Large Firm Advantage and Entrepreneurial Disadvantage:
Non-competes and Market Concentration[*]

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Abstract

If covenants not to compete provide advantage to larger and more established firms, relative to entrepreneurs and small firms, then markets could become more concentrated over time. We isolate the impact of non-compete enforcement on market concentration by focusing on Florida's 1996 legislative change that eased restrictions on the enforcement of non-competes. We first establish an unambiguous contrast between legal regimes before and after 1996. Differences-in-differences models show that following the change, establishments of large firms were more likely to enter Florida. They 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 created a smaller proportion of new jobs and decreased their share of employment in the state. Consistent with these location and job creation dynamics, a variety of market concentration measures increased significantly following the law change in Florida. Consistent with the dynamics illustrated in Florida, a nationwide cross-section also demonstrates a positive correlation between stronger enforcement of non-competes and market concentration.

Keywords: Entrepreneurship; Firm Sorting; Job Creation; Market Concentration; Employee Mobility; Non-compete agreement

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I. INTRODUCTION

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).

Employers typically prefer labor contracts with mechanisms that aid in the retention of employees, for at least two reasons. First, strong retention mechanisms give firms incentives to invest in their employees. If employers fear that an employee will leave just after training, they are less likely to invest in that training. This situation is not optimal for employers or employees. Second, and perhaps more importantly, employers often provide their employees with confidential and valuable information, such as business secrets and customer lists. 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). Hence, without retention mechanisms, firms might under-invest in both employee training and research and development (R&D).

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. This restriction constitutes a natural response to concerns about employee separation. With non-competes, 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. This restriction can at least partially solve the market failure of under-investment in employee training and research activities.

However, addressing the under-investment in training and research problem may have other effects on firms, industries, and the regions they both operate in. 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 they imply that prior employees cannot utilize their expertise and experience in the same field for a certain amount of time. Non-competes not only restrict prior employees' competition against their former employers, they also hinder employers' competition for prospective employees. In some cases, employers leverage their control over employees (more than allowed by law) 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 make it more difficult for firms to hire the talent they need, slow the optimal matching of human capital and opportunities, and retard the diffusion of knowledge and expertise within a region.

Empirical work has elaborated a variety of desirable and less desirable impacts of 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 promoted 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), inter-state brain drain of inventors (Marx, Singh, and Fleming, 2015), increased likelihood of acquisition (Younge, Tong, and Fleming, 2014), and temporarily increased valuation of firms (Younge and Marx, 2016). 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. Starr (2014) 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. 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.

To date, however, few studies have examined the asymmetric consequences of non-competes on firms of different sizes. If all of a region's employees have signed non-competes, smaller and typically start-up companies face greater difficulties in recruiting for those workers' current expertise; even some of the unemployed labor force may be bound by non-competes, as these agreements often stay valid even after layoffs or firings. Conglomerates, on the other hand, are more likely to have enough employees (compared to start-ups or small firms) and have abundant financial or non-financial resources to tie up their important workers or buy out the non-competes of new employees. As large firms run businesses in various fields, moreover, they also have an expedient way to circumvent the restrictions: allocate new employees to irrelevant business departments until their non-competes have expired. These mechanisms imply a very different response to non-competes, depending on firm size, in entrepreneurship, location choices, and employment; these choices in turn should influence market outcomes such as concentration or competitiveness. Advantages to larger and typically incumbent firms should accrete over time and could

fundamentally change a regional economy.

The main purpose of this study is thus to estimate the differential effects of stronger non-compete enforcement by firm size. We first establish that Florida's 1996 non-compete law provides a precise step change in non-compete law. We exploit this change as a quasi-natural experiment to demonstrate that stronger enforcement of non-competes alters the regional distribution of firms. It favored and attracted establishments of large incumbent firms; such firms were more likely to enter Florida and create new jobs. On the other hand, it appears that small firms or their establishments were crowded out. Stronger enforcement appears to have hindered the establishment of start-ups, the arrival of small firms to the state, and job creation by such firms. In analyses provided in the Electronic Companion, we explicitly consider how non-compete enforcement can influence the job creation and employment of existing in-state firms (intensive margin) but also those of new or incoming firms (extensive margin). Consistent with these trends, we also demonstrate a significant increase in market concentration measures following Florida's strengthening of non-competes. Finally, we illustrate a positive correlation between states' strength of enforcement and their industry concentration.

II. THE 1996 FLORIDA AMENDMENT TO NON-COMPETE ENFORCEABILITY

Non-competes are prevalent in many industries besides high technology. 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] Employers have used non-competes with increasing frequency up to the present day (Starr, 2015), and the introductory quote from The New York Times reflects their prevalence across many types of jobs and professions besides management and engineering. For example, Amazon requires their warehouse workers, 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: Disclosed contract)".[2] Physicians, dentists, accountants, and even lawyers can be subject to non-competes (Tanick and Troubaugh, 2012).

In order to identify 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.[3] This is

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[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] Other U.S. states have changed their non-compete laws, however, in each case, the intent or details of the change could obscure the impact of non-competes on concentration. For example, the intent of the 1986 change in Michigan law (MARA, 1986) was to increase competition, making it difficult to disentangle the effect of non-compete enforceability from the effect of the antitrust legislation itself (particularly the effect on market concentration). Changes made by court decisions – e.g, Light v. Centel Cellular Co., 883 S.W.2d 642, 647 n.6 (Tex. 1994) – strengthened (or weakened) enforceability of both existing and future contracts. In contrast, Florida affected only

because 1) the legislation was focused purely on restrictive covenants, notably non-competes (in contrast to Michigan's Anti-trust Reform Act of 1985, which was intended to increase competition, and thus would conflate our analysis of firm entry and market concentration), and 2) it clearly intended to strengthen non-competes enforcement in the state.

At least three important features of the 1996 amendment support the validity of the natural experiment. First, the amendment explicitly stated and thereby clarified which rule governed which 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. For example, the new law was to be construed in favor of business protection, and courts could not refuse non-competes enforcement on the grounds of employee economic hardship or public policy concerns. 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).[4] Further details and excerpts are provided in the electronic companion. Third, the 1996 amendment marked a sharp contrast to the preceding 1990 amendment. The post 1990-amendment statute 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)."

Considering the presence of non-compete law in Florida for the preceding four decades, employers and employees were probably familiar with and accustomed to non-competes. It is less likely that employers, in particular, were unaware of non-competes or changes in the associated rules that govern non-competes. The nature of the 1996 amendment and clear contrast with the prior amendment strengthens the argument for its use as a natural experiment.

III. THEORY: 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 impact

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future contracts, hence, it was possible to poach an employee under an old contract, thus making the impact of the law change on these outcomes more salient. The Louisiana change was brief and temporary (enacted in 2001, the law reverted back in 2003) and 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.
[4] Please see the Electronic Companion for further discussion and full text of §542.33B and §542.335.

firms of different sizes. We consider the differential effects of non-competes by firm size on location choice (at birth or in movement of extant establishments), job creation, employment, and market concentration. If our informal model is correct, we expect following the law change in Florida to observe (1) a shift in the distribution of firm size away from small firms towards large firms, (2) a shift in the sources of new job creation and employment away from small firms towards large firms, and (3) an increase in market concentration.

III.1. Business size and location preferences

Large firms probably prefer regions with stronger non-compete enforcement. It is generally easier for large firms to attract new employees, such that retention becomes a bigger concern, relative to recruiting. Furthermore, large firms possess more ways to exploit the upside and mitigate the downside of strong non-compete regimes. As a result, we expect a shift in the distribution of establishments following Florida's strengthening of non-competes enforcement, from smaller to larger firms, due to both cross-border relocation and organic growth within borders.

Small and large firms have different abilities and incentives to hire new workers and protect extant workers. The importance of scaling and hiring new employees is generally greater for small firms. The recruitment of high quality and experienced employees indeed constitutes one of the greatest challenges in the founding and scaling of a new business. Small 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. 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. As the competition for workers increases due to stronger non-compete enforcement, start-ups and small firms should find themselves at a disadvantage, given their inferior resources, reputations, and prospects compared to large incumbent firms. Therefore, potential entrepreneurs or proprietors of small firms should prefer weaker enforcement of non-competes and thus be less likely to locate in regions that strongly enforce non-competes. Difficulties in recruiting workers and coping with legal challenges to hiring effectively create an entry barrier for these start-ups or small businesses.

Large firms, on the other hand, are more likely to already possess most of their necessary workforce and should prefer stronger non-competes enforcement. While large firms also need to hire new employees, the strategic importance of hiring is less relative to a small firm that is trying to scale. For large firms, the strategic importance of retaining existing employees is generally greater. 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, incumbents could lose experienced members of their workforce that they have trained; furthermore, mobile employees might also unwillingly transfer important strategic assets of former employers, either implicitly or explicitly, to the competing firms. Therefore, large firms gain more than they lose from immobilized employees and thus should prefer to keep current rather than hiring new employees.

Stronger enforceability also favors large firms asymmetrically in that these 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 serve as a "holding tank" (Marx and Fleming, 2012) for new employees who might be bound by non-competes. Small firms, on the other hand, are more likely to focus on a specific area and lack diversified business units that could serve as holding tanks.

Finally, large firms generally possess a number of intrinsic advantages such as superior financial resources. For instance, large firms are more likely 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.

We therefore expect that firms will exhibit opposite responses to stronger enforceability of non-competes, depending on their size. Large firms are more likely to open new establishments in Florida or move extant establishments to Florida, after the amendment. Small out-of-state firms, on the other hand, are less likely to do so; they or their establishments are essentially crowded out. Entrepreneurship may also be hampered, implying the presence of fewer start-up launches in areas with stronger non-competes enforcement. Analogous to "voting with feet (Tiebout, 1956)," firms (re-)locate in the municipality that offers their preferred business environment, essentially shopping for advantageous policies.

III.2. Job Creation and Employment

The enforceability of non-competes should also asymmetrically affect the creation of new jobs and employment, depending on a company's size. All other things being equal, it becomes harder for small and entrepreneurial firms to hire new workers, if labor forces in the region are restrained by stronger non-compete enforcement. We would expect to observe this in a shift in the distribution of sources of new jobs, in favor of larger firms, following the 1996 law change.

Regional mobility decreases with stronger enforcement (Marx, Strumsky, and Fleming 2009) and this decrease provides an advantage to larger and more established firms. If workers expect that they will be bound by non-competes and thus unable to hop between jobs, they will prefer a more secure job

(typically offered by large firms) in the first place. This is more so when (typically) non-competes remain in force after an employee is laid off. 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 career transfers, and other non-pecuniary incentives. Furthermore, workers who sign non-competes bear additional risks should the business go awry, because they remain bound by commitment (and small business are more likely to go awry). If they fear being unable to flee a sinking ship, job seekers will prefer large and stable employers. This gives established employers an additional advantage in boosting their job creation and employment.

Small firms are typically less able to offer appealing incentives to prospective employees. Considering the workers' perspective, fewer prospective employees will choose small firms that are riskier, pay less, and offer less protection from potential non-compete prosecution by larger firms with intimidating legal resources. In contrast, if employees are not bound by non-competes, (marginal) job seekers may choose small firms that are riskier, because they can leave the job and get another job more easily. Consequently, small firms will be at a relative disadvantage in creating jobs and hiring when non-compete enforcement strengthens; this should be observable in decreased employment for smaller firms.

Firms that benefit from non-competes will also accrue resources that in turn enable future growth in their work force. The greater enforceability of non-competes reduces an employee's outside alternatives. For example, 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 resources to a current employer that can be invested in the expansion of the firm's work force.

These arguments imply that stronger enforcement of non-competes shifts the distribution of job creation to larger firms. This should make large firms even larger, whereas it should hamper the birth and growth of small and entrepreneurial firms.

III.3. Market Concentration

A demographic shift towards establishments of large firms and a proportional increase in job creation and employment by large firms implies a restructuring of the local economy towards higher market concentration. Strong enforcement of non-competes should alter the distribution of firms and thereby increase market concentration through two major channels.

First, a lenient business environment for non-competes changes the spatial distribution of firms (extensive margin). As discussed earlier, firms or their establishments position themselves ("vote with their feet") to places with institutional and market environments that favor their business. For large firms, whose (net) incentives include retaining and increasing their control over existing employees, it becomes more attractive to locate in regions with stronger non-compete enforcement. On the other hand, start-ups and small firms are at a stage where the number of employees is small yet grows quickly. Under the enforcement of non-competes, they will have more difficulty attracting employees and thus prefer regions with policies against non-compete enforcement. Attracting large firms and repelling or crowding out small businesses or potential start-up establishments should therefore increase a local economy's proportion of large firms.

Second, in addition to these location effects, the endogenous growth in the number of employees should be greater for large firms under stronger enforcement (intensive margin). As discussed in Section 3.2, large firms enjoy an advantage over smaller ones when hiring new employees and retaining existing employees. Large firms can train inexperienced and unemployed members of the work force through their typically more systematic training systems. The existence of holding tanks makes it easier to circumvent or evade incoming employees' non-compete restrictions. Small firms or potential entrepreneurs, on the other hand, have difficulty recruiting the experienced workers they need. Compared to regions where non-competes are seldom enforced or proscribed, enforcing regions probably experience slower growth of small firms. This retards endogenous growth for small firms, and encourages such growth for large firms.

In sum, with stronger enforcement of non-competes, (1) the market is restructured in a way that attracts large firms and crowds out small firms and (2) large firm growth is favored and small firm growth is not supported. These two mechanisms imply that the enforcement of non-competes will result in the dominance of large firms and their jobs in the local economy and in turn, significant increases in market concentration.

IV. EMPIRICAL DESIGN

We now describe the data and empirical models to test our predictions. If our arguments hold, we expect to observe 1) a shift in the distribution of firm size away from small towards large firms, 2) a shift in the sources of new job creation away from small towards large firms, and 3) an increase in market concentration.

IV.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 is 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 VII.1, we use industry information from a separate data, the Current Business Patterns (CBP), and match treated and controlled MSAs on their industry composition. Table 2 provides descriptive statistics and correlation table. There is little evidence of high correlations across variables in our models.

IV.2. Difference-in-Differences Model

To empirically test our hypothesized causal 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:

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

where the 0 in parentheses indicates a lack of treatment (i.e., no amendment). Although impossible to have any data on the left-hand side ("the fundamental problem of causal inference"), 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.

     Empirical specification 1: In our difference-in-differences regressions, we consider an indicator variable that adopts a value of unity for years including and 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 (± three years from the year of the amendment, 1996)[5]:

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

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[5] A variation of the window i.e., ± two, three, or five years does not qualitatively change our result.

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 ("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.

$$\log Y_{it} = \mu + \alpha_i + \delta_t + \sum_{t \neq 1996} \tau \cdot FL_i \cdot \mathbf{1}\{Year = t\} + X'_{it} \cdot \beta + \epsilon_{it} \ldots (2)$$

Empirical specification 2: An alternative approach is to include an ordinal firm size variable in the model ($Size$): 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.[6] We then run the difference-in-differences estimation in Equation (3) for the period ranging from 1993 to 1999 with full sample:

$$\log Y_{ist} = \mu + \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + \pi \cdot Post_t \cdot FL_i \cdot Size_s + X'_{it} \cdot \beta + \epsilon_{ist} \ldots (3)$$

where X'_{it} includes two-way interactions, $Post_t \cdot Size_s$ and $FL_i \cdot Size_s$, and $Size_s$. Note that FL and $Post$ variables are absorbed by the MSA and year fixed effects. The parameter of interest is τ and π. We need to be careful in interpreting this model, as it imposes a linearity assumption on the variable, $Size$, even though it is a discrete, categorical variable. We also run an unconstrained model as a robustness check.

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]

V. RESULTS

V.1. Business size and location preferences

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[6] This categorization is given by the Business Dynamics Statistics (BDS) data of the U.S. Census Bureau.
[7] We also present an analysis including the 1996 sample as treatment year in the Electronic Companion.

We anticipate that establishments of large firms are more likely to launch in or move to Florida, while start-ups or smaller firms respond in the opposite manner. Consequently, the distribution of establishments in Florida should shift in favor of larger firms.

We first present graphical evidence. Figure 1 shows the density change of the main outcomes in Florida between 1995 and 1997, by firm size. The black real line represents the density in 1995, while the black 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 1, the entry of establishments of small firms (or 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.

As additional evidence, Figure 2 splits firms *within* Florida by their size. The black real line and left-hand side y-axis represent "Small" Florida firms that have less than 50 workers. The black dashed line and right-hand side y-axis represent "Large" Florida firms with more than 1,000 workers. 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 (1989-1995) by rescaling the y-axis ranges. We generally find a parallel trend between the Small and Large firms for pre-amendment years, 1989-1995. 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 3 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. 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 3.

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 2,500 employees). In Figure 3, the blue real line represents Florida, while the red 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 Florida, beginning from 1996. In contrast, the number of establishments of Large firms shows the opposite trend: it becomes higher than counterfactual Florida.

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 6.8 percent, whereas large firm entry increased by 8.7 percent. The number of establishments in Column (2) shows a similar pattern though the Small sample does not achieve statistical significance.

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 $Post_t$ indicator). The results are summarized in Panel (a) of Figure 4, where the black real line shows estimates by year and blue dashed lines represent a 95% confidence interval. The number of establishments of Small firms decreases (left-hand side of Panel (a)), whereas that of Large firms increases (right-hand side of Panel (a)). The changes primarily come from Large firm response, as they show sharp and significant increases right after the treatment year, 1996.

Table 4 shows the results from continuous models where we interact an ordinal firm size variable with $FL \times Post$. For the establishment entry in Column (1), we consistently find opposite signs for $FL \times Post$ and $FL \times Post \times Size$ variables. In other words, the effects are negative for small firms, and the smaller the firm is, the larger the magnitude. In contrast, the effects are positive for large firms, and the larger the firm is, the larger the magnitude. The sign for combined coefficient ($\tau \cdot Post \cdot FL + \pi \cdot Post \cdot FL \cdot Size$) reverses when the $Size$ variable changes from 5 to 6. This suggests a reduced entry for small firms that have less than 100 employees ($Size \leq 5$). When the $Size$ variable takes a value larger than 5, the sign of the net effect reverses. That is, establishments of Large firms ($Size \geq 6$) increase their entry into MSAs in Florida. Column (2) illustrates consistent results for the total number of establishments. Small firms with 1-4 employees in the local economy decreased by about 1.9 percent ($-0.0290 + 0.0099 \times 1$), and the result becomes positive and large as firm size increases (i.e., when $Size \geq 3$).

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). In our result in Column (1) in Table 4, the entry of firms with 1 to 4 employees decreased by 7.5 percent ($-0.0913 + 0.0167 \times 1$), 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%.

As discussed, an assumption behind the approach in Equation (3) is that the effects are linear in firm size. To estimate the effects more generally and to circumvent this linearity assumption, we further conduct estimations in Equation (1), but separately for each firm size categories. The results are summarized

in Figure 5, where each dot represents an estimate for $Post \times FL$ from twelve separate regressions for each firm size categories. In both Panel (a) and (b), we find that the effects primarily come from responses by large firms, as their estimates are larger and statistically significant.[8] All these findings support the first hypothesis; large firms prefer to locate in regions that enforce non-competes.[9]

V.2. Job Creation and Employment

We anticipate that non-competes will provide an advantage to large firms in job creation and employment, because (1) large firms can provide more stable jobs, (2) small firms will be less able to poach the experienced employees they need from large firms, and (3) large firms can use their advantage to negotiate for cheaper labor and use those resources to hire more employees. Thus, large firms should create a greater proportion of new jobs, relative to small firms, following a strengthening of non-compete enforcement.

As shown in Panel (c) of Figure 1, job creation by small firms decreased, whereas that of large firms increased in Florida between 1995 and 1997. This is the case for new and incoming as well as continuing firms in Florida (see Electronic Companion). Similarly, employment in Panel (d) of Figure 1 shows a similar pattern. Figure 2 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.[10] Finally, an inter-state comparison with the Synthetic Control in Panel (b) of Figure 3 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 parallel trend for pre-amendment years, 1989-1995.

Table 3 estimates split sample models and illustrates that small firms decreased their job creation, whereas large firms did the opposite, though the small firm sample is not significant. The continuous (though ordinal) interaction specification in Table 4 finds similar and consistently significant results. After the amendment, job creation by entrepreneurs with 1 to 4 employees decreased by 1.25 percent.

Column (4) of Table 3 shows that small firms decrease their employment (though not significantly) and the growth of employment comes from large firms. The sign for the combined coefficient ($\tau \cdot Post \cdot$

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[8] Interestingly, the estimate is positive and significant for small firms that have 1-4 workers. We believe that this effect is driven by "born-small" firms that do not intend to grow. For example, one-person businesses (e.g., dry cleaners) are intrinsically unlikely to hire and therefore not affected by non-competes enforcement. Note also that the "10K+" category is right-censored.

[9] The results are robust to the inclusion of 1996 as treatment year (See Electronic Companion).

[10] Figures on job creation are provided in Electronic Companion. BDS data is MSA-level panel, and the identity of starting/incoming and continuing establishments are re-defined every year. For example, a firm who entered Florida in 1997 is classified as a continuing firm in the 1998 data. In our research setting, incoming and continuing firms need to be defined as of July 1, 1996, which is not possible with the BDS data. We thus focus on *total* job creation and the level of employment in our main analyses.

$FL + \pi \cdot Post \cdot FL \cdot Size$) is reversed when $Size \approx 3$. We find consistent effects in a more flexible estimation specified in Equation (2). In Panel (b) of Figure 4, employment in small firms decreases (left-hand side figure), whereas that of large firms increases (right-hand side figure).

In an alternative specification with the continuous and ordinal interaction term, in Column (4) of Table 4, we find consistent and significant results. We then more generally estimate the effects separately for twelve firm size categories. The results are summarized in Panel (b) of Figure 5. Each dot represents an estimate for $t \times FL$, and we again find that the effects primarily come from hiring expansions by large firms (rather than shrinking employments by small firms).

Consistent with the second prediction, 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 fewer jobs.

V.3. Market Concentration

The first two predictions imply an increase in market concentration for two reasons. First, large firms prefer a region that enforces non-competes when they launch or relocate establishments; small firms will be crowded out (extensive margin). Second, large firms will grow at a faster rate and small firms will grow at a slower rate (intensive margin).

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 market concentration using the following three measures: (1) share of establishments that belong to large firms, (2) share of workers that belong to large firms, and (3) *pseudo* Herfindahl-Herschman Index (HHI). In this measure, we use the share of employees instead of the market share of their products. Pseudo-HHI 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 average 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 an average over all twelve categories. This measure mimics the way we calculate the original firm-share based HHI and captures the degree of market concentration at the MSA-year level.

Figure 6 shows the results from the Synthetic Control Method. In both Panel (a) and Panel (b), we consistently find that market concentration increases on and after the year of law change, 1996. We then run the differences-in-difference regression in Equation (4) with the three different measures of market

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 500 employees increased by about 3.9% (statistically significant at the 0.01 level). Column (2) confirms this finding with the share of firms that have more than 2,500 employees as dependent variable. Columns (3) and (4) show the employment-based share of large firms. Consistent with the third prediction, the results show an increase by 9.7% and 8.8% for Large firms that have more than 500 and 2,500 employees, respectively. In Column (5), Table 5, again illustrates that market concentration measured by the pseudo-HHI increases by 14.9% after stronger non-compete enforcement.

VI. Potential Threats to Identification

Since we exploit 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 threats to identification: Enterprise Florida, Inc. and electoral changes.

VI.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 favor 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 7 reveals, however, no noticeable change in Florida's industry composition for 1993-2001. Second, even if the EFI actively operated beginning from 1996, its website stated that EFI "…supports small and minority

businesses through its capital programs," and other entrepreneurial goals.

VI.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.[11] 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.

VII. Robustness checks

VII.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 cannot control directly for industry composition because the BDS data lack information by industry. As an alternative way of controlling for industry, we look at the County Business Patterns (CBP) data that provides information on county or MSA-level industry composition. Figure 7 shows Florida's industry composition around 1996. The idea is that, using industry information in the CBP, 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.

Two-digit Standard Industrial Classification (SIC) code (01-99) and its share in each MSAs are used to calculate the Euclidean distance between industry compositions of any two MSAs:

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

$$Industry\ Distance_{A,B} = \sum_{SIC=01}^{99} \left(Share_{A,SIC} - Share_{B,SIC}\right)^2$$

where $Share_{A,SIC}$ and $Share_{B,SIC}$ are the share of establishments in industry SIC in MSA A and B, respectively. For each Florida MSA, we identify five 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 7 (odd-numbered columns) 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.

VII.2. State-Bordering MSAs

One concern is 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.[12] 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 5. 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 Tables 6 and 7 (even-numbered columns). The results are not qualitatively different from those in Table 3 and 4 (and matching results in odd-numbered columns in Table 6 and 7), though with a much smaller number of observations, the estimates become less precise.

Importantly, the magnitudes are generally larger in the model only with border MSAs. One potential explanation for this result (which can 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

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[12] Note that more than half of the counties in these borderline areas do not belong to any MSAs.

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.

VIII. 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), and researchers do not observe individual labor contracts (i.e., whether each employee signed non-competes or not). The stark change in non-compete enforcement makes Florida a good research case, though, 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.

Though the growth and scale-up of entrepreneurial firms has recently increased in the U.S., the long-term trend has been negative (Kauffman, 2016). It would be interesting to investigate whether non-compete laws or enforcement have increased and possibly contributed to the long-term trend. While the results presented here focus only on Florida following a strengthening of non-compete enforcement, they imply that states that enforce non-competes should experience an increase in their industry concentration. Figure 9 illustrates a positive and consistent relationship between a state's strength of enforcement and its industry concentration as measured by share of employment by large firms and pseudo HHI (similar relationships, not shown, exist for the share of establishments by large firms). The relationships hold consistently for two indices of enforceability (Garmaise 2009, Starr 2016) and without the outliers of California and North Dakota. [13] This cross-sectional correlation is consistent with the illustrated

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[13] 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. Bishara (2011) assigned each state a score between 0 to 10 on seven dimensions of enforceability for 1991 and 2009 and aggregated them. Starr (2016) applied confirmatory factor analysis on these seven scores to generate weights for each dimension. The Starr measure also incorporates how frequently the factors are predicted to occur in the non-

mechanisms in Florida.

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. If both human and organizational capital leaves states that enforce non-competes for states that do not, it is not 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? Compounding 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. Weighed against this concern is the empirical fact demonstrated here that more jobs were created in Florida immediately following the strengthening of non-compete enforcement.

Though beyond the scope of this paper, another empirical question would be whether jobs at start-ups and large conglomerates play different roles in firms and the economy, and how. Asymmetries in firm positioning and employment growth (i.e., the dominance of large firms and the jobs they offer) could lead to potential welfare losses for consumers as well as 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, state governments should want to attract entrepreneurs and the jobs they create.

VIIII. Concluding Remarks

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

We examined how the stronger enforcement of non-competes influences various aspects of a 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. The enforcement of non-competes not only affects inter-state competition for attracting businesses, but also the in-state distribution of businesses and jobs. Small and large firms respond to non-compete enforceability in opposing ways: large firms are more likely to locate (either launch or move) their establishments in Florida and small firms

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compete-signing population of each state (Starr, 2016; p. 9).

are less likely. Regardless of whether they were new or existing firms, small firms are reluctant or less able to create new jobs. In contrast, large firms boost their rate of new job creation. Likewise, the level of employment decreases for small firms and increases for large firms. Consistent with these results, we observed an increase in the market concentration in Florida, following strengthened non-competes enforcement. We also observe a positive correlation between non-compete enforcement and industry concentration, across all U.S. states.

These differential effects on firm (re-)location and employment by firm sizes have important yet overlooked managerial and policy implications. Stronger enforcement may have lowered the "birth" rate and/or move-in of establishments of small firms and simultaneously attracted large firms. Though we were unable to measure firm starts directly, non-compete enforcement could have hampered entrepreneurship and distorted the dynamism of the local economy (i.e., quality of jobs, decreased creativity of innovation and product development, and aging of firms). To the extent that small and large firms provide different values and jobs to local economies (e.g., break-through and newer technologies versus incremental innovations), the (potentially undesirable) effects of non-competes on a local economy could be large.

These processes ultimately imply a higher market concentration in the local economy. 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 further implications for consumer, social welfare, inter-state competition in attracting businesses, and intra-state competition for labor forces. Policy makers and legislators should take these broader impacts into account.

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Figure 1 Comparison of Main Outcomes in Florida by Firm Size: 1995 vs. 1997

(a). Establishment Entry

(b). Establishments

(c). Job Creation

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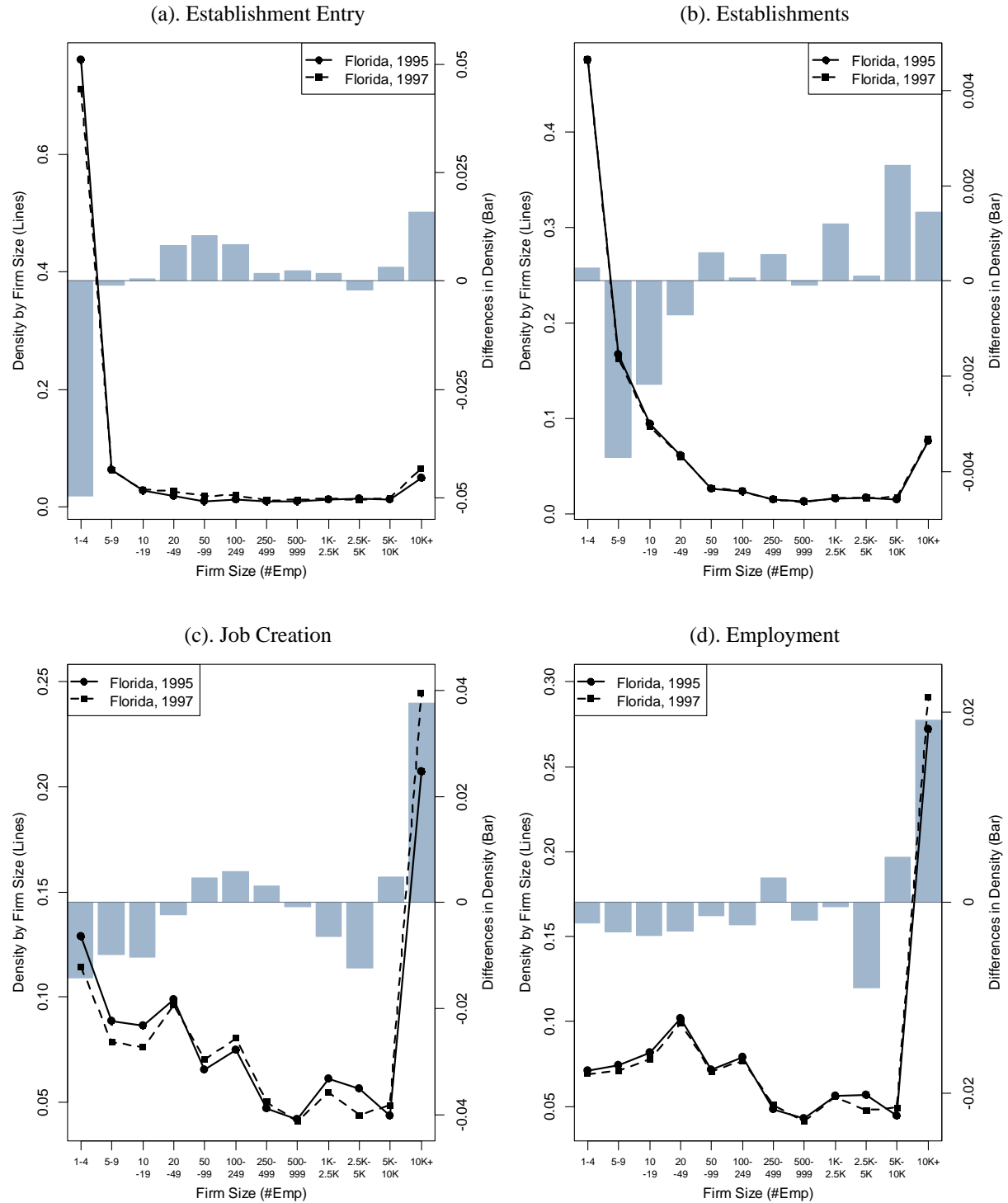
Data: Business Dynamics Statistics (BDS), 1995 and 1997.

Figure 2 Comparison of Main Outcomes in Florida by Year: Split Sample
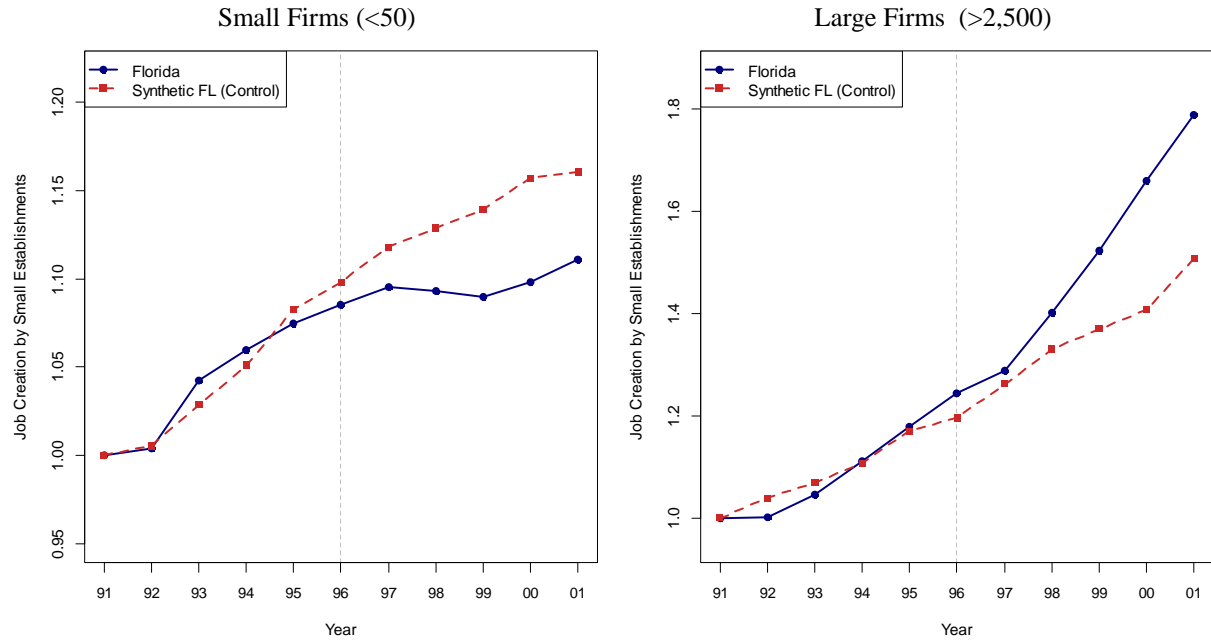
(a). Establishments
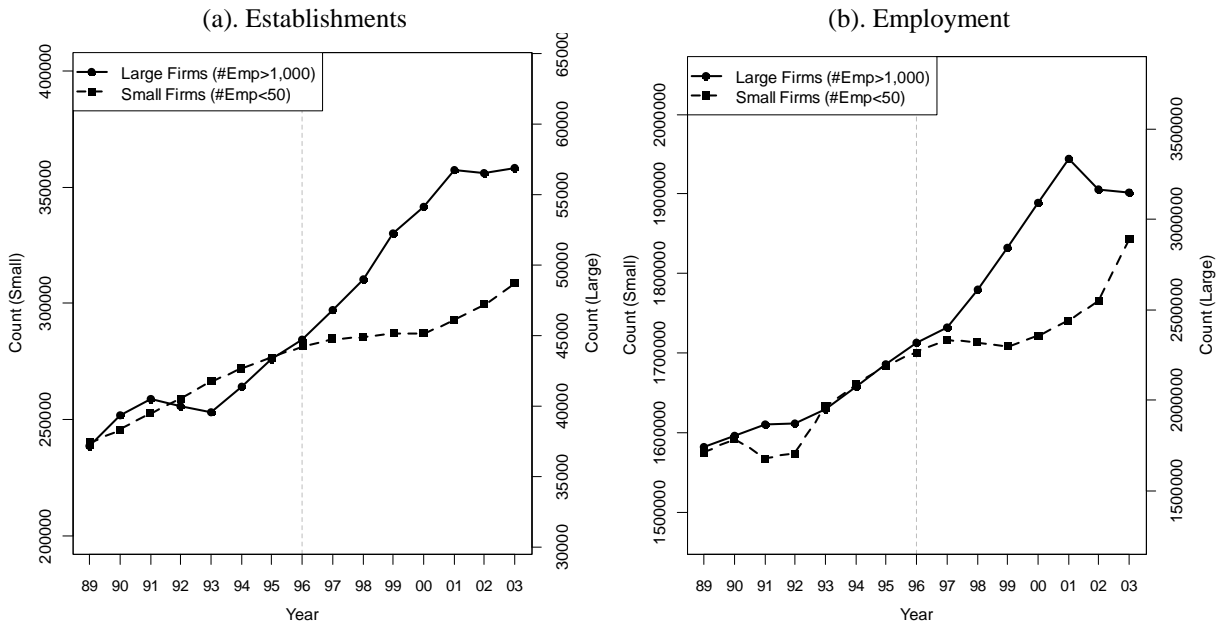
(b). Employment

Data: Business Dynamics Statistics (BDS), 1989-2003.

Figure 3 Comparison of Main Outcomes between Florida and Synthetic Control

(a). Establishments

Small Firms (<50)

Large Firms (>2,500)

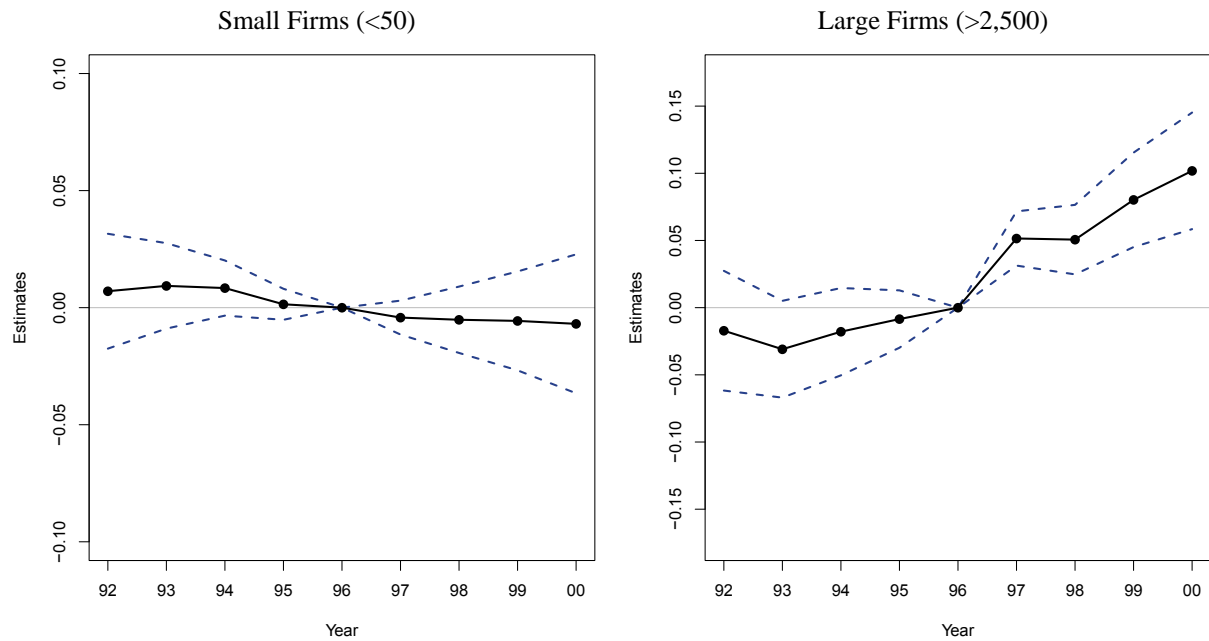(b). Employment

Small Firms (<50)

Large Firms (>2,500)
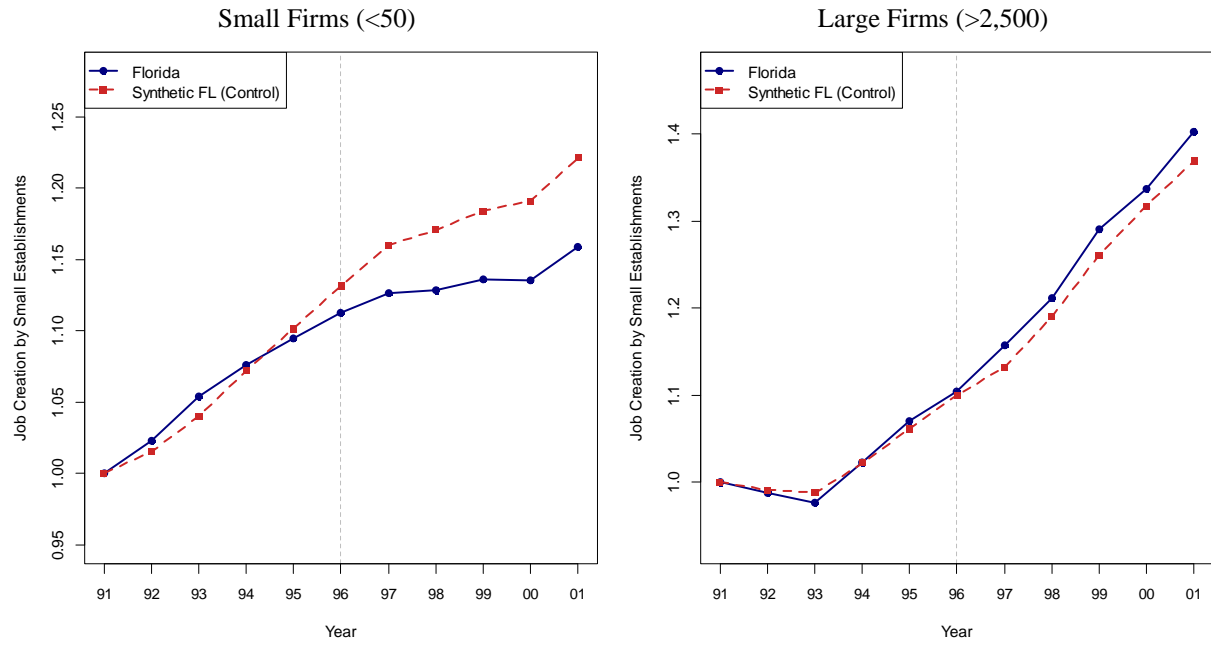
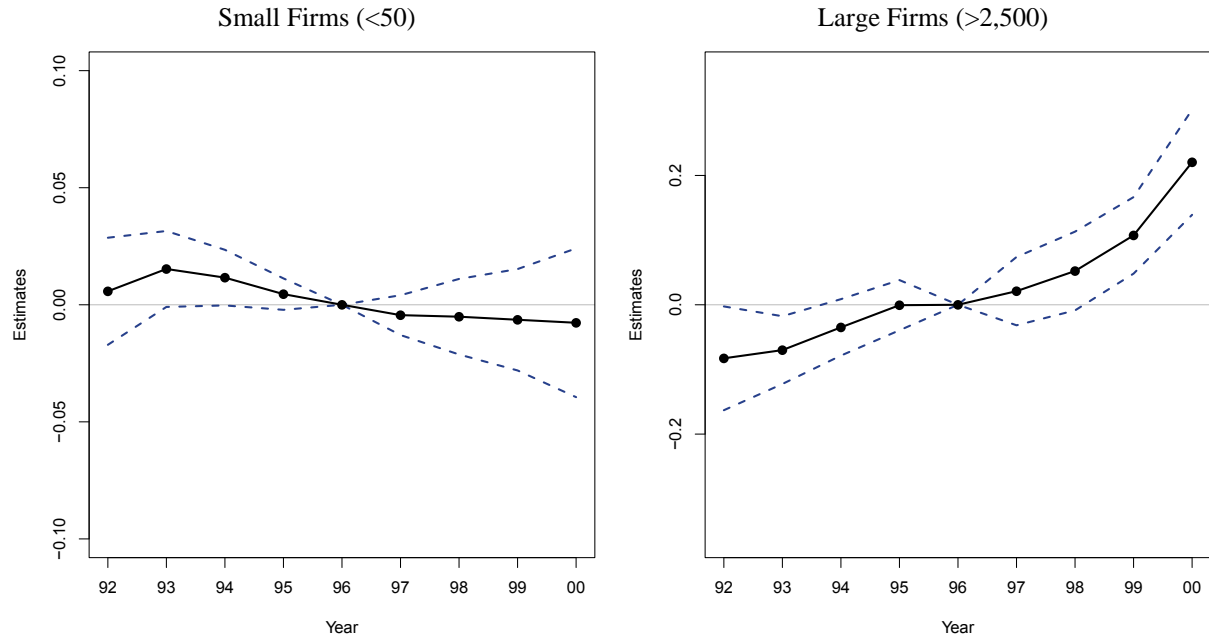Data: Business Dynamics Statistics (BDS), 1991-2001.
Note: The outcome variables for Florida are normalized relative to their 1991 value.

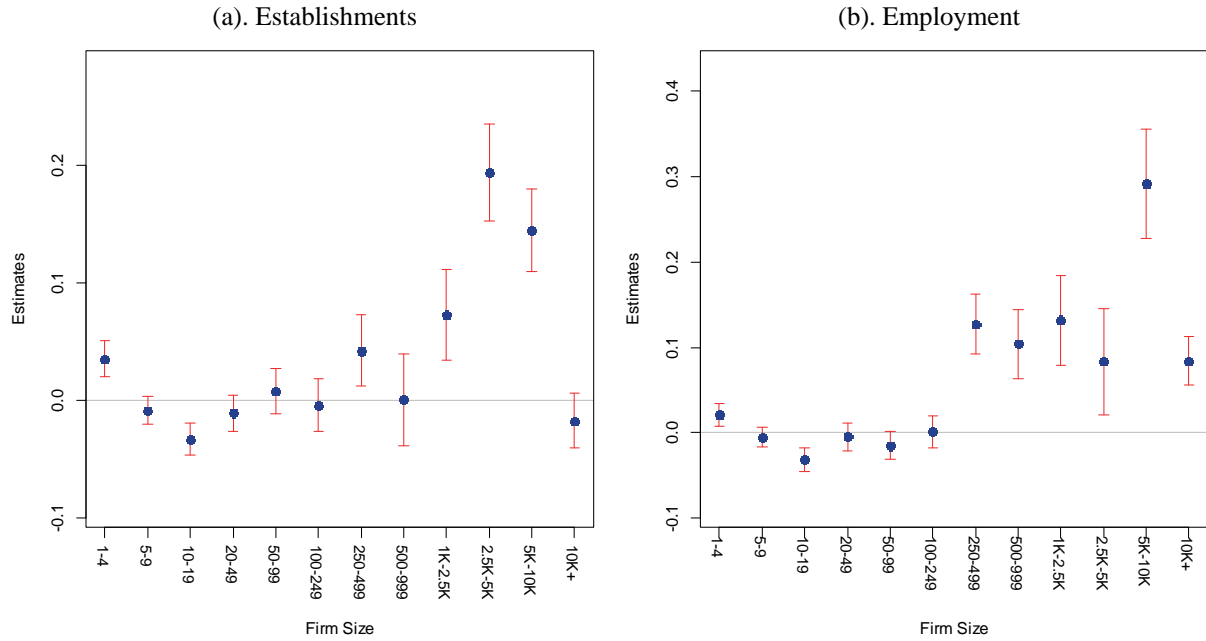**Figure 4** Yearly Estimates from Event Study Techniques

(a). Establishments

(b). Employment

Estimates from event study technique based on industry matched sample.
Dashed line stands for 95% confidence interval based on standard errors clustered at the state level.

Figure 5 Estimates from Separate Regressions by Firm Size

(a). Establishments

(b). Employment

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 internal based on standard errors clustered at the state level.

Figure 6 Comparisons of Market Concentration between Florida and Synthetic Control

(a).Employment Share of Large Firms
(>2,500 employees)

(b). Pseudo-HHI
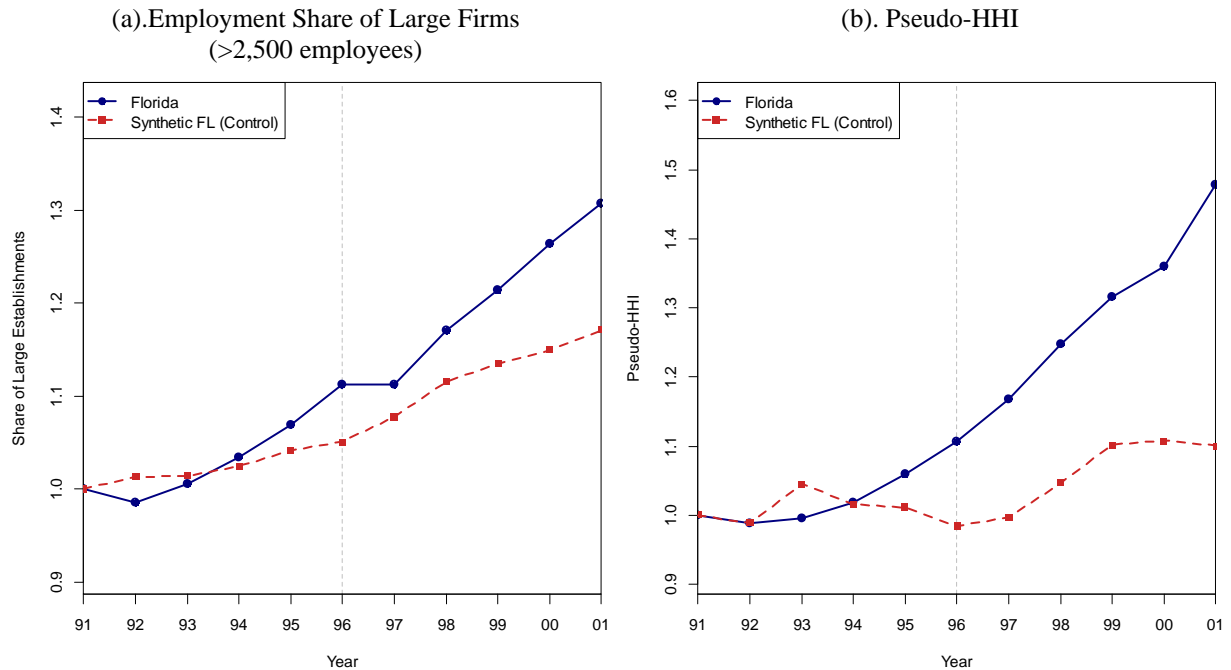
Data: Business Dynamics Statistics (BDS), 1991-2001.
Note: The outcome variables are normalized relative to their 1991 value.

Figure 7 Industry Composition in Florida, 1993-2001
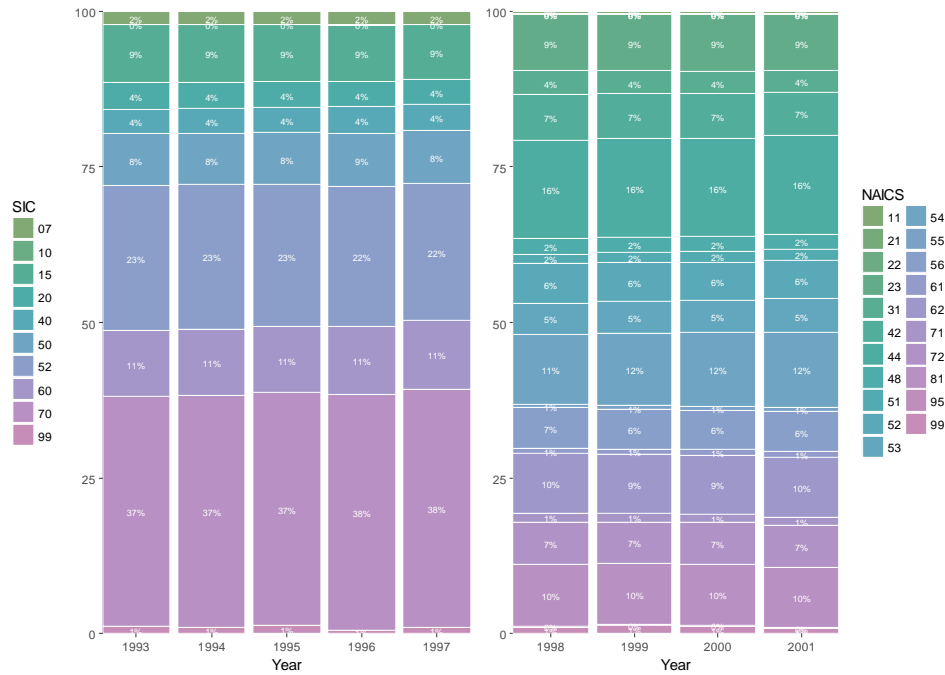
Data: Country Business Patterns (CBP).
Share of industries calculated based on the number of establishments in each industry.
U.S. Census Bureau has been using the North American Industry Classification System (NAICS) since 1998,
replacing the Standard Industrial Classification (SIC) system.

Figure 8 MSAs Near the Florida Border
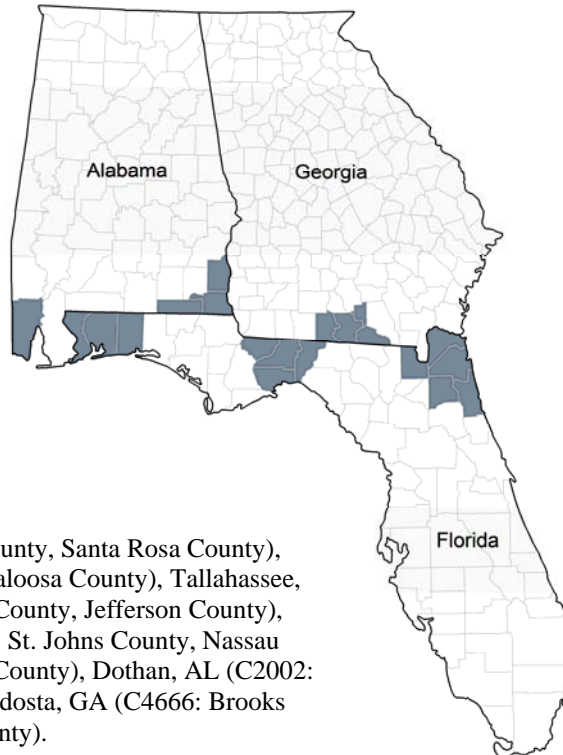
Border MSAs included:
Pensacola-Ferry Pass-Brent, FL (C3786: Escambia County, Santa Rosa County),
Fort Walton Beach-Crestview-Destin, FL (C2302: Okaloosa County), Tallahassee,
FL (C4522: Leon County, Gadsden County, Wakulla County, Jefferson County),
Jacksonville, FL (C2726: Duval County, Clay County, St. Johns County, Nassau
County, Baker County), Mobile, AL (C3366: Mobile County), Dothan, AL (C2002:
Geneva County, Henry County, Houston County), Valdosta, GA (C4666: Brooks
County, Echols County, Lanier County, Lowndes County).

Figure 9 Non-competes Enforceability and Market Concentration by State

(a). Employment-based share of large firms
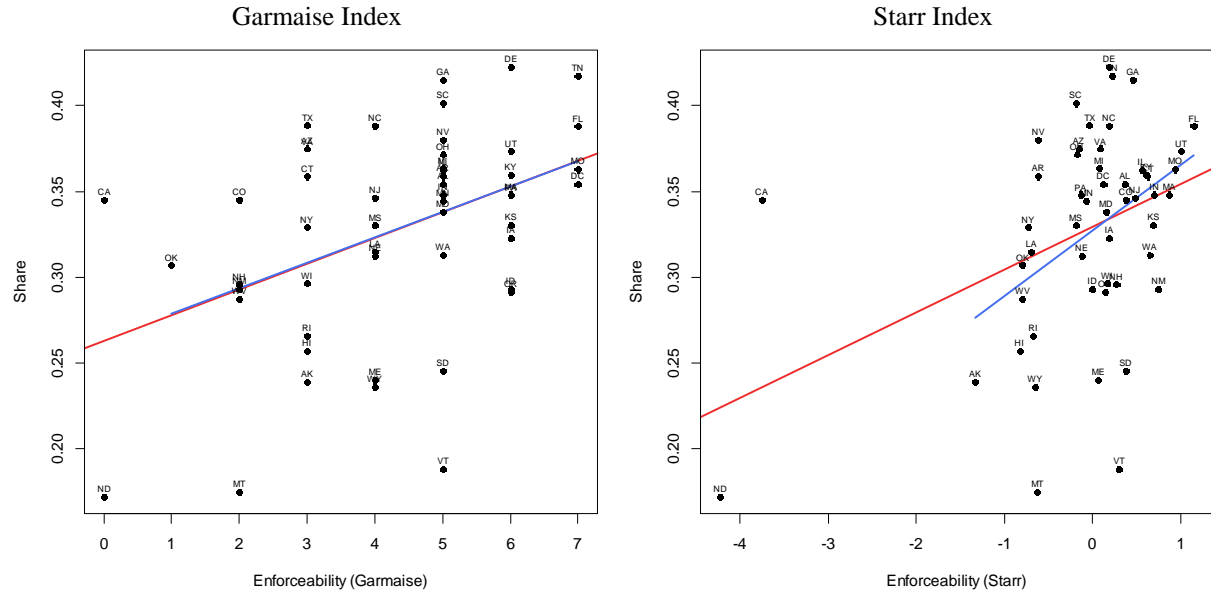
(b). Pseudo-HHI based on number of employees
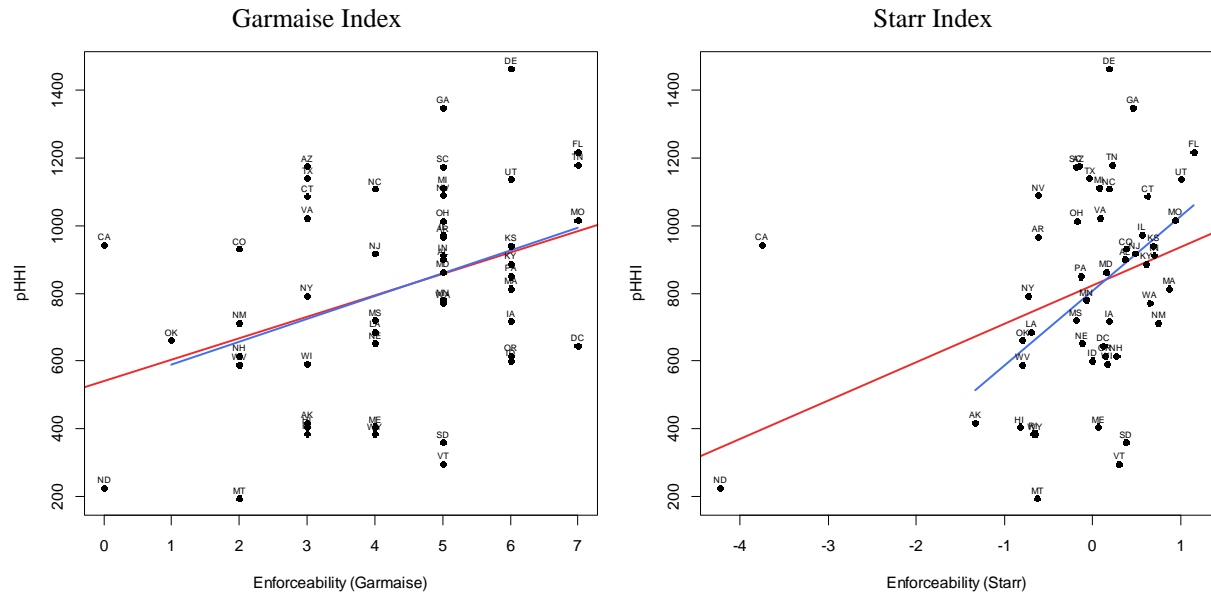
Red lines represent a fitted regression line with full sample. Blue lines represent a fitted regression line without the outlier states of CA and ND. Regressed market concentration on non-compete enforceability, including an intercept. Results for full sample regressions: top-left panel (coefficient: 0.015, standard error: 0.004, p-value 0.001); top-right panel (coefficient: 0.025, standard error: 0.008, p-value 0.003), bottom-left panel (coefficient: 63.51, standard error: 22.78, p-value 0.008), bottom-right panel (coefficient: 113.63, standard error: 41.17, p-value 0.008). Data: Business Dynamics Statistics (BDS), 1996.

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 *non-exclusive* 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
MSA-FSIZE-YEAR Level														
1 Establishment Entry	123.7	867.7	0.0	43299.0	1.00	0.95	0.65	0.25	0.42	0.25	0.02	0.01	-0.16	0.01
2 Establishments (Total)	1031.8	4910.7	2.0	230333.0	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	1108.4	3494.4	0.0	99261.0	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	1956.2	5707.1	0.0	193425.0	0.25	0.41	0.85	1.00	0.98	0.97	0.01	0.02	0.04	0.02
5 Job Creation (Total)	3064.6	8874.3	0.0	291020.0	0.42	0.56	0.94	0.98	1.00	0.97	0.02	0.03	0.01	0.03
6 Employment	18484.3	54145.2	7.0	1673631.0	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 (Category)	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	1996	2.2	1993.0	1999.0	0.01	0.01	0.03	0.02	0.03	0.02	0.00	0.93	0.00	1.00
MSA-YEAR Level														
1 Share of Large Firms (%, >500 employees)	2.4	0.5	1.1	4	1.00	0.96	0.72	0.64	0.51					
2 Share of Large Firms (%, >2,500 employees)	1.8	0.4	0.8	3.1	0.96	1.00	0.70	0.69	0.57					
3 Share of Employees in Large Firms (%, >500 employees)	7.8	1.4	2.9	12.5	0.72	0.70	1.00	0.89	0.77					
4 Share of Employees in Large Firms (%, 2,500 employees)	5.7	1.4	2	10.6	0.64	0.69	0.89	1.00	0.86					
5 Pseudo-HHI	295.8	121.3	87.1	1116.2	0.51	0.57	0.77	0.86	1.00					

Table 3 Heterogeneous Effects of Non-competes by Firm Size: Split Samples

	Establishment Entry (1)	*Establishment* (2)	*Job Creation* (3)	*Employment* (4)
	Dependent variables:			
	A. Split Sample: Small Firms (#Employees<50)			
Post×FL	−0.0677*** (0.0097)	−0.0042 (0.0062)	−0.0205 (0.0073)	−0.0053 (0.0060)
	B. Split Sample: Large Firms (#Employees>1,000)			
Post×FL	0.0866*** (0.0153)	0.0986*** (0.0073)	0.0824*** (0.0186)	0.1476*** (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 Heterogeneous Effects of Non-competes by Firm Size: Interaction

	Establishment Entry (1)	*Establishment* (2)	*Job Creation* (3)	*Employment* (4)
	Dependent variables:			
Post×FL	−0.0913*** (0.0100)	−0.0290*** (0.0062)	−0.0249*** (0.0087)	−0.0558*** (0.0062)
Post×FL×Size	0.0167*** (0.0016)	0.0099*** (0.0006)	0.0124*** (0.0021)	0.0187*** (0.0011)
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 Non-competes and Market Concentration

	Establishment-based Measures		Employee-based Measures		
	Share of Large Firms (>500)	Share of Large Firms (>2.5K))	Share of Large Firms (>500)	Share of Large Firms (>2.5K))	Pseudo-HHI
	(1)	(2)	(3)	(4)	(5)
Post×FL	0.0388***	0.0351***	0.0965***	0.0881***	0.1739***
	(0.0076)	(0.0092)	(0.0104)	(0.0119)	(0.0273)
MSA F.E.	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y
Observations	1,872	1,872	1,872	1,872	1,872

Dependent variables: Market Concentration

*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.

Table 6 Heterogeneous Effects of Non-competes by Firm Size: Split Samples (Border & Matching)

	Establishment Entry		Establishment		Job Creation		Employment	
	Matching	Border	Matching	Border	Matching	Border	Matching	Border
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Split Sample: Small Firms (#Employees<50)								
Post×FL	–0.0583***	0.0333	–0.0114*	–0.0042	–0.0449***	–0.0239	0.0655	–0.0266*
	(0.0198)	(0.0282)	(0.0065)	(0.0131)	(0.0110)	(0.0425)	(0.1427)	(0.0152)
B. Split Sample: Large Firms (#Employees>1,000)								
Post×FL	0.0648***	0.2439***	0.0799***	0.1623***	0.0220	0.2662	0.0954***	0.0973***
	(0.0191)	(0.0779)	(0.0124)	(0.0262)	(0.0327)	(0.1998)	(0.0167)	(0.0446)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	2,880	168	2,880	168	2,880	168	2,880	168

Dependent variables:

*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 Heterogeneous Effects of Non-competes by Firm Size: Interaction (Border & 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)
Post×FL	−0.0675** (0.0271)	−0.0812*** (0.0292)	−0.0312*** (0.0058)	−0.0397*** (0.0112)	−0.0441*** (0.0139)	−0.1956*** (0.0643)	−0.0510*** (0.0071)	−0.1290*** (0.0257)
Post×FL×Size	0.0113*** (0.0030)	0.0303** (0.0120)	0.0080*** (0.0008)	0.0159*** (0.0002)	0.0071* (0.0031)	0.0388* (0.0218)	0.0132*** (0.0014)	0.0201*** (0.0032)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	8,640	504	8,640	504	8,640	504	8,640	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 Non-competes and Market Concentration (Matching)

	Dependent variables: Market Concentration				
	Establishment-based Measures		Employee-based Measures		
	Share of Large Firms (>500) (1)	*Share of Large Firms (>2.5K))* (2)	*Share of Large Firms (>500)* (3)	*Share of Large Firms (>2.5K)* (4)	*Pseudo-HHI* (5)
Post×FL	0.0173** (0.0085)	0.0088 (0.0105)	0.0572*** (0.0121)	0.0492*** (0.0160)	0.1749** (0.0873)
MSA F.E.	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y
Observations	498	498	498	498	498

*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.