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Non-competes, business dynamism, and concentration: Evidence from a Florida case study

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Abstract

Most research on non-competes has focused on employees; here we study how non-competes affect firm location choice, growth, and consequent regional concentration, using Florida's 1996 legislative change that eased restrictions on their enforcement. 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, regional business concentration increased following the law change in Florida. Nationwide cross-sections demonstrate consistent correlations between state-level non-compete enforcement and the location, employment, and concentration dynamics illustrated in Florida.

KEYWORDS

business concentration, business dynamism, employee mobility, entrepreneurship, firm sorting, non-compete agreement

JEL CLASSIFICATION

J61; L22; L26; M13; M51

1 | INTRODUCTION

Most research on non-competes to date has focused on how such laws impact employees (Garmaise, 2009; Marx, Strumsky, & Fleming, 2009; Starr, 2019), patenting (Conti, 2014), and entrepreneurship (Samila & Sorenson, 2011; Starr, Balasubramanian, & Sakakibara, 2017). Less research has considered how non-competes impact firms' decisions, how those decisions might vary by the type of firm, and the ultimate impact of those decisions on industries and regions. Here we document how one state's change in non-compete laws influenced firms' strategic choices and preceded change in the competitive dynamics and industry concentration in that state.

Recent work has documented trends of increasing industry concentration, possibly due to scale and network effects (Shambaugh, Nunn, Breitwieser, & Liu, 2018), deregulation (De Loecker, Eeckhout, & 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 United States, including a flat trend in firm exit and declining trends in firm entry and job reallocation (Hathaway & Litan, 2014) and a decrease in

entrepreneurship (Haltiwanger, Jarmin, & Miranda, 2013). Hathaway and Litan (2014) comment that, “Whatever the reason, older and larger businesses are doing relatively better to younger and smaller ones.” A White House (2016) policy brief 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, also reveal positive relationships between enforcement of 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 et al., 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 et al., 2009). Florida’s experience appears internally consistent and provides an example of 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 illustrates how non-compete laws can alter business dynamism and the regional size distribution of firms. The law change appears to have favored 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’ non-compete enforcement and shares of establishment entry, employment growth, and business concentration.

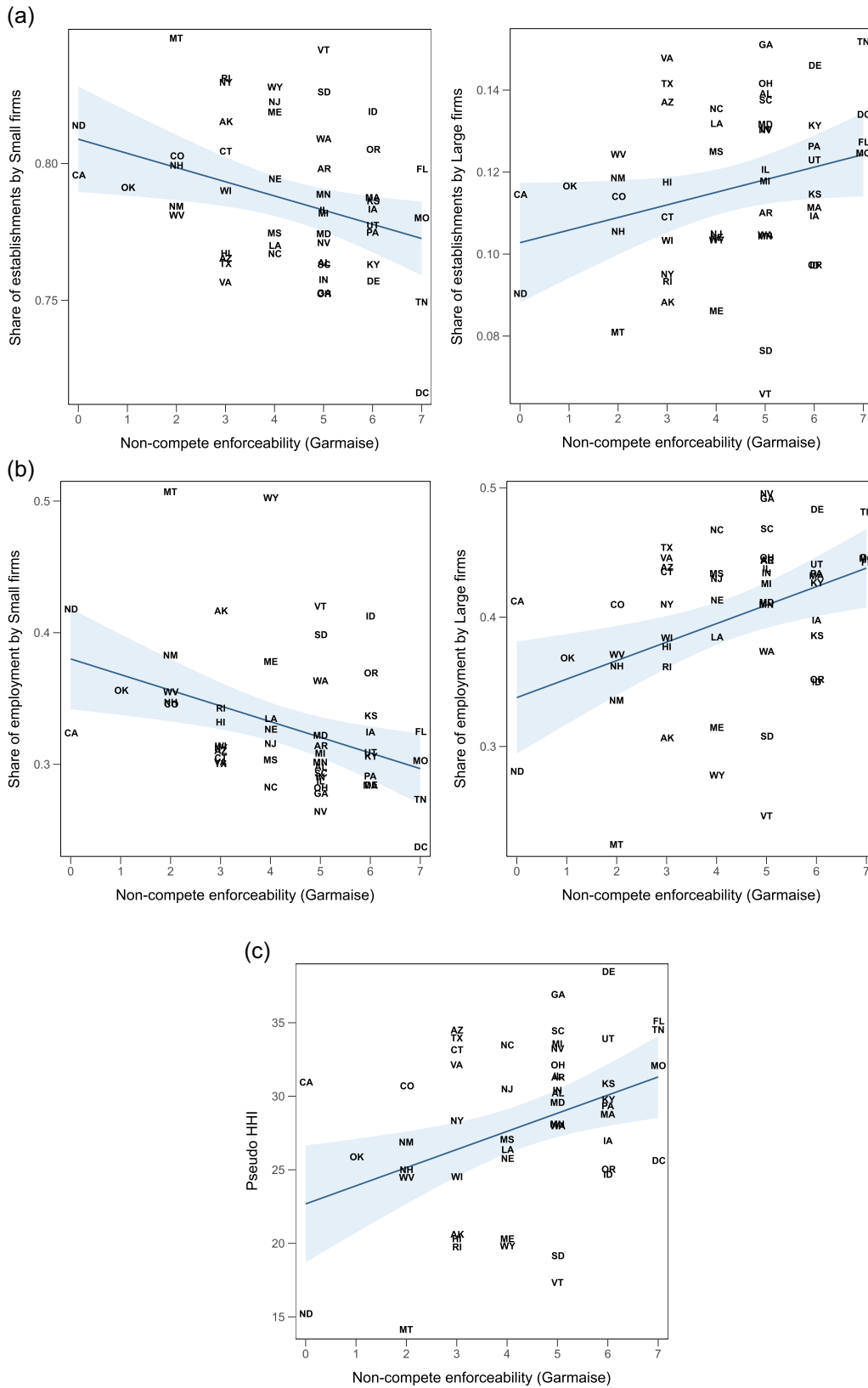
2 | EMPLOYEE NON-COMPETES

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 (Irwin 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, 2019); 351 of 500 U.S. firms (70.2%) reported non-compete agreements for their top executives (Garmaise, 2009).¹ Amazon requires their 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 (Woodman, 2015).”² Physicians, dentists, accountants, and even lawyers can be subject to non-competes (Tanick & Trobaugh, 2012).

Non-competes have developed in part because employers typically prefer labor contracts that aid in the retention of desirable employees. Such contracts intend to mitigate the market failure of under-investment in employee training and research activities (Samila & 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 commit that they will not use the training and information for the benefit of a competitors.

Empirical work has established a variety of relationships with non-compete enforcement, though little has focused on how non-competes affect existing firms. Stuart and Sorenson (2003) established that greater entrepreneurship



followed IPOs in regions that lacked enforcement. 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 et al., 2009), career detours (Marx, 2011), and inter-state brain drain of inventors (Marx, Singh, & Fleming, 2015). Using 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 a correlation in breakthrough and failed inventions in states that enforced non-competes, arguably due to greater risk-taking by firms that were less afraid of losing their technical personnel. Starr et al. (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, Chang, Sakakibara, Sivadasan, and Starr (2019) 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 existing firms of different sizes and in particular, their location decisions and rates of job creation, and ultimately, on the distribution of firm sizes. 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 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-compete enforcement as a case study

Florida's 1996 strengthening of non-compete enforcement offers an attractive case study, in contrast to law changes in other states (see Supporting Information Appendix D for a list of states that have changed their non-compete laws and a discussion of their suitability and comparability to Florida). Florida provides a close to ideal site because (a) the legislation focused purely on restrictive covenants, notably non-competes, (b) it was clearly intended to strengthen enforcement in the state, and (c) Florida had a four decade history with the laws governing non-competes, such that employers and employees were probably familiar with and accustomed to non-competes.

Additional features of the amendment support its use as a quasi-natural experiment. First, it explicitly stated and thereby clarified which rule governed a contract and stipulated a clear break on July 1, 1996. Second, 1996 amendments to earlier statutes (please see Table A1 in Appendix) illustrate significant strengthening of the employer's enforceability of non-compete covenants. The number of words almost tripled, from 455 in §542.33A to 1,211 in §542.33B, in the direction of strengthening employers' enforcement, and courts could no longer refuse non-compete enforcement on the grounds of employee economic hardship or public policy concerns. Third, legal commentary construed these changes as favoring business, for example, the 1996 amendment "... has once again swung the pendulum representing the enforceability of noncompetition agreements more in favor of employers (FindLaw, 2008)."³

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

Despite a growing literature on non-competes, most of the work to date has focused on individuals and their ability to pursue outside opportunities. Less work has investigated whether enforcement favors certain types of firms over others and, in particular, how non-competes might impact firm location and employment, which might in turn influence business

FIGURE 1 The 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-compete enforceability, including an intercept. The shaded area represents the 95% confidence interval. Results for regressions: (a) left panel: coefficient, -0.0052 ; standard error, 0.0021 ; p value, $.0162$; right panel: coefficient, 0.0031 ; standard error, 0.0016 ; p value, $.0570$; (b) left panel: coefficient, -0.0120 ; standard error, 0.0041 ; p value $.0057$; right panel: coefficient, 0.0143 ; standard error, 0.0047 ; p value $.0036$; (c) coefficient, 1.233 ; standard error, 0.4289 ; p value, $.0060$. Small firms: <50 employees. Large firms: $>1,000$ employees. To construct the index of non-compete enforceability, Garmaise (2009) examined 12 questions on the state-level enforceability of non-competes surveyed by Malsberger (2004). Garmaise assigned one point to each question if the state's enforcement of that dimension of non-competes exceeded a given threshold. The index ranges from 0 to 12. Data: Business Dynamics Statistics, 1996. [Color figure can be viewed at wileyonlinelibrary.com]

concentration, if there were different effects on small versus large firms. We consider the differential effects of non-competes by firm size on location (at birth or in movement of extant establishments) and employment choice of firms and regional business concentration. We discuss how the law change in Florida might cause a (a) shift in the distribution of businesses by firm size, (b) shift in the sources of new job creation and employment by firm size, and (c) change in regional business concentration. We discuss possible 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 & Sorenson, 2003). Entrepreneurial companies in particular need to hire already capable and experienced workers because (a) they do not have the resources or time to invest in employee training, and (b) 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 for a competitor). Hiring unemployed workers remains unattractive because they are generally less experienced than active employees; furthermore, an unemployed worker can still be bound by a non-compete. Startups also may not value as highly the legal strategies enabled by non-competes. Since startups by construction cover narrower businesses and geographies, an employee departing a startup will have a wider range of employment opportunities that do not include competitors. This wider range will make it more likely that an employee can leave for a company that is not an obvious competitor and hence not covered by non-competes. Add to this the greater likelihood that a startup will lack the resources to pursue legal action against former employees, and a startup would likely 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 probably 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 et al., 2017). Empirically, if startups find strong non-competes attractive, we would expect to find an increase in the number of small firms and their establishments, 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 multiply 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. Bigger 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 large and mature 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 such firms 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 can serve as “holding tanks” (Marx & 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 might (re-)locate their establishments to municipalities that offer a preferred business environment, essentially shopping for advantageous policies. As described above, large firms might prefer strong non-compete regions and hence open new establishments in Florida or move extant establishments to Florida, following the amendment. The advantages to entrepreneurial firms, on the other hand, remain mixed (and it is very possible that there is no monotonic relationship between firm size and location preference—we leave it as an empirical question). 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 their establishments, following a shift to stronger 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 (e.g., assuming that all firms want to hire and grow), if it becomes easier (harder) for larger (smaller) firms to hire new workers, we would expect to observe a shift in the distribution of sources of new jobs and employment by firm size, following the 1996 law change. Since we do not observe whether individual firms attempt to grow or employee's preferences or responses to employment offers, we will consider how non-competes could make it more or less difficult for different types of firms—small versus large—to hire.

Regional mobility of workers decreases with stronger enforcement (Balasubramanian et al., 2019; Marx et al., 2009), 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, 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; in this situation, 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 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 pursue a departing employee and 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 not yet developed firm-specific proprietary knowledge, they also might be less likely to prosecute a non-compete, making them more attractive to employees (and thus making it 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 and small firms (difficulty in attracting risk-averse talent, inability to offer competitive compensation, and weaker legal resources in non-compete litigation). Large, established firms will probably 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, that is 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 (Starr, 2019). 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 toward 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 (re)location, and endogenous growth. We will not repeat the mechanisms detailed above, and here focus on the impact of those mechanisms on regional business concentration.⁴

With regards the 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. On the other hand, if larger (and assumedly incumbent) firms are attracted, they will move to or open more establishments in the region, which will increase the density of large firms there (at least on the margin). With regards job creation and employment growth, any differential impact will be observable in the sources of jobs; if startups and small firms are advantaged, they will exhibit an increase in job creation and employment, likewise, 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, this should be observable in an overall increase in regional business concentration.

4 | EMPIRICAL DESIGN

4.1 | Data and sample

We use the Business Dynamics Statistics (BDS) provided by the U.S. Census Bureau for our main analysis. This data covers almost the universe of firms and their business units ("establishments") in the U.S. 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 12 firm size categories: 1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1,000–2,499, 2,500–4,999, 5,000–9,999 employees, and 10,000 or more employees.

One limitation is that the data are not available at the MSA-Industry-Firm Size-Year level; in other words, we cannot run industry-specific analyses. To ameliorate this restriction, in Section 7.1, we use industry information from a separate data source, the Quarterly Census of Employment and Wages (QCEW), 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 pair MSAs in Florida and other comparison states based on their industry composition and calculate the distance in industry composition as the squared sum of differences in employment share by 5-digit NAICS industries. For each MSA in Florida, we select and pair ten MSAs in comparison states that have the most similar industry composition (i.e., the lowest distance score). Table 1 provides descriptive statistics and a correlation table. There is little evidence of high correlations across variables.

4.2 | Difference-in-differences model

We mainly estimate difference-in-differences (DiD) models. 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. In other words, we assume that MSAs in our treatment state (Florida) and control states (non-Florida) would 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

TABLE 1 Descriptive statistics and correlations

Variable	Mean	SD	Min	Max	1	2	3	4	5	6	7	8	9	10
<i>MSA-FSIZ-EYEAR Level</i>														
1 Establishment Entry	123.7	867.7	0.0	43,298.0	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	4,911.0	2.0	230,308.0	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	1,107.8	3,494.7	0.0	99,242.0	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	1,955.5	5,706.9	0.0	193,565.0	0.25	0.41	0.85	1.00	0.98	0.97	0.01	0.02	0.04	0.02
5 Job Creation (Total)	3,063.3	8,874.8	0.0	291,160.0	0.42	0.56	0.94	0.98	1.00	0.97	0.02	0.03	0.01	0.03
6 Employment	18,483.3	54,145.9	7.0	1,673,859.0	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 (%)	77.1	3.2	67.3	88.3	1.00	-0.88	0.72	-0.55	-0.39					
2 Large Firms' (>1,000 employees) Share of Establishments (%)	12.7	2.4	5.8	21.2	-0.88	1.00	-0.64	0.60	0.53					
3 Small Firms' (<50 employees) Share of Employment (%)	32.2	6.3	19.4	59.2	0.72	-0.64	1.00	-0.84	-0.61					
4 Large Firms' (>1,000 employees) Share of Employment (%)	41.4	7.9	15.2	66.7	-0.55	0.65	-0.84	1.00	0.80					
5 Pseudo HHI	29.7	8.1	12.1	66.8	-0.39	0.53	-0.61	0.80	1.00					

group, as it is widely accepted that they are quite different from other states in economic and geographic characteristics (results remain robust to the inclusion of MSAs in these states). To further minimize the possibility of unobservable, confounding variables, we provide two robustness checks focusing exclusively on treated (Florida) and control (non-Florida) MSAs that (a) have very similar industry composition and (b) 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,” the first four size categories in the BDS data) and another for firms with more than 1,000 employees (“Large,” the last four size categories in the BDS data, “Large”). We then run separate log-linear regressions in Equation (1) for the split samples for 1993–1999 (± 3 years from the year of the amendment)⁵:

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

where Y_{it} is an outcome of interest, 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—that is stronger enforcement of non-competes—and the parameter of interest is τ .

The difference-in-differences estimation in Equation (1) forces estimates to be the same within pre- or posttreatment years. In Supporting Information Appendix G, we run a more flexible econometric model with distributed leads and lags (“event study regression techniques”). We interact the treatment indicator with year indicators, rather than uniformly assigning zero and unity for all pre- and posttreatment years.

An alternate approach compares the effects by firm size in the same model. Based on the 12 firm size categories provided in the BDS data, we created four dummy variables for firm size by collapsing the 12 categories into four: *SizeS* (1–19), *SizeM* (20–249), *SizeL* (250–2,500), and *SizeXL* (more than 2,500 employees).⁶ We then run the difference-in-differences estimation in Equation (2) for the period ranging from 1993 to 1999 with full sample.

$$\log Y_{ist} = \pi^M \cdot Post_t \cdot FL_i \cdot SizeM_s + \pi^L \cdot Post_t \cdot FL_i \cdot SizeL_s + \pi^{XL} \cdot Post_t \cdot FL_i \cdot SizeXL_s + \alpha_i + \delta_t + X'_{it} \cdot \beta + \epsilon_{ist}, \quad (2)$$

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_s*, *SizeL_s*, and *SizeXL_s*). Note that FL_i and $Post_t$ variables are absorbed by the MSA and year fixed effects. *Size S* is the omitted baseline and the parameters of interest are π^M , π^L , and π^{XL} .

The data are yearly, and since the new law applied to the contracts written on and after July 1, 1996, the inclusion of 1996 in the sample might bias the estimates. In addition, since the amendment was introduced by the Florida legislature, it is possible that employers and employees anticipated the change ex ante and adjusted their behavior before the effective date, July 1, 1996 (Barnett & Sichelman, 2016). We, therefore, exclude 6 months before and after the effective date and run the regressions in Equations (1, 3) and supplementary Equation (2) for 1993–1999, leaving out the year of amendment, 1996.⁷

5 | RESULTS

5.1 | Business size and location preferences

Figure 2 compares the densities of a set outcomes in Florida by firm size 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) of small firms (including small single-unit firms) decreased in 1997, whereas that of large firms increased. As might be expected due to the large number of establishments that do not move, differences in density lines are less discernible 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 the increase from large firms, following the amendment.

Figure 3 compares Florida to 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 & Gardeazabal, 2003; Abadie, Diamond, &

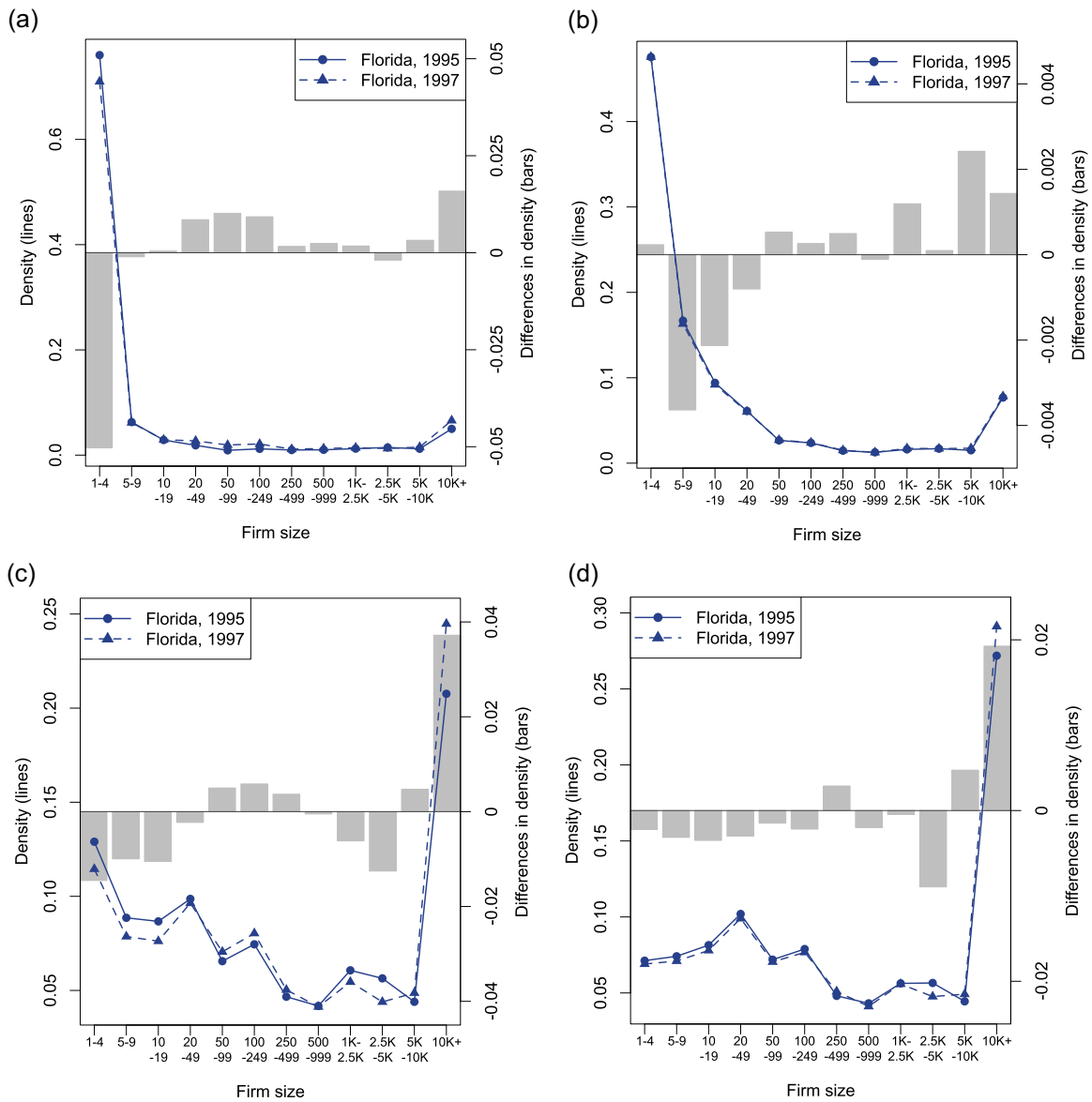


FIGURE 2 Density of establishments and employment of Florida firms by size: 1995 vs. 1997. (a) Establishment entry, (b) establishments, (c) job creation, (d) employment. The solid and dashed lines represent the distribution (density) of each outcome by firm size in Florida in 1995 and 1997, respectively. Gray bars represent the difference in density between 1995 and 1997. *Source:* Business Dynamics Statistics, 1995 and 1997. [Color figure can be viewed at wileyonlinelibrary.com]

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 outcomes of interest in 1991, 1992, 1993, 1994, and 1995 after normalizing values relative to the 1994 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. To construct a single, representative weight used for all our analyses with four different outcomes of interest, we calculate the optimal weights for each of four outcomes of interest and then take the arithmetic mean across the four weights for each state. With a single weight, we could construct a parallel trend for Florida and its synthetic control for pretreatment periods in all four graphs in Figure 3. Individual weights and analyses returned similar results.⁸

Since we study differential effects by firm size, we split the sample and plot the result by small versus large firms. In Figure 3, the red solid line represents Florida, while the brown dashed line represents the counterfactual synthetic Florida.

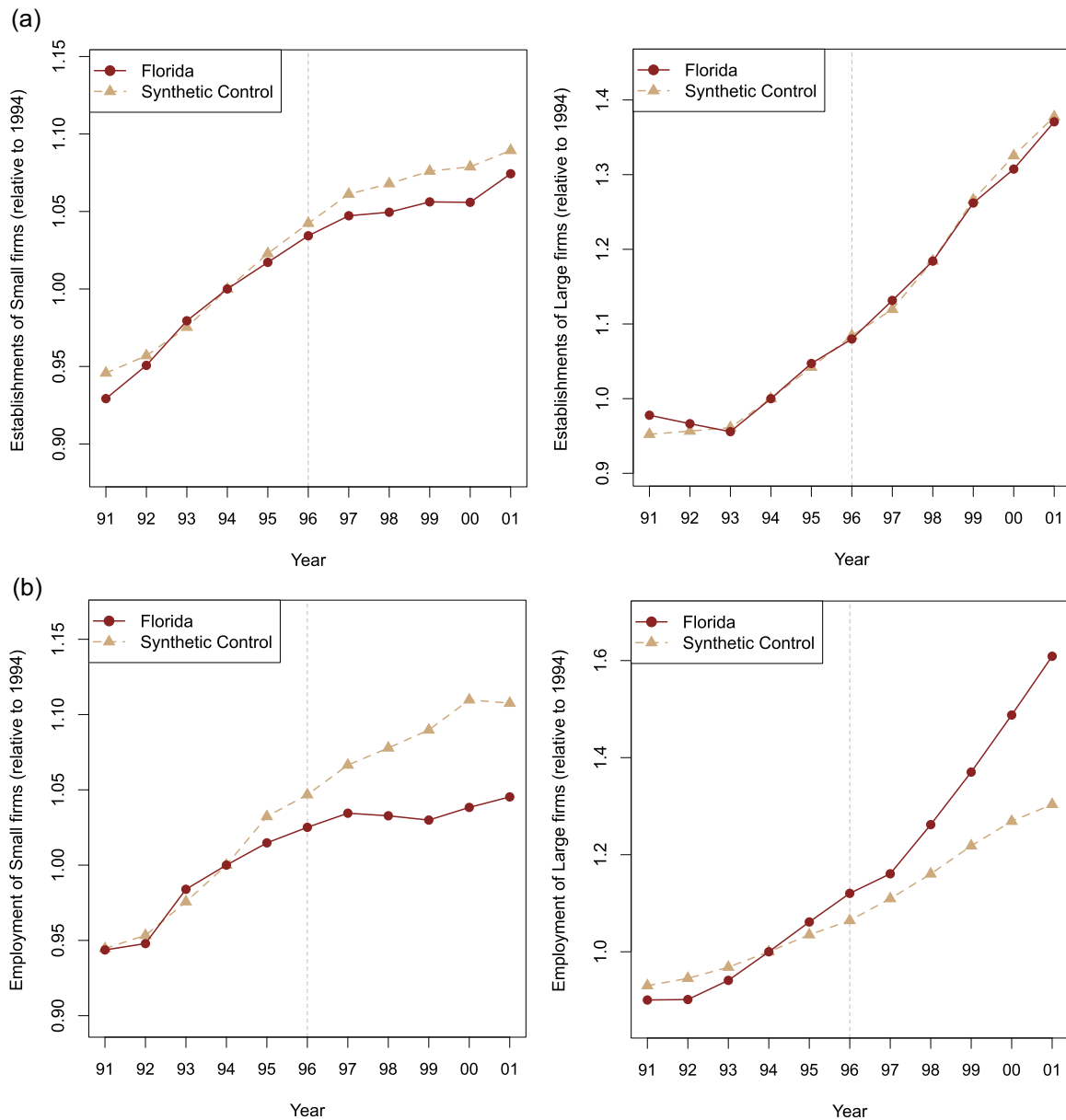


FIGURE 3 Synthetic control method: establishments and employment by firm size. (a) Establishments, small firms (<50), Large firms (>1,000), (b) employment, small firms (<50), Large firms (>1,000). The outcome variables for Florida are normalized relative to their 1994 value. Data: Business Dynamics Statistics, 1991–2001. [Color figure can be viewed at wileyonlinelibrary.com]

We find in Panel (a) that the number of establishments of small firms in Florida becomes significantly lower than that in synthetic control or counterfactual Florida, beginning from 1996. In contrast, the number of establishments of Large firms shows the opposite trend in the short run: it becomes higher than counterfactual Florida in the following few years, although the long-term trend seems less clear. We generally find the opposite outcomes for establishments by small versus large firms. Supporting Information Appendix E presents consistent results from a set of placebo treatments under the null hypothesis that the law change had no effect and Appendix K documents additional split sample analyses that align pretreatment years.

Table 2 presents results from formal difference-in-differences models. Equation (1) estimates a split sample model. As hypothesized, for 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. Establishment entry by small firms decreases by 5.6%, whereas that of Large firms increased by 8.5%. The number of establishments in Column (2) shows a similar pattern though the estimate from small sample is imprecisely estimated.

Table 3 shows the results from alternate models (with full sample) where we interact indicators for the four collapsed firm size categories with $FL \times Post$. For the establishment entry in Column (1), we consistently find that the

TABLE 2 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)
A. Split sample: Small firms (#Employees < 50)				
FL × Post	-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
B. Split sample: Large firms (#Employees > 1,000)				
FL × Post	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

Note: the two panels show the results from small (panel A) and large (panel B) firm split samples. Robust standard errors, clustered at the state level, are presented in parentheses.

* $p < .01$.

Source: Business Dynamics Statistics, 1993–1999.

estimates are positive and large for bigger firms. Entry of establishments of firms with 20–249 workers (*SizeM*) is 3.7% larger than that of firms with 1–19 workers (*SizeS*). Entry of establishments of firms with 250–2,500 (*SizeL*) and more than 2,500 workers (*SizeXL*) is 15.3% and 12.4% larger than that of firms with 1–19 workers (*SizeS*), respectively. Column (2) illustrates consistent results for the total number of establishments. The number of establishments of firms with 250–2,500 (*SizeL*) and more than 2,500 workers (*SizeXL*) increased by 4% and 11% compared to that of firms with 1–19 workers (*SizeS*).

This approach estimates the effects for larger firms relative to the smallest firm size category, *Size*. To estimate the effects more generally, we estimate separately for each firm size category with regression models in Equation (1). The results for the

TABLE 3 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)
FL × Post	-0.0541* (0.0105)	-0.0011 (0.0060)	-0.0273* (0.0073)	-0.0047 (0.0058)
FL × Post × Size M (20–249)	0.0372* (0.0131)	-0.0014 (0.0039)	0.0241* (0.0084)	-0.0018 (0.0039)
FL × Post × Size L (250–2,500)	0.1526* (0.0140)	0.0397* (0.0086)	0.2357* (0.0210)	0.1277* (0.0095)
FL × Post × 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

Note: This table shows the results from full sample. Robust standard errors, clustered at the state level, are presented in parentheses.

* $p < .01$.

Source: Business Dynamics Statistics, 1993–1999.

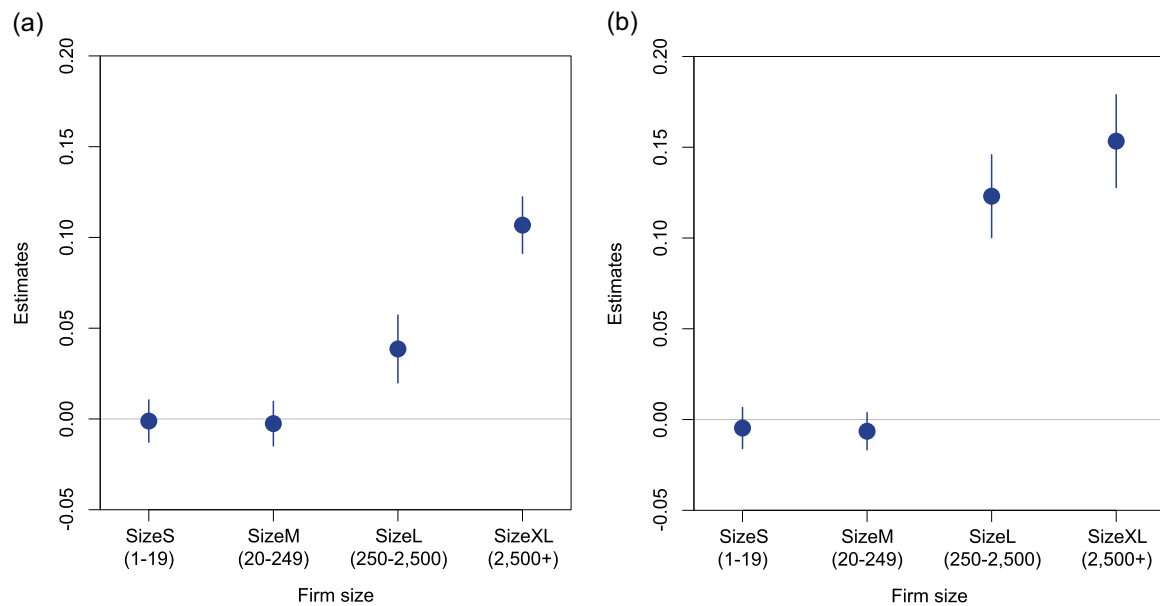


FIGURE 4 Establishments and employment of Florida firms by size: split-sample regressions. Each point stands for an estimate ($Post \times FL$) from separate regressions by firm size category. The vertical lines stand for 95% confidence interval based on standard errors clustered at the state level. *Source:* Business Dynamics Statistics, 1993–1999. [Color figure can be viewed at wileyonlinelibrary.com]

number of establishments by firm size are summarized in Panel (a) of Figure 4, where each dot represents an estimate for $FL \times Post$ from four separate regressions for each firm size category: *SizeS*, *SizeM*, *SizeL*, and *SizeXL*. This result suggests that the effects primarily come from responses by large firms, as their magnitude of effects are much larger and more precisely estimated. Large firms prefer to (re)locate in regions that strongly enforce non-competes.

Small firm (less than 19 employees) entry appears to weaken after 1996. Table 2, column 1, shows a 5.6% decrease in the entry of small firms or their establishments. An alternative, full-sample specification with the four firm size categories, shown in Table 3, produces the similar result that the entry of establishments with less than 20 workers (*SizeM*) decreased by 5.4%. Appendix F illustrates consistent results using more granular estimations with all 12 categories. If one accepts that firms with less than 19 employees are more likely to be entrepreneurial (see Starr, 2019), the change in non-compete law appears to have made Florida a more attractive location for large firms and a less attractive location for entrepreneurs.

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. Job creation by the four smallest (<50 employees) and two largest (>5,000) categories decreased and increased, respectively, while the results in the middle of the distribution are mixed. Employment in Panel (d) of Figure 2 shows a similar pattern. Employment in the six smallest categories consistently decreased, whereas that of the two largest categories significantly increased. Figure K.1 in Supporting Information Appendix K splits the data between small (<50 workers) and large firms (>1,000 workers) *within* Florida. In Panel (b), employment in small firms (dashed line) decreased, as opposed to that in large firms (solid line), following the 1996 amendment. Finally, an inter-state comparison with the Synthetic Control in Panel (b) of Figure 3 shows consistent results. Both figures in Panel (b) show a parallel trend for preamendment years, 1991–1995, satisfying the rationale behind this approach. 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. In contrast, increased employment by large firms is found in the right-hand side, relative to the synthetic control.

Table 2 estimates split sample models from Equation (1) and illustrates that Small firms decreased their job creation by 1.8%, whereas Large firms increased by 7.6%, though the estimate for the Small firm sample remains imprecise. The alternative specification with the full sample and four categories for size, shown in Column (3)–(4), Table 3, finds consistent results. Large firms that have more than 250 workers (*SizeL* and *SizeXL*) increased their job creation and employment by

8–24% and 13–16%, respectively, compared to small firms that have 1–19 workers (*SizeS*). The results from separate estimations for four firm size categories are summarized in Figure 4b. Each dot represents an estimate for *Post* × *FL*, and we again find that the effects primarily come from hiring expansions by large firms (rather than shrinking employment by small firms). More granular interactions in Supporting Information Appendix F find consistent results for smaller firms.

The change in non-compete law appears to have altered job creation and employment by small and large firms in opposite ways. The total number of jobs in Florida clearly increased after the amendment was instituted, and 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 appeared to prefer a region that enforces non-competes when they launch or relocate establishments; small firms appeared to be relatively 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, we can estimate changes in business concentration using the following three measures: (a) share of establishments that belong to large firms (“establishment concentration”), (b) share of workers that belong to large firms (“employment concentration”), and (c) a 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:

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

where $\frac{Min_s + Max_s}{2}$ is the representative firm size in each firm size category *s* (“weight”), and $\frac{Number\ of\ Employees_{ist}}{\sum_s Number\ of\ Employees_{ist}}$ is the share of employees in size category *s* in MSA *i* in year *t* (“share”). It then requires the square root of the sum over all 12 categories.⁹ This measure mimics calculations for the product market share-based HHI and captures the degree of business concentration at the MSA-year level.

Figure 5 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 (3) with the three different measures of business concentration. We do not take the logarithm on the dependent variable because the outcomes of interest are bound (shares ∈ [0,1], *PseudoHHI* ∈ [0,122.5]) and close to the Normal distribution:

$$Y_{it} = \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + \epsilon_{it}. \quad (3)$$

In our result in Column (1) in Table 4, we find that the establishment concentration increased by 0.0036 points or about 2.82% (the establishment concentration in Florida was on average 0.1278 before the 1996 law change). Column (2) shows the employment concentration. Consistent with our prediction, the results show an increase by 0.0209 points or 5.15% (the employment concentration in Florida was on average 0.406 before the 1996 law change). Column (3) again illustrates that business concentration measured by the Pseudo-HHI increases after stronger non-compete enforcement, by 1.45 points or 4.41% (the Pseudo-HHI in Florida was on average 32.90 before the 1996 law change). In Supporting Information Appendix G, we additionally run a more flexible econometric model with distributed leads and lags (“event study techniques”) and conduct robustness checks with industry paired sample and state-bordering MSAs, all of which produce similar results. In summary, the change in non-compete law appears to have preceded increased business concentration, arguably 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 remain vulnerable to other simultaneous and confounding events, particularly if there was a change that operated in the same direction as the

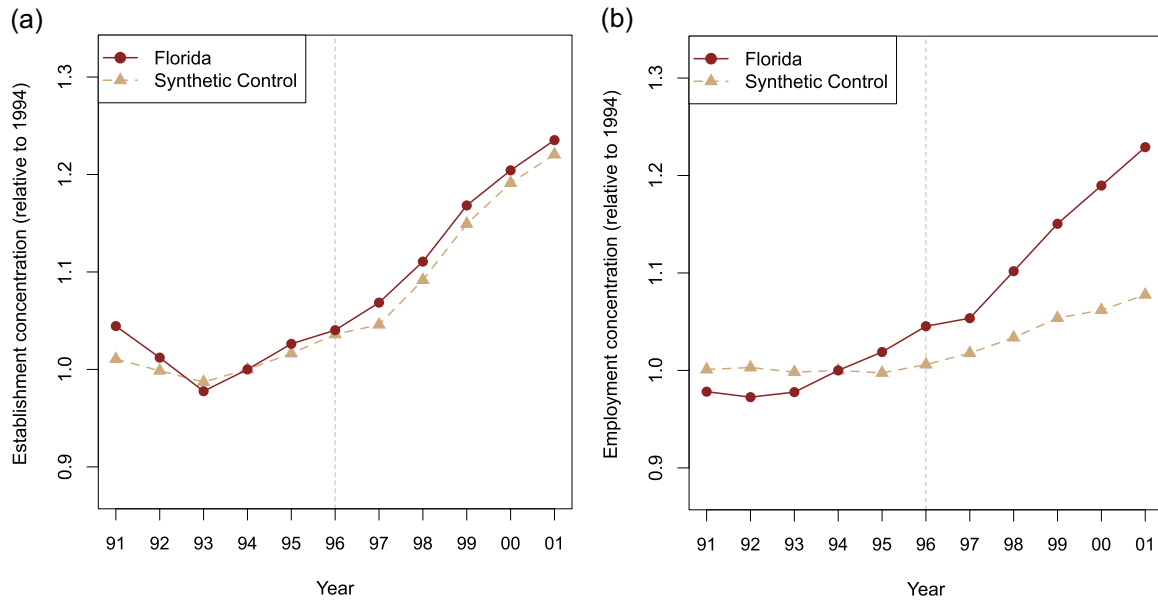


FIGURE 5 Synthetic Control method: business concentration. The outcome variables are normalized relative to their 1994 value. We measure “establishment (or business-unit) concentration” as the share of establishments by large firms (that have more than 1,000 employees) and “employment concentration” as the share of employment by Large firms. *Source:* Business Dynamics Statistics, 1991–2001. [Color figure can be viewed at wileyonlinelibrary.com]

non-compete amendment. While it is not possible to consider every event that happened in 1996, we discuss two potential threats to identification: Enterprise Florida, Inc. and electoral changes. Furthermore, in the Supporting Information Appendix C, we also establish that wage trends changed little before and after 1996. This eases concern that the law change impacted the economy through wage changes and that the effects of non-competes were 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,

TABLE 4 Effects of non-competes on regional business concentration

	Dependent variables		
	Establishment concentration (1)	Employment concentration (2)	Pseudo HHI (3)
FL × Post	0.0036* (0.0006)	0.0209* (0.0017)	1.4514* (0.2369)
MSA F.E.	Y	Y	Y
Year F.E.	Y	Y	Y
Observations	1,872	1,872	1,872

Note: This tables shows the results from linear regressions with full sample (dependent variables not logarithmized). We measure “establishment (or business-unit) concentration” as the share of establishments by Large firms ($\in[0,1]$) and “employment concentration” as the share of employment by Large firms ($\in[0,1]$). Large firms are defined as firms that have more than 1,000 employees. The maximum possible value of Pseudo-HHI is 122.5 (and the minimum is 0). Robust standard errors, clustered at the state level, are presented in parentheses.

* $p < .01$.

Source: Business Dynamics Statistics, 1993–1999.

“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 (a) could affect Florida businesses and (b) 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 H.1 in Supporting Information Appendix H reveals no noticeable change in Florida’s industry composition for 1991–2001, indicating minimal if any influence from EFI activities in the 1990s. 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 for pro-big business candidates, the findings might result from other 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 1992.¹⁰ 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 some firms over others 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 (2019) 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 control 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 provide information on county, MSA, and state-level industry composition. Figure H.1 in Supporting Information Appendix H shows Florida’s industry composition from 1991 to 2001. The idea is that, using industry information in the QCEW, we can control for confounding effects of industry composition by pairing MSAs of Florida with control states that share the similar industry composition. We then use these treatment-control MSA pairs in the BDS data and rerun the regressions.

We use employment in five-digit North American Industry Classification System (NAICS) sector (11111-99999) in each MSA to calculate the Euclidean distance between industry compositions of any two MSAs

$$\text{Industry Distance}_{A,B} = \sum_{\text{NAICS}(5\text{-digit})} (\text{Emp}_{A,\text{NAICS}} - \text{Emp}_{B,\text{NAICS}})^2,$$

where $\text{Emp}_{A,\text{NAICS}}$ and $\text{Emp}_{B,\text{NAICS}}$ are the employment by 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 (i.e., that have the lowest industry distance). We then run the same difference-in-differences estimation using the paired MSA data. Results provided in Tables 5 and 6 (odd-numbered columns) and Supporting Information Appendix Table G.1. in are qualitatively similar to our main findings, making it less likely that the results are driven by a discrepancy in industry composition between the treated and control MSAs.

TABLE 5 Effects of non-competes on establishments and employment of Florida firms by size: Split samples (border and matching)

	Dependent variables							
	Establishment entry		Establishment		Job creation		Employment	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
A. Split sample: Small firms (#Employees < 50)								
FL × Post	-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
B. Split sample: Large firms (#Employees > 1,000)								
FL × Post	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

Note: The two panels show the results from small (panel A) and large (panel B) firm with split samples. For each Florida MSA, we paired 10 non-Florida MSAs that have the most similar industry composition. The results are presented in columns (1), (3), (5), and (7). Only borderline MSAs are included in columns (2), (4), (6), and (8). Robust standard errors, clustered at the state level, are presented in parentheses.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

Source: Business Dynamics Statistics, 1993–1999.

TABLE 6 Effects of non-competes on establishments and employment of Florida firms by size: interaction (border and matching)

	Dependent variables							
	Establishment entry		Establishment		Job creation		Employment	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
FL × Post	-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)
FL × Post × Size M (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)
FL × Post × Size L (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)
FL × Post × Size XL (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

Note: This tables shows the results from log-linear regressions with full sample. For each Florida MSA, we paired 10 non-Florida MSAs that have the most similar industry composition. The results are presented in columns (1), (3), (5), and (7). Only borderline MSAs are included in columns (2), (4), (6), and (8). Robust standard errors, clustered at the state level, are presented in parentheses.

* $p < .05$.

** $p < .01$.

Source: Business Dynamics Statistics, 1993–1999.

7.2 | State-bordering MSAs

Unobservable characteristics might still differ between the treatment group (MSAs in Florida) and control group (MSAs in states other than Florida). To mitigate this concern, we restrict our sample to MSAs that adjoin the Florida border or that are located within 50 miles from the border. It is expected that the MSAs near the Florida borderline would share many unobservable characteristics, further ensuring 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 Supporting Information Appendix Figure I.1. 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, and so forth. The results of the formal regression, Equations (1) and supplementary Equation (2), are presented in Tables 5 and 6 (even-numbered columns). The results are not qualitatively different from those in Tables 2 and 3 (and industry matching results in odd-numbered columns in Tables 5 and 6), though with a much smaller number of observations, the estimates become less precise.

The magnitudes are generally larger in the models with only 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 particular firm (re)locates to Florida, *in direct response to* the 1996 Florida amendment. For example, it is much more likely that potential new entrants choose between Tallahassee MSA (Florida) versus Valdosta MSA (Georgia) than Tallahassee MSA (Florida) versus San Francisco MSA (California). The borderline sample captures this substitution effect to a greater extent than the full sample. In other words, a move between state-bordering MSAs will more likely lead to *double-counting* of the effect when a large firm moves into Florida and a small firm leaves, because a move of single establishment (or employee) from control MSA to treatment MSA is counted twice when we calculate the difference in the number of establishments (or employees) between the two groups.

This argument implies that our control MSAs from Alabama and Georgia borders are also affected by the 1996 Florida amendment. This magnified result for state-bordering MSAs increases our confidence that firms make (re) location and employment decisions in response to changes in non-compete enforceability. We find greater effects even if the two MSAs share most a similar business environment *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 (re)location of businesses or employment after the 1996 Florida amendment.

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 (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.

The search for additional research sites 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 different 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 completely flipped from one regime to the other, instead, most changes have been more subtle differences of degree and types of enforcement mechanisms. It should also be noted that the stronger versus weaker enforcement may not necessarily have symmetric effects. This calls into question the assumption that every state's change in non-competes—in either direction—can be used as *binary* and *opposite experiments*. Researchers cannot unfortunately bin all changes in non-compete laws into two discrete buckets and then estimate aggregated models.

Following our own recommendations then, we present Florida as a single case study. Florida's experience implies that enforcement of non-competes could crowd out small firms, favor large firms, and thereby increase regional

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 outcomes of interest. 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 versus 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 HHI. These relationships hold consistently for two indices of enforceability (Garmaise, 2009; Starr, 2019) and without the outliers of California and North Dakota (see Supporting Information Appendix J, for more detail).

Analogous to the brain drain of talented individuals from non-competes (Marx et al., 2015), these results could be also labeled as a small—and probably entrepreneurial—firm drain (though Florida obviously benefited from the location choices and increased employment of large firms). If the human and organizational capital of small firms 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 & 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 the downsides of entrepreneurial brain drain is that large firms tend to do better than smaller ones (Hathaway & 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

Most research on non-competes has examined individual outcomes on mobility, other has considered entrepreneurship and innovation, and very little has considered how firms respond to changes in or types of non-compete regimes. Here we examined how the stronger enforcement of non-competes differentially influenced small versus large firms and consequently business dynamism in one local economy, using 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, employment growth, and business concentration. The enforcement of non-competes appears to have affected not only spatial (re)location of businesses and workers across states but also endogenous growth within states by firm size. Small, entrepreneurial firms and large incumbent 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 versus existing firms, small firms appeared reluctant or less able to create new jobs. In contrast, large firms boosted their rate of new job creation and the level of employment, following the law change. Consistent with these results, we observed an increase in the business concentration in Florida, following strengthened non-compete enforcement.

Furthermore, across all the 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 (negative) relationship exists between non-compete enforcement and large (small) firms' establishment and employment. Business concentrations also exhibit positive relationships with non-compete enforcement across all the U.S. states.

While intending to address leakage and lack of investment, 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 affect the labor market and can 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 (Belenzon & Schankerman, 2013; Fallick et al., 2006).

While most research on non-competes has to date focused on individuals and entrepreneurship, the differential effects on firm (re-)location and employment by firm size and consequent regional business concentration have received little attention, despite having important managerial and policy implications. Firm strategies for R&D and innovation differ by their size (e.g., Cohen & Klepper, 1996a, 1996b), 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 competition and the broader market environment, most importantly, through the redistribution of firm size and increased concentration. Stronger enforcement may attract and favor large established firms, while lowering the birth and/or move-in rate and growth of small entrepreneurial firms. Non-competes impact many firm decisions, not only in location choice and hiring and competing in a less mobile labor market, but also in choices for growth, product development, alliance partners or acquisition targets, supply chain design, and competitive strategy. All of these choices directly influence ultimate performance.

Furthermore, to the extent that small and large firms provide different values and jobs to local economies (e.g., incremental versus 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 (e.g., 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, endogenous business growth, and business dynamism. Policy makers and legislators should take these broader impacts into account.

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DATA AVAILABILITY STATEMENT

All data are from public sources.

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.
- 3 Please see the Supporting Information Appendices 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. The Supporting Information Appendices also provide a summary of other state changes in non-compete enforceability and highlight their unsuitability for use 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 to establish a wider validity of that path.

- 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 that is ± 2 , 3, or 5 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 1). We ran the linear interaction approach (where the size variable takes values from one to 12) nonetheless and found consistent results. In addition, creating 12 dummy variables (rather than three) produces the qualitatively same results. See Supporting Information Appendix F for a more detailed description and results.
- 7 The results are robust to the inclusion of 1996 as treatment year.
- 8 Here we make a stringent assumption that our dependent variables are closely interconnected and use a single representative weight to construct the counterfactual Florida for all outcomes of interest. However, it is possible that the drivers for our outcomes are different; for example, states that show a similar trend in small establishment entry to that of Florida do not necessarily coincide with the states that show a similar trend in large employment growth to that of Florida. We thus conducted the same analysis with different weights for each of our dependent variables. This approach, by design, produces better parallel trends for pre-1996 period, and the results are very similar to those with a single weight.
- 9 The last firm size category is "10,000 or more employees." For this category, we conservatively assume that the maximum number of employees is 20,000, and the resulting Pseudo-HHI measure ranges from 0 to 122.5. The result is robust to a wide range of alternative assumptions.
- 10 This assumes that pro-big business policies are most likely to be adopted by Republicans.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section.

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APPENDIX A

TABLE A.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>nonexclusive</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

Source: The authors’ assessment of the statutes.