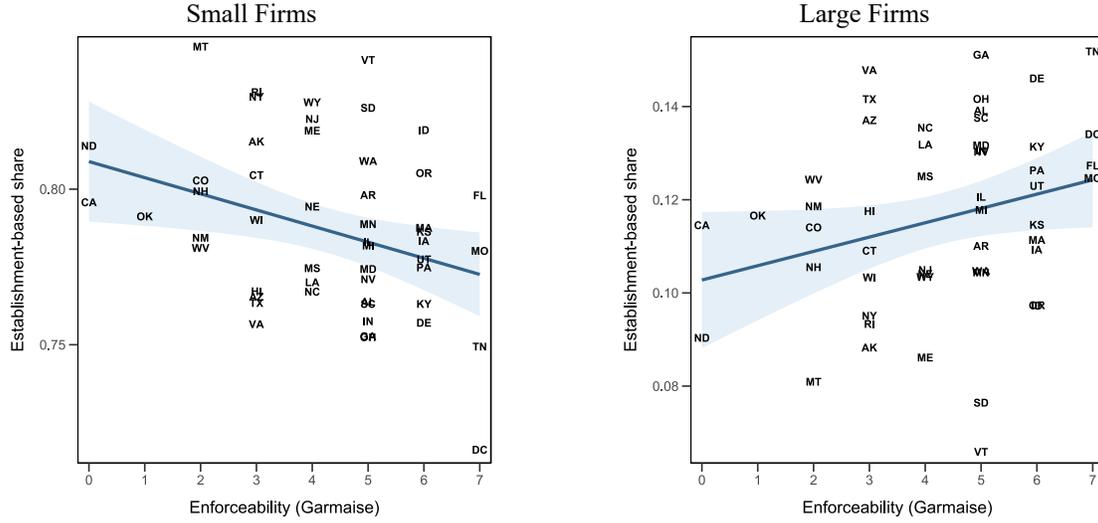
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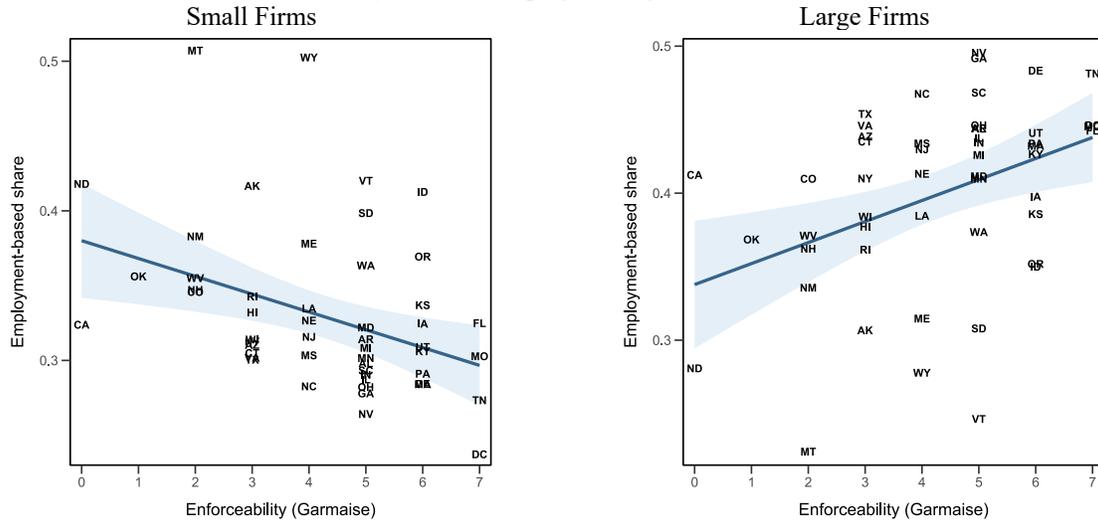
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Figure 1 U.S. State Non-competes Enforceability and Regional Business Concentration

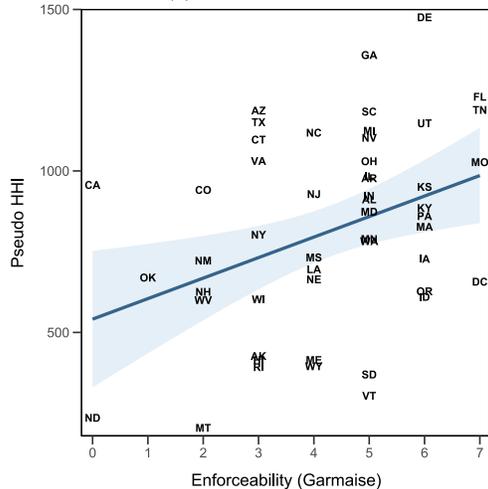
(a). Share of establishments by firm size



(b). Share of employment by firm size



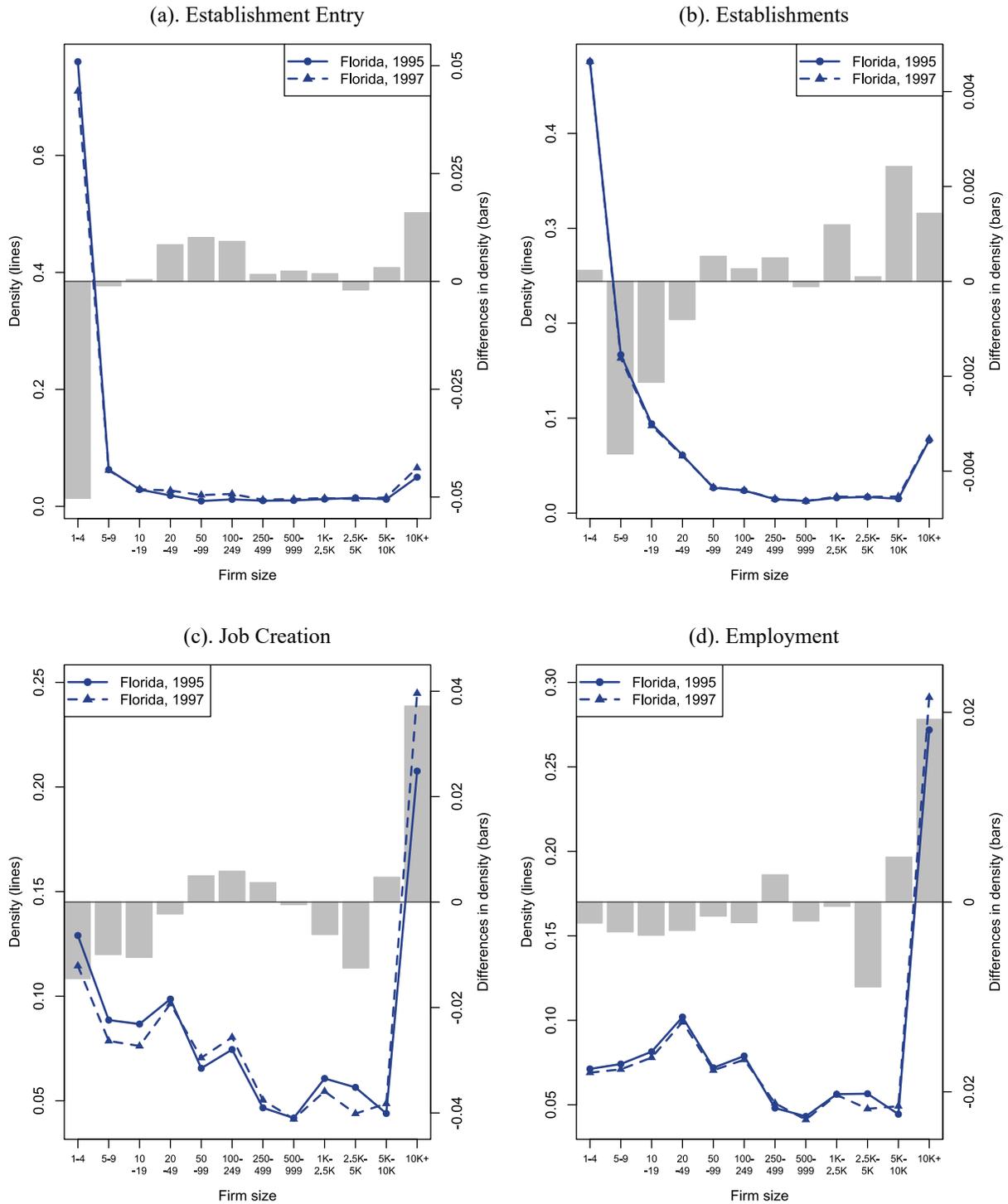
(c). Pseudo HHI



Blue solid line represents a fitted (bivariate) regression line with full sample: regressed each outcomes on non-competes enforceability, including an intercept. Results for regressions: (a). left panel: coefficient -0.0052, standard error 0.0021, p-value 0.0162; right panel: coefficient 0.0031, standard error 0.0016, p-value 0.0570; (b). left panel: coefficient -0.0120, standard error 0.0041, p-value 0.0057; right panel: coefficient 0.0143, standard error 0.0047, p-value 0.0036; (c). coefficient 63.51, standard error 22.79, p-value 0.0076. Small firms: <50 employees. Large firms: >1,000 employees.

Data: Business Dynamics Statistics (BDS), 1996.

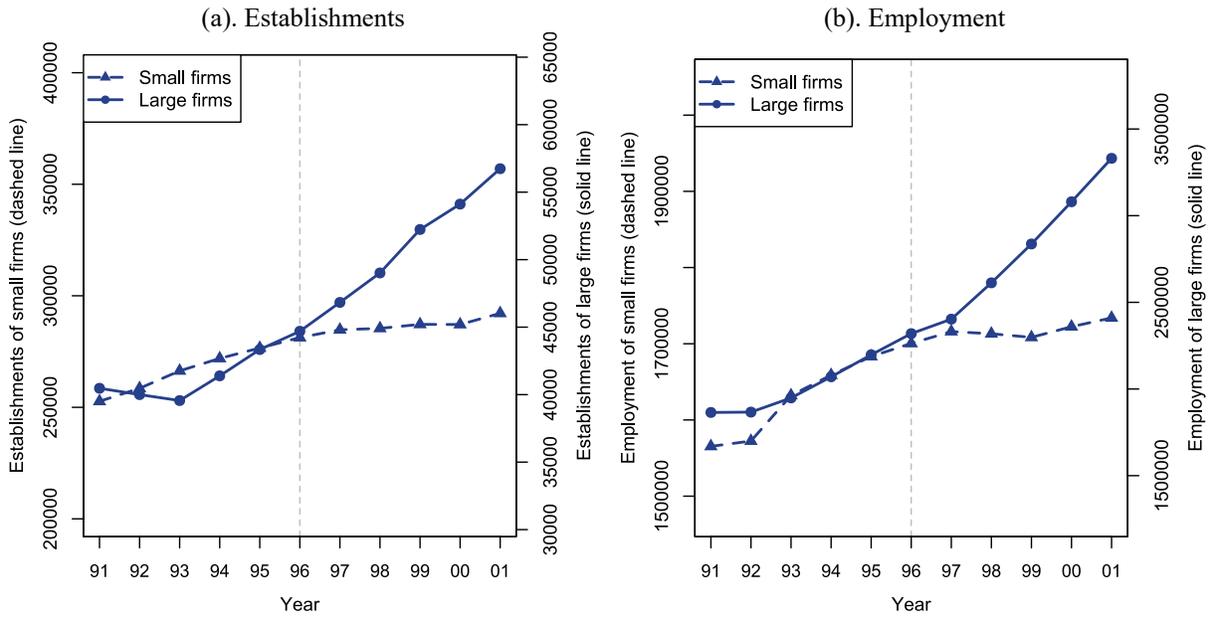
Figure 2 Density of Establishments and Employment of Florida Firms by Size: 1995 vs. 1997



Data: Business Dynamics Statistics (BDS), 1995 and 1997.

Lines represent firm size distribution (density), while bars represent the difference in density between 1995 and 1997.

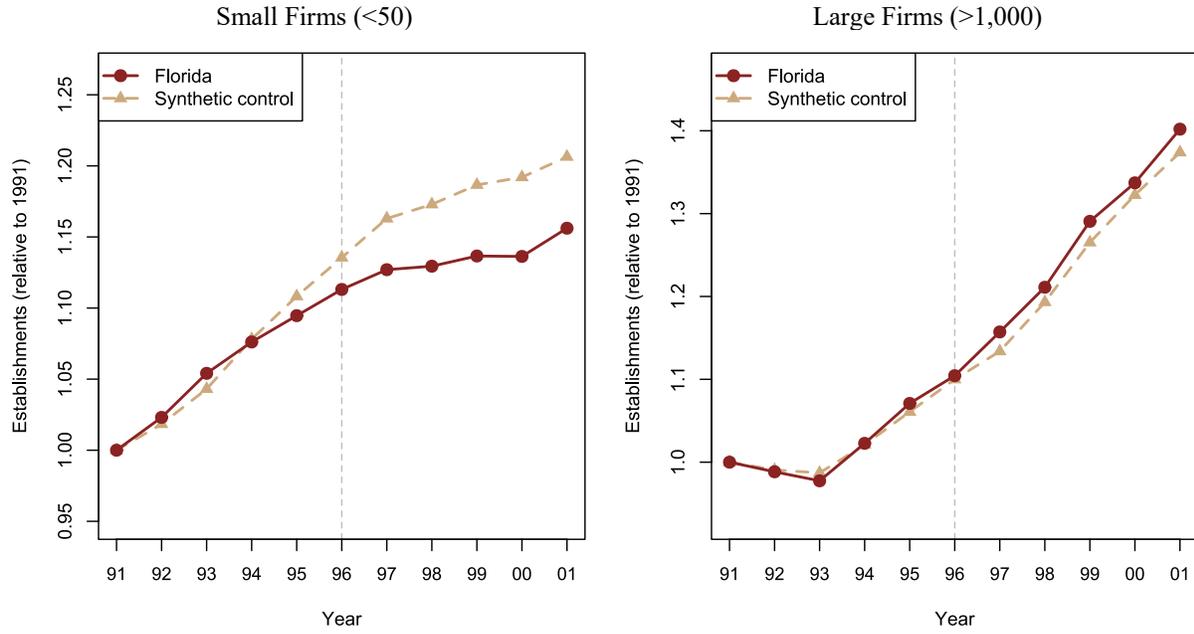
Figure 3 Trends in Establishments and Employment of Florida Firms by Size: Split Sample



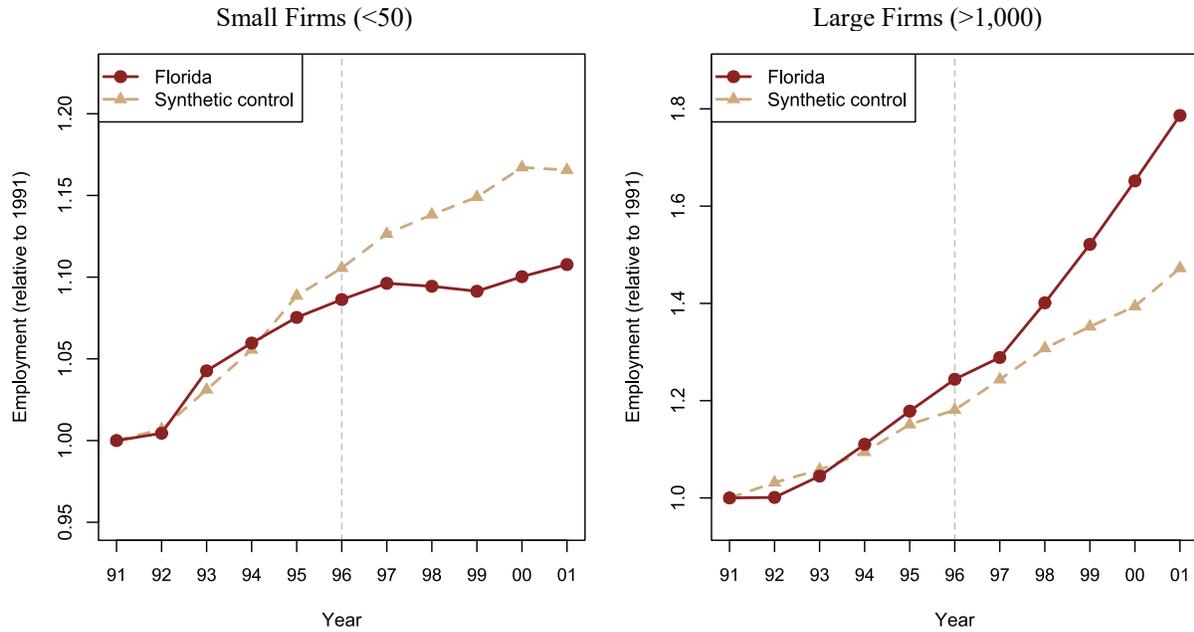
Data: Business Dynamics Statistics (BDS), 1991-2001.

Figure 4 Synthetic Control Method: Establishments and Employment by Firm Size

(a). Establishments



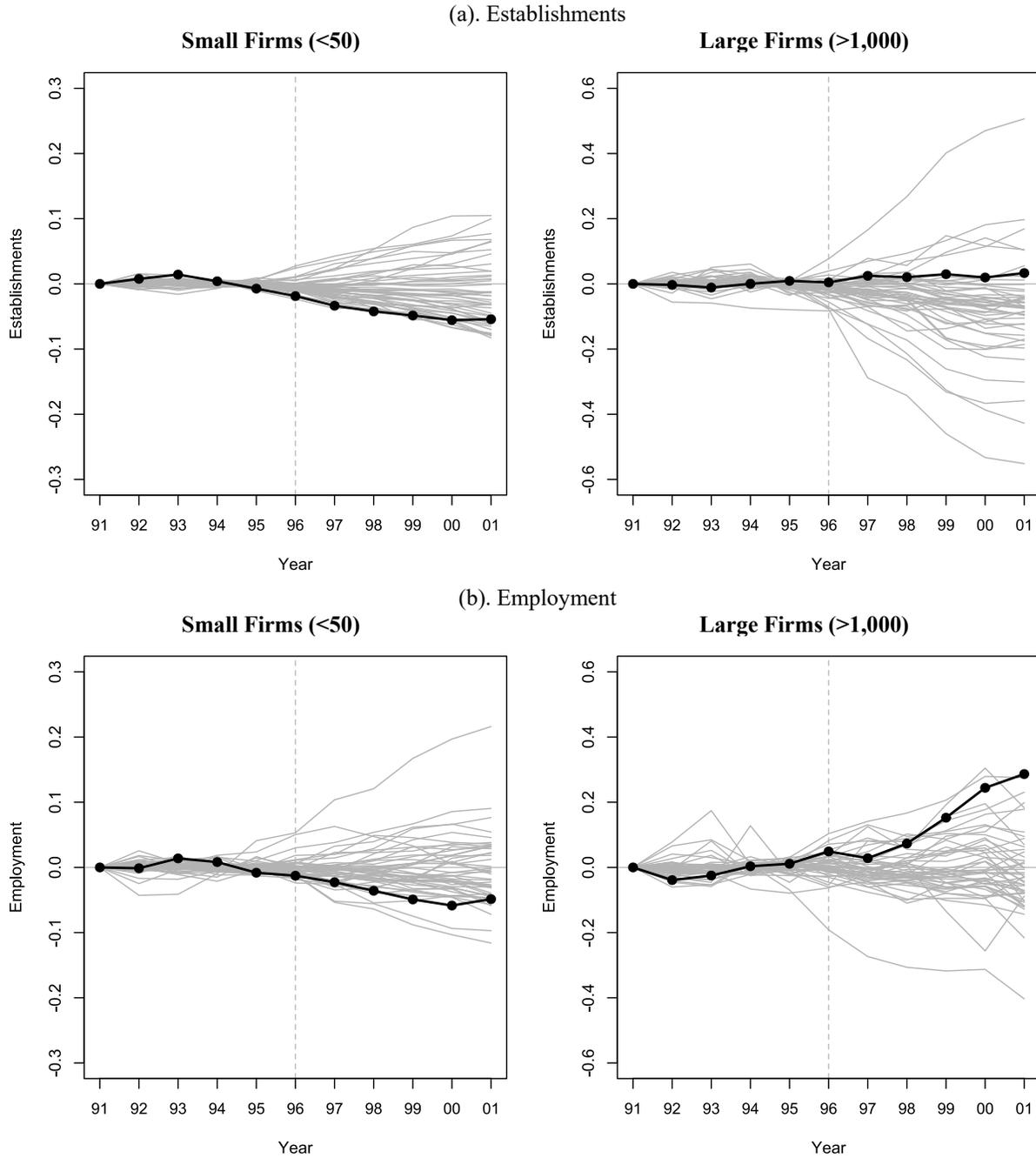
(b). Employment



Data: Business Dynamics Statistics (BDS), 1991-2001.

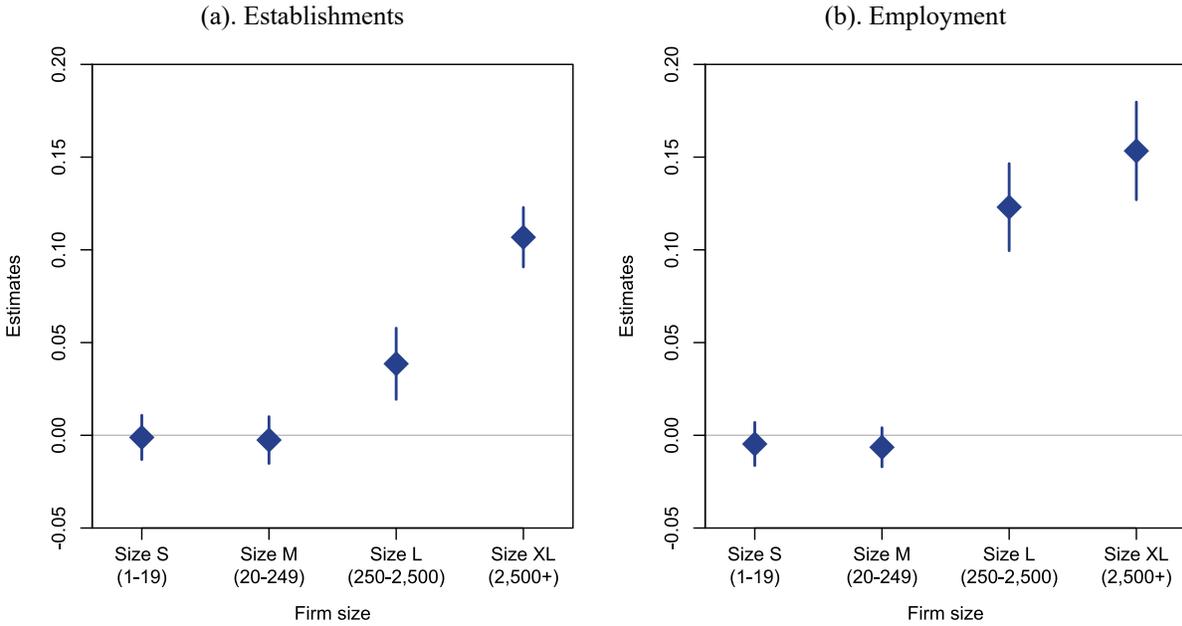
Note: The outcome variables for Florida are normalized relative to their 1991 value.

Figure 5 Placebo Tests for Synthetic Controls: Establishments and Employment by Firm Size



These figures show the *gaps* in the outcome variable for the treated state and the synthetic control. The black line represents our test for Florida (the actual treated state). We additionally perform *placebo tests*, pretending that the states in our control group were treated. Each of these placebo tests are presented in the gray lines. The outcome variables for Florida are normalized relative to their 1991 value. Data: Business Dynamics Statistics (BDS), 1991-2001.

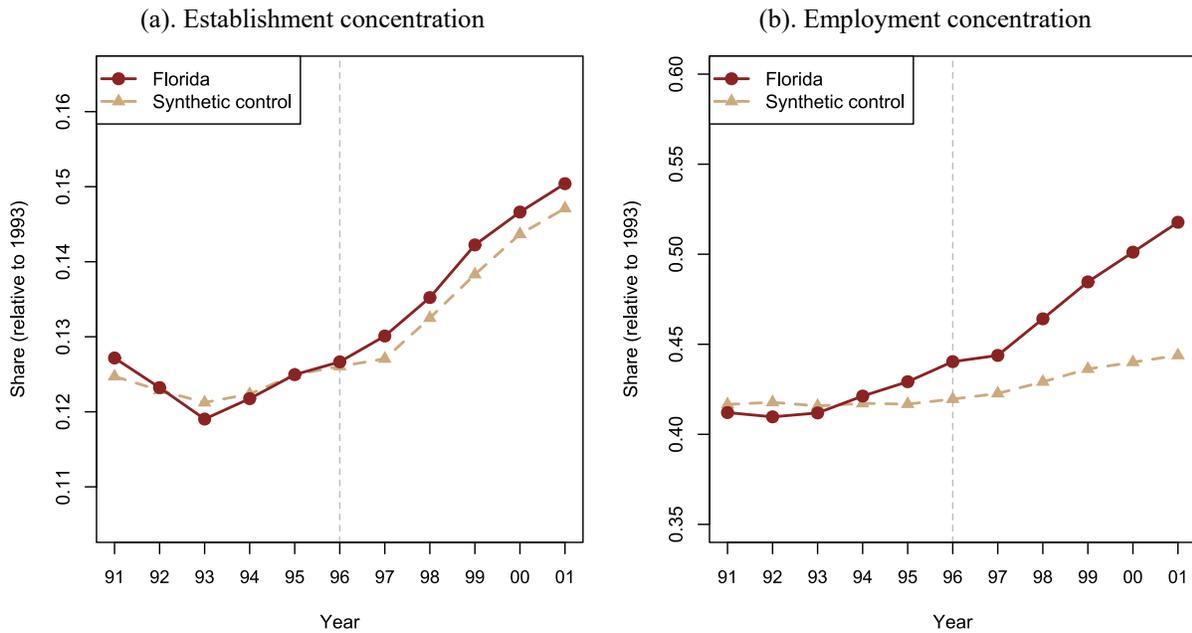
Figure 6 Establishments and Employment of Florida Firms by Size: Split-Sample Regressions



Data: Business Dynamics Statistics (BDS), 1993-1999.

Note: Each point stands for an estimate ($Post \times FL$) from separate regressions by firm size category. Red real lines stand for 95% confidence interval based on standard errors clustered at the state level.

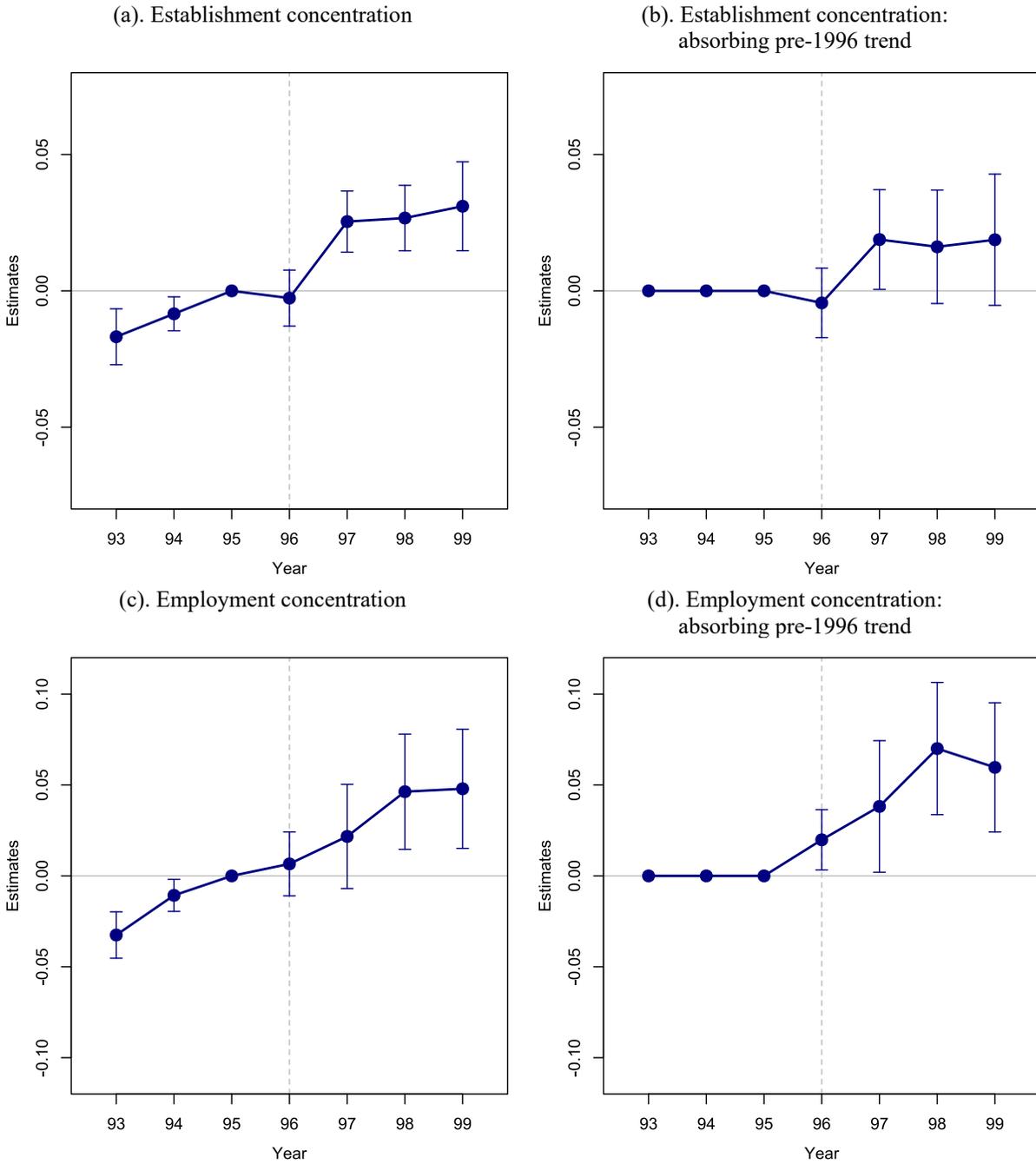
Figure 7 Synthetic Control Method: Business Concentration



Data: Business Dynamics Statistics (BDS), 1991-2001.

Note: The outcome variables are normalized relative to their 1991 value. We measure “establishment (or business-unit) concentration” as the share of establishments by Large firms (that have more than 1,000 employees) and “employment concentration” as the share of employment by Large firms.

Figure 8 Regional Business Concentration in Florida: Even Study Approach



Data: Business Dynamics Statistics (BDS), 1993-1999.

Table 1 The 1996 Amendment to the Florida Statutes and Non-competes Enforceability

	§542.33B (1990 – Jun 1996)	§542.335 (July 1996 – Present)	Note
Protection of business interests	Not specified	Lists five <i>non-exclusive</i> legitimate business interests that can be protected	Provides an open-ended enumeration of what the employers can do (but not what they cannot do)
The modification of over-broad covenants (“Blue pencil”)	Courts have option either to modify or not to enforce	Courts can only modify the excessive restraints rather than declaring it non-enforceable	Made it easier for employers to write highly restrictive covenants (without fearing it being overturned)
Burden of proof	Not specified	Once an employer proves that the non-competes meet the “legitimate business interests” restriction, the burden of proof shifts to employee	§542.335(1)(c): “the person opposing enforcement has the burden of establishing that the restraint is over-broad, overlong, or otherwise not reasonably necessary ...”
Injunctions and the presumption of irreparable injury	Not specified	Once an employer shows the intentional breach of non-competes, irreparable harm is presumed. Courts may issue an injunction that prohibits competition not only by the former employee, but also by his/her new employer	Made it easier for employers to receive injunctions. Courts may also award damages for a violation of non-competes, including lost profits and damages
Limitations on public policy defense	Allows the courts to consider public policy and welfare (when entering injunction)	Courts could not refuse enforcement on the grounds that it violated public policy, with few exceptions	Sharply limited the use of the “contrary to public policy” defense against the enforcement of non-competes
Consideration of individual economic hardship	Not specified	Not allowed to consider an employee’s individual hardship	
An interpretation favoring business protection	Not specified	Required to construe covenants “in favor of providing reasonable protection to all legitimate business interests established by the person seeking enforcement”	Not allowed to construe the covenant narrowly against the drafter or against enforcement
Enforcement despite the discontinuation of business	Not specified	An employee has to prove that the discontinuation had nothing to do with his or her work for the competitor	
Award of attorney’s fees	Not specified	Allowed for the awarding of attorney’s fees and costs to the prevailing party	Imposed asymmetric burden to an employee

Table 2 Descriptive Statistics and Correlations

Variable	Mean	Std. dev	Min	Max	1	2	3	4	5	6	7	8	9	10
<i>MSA-FSIZE-YEAR Level</i>														
1 Establishment Entry	123.7	867.7	0.0	43,299.0	1.00	0.95	0.65	0.25	0.42	0.25	0.02	0.01	-0.16	0.01
2 Establishments (Total)	1,031.8	4,910.7	2.0	230,333.0	0.95	1.00	0.75	0.41	0.56	0.39	0.01	0.01	-0.19	0.01
3 Job Creation by Incoming Firms	1,108.4	3,494.4	0.0	99,261.0	0.65	0.75	1.00	0.85	0.94	0.86	0.03	0.03	-0.04	0.03
4 Job Creation by Continuing Firms	1,956.2	5,707.1	0.0	193,425.0	0.25	0.41	0.85	1.00	0.98	0.97	0.01	0.02	0.04	0.02
5 Job Creation (Total)	3,064.6	8,874.3	0.0	291,020.0	0.42	0.56	0.94	0.98	1.00	0.97	0.02	0.03	0.01	0.03
6 Employment	18,484.3	54,145.2	7.0	1,673,631.0	0.25	0.39	0.86	0.97	0.97	1.00	0.00	0.02	0.09	0.02
7 Florida (Indicator)	0.1	0.2	0.0	1.0	0.02	0.01	0.03	0.01	0.02	0.00	1.00	0.00	0.00	0.00
8 Post 1996 (Indicator)	0.5	0.5	0.0	1.0	0.01	0.01	0.03	0.02	0.03	0.02	0.00	1.00	0.00	0.93
9 Firm Size (Categorical)	6.5	3.5	1.0	12.0	-0.16	-0.19	-0.04	0.04	0.01	0.09	0.00	0.00	1.00	0.00
10 Year	1,996	2.2	1,993.0	1,999.0	0.01	0.01	0.03	0.02	0.03	0.02	0.00	0.93	0.00	1.00
<i>MSA-YEAR Level</i>														
1 Small Firms' (<50 employees) Share of Establishments (%)	11.0	0.6	8.8	14.2	1.00	-0.29	0.66	-0.12	-0.05					
2 Large Firms' (>1,000 employees) Share of Establishments (%)	1.8	0.4	0.8	3.1	-0.29	1.00	-0.46	0.70	0.51					
3 Small Firms' (<50 employees) Share of Employment (%)	4.6	0.9	2.7	8.9	0.66	-0.46	1.00	-0.65	-0.43					
4 Large Firms' (>1,000 employees) Share of Employment	5.9	1.2	2.0	10.3	-0.12	0.70	-0.65	1.00	0.78					
5 Pseudo HHI	19.6	12.0	2.1	98.2	-0.05	0.51	-0.43	0.78	1.00					

Table 3 Effects of Non-competes on Establishments and Employment of Florida Firms by Size:
Split Samples

	<i>Dependent variables:</i>			
	Establishment Entry (1)	Establishment (2)	Job Creation (3)	Employment (4)
<i>A. Split Sample: Small Firms (#Employees<50)</i>				
Post×FL	-0.0562*** (0.0101)	-0.0033 (0.0062)	-0.0183 (0.0074)	-0.0048 (0.0060)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	7,488	7,488	7,488	7,488
<i>B. Split Sample: Large Firms (#Employees>1,000)</i>				
Post×FL	0.0849*** (0.0154)	0.0981*** (0.0073)	0.0760*** (0.0187)	0.1468*** (0.0121)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	7,488	7,488	7,488	7,488

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level

Data: Business Dynamics Statistics (BDS), 1993-1999. Small (Panel A) and Large (Panel B) firm split samples.

Table 4 Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Interaction

	<i>Dependent variables:</i>			
	Establishment Entry (1)	Establishment (2)	Job Creation (3)	Employment (4)
Post×FL	-0.0541*** (0.0105)	-0.0011 (0.0060)	-0.0273*** (0.0073)	-0.0047 (0.0058)
Post×FL×Size M (20-249)	0.0372*** (0.0131)	-0.0014 (0.0039)	0.0241*** (0.0084)	-0.0018 (0.0039)
Post×FL×Size L (250-2,500)	0.1526*** (0.0140)	0.0397*** (0.0086)	0.2357*** (0.0210)	0.1277*** (0.0095)
Post×FL×Size XL (2,500+)	0.1236*** (0.0181)	0.1079*** (0.0066)	0.0832*** (0.0200)	0.1580*** (0.0123)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	22,464	22,464	22,464	22,464

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level

Data: Business Dynamics Statistics (BDS), 1993-1999. Full sample.

Table 5 Effects of Non-competes on Regional Business Concentration

	<i>Dependent variables: Business Concentration</i>		
	Establishment concentration (1)	Employment concentration (2)	Pseudo HHI (3)
Post×FL	0.0244*** (0.0051)	0.0467*** (0.0055)	0.1739*** (0.0273)
MSA F.E.	Y	Y	Y
Year F.E.	Y	Y	Y
Observations	1,872	1,872	1,872

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level.

Data: Business Dynamics Statistics (BDS), 1993-1999.

Log-Linear regression with full sample. We measure “establishment (or business-unit) concentration” as the share of establishments by Large firms and “employment concentration” as the share of employment by Large firms. Large firms are defined as firms that have more than 1,000 employees.

Table 6 Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Split Samples (Border & Matching)

	<i>Dependent variables:</i>							
	Establishment Entry		Establishment		Job Creation		Employment	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
<i>A. Split Sample: Small Firms (#Employees<50)</i>								
Post×FL	-0.0506*** (0.0178)	-0.0022 (0.0169)	-0.0161 (0.0106)	-0.0058 (0.0139)	-0.0315*** (0.0114)	-0.0341 (0.0519)	-0.0180* (0.0109)	-0.0287** (0.0159)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,248	168	1,248	168	1,248	168	1,248	168
<i>B. Split Sample: Large Firms (#Employees>1,000)</i>								
Post×FL	0.1368*** (0.0328)	0.2439*** (0.0781)	0.1168*** (0.0188)	0.1622*** (0.0263)	0.0847** (0.0376)	0.2658 (0.2007)	0.169*** (0.0400)	0.0969*** (0.0445)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,248	168	1,248	168	1,248	168	1,248	168

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level

Data: Business Dynamics Statistics (BDS), 1993-1999. Small (Panel A) and Large (Panel B) firm split samples. Only borderline MSAs are included in columns (2), (4), (6), and (8).

Table 7 Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Interaction (Border & Matching)

	<i>Dependent variables:</i>							
	Establishment Entry		Establishment		Job Creation		Employment	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
Post×FL	-0.0396 (0.0171)	-0.0292 (0.0340)	-0.0131 (0.0103)	-0.0082 (0.0202)	-0.0328 (0.0119)	-0.0437*** (0.0202)	-0.0166 (0.0097)	-0.0209*** (0.0035)
Post×FL×Size (20-249)	0.0561 (0.0369)	0.0611*** (0.0095)	0.0040 (0.0103)	0.0181 (0.0555)	0.0345*** (0.0139)	0.0188** (0.0099)	-0.0016 (0.0104)	-0.0434*** (0.0161)
Post×FL×Size (250-2,500)	0.1521*** (0.0383)	0.0672 (0.0417)	0.0325*** (0.0137)	0.1450*** (0.0296)	0.1638*** (0.0413)	-0.0926 (0.3020)	0.1181*** (0.0295)	-0.1268 (0.0890)
Post×FL×Size (2,500+)	0.1669*** (0.0278)	0.3211** (0.1835)	0.1355*** (0.0136)	0.1169*** (0.0079)	0.1091*** (0.0349)	0.4576*** (0.0638)	0.1911*** (0.0334)	0.2645*** (0.0691)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,744	504	3,744	504	3,744	504	3,744	504

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level

Data: Business Dynamics Statistics (BDS), 1993-1999.

Only borderline MSAs are included in columns (2), (4), (6), and (8).

Table 8 Effects of Non-competes on Regional Business Concentration (Matching)

	<i>Dependent variables: Business Concentration</i>		
	Establishment concentration (1)	Employment concentration (2)	Pseudo-HHI (3)
	Post×FL	0.0361*** (0.0072)	0.0530*** (0.0145)
MSA F.E.	Y	Y	Y
Year F.E.	Y	Y	Y
Observations	276	276	276

*p<0.1; **<0.05; ***p<0.01

Robust standard error, clustered at the state level

Data: Business Dynamics Statistics (BDS), 1993-1999.

Log-Linear regression with full sample. We measure “establishment (or business-unit) concentration” as the share of establishments by Large firms and “employment concentration” as the share of employment by Large firms. Large firms are defined as firms that have more than 1,000 employees.

Endnotes

¹ Garmaise (2009) selected a random sample of 500 firms from the Execcomp database (1992-2004). This is only a lower bound because firms are not required to disclose this information.

² Amazon removed non-competes after intense media coverage and controversy in 2015 (Business Insider, 2015).

³ Please see the Online Appendix, Sections A and B, for full text of §542.33B and §542.335 and further discussion on why the post-1996 legal regime offers much more leniency to employers seeking non-compete enforcement.

⁴ The literature provides varying definitions of “market concentration” or “industry concentration”. In some cases, researchers use market concentration to refer to product sales concentration, and define industry concentration by firm within SIC or NAICS categories. To avoid confusion, we use the term “(regional) business concentration” that consists of the following three measures: “establishment (or business-unit) concentration” when looking at the share of establishments by large firms, “employment concentration” when looking at the share of employment by large firms, and Pseudo HHI (as defined in Section 5.3).

⁵ A variation of the window i.e., \pm two, three, or five years does not qualitatively change the result.

⁶ This approach is based on our understanding that the effects do not change linearly as a function of firm size (as in Figure 2). We ran the linear interaction approach nonetheless and found consistent results. See Online Appendix for a more detailed description and results.

⁷ The results are robust to the inclusion of 1996 as treatment year.

⁸ Figures on job creation are provided in Online Appendix, Section D.

⁹ Note that it is generally believed that pro-big business policies are most likely to be adopted by Republicans.

¹⁰ One caveat for generalizing their finding is their use of cross-sectional variations in the non-compete enforceability to compare high vs. low tech and high vs. low wage workers. In other words, their results are correlations between non-compete enforceability and wages of high-tech [high-wage] workers, relative to non-high-tech [non-high-wage] workers. The current study does not exclusively focus on high-tech or high-wage workers.

¹¹ Unfortunately, the BDS data does not provide wage information. We obtain wage data from the Quarterly Census of Employment and Wages where total quarterly wages and the number of establishments are provided by nine “establishment” size categories.