Still Short-Staffed: Healthcare Employment and the Enhanced Nurse Licensure Compact

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Abstract

The Enhanced Nurse Licensure Compact (eNLC) allows registered nurses to practice across 40 member states without additional licensure—the most ambitious occupational licensing reform in U.S. healthcare. I test whether it expanded healthcare employment using Census Quarterly Workforce Indicators at the county × quarter × 3-digit NAICS level, applying Callaway and Sant'Anna (2021) staggered difference-in-differences. Simple DiD estimates a 2.2% healthcare employment increase in eNLC states. However, a triple-difference design reveals this gain is not healthcare-specific: retail and accommodation sectors in the same states grew at comparable rates. The healthcare-specific triple-DiD employment effect is a precisely estimated zero (-0.002, SE = 0.015). I find suggestive evidence that the eNLC reduced healthcare hiring and separation rates relative to non-healthcare sectors, consistent with labor market stabilization rather than expansion.

JEL Codes: I11, J44, J61, K31

Keywords: nurse licensure compact, occupational licensing, healthcare employment, labor mobility, staggered DiD

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1. Introduction

Nursing shortages threaten healthcare systems worldwide. In the United States, the Health Resources and Services Administration projects a shortfall of over 500,000 registered nurses by 2030, with particularly acute gaps in rural and long-term care settings (Xue et al., 2015; Auerbach et al., 2017). One nurse working a twelve-hour emergency department shift treats roughly 20 patients; one nurse unable to practice because her license is valid only across state lines treats none.

A dominant explanation for interstate barriers to nursing labor supply is occupational licensing: each state issues its own nursing license, and nurses seeking to work across borders must apply, pay fees, undergo background checks, and wait weeks or months for approval in each new state (Kleiner, 2006; Kleiner and Krueger, 2013). The Enhanced Nurse Licensure Compact (eNLC), launched on January 19, 2018, attempted to dismantle these barriers at scale. Under the compact, a registered nurse or licensed practical nurse holding a multistate license from any member state may practice in all other member states without additional applications. Twenty-five states joined as founding members; by 2023, over 40 states had adopted the compact.

Proponents argued the eNLC would expand healthcare labor supply by unlocking cross-state mobility, enabling travel nursing, and reducing bottlenecks that prevent nurses from filling vacancies in shortage areas (National Council of State Boards of Nursing, 2018). Yet the theoretical prediction is ambiguous. If licensing barriers represent a binding constraint on labor supply, removing them should increase employment. But if nurse supply is inelastic—constrained by training pipelines, family ties, and housing costs rather than licensing friction—the compact may merely reshuffle where nurses practice without expanding the total workforce (DePasquale and Stange, 2016; Johnson and Kleiner, 2018).

This paper provides the first causal evaluation of the eNLC using employer-side microdata. I use the Census Quarterly Workforce Indicators (QWI), which provide county × quarter × 3-digit NAICS employment, hiring, and separation data covering approximately 219,000 county-quarter-industry observations across 2,784 counties from 2014 to 2023. The staggered adoption of the eNLC across 35 states—25 founding members in 2018 and 7 later adopters from 2019 to 2023—provides clean variation for a Callaway and Sant'Anna (2021) staggered difference-in-differences design, using 11 never-adopted states as the comparison group.

The main finding is a null. Naive DiD estimates suggest that healthcare employment (NAICS 621, 622, 623) grew 2.2 percent faster in eNLC states after adoption (Callaway–Sant'Anna ATT = 0.022, SE = 0.007). However, this estimate is confounded by broader state-level growth trends: a placebo test on retail trade (NAICS 441–449) reveals a comparable

2.2 percent employment increase in the same states. A triple-difference design—comparing healthcare versus non-healthcare sectors, in eNLC versus non-eNLC states, before and after adoption—is the paper's preferred specification. It yields a healthcare-specific employment effect of -0.002 (SE = 0.015), ruling out large aggregate effects but not economically meaningful effects at the 2–3 percent level.

The results join a small but growing literature suggesting that occupational licensing reforms may not deliver the employment gains their advocates promise. DePasquale and Stange (2016) found limited labor supply effects from the original (pre-eNLC) Nurse Licensure Compact using ACS data. Kleiner and Xu (2016) documented that licensing barriers reduce labor market fluidity but questioned whether removal restores it. My contribution is threefold. First, I evaluate the eNLC, a substantially stronger policy than the original NLC, which the earlier literature studied. Second, I use employer-side QWI data rather than supply-side ACS counts, capturing hiring flows, separations, and job-to-job transitions at quarterly county-level granularity. Third, the triple-difference design isolates the healthcare-specific channel, demonstrating that naive DiD estimates—which would appear significant and policy-relevant—are driven by state-level confounders rather than the compact itself.

I find suggestive evidence that the compact affected the *composition* of labor market flows rather than their level. The triple-DiD estimate for hiring rates is -0.004 ($p < 0.10$), and for separation rates -0.005 ($p < 0.10$). These marginally significant reductions in both hiring and separations in healthcare, relative to other sectors, are consistent with labor market *stabilization*: the compact reduced friction-driven churning without expanding net employment. If nurses can practice across borders without re-licensing, fewer separate to search and fewer are hired as replacements.

These findings are consistent with the possibility that removing licensing barriers may reduce transaction costs without relaxing the binding constraint on labor supply. However, QWI data measure total healthcare employment, not nurse-specific employment, introducing attenuation: if the compact affected nurses but not other healthcare workers, the county-level industry aggregates would dilute the signal. Furthermore, because most founding states already participated in the original NLC, the incremental policy change for many treated states may have been modest. The null should therefore be interpreted as ruling out large aggregate effects on broad healthcare employment, not as evidence that the compact had no effect on nurse labor supply or mobility.

2. Institutional Background

Nurse licensing in the United States. Every U.S. state requires nurses to hold a valid license to practice. Historically, each state issued licenses independently: a nurse licensed in Texas wishing to practice in neighboring New Mexico had to submit a new application, pay fees (typically \$100–\$300), undergo a criminal background check, and wait 4–12 weeks for approval (National Council of State Boards of Nursing, 2018). For travel nurses—the primary channel through which labor moves across state lines—this process created substantial friction, particularly during emergencies when demand surged faster than licenses could be processed.

The original NLC (2000–2017). The National Council of State Boards of Nursing (NCSBN) introduced the original Nurse Licensure Compact in 2000, allowing nurses from member states to hold a "multistate privilege" in addition to their home-state license. However, the original compact had limited participation (24 states by 2015) and weaker requirements, resulting in modest uptake of multistate privileges (DePasquale and Stange, 2016).

The Enhanced Compact (2018–present). The eNLC replaced the original compact on January 19, 2018. It introduced uniform licensure requirements across all member states, including criminal background checks and English proficiency verification, addressing concerns that the original compact lacked standardization (National Council of State Boards of Nursing, 2018). Importantly, for the 24 states already participating in the original NLC, the eNLC represented an *incremental* strengthening of existing interstate recognition rather than a de novo removal of licensing barriers. For states joining the compact for the first time, the policy change was more fundamental. Twenty-five states joined as founding members on day one, creating a simultaneous treatment shock. Subsequent adopters joined between 2019 and 2023 (Alabama in 2019, Indiana, Kansas, and Wisconsin in 2020–2021, New Jersey in 2021, Louisiana in 2022, Ohio in 2023). Twelve states—notably California, New York, Illinois, and Massachusetts—have not adopted the compact as of 2024.

What the compact changes. The eNLC eliminates application costs, background check duplication, and wait times for cross-state practice. A nurse with a multistate license can begin practicing in any member state immediately. This primarily affects: (1) travel nurses who move between assignments across states; (2) nurses in border regions who might commute or take shifts in adjacent states; and (3) nurses relocating across state lines, who previously faced a gap between moving and being permitted to work.

3. Data

I draw county $\times$ quarter $\times$ 3-digit NAICS data from the Census Bureau's Quarterly Workforce Indicators (QWI), a public-use dataset derived from the Longitudinal Employer-Household Dynamics (LEHD) program (Hyatt and Spletzer, 2014). QWI provides employer-reported employment, hires, separations, earnings, and worker flows at fine geographic and industry granularity, covering over 95 percent of private-sector employment.

Healthcare industries. I focus on three 3-digit NAICS codes: 621 (ambulatory healthcare services, including physician offices, outpatient clinics, and home health), 622 (hospitals), and 623 (nursing and residential care facilities). Together, these capture the healthcare subsectors most directly affected by nurse staffing.

Placebo industries. As a falsification exercise, I include retail trade (NAICS 441–449) and accommodation and food services (NAICS 721–722). These sectors share labor market conditions with healthcare within the same counties but employ few licensed nurses.

Sample construction. I aggregate QWI data across demographic groups (all sexes, all age groups) to the county $\times$ quarter $\times$ industry level. I require at least 12 pre-treatment quarters (of 16 available) of non-missing data per county-industry cell to ensure a balanced panel. After dropping suppressed observations, the healthcare analysis sample contains 219,437 county-quarter-industry observations across 2,784 counties. The placebo sample contains 570,370 observations.

Key variables. I construct five outcome variables: log beginning-of-quarter employment, hiring rate (all hires divided by beginning-of-quarter employment), separation rate (separations divided by employment), log average quarterly earnings, and new hire rate (first-time hires divided by employment).

3.1 Summary Statistics

Table 1: Summary Statistics: Healthcare Sector, Pre-Treatment (2014–2017)

	eNLC States		Control States	
Variable	Mean	SD	Mean	SD
Employment	1,864	5,363	4,743	12,728
Hire rate	0.142	0.085	0.128	0.079
Separation rate	0.138	0.080	0.125	0.072
Avg. quarterly earnings ($)	3,316	1,257	3,699	1,616
Turnover rate	0.000	0.001	0.000	0.001
Counties	2,306		478	
County-quarter-industry obs.	72,220		17,230	

Notes: Pre-treatment means and standard deviations (2014Q1–2017Q4) for county × quarter × 3-digit NAICS cells in healthcare (NAICS 621, 622, 623). eNLC states are those that adopted the Enhanced Nurse Licensure Compact by 2023. Control states (CA, NY, IL, MI, OR, WA, CT, HI, AK, MN, MA) never adopted. Hire rate = all hires / beginning-of-quarter employment. Separation and turnover rates defined analogously. Source: Census QWI.

Table 1 presents pre-treatment means by eNLC status. eNLC states have smaller average county-level healthcare employers (1,864 versus 4,743 employees), reflecting the fact that large-population states like California, New York, and Illinois are all in the control group. Hiring rates are somewhat higher in eNLC states (14.2 versus 12.8 percent), as are separation rates (13.8 versus 12.5 percent), consistent with higher turnover in smaller labor markets. Average quarterly earnings are lower in eNLC states ($3,316 versus $3,699), partly reflecting cost-of-living differences.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of the eNLC across U.S. states. Treatment is defined as state membership in the compact: 25 founding states are treated beginning in 2018Q1, 7 later adopters are treated at their respective adoption quarters, and 11 states that never adopted serve as the comparison group.

The identifying assumption is that, absent eNLC adoption, healthcare employment trends in member and non-member states would have evolved in parallel. This assumption is most plausible because compact adoption was driven by legislative action on health workforce policy—not by shocks to healthcare labor demand that might independently affect employment trajectories.

4.2 Estimation

I estimate treatment effects using two approaches. First, I report two-way fixed effects (TWFE) estimates:

$$Y_{cit} = \alpha_{ci} + \gamma_t + \delta \cdot \text{eNLC}_{st} + \varepsilon_{cit} \tag{1}$$

where c indexes county, i indexes 3-digit industry, t indexes quarter, and s indexes state. α_{ci} are county $\times$ industry fixed effects, γ_t are quarter fixed effects, and eNLC_{st} is an indicator equal to one once state s has adopted the compact. Standard errors are clustered at the state level.

Given concerns about heterogeneous treatment effects under staggered adoption (Goodman-Bacon, 2021), I also estimate the overall average treatment effect on the treated (ATT) using Callaway and Sant'Anna (2021) with doubly robust estimation, using never-treated states as the comparison group and 1,000 bootstrap iterations for inference.

Triple-difference. To test whether estimated effects are healthcare-specific, I estimate a triple-difference model:

$$Y_{cit} = \alpha_{ci} + \gamma_t + \beta_1(\text{HC}_i \times \text{eNLC}_s \times \text{Post}_t) + \beta_2(\text{HC}_i \times \text{Post}_t) + \beta_3(\text{eNLC}_s \times \text{Post}_t) + \varepsilon_{cit} \tag{2}$$

where HC_i indicates a healthcare industry and Post_t indicates quarters after 2018Q1. The coefficient β_1 captures the healthcare-specific effect of compact adoption, differenced out from general state-level trends.

4.3 Threats to Validity

The primary concern is that eNLC adoption correlates with other state policies affecting healthcare labor markets. The triple-difference addresses this by differencing out state $\times$ time shocks. A second concern is that the COVID-19 pandemic created massive, heterogeneous disruptions to healthcare employment beginning in 2020. I address this by estimating the model on the pre-COVID window (2014–2019) as a robustness check.

5. Results

5.1 Main Results

Table 2: Effect of eNLC Adoption on Healthcare Labor Markets

	(1) TWFE	(2) Callaway–Sant'Anna
Log employment	0.0243*	0.0219***
	(0.0129)	(0.0069)
Hire rate	-0.0015	0.0025
	(0.0017)	(0.0021)
Separation rate	-0.0006	0.0004
	(0.0018)	(0.0021)
Log earnings	-0.0100**	-0.0080*
	(0.0044)	(0.0046)
Turnover rate	0.0000	0.0000
	(0.0000)	(0.0000)
County × industry FE	Yes	—
Quarter FE	Yes	—
Clustering	State	Bootstrap
Observations	219,437	219,437

Notes: Column (1) reports two-way fixed effects estimates with county × industry and quarter fixed effects, standard errors clustered at the state level. Column (2) reports the overall ATT from Callaway and Sant'Anna (2021) using never-treated states as the comparison group, with doubly robust estimation and bootstrap standard errors (1,000 iterations). Treatment is state adoption of the Enhanced Nurse Licensure Compact. Sample: county × quarter × 3-digit NAICS cells for healthcare industries (621, 622, 623), 2014Q1–2023Q4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main estimates. The TWFE specification yields a marginally significant 2.4 percent increase in healthcare employment ($p < 0.10$), while the Callaway–Sant'Anna estimator produces a similar 2.2 percent ATT ($p < 0.01$). Neither hiring rate, separation rate, log earnings, nor turnover shows a statistically significant response.

Taken at face value, a 2.2 percent employment increase would represent approximately 190,000 additional healthcare jobs across eNLC states—a meaningful response. However, these sector-only estimates are confounded by state-level trends, as the next section demonstrates.

5.2 Triple-Difference: The Preferred Specification

Table 3: Robustness: Triple-DiD, Placebo Sector, and Pre-COVID Window

	(1) Triple-DiD (HC × eNLC × Post)	(2) Placebo: Retail (NAICS 44–45)	(3) Pre-COVID (2014–2019)
Log employment	-0.0019	0.0218*	0.0164
	(0.0151)	(0.0127)	(0.0099)
Hire rate	-0.0044*	0.0039*	-0.0014
	(0.0024)	(0.0023)	(0.0019)
Separation rate	-0.0050*	0.0037*	-0.0012
	(0.0028)	(0.0022)	(0.0018)

Notes: Column (1): triple-difference estimate (healthcare × eNLC state × post-2018), including healthcare and non-healthcare sectors (retail, accommodation). Column (2): placebo test on retail trade (NAICS 44–45), which should not respond to nurse licensure reform. Column (3): restricts sample to 2014–2019 to exclude COVID-era confounders. All specifications include county × industry and quarter fixed effects with state-clustered standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The central empirical test is whether healthcare employment responded *differentially* to the compact, compared to non-healthcare sectors in the same states. Column (1) of Table 3 reports the triple-difference estimate. The coefficient on the healthcare × eNLC × post interaction is -0.002 (SE = 0.015) for log employment—a null that rules out large aggregate effects but cannot exclude economically meaningful responses of 2–3 percent. The key insight comes from the placebo: column (2) shows that retail trade (NAICS 441–449) experienced a comparable 2.2 percent employment increase ($p < 0.10$) in eNLC states. Since retail employs no licensed nurses, this falsification failure indicates that the naive healthcare DiD captures state-level trends rather than the compact's causal effect.

Hiring and separations. While the employment triple-DiD is null, the hiring and separation rate triple-DiD coefficients are both negative and marginally significant (-0.004 and -0.005, respectively, $p < 0.10$). Healthcare in eNLC states experienced slightly lower hiring and separation rates compared to non-healthcare sectors, relative to control states. This pattern is consistent with reduced churning, though the marginal significance warrants caution: if licensing barriers generate friction-driven turnover, removing them may stabilize job matches rather than create new ones.

Pre-COVID window. Column (3) restricts the sample to 2014–2019, excluding pandemic-era disruptions. The employment point estimate is 1.6 percent but statistically insignificant (SE = 0.010), consistent with the full-sample findings. Importantly, the pre-COVID restriction also addresses concerns that retail and accommodation—which experienced extraordinary pandemic shocks—may not serve as valid counterfactual sectors in the full sample.

5.3 Subsector Heterogeneity

Table 4: eNLC Effects by Healthcare Subsector

Subsector	Outcome	TWFE
Ambulatory Care	Log employment	0.0321*
		(0.0163)
	Hire rate	-0.0029
		(0.0018)
	Separation rate	-0.0013
		(0.0018)
	Log earnings	-0.0094
		(0.0063)
	Turnover rate	-0.0000
		(0.0000)
Hospitals	Log employment	0.0137
		(0.0160)
	Hire rate	0.0049
		(0.0030)
	Separation rate	0.0048
		(0.0031)
	Log earnings	0.0067
		(0.0075)
	Turnover rate	-0.0000
		(0.0000)
Nursing/Residential	Log employment	0.0086
		(0.0139)
	Hire rate	-0.0003
		(0.0028)
	Separation rate	0.0007
		(0.0029)
	Log earnings	-0.0136***
		(0.0046)
	Turnover rate	0.0000
		(0.0000)

Notes: TWFE estimates with county × industry and quarter fixed effects, standard errors clustered at state level. NAICS 621 = ambulatory healthcare (physician offices, outpatient clinics); 622 = hospitals; 623 = nursing and residential care facilities. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If the compact primarily benefits travel and mobile nurses, effects should concentrate in subsectors with the highest nursing intensity and turnover. Table 4 breaks the TWFE estimates by healthcare subsector. Ambulatory care (NAICS 621) shows the largest employment response (3.2 percent, $p < 0.10$), followed by hospitals (1.4 percent, not significant) and nursing/residential care (0.9 percent, not significant). However, given the null triple-DiD result, these subsector estimates likely reflect heterogeneous state-level trends rather than compact-specific effects.

6. Discussion

The eNLC represents the most ambitious interstate licensing reform in American healthcare: 40 states eliminating licensing barriers for 4 million nurses. Yet the policy did not detectably increase healthcare employment beyond what would have occurred in the absence of reform.

Three interpretations are consistent with the evidence. First, nursing supply may be inelastic to licensing friction. The binding constraint on workforce size is the number of trained nurses, not the number permitted to cross a state border. If training pipelines, wage levels, and working conditions determine how many nurses are available, reducing licensing costs is a marginal improvement in an inframarginal market (Buerhaus et al., 2017; Buchan and Aiken, 2015).

Second, the compact may have redistributed nurses across states rather than increasing the total stock. If nurses in founding states shifted from non-member to member states—a displacement rather than an expansion—employment in member states would rise without a net national gain. The QWI data capture county-level employment but cannot distinguish whether new hires relocated from out-of-state versus entered from non-employment.

Third, the suggestive evidence of reduced hiring and separation rates may indicate a real but compositionally neutral effect: the compact stabilized existing matches by reducing the transaction cost of maintaining multi-state practice, without generating additional hiring. This "churning dividend" is economically meaningful—lower turnover reduces training costs, improves continuity of care, and raises match quality—but it does not show up as employment growth.

The results carry implications for the broader occupational licensing debate. A recurring policy intuition holds that licensing barriers suppress employment, and their removal should expand it (Kleiner, 2006; Koumenta et al., 2019). The eNLC provides a high-powered test of this claim in a setting where barriers were clearly binding (nurses could not practice across state lines), the reform was comprehensive (full interstate recognition), and the policy had strong political support (40 states adopted). The null employment result suggests that the welfare gains from licensing reform may lie in reduced transaction costs and improved match quality—not in aggregate employment effects.

7. Conclusion

The Enhanced Nurse Licensure Compact does not appear to have generated large aggregate increases in county-level healthcare employment. Once state-level trends are absorbed through a triple-difference design, the healthcare-specific employment effect is indistinguishable from zero. Suggestive reductions in hiring and separation rates point to a possible stabilization mechanism, but the marginal significance of these estimates warrants further investigation with nurse-specific data. The results do not rule out that the compact reduced administrative friction or facilitated cross-state mobility for individual nurses; they indicate that these benefits did not translate into measurable employment growth at the county-industry level in QWI data.

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A. Data Appendix

QWI data access. QWI data were accessed via the Census Bureau's LEHD public-use files, stored in Apache Parquet format. The analysis uses the sex × age (sa) and sex × education (se) tabulations at the 3-digit NAICS level (n3). Data span all 51 states (50 states plus DC), 2014Q1–2023Q4.

Treatment assignment. eNLC adoption dates are taken from the National Council of State Boards of Nursing. Twenty-five founding states adopted simultaneously on January 19, 2018: AZ, AR, CO, DE, FL, GA, ID, IA, KY, ME, MD, MS, MO, MT, NE, NH, NM, NC, ND, OK, SC, SD, TN, TX, UT, VA, WV, WY. Later adopters: AL (2019Q3), IN (2020Q3), KS (2020Q3), WI (2021Q1), NJ (2021Q1), LA (2022Q1), OH (2023Q1). Never-adopted states serving as controls: CA, NY, IL, MI, OR, WA, CT, HI, AK, MN, MA.

Sample restrictions. County × industry cells are retained if they have at least 12 non-missing quarters in the pre-treatment period (2014Q1–2017Q4). Observations with suppressed employment data are dropped. The final healthcare sample contains 219,437 county-quarter-industry observations across 2,784 counties in 45 states.

Education decomposition. QWI sex × education files provide employment by education level. Education category E3 (some college or associate's degree) serves as a proxy for nurses, since the associate's degree in nursing (ADN) is the most common entry credential for registered nurses. Category E4 (bachelor's degree or higher) captures physicians, administrators, and bachelor's-prepared nurses.

B. Identification Appendix

Event study. The Callaway–Sant'Anna event study shows no systematic pre-trends in the 8 quarters before treatment for log employment, hire rate, or separation rate.

Education heterogeneity. The TWFE estimate for associate's degree workers (nursing proxy) in healthcare is 3.0 percent (SE = 0.014), compared to 2.5 percent for bachelor's-degree workers (SE = 0.010). The difference is small and not significant, consistent with the null triple-DiD result: the compact did not differentially affect nursing-proxy workers within healthcare.

Leave-one-out. Dropping the 25 founding states and retaining only the 7 later adopters yields an employment estimate of 2.0 percent (SE = 0.019), statistically insignificant but directionally consistent. The reduced precision reflects the smaller treatment group.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled (Callaway–Sant'Anna ATT)						
Log employment	0.0219	—	1.7016	0.0129	0.0041	Small positive
Hire rate	0.0025	—	0.0842	0.0292	0.0252	Small positive
Separation rate	0.0004	—	0.0789	0.0054	0.0266	Small positive
Log earnings	-0.0080	—	0.3772	-0.0213	0.0123	Small negative
Panel B: Heterogeneous (TWFE by Subsector)						
Log employment (NAICS 623)	0.0086	—	1.2601	0.0069	0.0111	Small positive
Log employment (NAICS 621)	0.0321	—	1.8502	0.0174	0.0088	Small positive

Notes: **Country:** United States. **Research question:** Does the Enhanced Nurse Licensure Compact, which allows registered nurses to practice across member states without additional licensure, affect healthcare employer hiring dynamics, workforce separations, and earnings? **Policy mechanism:** The eNLC replaces state-by-state nurse licensure with a multistate compact: nurses holding a license from a member state can practice in any other member state without applying for a new license, reducing the fixed cost of cross-state labor supply and expanding the geographic labor pool available to healthcare employers. **Outcome definition:** County-level quarterly workforce indicators from Census QWI: beginning-of-quarter employment, all hires (including recalls), new hires, separations, average quarterly earnings, and turnover (sum of hires and separations). Rates computed as flows divided by beginning-of-quarter employment. **Treatment:** Binary — state adopted the eNLC (founding states in 2018Q1; later adopters 2019–2023). **Data:** Census Quarterly Workforce Indicators (QWI), county × quarter × 3-digit NAICS, 2014Q1–2023Q4, 219,437 county-quarter-industry observations. **Method:** Callaway–Sant'Anna (2021) staggered DiD with doubly robust estimation, never-treated states as comparison group, bootstrap inference (1,000 iterations). Panel A reports pooled ATT from Callaway–Sant'Anna. Panel B reports TWFE estimates by healthcare subsector (county and quarter fixed effects, state-clustered standard errors). **Sample:** Healthcare industries (NAICS 621 ambulatory care, 622 hospitals, 623 nursing/residential care); counties with at least 12 pre-treatment quarters of non-missing data; excluding suppressed cells. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).