ECONOMIC INNOVATION GROUP

Research Note Series

The Effects of Noncompete Agreement Reforms on Business Formation: A Comparison of Hawaii and Oregon

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Research Question

What happens to new business formation when states introduce legislation limiting the enforceability of noncompete agreements?

Context

In order to better understand the implications of noncompete agreements and their enforceability, we compare how targeted noncompete agreement reforms in Hawaii and Oregon affected business formation in each state. While Hawaii's reform focused on noncompete agreements among technology workers, Oregon's reform focused on low-wage hourly workers. Following previous work on entrepreneurship and noncompete agreements, we expect that the two reforms would have different impacts on business formation. Because technology workers are more likely to have the technical expertise and access to the financial assets necessary to start a business in their chosen field relative to low-wage hourly workers, we expect that the Hawaii reform will have a greater positive impact on business formation.

Summary of Results

- Legislation limiting the enforceability of noncompete agreements can change patterns of business formation.
- The Hawaiian reform, which exempted workers in technology focused industries, resulted in a 10.2 percent increase in the number of technology establishments and a diffusion of skilled technology workers across the labor market.
- The Oregon reform, which exempted low-wage workers, did not result in a statistically significant increase in the number of establishments or a significant shift in employment.

Why We Care

This research suggests that policymakers must ensure that noncompete agreement reforms include higher-earning knowledge workers if they aim to encourage entrepreneurship and foster economic dynamism with their efforts. This research finds that legislation limiting the enforceability of noncompete agreements among a subset of high-wage workers with in-demand skills resulted in the formation of new businesses and increased transfer of knowledge as workers changed jobs. It found no such impact to reforms covering only lower-earning workers. There are many reasons to curtail the use of noncompetes for low-wage workers, and this research helps build the case for curtailing the use among higher-wage workers, too.

Contents

1 Literature Review

Noncompete agreements (NCAs) have been a growing element of the U.S. labor market and a recent focus for state and federal regulation. Since 2011, 28 states and Washington, DC, have enacted regulations and bans or modified their laws on the use of NCAs. Subsequently, enforcement regimes across states have become increasingly dissimilar. These changes have inspired a great deal of research exploring the effects of NCAs on wages, innovations, entrepreneurship, and the labor market as a whole [26].

The motivation for, and assumed effects of, NCAs are a necessary component of our understanding of the literature. Arrow's [2] work on innovation across firms offers a great perspective on the potential motivations for NCAs: Firms are caught in a balancing act of developing innovative ideas and training skilled workers, while also maintaining control over their intellectual property and keeping the cost of retaining workers low. Although NCAs are just one tool at a firm's disposal, they are used frequently. It is estimated that roughly 18 percent of workers are covered by NCAs [25], though this share could be even higher. [6] This high level of coverage results in two major negative effects.

First, a noncompete agreement limits the capacity of workers to transition to competing firms. While this may be in the interest of an employer looking to capture an investment, it significantly limits one of the primary avenues for a worker to acquire higher wages. NCAs have been shown to prevent workers from transitioning out of jobs, [10;11;15;19;23;25] make it more difficult to hire workers, [9;23;24]], and reduce the earnings of workers. [17;23] This limitation on labor market churn can also significantly impede new firms from accessing talent and growing, which can act as a support for large incumbent firms, limit competition, reduce innovation, and increase consumer prices.

Second, NCAs act as a barrier to the entrepreneurial efforts of individuals looking to start a business in their chosen field. For those workers who feel capable of striking out on their own, a noncompete can act as a deterrent, limiting the number of innovative thinkers allowed to shake up an industry. Together, these detrimental impacts can harm workers embedded in an industry as well as firms that are new entrants. When considering the effect of NCAs on entrepreneurship, the anticipated negative effect appears to be true, with new firms being significantly less likely to form in areas with greater enforceability of NCAs. [4;15;16;18;21;27]

Given that numerous alternative solutions exist to preserve a firm's investment in the research and innovation pipeline (non-disclosure agreements, non-solicitation of client agreements, etc.), NCAs have attracted attention from policymakers, and most recently, the Federal Trade Commission.

As policymakers have looked to support workers, stoke entrepreneurship, and rein in the excesses of noncompetes, subsets of the labor force have been offered exemptions. In Oregon, low-wage and hourly workers were exempted from NCAs which were voided for new contracts for individuals earning less than the "median family income for a four-person family." But the exceptions for this law, workers who could still be subject to NCAs, included workers "engaged in administrative, executive or professional work who: (a) Performs predominantly intellectual, managerial or creative tasks; (b) Exercises discretion and independent judgement; and (c) Earns a salary and is paid on a salary basis" (ORS 653.295 section (1)(b) and ORS 653.020 (3)). Hawaii opted for a very different approach, banning NCAs for technology workers. The Hawaii policy prohibits noncompete clauses for "any employment contract relating to an employee of a technology business" (HB 1090 H.D 2 S.D.2 C.D.1).

Importantly, the Hawaiian reform also included a ban on co-worker non-solicitation covenants, which limit the capacity of employees to "recruit" their colleagues to start a new venture and can exhibit similar tendencies as NCAs.[14] These two factors were implemented simultaneously and are likely to both support entrepreneurial activities.

The differences in policy design offer an opportunity to explore how variation in legislative approaches might alter the effects of partial noncompete bans: In particular, how entrepreneurship may vary across exempted industries and workers. Following the literature on self-employment and entrepreneurship, we would expect that individuals with higher levels of human capital,[7] prior work experience,[5] knowledge of a system, market, or technology,[1;22] and access to non-banking networks of capital[1;8;12;13;22;28] are more likely to create businesses that survive and grow. These features of entrepreneurship support a hypothesis that bans on NCAs for technology workers are more likely to produce an increase in entrepreneurial activities relative to bans for hourly and low-wage workers.

2 Data

Our analysis uses the U.S. Census Bureau's County Business Patterns (CBP) data set from 2000 to 2020 to explore what effect the Hawaii and Oregon noncompete agreement reforms had on new business formation and employer establishment size. The CBP is an annual series which is available at the county level by industry according to the North American Industry Classification System (NAICS). The CBP includes the number of employer establishments within a NAICS-county as of March 12th of a given year and breaks this count out across establishment size bins, as defined by the number of employees of each establishment. Using the CBP we can build a panel of establishment counts across multiple NAICS categories at the county-year level.

We support our analysis with the use of Quarterly Workforce Indicators (QWI) data from the U.S. Census Bureau. The QWI measures employment flows at the county-industry-year level, allowing us to validate the results on employment estimates from the CBP using an additional source of data.

Following Lipsitz and Starr[17], we restrict our analysis of the Oregon reform to cover five years before the ban (2003) and until 2014. Our analysis of the Hawaiian reform extends from 2000 to 2020. As of 2017, the CBP changed their policies on censorship of employee bins for establishment counts, which restricted binned analyses to data ranging from 2000 to 2016 in Hawaii (the analysis of the total count of establishments was unaffected). We do not perform binned analyses of the QWI data.

3 Methodology

Using the panel design of the CBP and QWI, we employ a difference-in-differences estimator, namely the Callaway and Sant'Anna difference-in-differences estimator (CSDID).[3] The CSDID addresses the recent literature on two-way fixed effect estimators, as it can accommodate multiple treatment periods, construct conditional parallel trends, and allow for disaggregated group-time treatment effects. The CSDID estimator is suitable for panel data with binary treatments.

For the purposes of this analysis, we favor a "Cross-State" design at the four-digit NAICS-county-year level, which allows us to assess the relative impact of the Hawaii and Oregon noncompete reforms by contrasting the four-digit NAICS industries in the treated states with states that have not instituted any noncompete legislation as of 2020. Where the two analytic strategies for both states differ is in the identification of the quasi-control groups within the treated states as a robustness check for our effect estimates.

The noncompete agreement reform implemented by Hawaii targeted technology workers within the state. Assuming that these workers are more likely to transition into entrepreneurial activities within their industries of occupation, the NAICS industries most likely to be impacted by the Hawaiian policy are technology-focused industries. Using the industry classification from Paytas and Berglund,[20] we define the four-digit NAICS codes with a technological focus and build two distinct industry subsets: (i) technology NAICS identified by Paytas and Berglund,[20] our technology group, and (ii) non-technology NAICS industry sectors where the Hawaii policy is less likely to have had an effect. It is possible that spillover into our non-technology focused industries could occur, and so these distinct industry subsets represent a quasi-treated and quasi-control group. Both samples will be contrasted with the out-of-state industries for the purpose of effect estimates.

4 Results and Conclusion

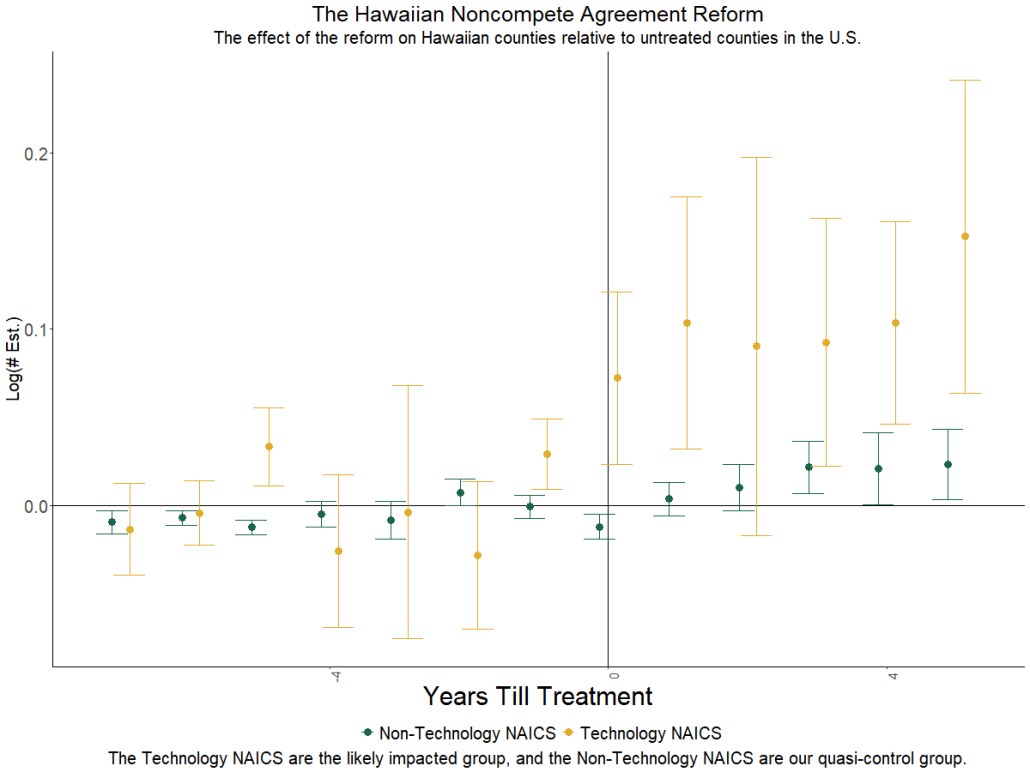
We find evidence that NCAs significantly hinder the entrepreneurial activities of highly-paid knowledge workers, and that curtailing the use of NCAs among such workers boosts establishment formation. While previous work has demonstrated the detrimental effects of NCAs on worker compensation, job match, and cross-firm innovation, we highlight how industry and wage cutoffs to noncompete agreement reforms can lead to significantly different levels of entrepreneurial activity. Table 2 describes the results of each reform on both defined industry subsets within Hawaii and Oregon, as well as the aggregate effect across all reported four-digit NAICS industry codes. The effect on employer establishments is shown in Figures 1 and 2. Tables 4 and 5 break out the effect of noncompete agreement reform across different employer size bins.

The Hawaiian policy leads to a significant increase in the number of employer establishments relative to untreated states, as seen in Table 2. We find that the Hawaiian reform resulted in a 10.2 percent increase in the number of technology establishments following the reform. This increase in technology establishments was paired with a decline in employment among identified technology industries of 9.7 percent. This decrease would be concerning if not for the increase in employment at the aggregate level, implying that any reduction in the work force among identified technology industries is actually a transition to alternative industry types, potentially on the periphery of the identified technology industries. This change is likely an indication that many workers were prevented from taking up jobs they were otherwise interested in as a result of noncompete and co-worker non-solicitation covenants.

Statewide, the number of workers in these occupations has risen modestly in the years since reform. Combined, the findings suggest that Hawaii's noncompetes reform had the dual effect of boosting establishment formation in technology industries while facilitating the diffusion of tech workers and their skills into other sectors of the economy. We validate this by checking for changes in the share of workers in Honolulu who fall under the "Computer and mathematical" major occupational group in the Occupational Employment Statistics

(OES) survey. In 2014, 1.8 percent of total employment in Honolulu was employed under the "Computer and mathematical" tag, and this share of the workforce remained consistent at 1.8 percent in 2015 and 2016. As anticipated, our non-technology set of NAICS industries appear largely unresponsive to the reform, given the targeted nature of the legislation. Statewide, the number of workers in these occupations has risen modestly in the years since reform. Combined, the findings suggest that Hawaii's noncompetes reform had the dual effect of boosting establishment formation in technology industries while facilitating the diffusion of tech workers and their skills into other sectors of the economy.

Figure 1



This figure plots the dynamic effect estimates of the Hawaiian reform to noncompete agreements. The blue lines and dots are the effect estimates of the reform among technology heavy NAICS industries. The red lines and dots are the effect estimates among NAICS industries which were not likely to be impacted by the reform.

The Oregon reform, on the other hand, led to a statistically insignificant 1.4 percent increase in the number of new employer establishments among likely low-wage NAICS industries relative to untreated counties. The Oregon reform does appear to have led to a reduction in employment among likely-low-wage industries when using the CBP, but no significant impact on aggregate employment. Given the timing of the reform lining up with the 2008 recession, and the observed negative effect on likely-high-wage industries, the Oregon reform had little impact on entrepreneurship among the treated population of workers.

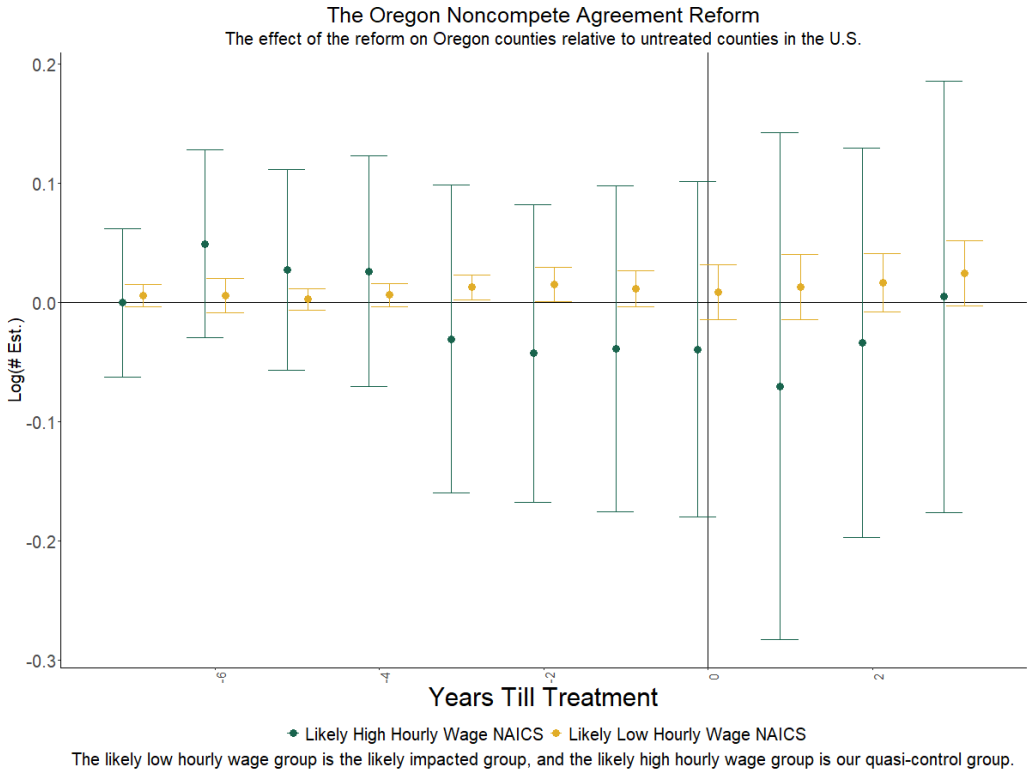
Using the QWI data, we validate the estimated effect on total employment in both states. Table 3, and

Figures 3 and 4, report that the estimated effect of the Hawaiian reform on employment is largely consistent across the CBP and QWI, with a reported negative effect on employment among technology industries but a positive effect on total employment. In Oregon, the QWI contradicts the results of the CBP, finding that the removal of NCAs had no significant effect on employment, likely doing a better job of accounting for the 2008 recession thanks to the increased frequency of data collection.

To test for the validity of the parallel trends assumption, we plot the dynamic treatment effect estimates for both Hawaii and Oregon in Figures 1 and 2. Figure 1 demonstrates that the Hawaiian noncompete agreement reform resulted in an increase in the number of business establishments in the years following the legislation. It appears that the primary increase in business formation occurred in the first two years after the policy was put in place before stabilizing. This is indicative of a stock of nascent entrepreneurs who were previously locked behind NCAs rather than the noncompete agreement reform inducing employees to convert to entrepreneurs. When comparing the technology industries to non-technology industries in Hawaii, we see an indication that the Hawaiian policy was well-targeted, with no clear spillover into non-technology focused industries.

Figure 2 indicates that the Oregon noncompete agreement reform had a substantially smaller effect on entrepreneurial activity relative to the Hawaii reform. These muted impacts on entrepreneurship in Oregon are distinct from the previous work showing that wages increased 2-3 percent on average, with as much as a 14-21 percent increase among noncompete agreement-bound workers. Given the effect estimates of the pretreatment period, it seems likely that the Oregon reform, while reporting significant aggregate estimates, had no significant effect after conditioning on pre-treatment trends. Unfortunately, the Oregon reform did occur during the 2008 recession, which may have limited our capacity to detect new establishment formation which would have occurred during an economic expansion period.

Figure 2



This figure plots the dynamic effect estimates of the Oregon reform to noncompete agreements. The blue lines and dots are the effect estimates of the reform among likely low-wage NAICS industries. The red lines and dots are the effect estimates among likely high-wage NAICS industries.

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Table 1: Descriptive Statistics

Statistic	N	Mean	St.Dev.	Min	Max
		Hawaiian Counties			
Employed	1,248	1,586.62	11,121.81	0	358,309
Employer Estab.	1,248	106.79	699.939	1	22,279
Nonemployer Estab.	792	458.824	2,381.42	3	58,293
Labor Force	4	168,411.80	193,287.40	36,835	455,815
Population	4	302,847.50	384,150.00	58,463	876,156
Urban	4	277,056.20	390,942.90	47,351	862,113
White	4	73,516.00	76,465.63	17,255	186,484
Black	4	5,500.75	10,081.13	177	20,619
Male	4	152,149.50	193,220.30	29,252	440,518
Avg. Household Size	4	2.87	0.086	2.75	2.95
		Oregon Counties			
Employed	9,075	605.58	5,880.57	0	404,018
Employer Estab.	9,075	49.903	397.784	1	24,622
Nonemployer Estab.	4,652	209.52	1,283.93	3	55,781
Labor Force	36	60,722.03	102,437.80	732	476,120
Population	36	95,038.86	143,655.70	1,547	660,486
Urban	36	74,837.33	135,392.90	0	649,010
White	36	82,267.31	118,696.40	1,444	522,825
Black	36	1,546.17	6,237.51	1	37,434
Male	36	47,126.39	71,161.42	782	326,886
Avg. Household Size	36	2.499	0.168	2.19	2.9
		Control Counties			
Employed	374,328	509.807	7,126.58	0	2,051,315
Employer Estab.	374,328	36.147	405.002	1	104,063
Nonemployer Estab.	181,302	187.877	1,633.98	3	220,602
Labor Force	1,723	40,731.82	99,631.77	230	1,174,908
Population	1,723	72,588.21	176,214.00	444	2,465,326
Urban	1,723	53,344.46	172,891.00	0	2,465,326
White	1,723	56,518.92	116,425.90	332	1,200,755
Black	1,723	10,606.15	48,714.10	0	898,350
Male	1,723	35,463.54	84,824.11	224	1,156,446
Avg. Household Size	1,723	2.522	0.174	2	3.87

This table reports a set of descriptive statistics across the counties and industries of interest. All of the reported information comes from 2007, the last year before either of our treated states instituted a reform to noncompete agreements.

Table 2: Effect of Noncompete Agreement Reforms

		Hawaii	
Outcome	Technology-NAICS	Other NAICS	All NAICS
Log(# Est.)	0.102***	0.011*	0.034**
	(0.035)	(0.006)	(0.016)
Log(# Employed)	-0.097***	-0.004	0.018*
	(0.029)	(0.014)	(0.01)
# Est.	3.141***	0.052	129.9***
	(0.247)	(0.084)	(18.985)
# Employed	-75.776***	0.567	2,679.19
	(22.406)	(4.789)	(2013.19)
		Oregon	
Outcome	Likely Low-wage NAICS	Likely High-Wage NAICS	All NAICS
Log(# Est.)	0.014	-0.036	-0.003
	(0.009)	(0.068)	(0.01)
Log(# Employed)	-0.027***	0.042	-0.026
	(0.01)	(0.086)	(0.018)
# Est.	-0.514***	-0.569	-73.315**
	(0.157)	(0.767)	(36.365)
# Employed	-6.197***	-77.583***	-1,258.257*
	(2.45)	(23.911)	(673.06)

Bootstrapped standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01 and standard errors are clustered at the county level. Results for Hawaii cover the years 2000 to 2020 while Oregon's results cover 2003 to 2014.

Table 3: Effect of Noncompete Agreement Reforms, QWI Data

		Hawaii	
Outcome	Technology-NAICS	Other NAICS	All NAICS
Employed #	-97.4***	19.57***	15.37***
	(15.31)	(3.57)	(3.66)
		Oregon	
Outcome	Likely Low-wage NAICS	Likely High-Wage NAICS	All NAICS
Employed #	2.82	12.21	9.45
	(8.12)	(68.51)	(8.43)

Bootstrapped standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01 and standard errors are clustered at the county level. Results for Hawaii cover the years 2000 to 2020 while Oregon's results cover 2003 to 2014.

Table 4: Effect of Noncompete Agreement Reforms – Mutually Exclusive Employer Establishment Bins

	Hawaii	
Outcome	Technology-NAICS	Other NAICS
Log(# Est.)	0.082***	0.01***
	(0.03)	(0.004)
Log(# Est. 1 – 4)	0.146***	0.025***
	(0.055)	(0.006)
Log(# Est. 5 – 9)	0.141	-0.021***
	(0.126)	(0.005)
Log(# Est. 10 – 19)	-0.147	0.002
	(0.172)	(0.005)
Log(# Est. 20 – 49)	-0.204	-0.034***
	(0.13)	(0.013)
Log(# Est. 50 – 99)	-0.041*	-0.057***
	(0.021)	(0.018)
Log(# Est. 100 – 249)	-0.048**	0.046***
	(0.023)	(0.007)
	Oregon	
Outcome	Likely Low-wage NAICS	Likely High-Wage NAICS
Log(# Est.)	0.014	-0.036
	(0.009)	(0.069)
Log(# Est. 1 – 4)	0.027	-0.055
	(0.016)	(0.101)
Log(# Est. 5 – 9)	-0.01	-0.113
	(0.017)	(0.123)
Log(# Est. 10 – 19)	-0.001	0.063
	(0.018)	(0.057)
Log(# Est. 20 – 49)	-0.023	-0.051
	(0.038)	(0.21)
Log(# Est. 50 – 99)	-0.023	-0.103
	(0.034)	(0.204)
Log(# Est. 100 – 249)	0.001	-0.052
	(0.05)	(0.442)

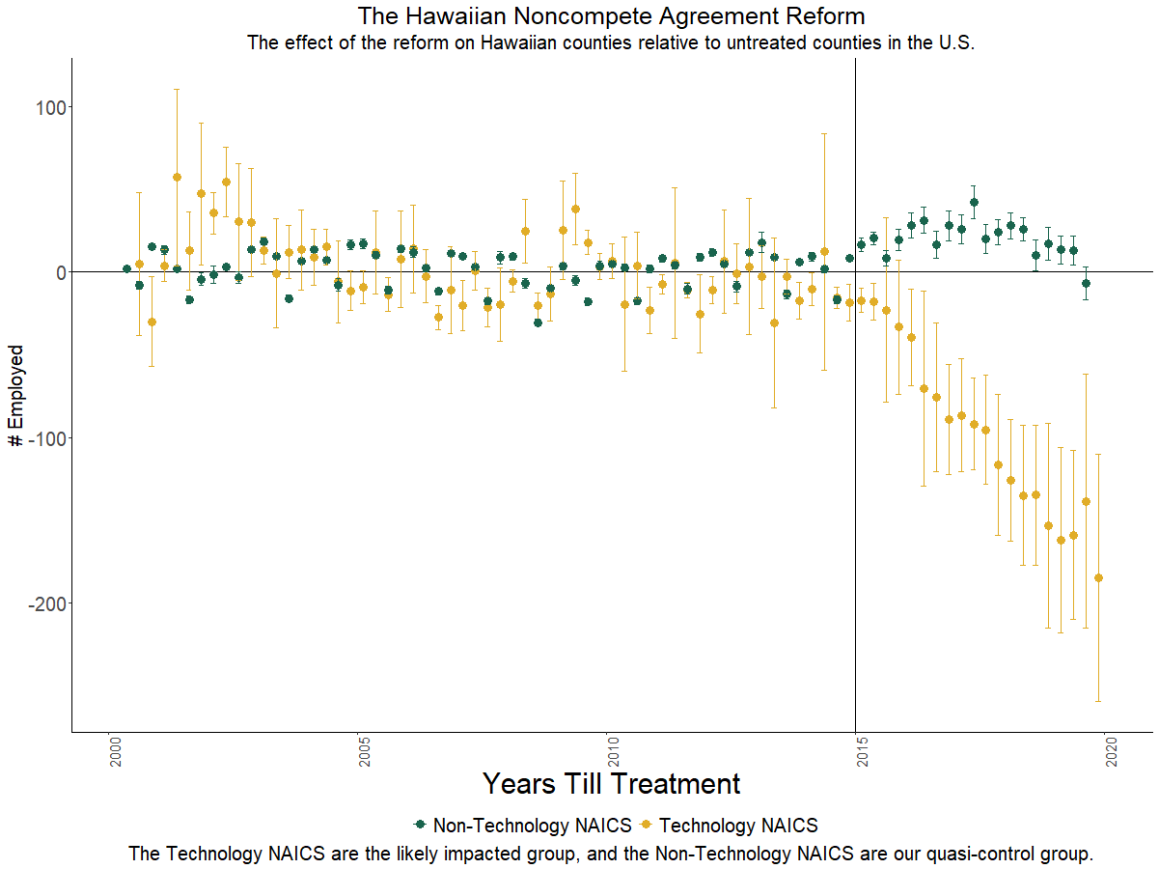
Bootstrapped standard errors in parentheses, * p <0.1, ** p <0.05, *** p <0.01 and standard errors are clustered at the county level. Results for Hawaii cover the years 2000 to 2017 while Oregon's results cover 2003 to 2014.

Table 5: Effect of Noncompete Agreement Reforms – Aggregated Employer Establishment Bins

	Hawaii	
Outcome	Technology-NAICS	Other NAICS
Log(# Est.)	0.082***	0.01***
	(0.028)	(0.004)
Log(# Est. 1 – 4)	0.146***	0.025***
	(0.054)	(0.006)
Log(# Est. 1 – 9)	0.133***	0.01*
	(0.024)	(0.006)
Log(# Est. 1 – 19)	0.068***	0.006
	(0.023)	(0.005)
Log(# Est. 1 – 49)	0.078***	0.012***
	(0.025)	(0.003)
Log(# Est. 1 – 99)	0.088***	0.008*
	(0.021)	(0.004)
Log(# Est. 1 – 249)	0.083***	0.011***
	(0.031)	(0.004)
	Oregon	
Outcome	Likely Low-wage NAICS	Likely High-Wage NAICS
Log(# Est.)	0.014	-0.036
	(0.009)	(0.071)
Log(# Est. 1 – 4)	0.027	-0.055
	(0.018)	(0.098)
Log(# Est. 1 – 9)	-0.01	-0.113
	(0.017)	(0.132)
Log(# Est. 1 – 19)	-0.001	0.063
	(0.018)	(0.054)
Log(# Est. 1 – 49)	-0.023	-0.051
	(0.038)	(0.212)
Log(# Est. 1 – 99)	-0.023	-0.103
	(0.033)	(0.204)
Log(# Est. 1 – 249)	0.001	-0.052
	(0.05)	(0.442)

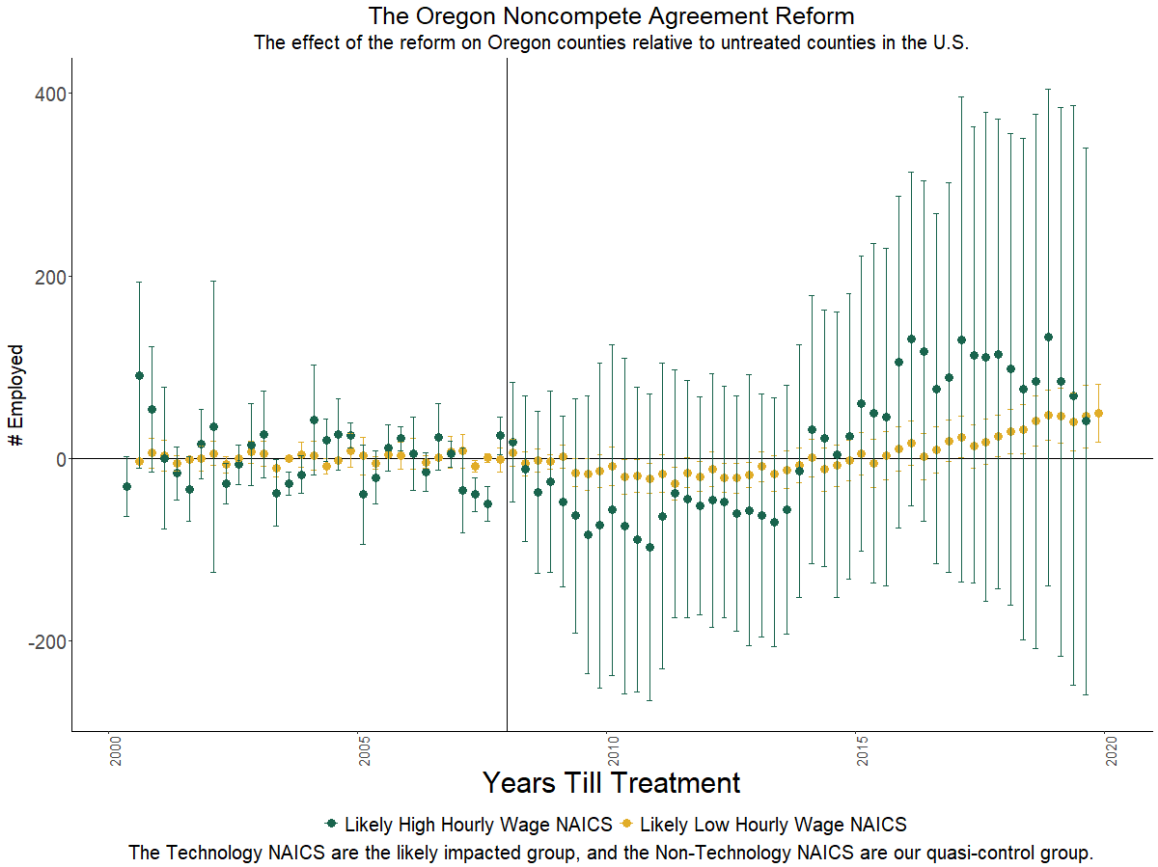
Bootstrapped standard errors in parentheses, * p <0.1, ** p <0.05, *** p <0.01 and standard errors are clustered at the county level. Results for Hawaii cover the years 2000 to 2017 while Oregon's results cover 2003 to 2014.

Figure 3



This figure plots the dynamic effect estimates of the Hawaiian reform to noncompete agreements using QWI data. The blue lines and dots are the effect estimates of the reform among technology heavy NAICS industries. The red lines and dots are the effect estimates among NAICS industries which were not likely to be impacted by the reform.

Figure 4



This figure plots the dynamic effect estimates of the Oregon reform to noncompete agreements using QWI data. The blue lines and dots are the effect estimates of the reform among likely low-wage NAICS industries. The red lines and dots are the effect estimates among likely high-wage NAICS industries.