Association between Physician Noncompete Agreements and Healthcare Access

April 2021

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ABSTRACT

Introduction: Several states have restricted noncompete agreements (NCAs) for physicians to protect public access to medical services and continuity of patient-physician relationships. However, little is known about the effect of these policies on healthcare access.

Methods: We used the U.S. Census Bureau's Longitudinal Business Database (1976 to 2010) to compare changes in measures of healthcare access, in four states that restricted physician NCAs (intervention group) with corresponding changes in 37 states that continued enforcing NCAs (control group).

Results: Relative to the control group, in states that restricted NCAs, the number of practices and practice-locations in a county increased by 3.6% and 3.7%, respectively. Practice employment increased at a rate of 1% per year while the probability of practice closure decreased by 4.9 percentage points.

Public Health Implication: Restricting physician NCAs was associated with an increase in the number of practices and practice-locations in a county and a reduction in the likelihood of practice closure and increase in practice employment.

<u>Registration</u>: Not Applicable

<u>Funding Source</u>: None

Introduction

Access to health services is a key issue in the U.S. Department of Health and Human Services' Healthy People 2030 initiative. In this regard, numerous studies have shown that healthcare access, particularly primary physician supply,[1] and continuity of care,[2-7] are important to overall health outcomes. In this paper, we examine one potential barrier to healthcare access—the legal enforceability of noncompete agreements ("NCAs")—which recent federal reports suggest may impede patient access to care and limit the supply of providers.

NCAs are contractual provisions that prohibit departing employees from working at or starting a competing organization within a specified geographic area for a pre-defined period following cessation of employment. NCAs have become an important concern in healthcare because employed physicians now outnumber self-employed physicians and because nearly half of US physicians are bound by NCAs [8,9]. NCAs are intended to prevent employees from misappropriating investments made by their employers (e.g., in client acquisition), but in studies of executives, inventors and high-tech workers they also have the effect causing workers to leave the state, their industry, and reducing their wages [10-12].

In healthcare, NCAs have consequences not only for physicians but also for patients. In particular, departing physicians bound by NCAs must leave the geographic area or stop practicing their specialty until the NCA expires, which simultaneously reduces patient access to care in the local area and compels existing patients to have to find new care providers. Since 1933, the American Medical Association (AMA) has recognized that NCAs can result in both reduced access to care and disruptions in continuity of care for patients[13,14]. Continuity of care, particularly with the same physician, has been consistently associated with greater quality of healthcare and cost-effectiveness [2-7]. Indeed, the AMA's longstanding position holds that physicians should not enter into covenants that “unreasonably restrict the right of a physician to practice” or “do not make reasonable accommodation for patients’ choice of physician” [15]. During the COVID-19 pandemic, the AMA bolstered its opposition to NCAs [16], writing that “[t]he ethical obligation for physicians to respond and provide care in the face of disaster is fundamental and exists independent of any contractual duty.” The AMA’s concerns have also become a subject of intense media discussion in the last few years [17].

In light of these concerns, over the last four decades, several states have banned or severely restricted NCAs for physicians, including Massachusetts (1977), Colorado (1982), Delaware (1983), Texas (1999), New Mexico and Arkansas (2015), and Rhode Island and New Hampshire (2016) [18]. However, 69% of U.S. states enforce “reasonable” NCAs [19]. One possible reason for these varied policies is the lack of empirical work examining the effect of physician-specific state NCA policies on healthcare access. Such an examination is critical since it has been suggested that permitting NCAs may in fact increase healthcare access and quality [20].

In this study, we analyzed the association between restricting physician NCAs and access to healthcare. Specifically, we examined whether the number of “practice-locations” (physician offices and clinics) and physician “practices” that own these locations, in a county changed after states restricted physician NCAs. We also examined the survival of physician practices and their level of employment. If NCAs have a detrimental effect, then restricting NCAs should improve healthcare access as measured along these dimensions—that is, there would be an increase in the number and survival of physician practices. Although one study has examined how NCAs affect physician compensation [21], to our knowledge, this is the first study to evaluate the association between restricting physician NCAs and healthcare access using large-scale data on multiple natural experiments in which states restricted physicians NCAs.

Methods

Study Design

This study received approval from the U.S. Census Bureau and the Internal Revenue Service to access the Longitudinal Business Database (LBD) for the years 1976 to 2010. (Data for more recent years are not yet available for researcher access.) The data contained no identifying information on the practices or practice-locations. No IRB approvals were required since no individual information was used.

We used a multivariable regression approach which reduced the potential biases from unobserved confounding variables. Specifically, we used a quasi-experimental difference-in-differences with time-trends approach often used to examine large-scale policy changes[22]. Counties in states that restricted NCAs between 1976 and 2010 (the intervention group) were compared on four measures of healthcare access with counties in states that did not restrict NCAs during this period (the control group). We further leveraged the detailed practice-level data available in the LBD and performed a similar comparison for physician practices. This allowed us to test whether any observed increase in the number of practices was merely due to the division of existing practices into smaller practices. For both analyses, we compared changes in time trends and levels of healthcare access in the intervention group before and after the NCA restrictions with the corresponding changes in the control group.

Data

We used the LBD to obtain annual employment and the years of entry and exit for all U.S. business “establishments” with at least 1 employee and in Standard Industrial Code (SIC) 8011 (offices and clinics of doctors of medicine). Each establishment was a single physical location where an organization operated, and thus represented a practice-location. Practices were identified based on ownership linkages among practice-locations, with each practice owning one or more locations [23].

The intervention—the restriction of physician NCAs—is a state policy change that occurred in four states during our study period: Colorado, Delaware, Massachusetts, and Texas. A summary of the changes is discussed in Table S1 in the Supplementary Material. In three states (Colorado, Delaware, and Massachusetts), physician NCAs became legally unenforceable after the restrictions were passed, while in Texas physician NCAs were subject to strong limitations that allowed physicians to buy out of the contract at a reasonable price. Hereinafter, we refer to these four changes collectively as "restricting NCAs".

Measures of healthcare access in these states were compared to those in 37 other states and the District of Columbia, which continued enforcing NCAs during the study period. To ensure comparability among the intervention and control groups, nine states (AL, CA, KY, LA, MI, ND, NJ, OK, and TN) were excluded, because they did not either permit NCAs at all or did not consistently restrict NCAs during the study period. Further, to focus on changes related to the intervention, we limited the analysis to the four-year period before and after the NCA restrictions. The final sample consisted of approximately 2.9 million practice-year observations, corresponding to an underlying 3.1 million practice-location-year observations. These data were aggregated to form 70,000 observations at the county-year-level for county-level analysis of healthcare access. Throughout, we rounded the number of observations to meet U.S. Census Bureau disclosure restrictions.

Statistical Analysis

Dependent variables

Four different dependent variables at two different levels of aggregation—at the county-level and practice-level—were used as measures of healthcare access. The county-level variables provided an aggregated view of healthcare access in a county. Specifically, the (i) natural logarithm of the number of practices, and (ii) natural logarithm of the number of practice-locations in a county were used as the primary aggregate measures of healthcare access in a county in a given year. The logarithmic transformation was used because the distribution of these variables was skewed [24]. The means and standard deviations of these variables are in Table 1.

The two measures at the practice-level were: (i) the logarithm of total employment of a practice in a county in a given year, and (ii) closure of a practice in a county in a given year (a dummy variable set to 1 if a practice ends its presence in a county in a year, and 0 otherwise). Broadly, these two measures assess the tendency of practices to expand, contract, continue or close their practices in a given county and year in response to changes in NCA policies.

These four measures were supplemented with four additional measures to check for consistency between the county-level and practice-level results. These included the log of the 25th and 75th percentiles of practice employment in a county, the logarithm of the average practice age in a county, and the Herfindahl-Hirschman Index (HHI) of competition in a county-year. HHI was computed as the sum of the squared employment shares of each practice in that county-year.

Regression model

For each measure of healthcare access, we evaluated the association between restricting NCAs and healthcare access by estimating three coefficients—*Pre-Trend*, *Level-Shift*, and *Post-Trend*—based on a difference-in-differences regression model with time trends. The exact specification is included in the Supplementary Material. In essence, our difference-in-differences model is equivalent to estimating two separate linear time trends for each measure of healthcare access, one prior to the restriction and one after the restriction, for each of the intervention and control groups, and then taking their difference between the two groups.

In particular, "*Pre-Trend*" is the pre-intervention, between-group time trend difference in the measure of healthcare access, and is calculated as the difference between the slopes of the intervention group and control group during the pre-intervention period. Thus, *Pre-Trend* would be zero if the measure of healthcare access showed a similar time trend in the intervention and control groups before the intervention. A positive *Pre-Trend* would indicate that before restricting NCAs, the measure of healthcare access increased at a faster rate (or decreased at a slower rate) in the intervention group compared with the control group. "*Level-Shift*," is the change in the average between-group difference in the measure of healthcare access in the year of intervention. "*Post-Trend*" is the post-intervention, between-group time trend difference in the measure compared with the pre-intervention between-group time trend difference. *Post-Trend* is calculated as the difference between the slopes of the intervention group and control group time-trends during the post-intervention period relative to the *Pre-Trend* (thus, difference-in-differences). Hence, the difference between the slopes of the intervention group and control group time-trends after the intervention would be the sum of *Pre-Trend* and *Post-Trend*. If the difference in the post-intervention time trends between the intervention and control groups were the same as in the pre-intervention period, then *Post-Trend* would be zero. Alternatively, if *Post-Trend* were positive and *Pre-Trend* were zero, then it indicates that after the intervention, the measure of healthcare access increased faster in the intervention group than in the control group.

We included fixed effects in all our regressions. For county-level measures of healthcare access, we included county fixed effects. Thus, we compared each county in the intervention group with itself before and after NCA restrictions and contrasted these changes with changes in counties in states that did not restrict NCAs. This ensured that we did not confound stable differences across counties for the effect of restricting NCAs by NCAs. Analogously, for the practice-level analyses, we compared each practice with itself over time using practice fixed effects. These fixed effects address potential biases due to unobserved practice-level differences between the intervention and control groups. Finally, to ensure that universal changes such as macroeconomic fluctuations or healthcare law changes, which affected all practices and counties similarly, were not mistaken for the effect of restricting NCAs, we included year fixed effects. Clustered standard errors were estimated, allowing for correlation of errors within states [25]. Significance was set to 0.05 and all tests were two-tailed. We used the Bonferroni-Holm method for multiple comparisons [26]. Stata 14 was used for statistical analyses.

Results

County-Level Analyses

The *Pre-Trend* for the number of practices in a county was not statistically significant (Table 2, row 1), indicating that the trends were similar in both the intervention and control groups before NCAs were restricted in the intervention group. Restricting NCAs was associated with a significant increase in the *Level-Shift*, by about 3.6% (95% CI, 1.8% to 5.4%, p<.001). The *Post-Trend* was close to zero. The results for the number of practice-locations in a county were similar (row 2). The *Level-Shift* was positive; the average level increased by 3.7% (95% CI, 2.1% to 5.3%, p<.001), and the *Pre-Trend* and *Post-Trend* were close to zero. The results are presented graphically in Figure 1. Based on the estimates in Table 2, it plots the time trends for the estimated difference in the number of practices (and practice-locations) between the intervention and control groups. Consistent with the positive *Level-Shift*, Figure 1 shows a large jump in the number of practices and practice-locations in the intervention states shortly after restriction of NCAs.

Practice-Level Analyses

The average number of practices per county was 9.25 ($e^{2.225}$) and the average number of practice-locations was 9.75 ($e^{2.277}$), indicating that most practices operated in one location (Table 1, row 1 and 2). The *Pre-Trend* for (log) practice employment in a county was nearly zero (Table 2, row 3), indicating that before the intervention, average practice employment had a similar trend in both the intervention and control groups. Based on the estimated *Post-Trend*, after restricting NCAs, average practice employment increased in the intervention group relative to the control group at a rate of about 1% per year (95% CI, 0.4% to 1.6%, p<.001). The left panel of Figure 2 presents these results, and shows almost no difference in the time-trend between the two groups before the interventions. In contrast, the difference in average practice employment between the two groups increases after the intervention, suggesting that practices in the intervention group started growing after the intervention. The corresponding *Level-Shift* was close to zero, indicating that there was no immediate increase in the average level of practice employment after restricting NCAs.

Practice closure in a county showed a significant change after restricting NCAs (Table 2, row 4). Pre-intervention, it increased at 3.8 percentage points per year (95% CI, 3.4 to 4.2 percentage points p<.001) in the intervention group relative to the control group (*Pre-Trend*). Post-intervention, there was a decrease of 4.9 percentage points (95% CI, 4.3 to 5.5 percentage points, p<.001) in the corresponding *Level-Shift*. Further, the probability of practices closing their presence in the intervention group showed a decreasing *Post-Trend* of 2.8 percentage points per year (95% CI, 2.4 to 3.2 percentage points, p<.001) relative to the trend before intervention.

Figure 2 (right panel) shows these changes. In the intervention group, the probability of practice closure was increasing before the intervention. It dropped immediately after the intervention, before continuing to increase at a slower rate in the intervention group.

Supplemental Analyses

The results on the supplemental variables at the county level were consistent with the practice-level results. The log of the 25th and 75th percentiles of practice employment showed increasing *Post-Trend* after restricting NCAs. This was consistent with the practice-level results and indicated that the increase in practice employment occurred throughout the practice employment distribution, not just in a few practices. Consistent with the lower probability of practice closure after the intervention, average practice age also showed an increasing *Post-Trend*. Finally, HHI of employment in a county showed a significant decrease in the post-intervention period, which was consistent with the increasing number of practices and practice-locations observed earlier.

Together, these results indicate that after restricting NCAs, practice presence in counties in the four intervention states improved through higher employment and fewer closures.

Discussion

We explored the association between restricting physician NCAs and access to healthcare. Our results indicate that restricting NCAs was associated with a trend towards stabilization and improvement of healthcare access. Specifically, we observed two changes. First, restriction of NCAs was associated with an increase in the number of practices and practice-locations in a county. Second, we observed that employment at practices increased and their closure rates decreased; that is, practices grew larger and survived for a longer period. Together, these results suggest that in states that restricted physician NCAs, after the restriction, patients had a larger choice of healthcare providers. The increase in the number of practices and practice-locations, as well as in the HHI, also suggest that restricting NCAs was associated with greater competition. Although we did not measure healthcare quality, prior studies suggest that greater competition among providers may improve healthcare quality [27].

Our results are likely driven by the two ways that NCAs affect physicians. First, NCAs impede physicians who want to form new practices. Our results show that the number of practices and practice-locations increased after restricting NCAs. This suggests that by facilitating greater physician mobility, restricting NCAs may increase the formation of new practices. This is consistent with evidence from other industries, where researchers have found greater entrepreneurial activity in states that restrict NCAs [28,29]. Second, NCAs can constrain physicians from moving across practices. From a practice's perspective, the mobility constraint associated with an NCA manifests as a hiring barrier, particularly in the same geographic area. Thus, restricting NCAs may make it easier for practices in the same geographic location to recruit physicians and grow. It may also buffer them against closure due to physician shortages, increasing their longevity. This effect is likely to be particularly true for older and smaller practices that need to hire physicians but lack the resources to support NCA-related litigation. Consistent with this, in untabulated analyses, we found that post-intervention increases in practice employment and decreases in the probability of closure were greater for small, older practices than for younger or larger practices.

The magnitudes of the observed associations are not ignorable. To see this, consider the control group excluding Arkansas, New Hampshire, New Mexico and Rhode Island that recently introduced restrictions on NCAs. There were about 126,400 practice-locations in these 33 states and the District of Columbia in 2016, the most recent year for which data are available [30]. Based on the estimate in Table 2 (3.7%), and assuming causality, if these states restricted NCAs, we can expect to see about 4,675 new practice-locations. To put this number in perspective, it is larger than the total number of practice-locations in 24 of the 33 states [30]. Moreover, this is very likely an underestimate of the true impact since the prevalence of NCAs today is likely much higher than in the earlier decades.

The study has two key strengths. First, the unique sample covers all practices in 41 states. This large sample not only allows for county-specific and practice-specific differences to be controlled, but also considerably reduces concerns about the generalizability of the findings. Second, our regression approach is superior to purely cross-sectional approaches in its ability to address confounding variables. Given the infeasibility of randomized clinical trials, this approach offers the most practical alternative to evaluating such large-scale policy changes.

The study nevertheless has several limitations. First, it is an observational study. Our difference-in-differences approach eliminates many potential confounds. However, it does not confirm a causal relationship between restricting NCAs and healthcare access. Second, practice closure showed an increasing trend in the intervention group before the introduction of the law restricting NCAs. Since this trend could have caused the NCA restrictions in the intervention group, we examined the legislative history of the policy changes and contemporaneous news media around the time of the restrictions. They showed no prominent discussions of practice closures, suggesting that the increasing practice closure before the restrictions was not likely related to the policy changes. Nonetheless, there could have been some connections that were not reported in the media or legislative histories. Accordingly, the closure results should be interpreted with caution. Third, we do not use data on individual physicians. Hence, we do not know how many physicians were bound by NCAs. Thus, our estimates are likely to underestimate the true effect. Fourth, our study does not directly measure continuity of care. Future work may address this gap directly. Fifth, the NCA restrictions studied here occurred several years ago. Even though there are a few states that recently changed their policies, the last year that the rich Census Bureau micro-data are available is 2010. Nonetheless, the fundamental issues surrounding physician NCAs have remained virtually unchanged over several decades [13]. So, we believe future work on these more recent changes will corroborate our analyses. Finally, the study did not evaluate if there were inter-regional differences. Future research can investigate if the impact of NCAs is higher in regions with physician shortages.

PUBLIC HEALTH IMPLICATIONS

Our study offers timely evidence that is relevant to the recent policy debate on NCAs in healthcare. To our knowledge, this is the first study that shows that restricting NCA enforceability for physicians is associated with an increase in local healthcare access. An earlier study[1] has highlighted the significance of physician availability on mortality, particularly in regions with income inequalities. Our results show that following restrictions on enforcing physician NCAs, the number of physician practices and practice-locations in a county increased relative to counties in states that continued enforcing physician NCAs. Further, after NCAs were restricted, practices were more likely to survive and grow larger relative to practices in states that continued enforcing physician NCAs. More broadly, considering the rising demand for healthcare and the increasing prevalence of physician-employees [9], our results suggest that it may be the right time to evaluate restricting physician NCAs as a means to increasing healthcare access.

Table 1: Mean and Standard Deviation of Dependent Variables

	Mean	**Std. Dev.**
County-Level Sample **(**N=70,000)		
(1) Log Number of Practices in County	2.225	1.653
(2) Log Number of Practice-Locations in County	2.277	1.665
(3) Log of 25th Percentile of Employment	1.311	0.553
(4) Log of 75th Percentile of Employment	2.191	0.620
(5) Log Average Practice Age (Years)	1.800	0.721
(6) Herfindahl-Hirschman Index of Employment	0.205	0.358
Practice-Level Sample (N=2,900,000)		
(7) Log Practice Employment in County	1.699	0.843
(8) Probability of Practice Closure in County	0.071	0.257

Note: Number of observations rounded to meet U.S. Census Bureau disclosure requirements.

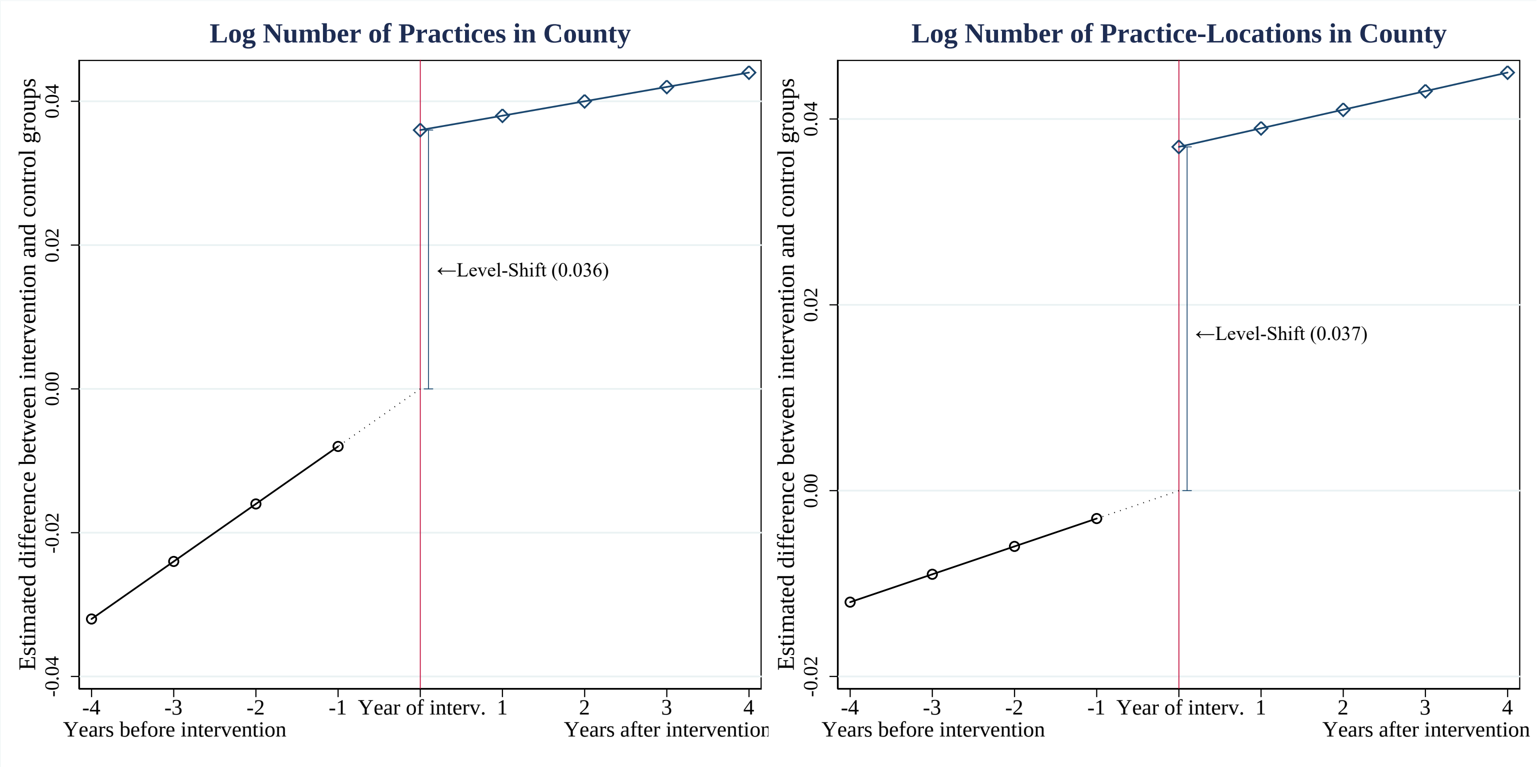
Table 2: Difference-in-Differences Estimates of Changes in Measures of Healthcare Access

	(1)		(2)		(3)		
	Pre-intervention Trend (*Pre-Trend*)	**Standard Error**	***Level-Shift***	**Standard Error**	**Post-intervention Trend Relative to Pre-trend (*Post-Trend*)**	**Standard Error**	R^2
County-level analyses							
(1) Log Number of Practices in County	0.008	0.004	0.036*	0.009	-0.006	0.006	0.95
(2) Log Number of Practice-Locations in County	0.003	0.003	0.037*	0.008	-0.001	0.007	0.95
Practice-level analyses							
(3) Log Employment in County	0.001	0.001	-0.000	0.005	0.010*	0.003	0.82
(4) Probability of Closure in County	0.038*	0.002	-0.049*	0.003	-0.028*	0.002	0.20
Supplemental county-level analyses							
(5) Log of 25th Percentile of Employment	-0.009	0.006	-0.027	0.022	0.016*	0.007	0.46
(6) Log of 75th Percentile of Employment	-0.004	0.004	-0.010	0.016	0.021*	0.007	0.58
(7) Log Average Practice Age	-0.009*	0.002	0.002	0.017	0.022*	0.004	0.79
(8) Herfindahl-Hirschman Index of Employment	0.005	0.007	-0.052*	0.007	0.001	0.008	0.64

Notes: All county-level analyses include county and year fixed effects; all practice-level analyses include practice and year fixed effects; clustered standard errors estimated after allowing for correlation of errors within states. *Significant at the 5% level, Bonferroni-Holm corrected. Number of observations rounded to meet U.S. Census Bureau disclosure requirements.

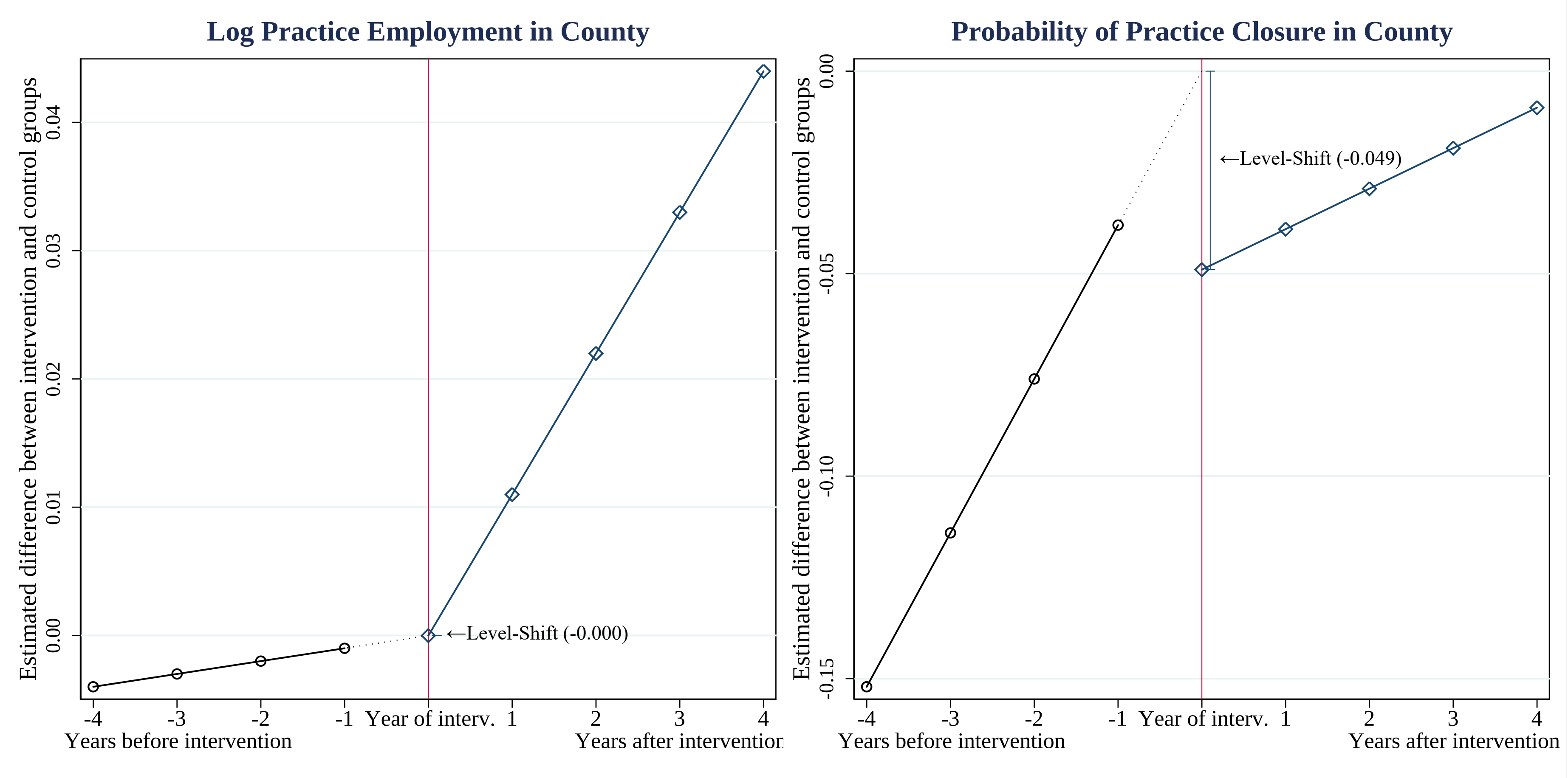
Pre-Trend: Pre-intervention, between-group time trend difference in the measure of healthcare access. *Level-Shift*: Change in the average level of the between-group difference in the measure of healthcare access after the intervention. *Post-Trend*: Post-intervention, between-group time trend difference in the measure compared with the pre-intervention between-group time trend difference.

Figure 1: Changes in Number of Practices and Practices in a County



Notes: Each graph presents the estimated difference in the respective dependent variable between the intervention group in four states (CO, DE, MA, TX) and the control group in the other 37 states and District of Columbia, based on the regression estimates in Table 2. The slope before the intervention is *Pre-Trend*, and the slope after the intervention is *Pre-Trend* + *Post-Trend*.

Figure 2: Changes in Practice Size and Survival in a County



Notes: Each graph presents the estimated difference in the respective dependent variable between the intervention group in four states (CO, DE, MA, TX) and the control group in the other 37 states and District of Columbia, based on the regression estimates in Table 2. The slope before the intervention is *Pre-Trend*, and the slope after the intervention is *Pre-Trend* + *Post-Trend*.

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Supplementary Material

Exhibit S1: Complete Ban or Severe Restriction on NCAs

State	Date	Statute: Impact on NCAs
Colorado	April 6, 1982	§ 8-2-113 (3): Complete ban
Delaware	July 13, 1983	6 Del. C. § 2707: Complete ban
Massachusetts	November 23, 1977	General Laws Chapter 112, §12X: Complete ban
Texas	September 1, 1999	Bus. & Com. Code §15.50-0.52, part (b). Severely limits enforceability of physician NCAs[a]

[a] The Texas law ensures that departing physicians have access to patient lists and medical records, that physicians will not be prohibited from providing "continuing care and treatment" and that the NCA must include a buyout provision at a reasonable price. Thus, though the law change in Texas is not a ban, it is a severe restriction that allows physicians to continue to treat prior patients if needed, along the lines expressed in AMA Code of Medical Ethics Opinion 11.2.3.1.(8)

Estimated Regression Specification

For county-level measures of healthcare access, we estimated the following specification using a sample of observations at the county-year level:

$$Y = \alpha.t + \beta.D_{post} + \gamma.D_{post}.t + \text{county fixed effects} + \text{year fixed effects}$$

where Y is a measure of healthcare access. D_{post} is 1 for a county in a state after the NCAs were restricted, and zero when NCAs were not restricted. For a county in a state that did not change NCAs, D_{post} is zero for all years. "t" is the number of years before and after the restriction for a county in a state that restricted NCAs, and 0 for a county in other states for all years. Thus, α corresponds to "Pre-Trend", β to "Level-Shift" and γ to "Post-Trend"**.**

A similar specification was used for practice-level measures of healthcare access using a sample of observations at the practice-year level. County fixed effects were replaced with practice fixed effects in those specifications.