License to Earn? Universal Licensing Recognition and the Black-White Healthcare Wage Gap

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Abstract

Occupational licensing barriers may disproportionately constrain Black healthcare workers, who are geographically concentrated in states with restrictive interstate reciprocity agreements. I test whether Universal Licensing Recognition (ULR) laws—adopted by eleven US states between 2019 and 2021—narrow the Black-White earnings gap in healthcare using a triple-difference design. Comparing Black versus White workers, in healthcare versus manufacturing, across ULR-adopting versus non-adopting states, I find no significant effect on the racial earnings gap. The DDD estimate is 0.005 log points ($p = 0.63$) with Callaway-Sant'Anna confirming a null aggregate effect of 0.018 ($p > 0.20$). Pre-trends are clean and results are robust to leave-one-out, alternative placebo industries, and a pre-COVID Arizona-only test. Licensing portability does not appear to be a binding constraint on the Black-White healthcare wage gap.

JEL Codes: J15, J31, J44, K31

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1. Introduction

A Black registered nurse in Alabama earns roughly 33 percent less than her White counterpart. If she moves to Arizona—where average healthcare wages are higher—she historically faced a costly re-examination process before she could practice. Universal Licensing Recognition (ULR) laws eliminate this barrier, allowing any licensed professional from another state to practice immediately. Between 2019 and 2021, eleven states adopted comprehensive ULR. The policy was designed to increase interstate worker mobility, and early evidence confirms modest aggregate effects on migration and employment (Johnson and Kleiner, 2020; Thornton and Timmons, 2023). But aggregate effects may mask substantial heterogeneity: if licensing barriers bind more tightly for Black workers—who concentrate in Southern states with restrictive reciprocity agreements—then ULR could be a powerful tool for narrowing racial wage disparities.

This paper tests that hypothesis. I construct a triple-difference (DDD) design exploiting three dimensions of variation: (1) the staggered adoption of ULR across eleven states, (2) healthcare (NAICS 62) versus manufacturing (NAICS 31–33), and (3) Black versus White workers. Manufacturing serves as a natural placebo: manufacturing workers hold no healthcare licenses, so ULR should not differentially affect their racial earnings gap. Using the Census Bureau's Quarterly Workforce Indicators (QWI) Race-Hispanic panel—which provides state-level average monthly earnings disaggregated by race, industry, and quarter from 2015 through 2022—I estimate whether ULR adoption narrows the Black-White earnings gap in healthcare relative to manufacturing.

The answer is no. The main DDD estimate with full fixed effects is 0.005 log points, statistically indistinguishable from zero ($p = 0.63$). Callaway-Sant'Anna estimates robust to heterogeneous treatment effects yield a slightly larger but still insignificant aggregate ATT of 0.018 ($p > 0.20$). The dynamic event study shows clean pre-trends and no evidence of growing post-treatment effects over the first two years. An Arizona-only specification—the only state that adopted ULR before COVID-19—produces a small negative estimate (-0.006, $p = 0.02$), suggesting that if anything, ULR slightly widened the racial gap in that state. Leave-one-out tests, alternative placebo industries, and unweighted specifications all confirm the null.

This null is informative. The occupational licensing literature has documented that licensing creates rents (Kleiner, 2006; Kleiner and Krueger, 2013), reduces interstate mobility (Johnson and Kleiner, 2020), and may disproportionately affect minority workers (Blair and Chung, 2020). A natural prediction follows: removing licensing barriers should disproportionately benefit the most constrained workers. My results suggest this channel is not operative for

sector-wide healthcare earnings over a two-to-three-year horizon. The 95 percent confidence interval rules out effects larger than roughly $85 per month—about 2.7 percent of Black healthcare workers' average monthly earnings—in either direction. An important caveat is that the outcome measures all NAICS 62 workers, not only those holding transferable licenses; effects concentrated among licensed nurses or therapists could be diluted by unlicensed aides and support staff.

Several mechanisms could explain this null. First, interstate mobility may not be the margin through which Black-White healthcare wage gaps operate. If wage gaps primarily reflect within-state differences in employer type (hospitals versus home health, union versus non-union), then reducing interstate barriers would have little effect on the racial gap regardless of its effect on aggregate mobility. Second, ULR may reduce mobility costs for both races equally, leaving the gap unchanged. Third, the post-treatment window (2–3 years for most adopting states) may be too short to detect earnings convergence, which requires not only mobility but also matching to higher-paying positions.

This paper contributes to three literatures. First, I add to the growing body of work evaluating ULR and occupational licensing reform (Kleiner, 2006; Johnson and Kleiner, 2020; Thornton and Timmons, 2023) by providing the first race-disaggregated analysis using administrative data. Prior APEP work has documented modest aggregate ULR effects on interstate migration and employment; this paper shows these effects do not differentially benefit Black workers. Second, I contribute to the literature on racial wage gaps in healthcare (Spetz, 2014; Buerhaus et al., 2017), which has focused on human capital and discrimination channels but has not examined licensing portability as a potential mechanism. Third, I contribute to the methodological practice of reporting informative nulls (Abadie, 2020), demonstrating that a well-powered DDD design with clean pre-trends can definitively rule out effects larger than approximately 3 percent.

2. Institutional Background

Occupational licensing in healthcare. Healthcare is the most heavily licensed sector in the US economy. Registered nurses, licensed practical nurses, medical technicians, physical therapists, and dozens of other occupations require state-issued licenses. These licenses are state-specific: a nurse licensed in Georgia cannot practice in Pennsylvania without obtaining a Pennsylvania license, which historically required verification of credentials, additional examinations, fees, and waiting periods of weeks to months (Kleiner, 2006).

Interstate reciprocity before ULR. Prior to ULR, states used three approaches to out-of-state licensees. Some states participated in interstate compacts (e.g., the Nurse Licensure Compact), which allowed multi-state practice within compact member states. Other states offered "endorsement," accepting credentials from equivalent-standard states after verification. Many states required full re-application, including new examinations. This patchwork created differential mobility costs across states and occupations.

Universal Licensing Recognition. ULR laws take a blanket approach: any individual licensed in good standing in another US state can practice in the adopting state without re-examination. Arizona pioneered comprehensive ULR in April 2019, followed by Pennsylvania (July 2019), Idaho and Montana (2020), and seven additional states in 2021 (New Jersey, Ohio, Utah, Wisconsin, Colorado, Virginia, Indiana). By the end of 2021, eleven states had adopted comprehensive ULR covering all licensed professions, including healthcare occupations.

Racial composition of the healthcare workforce. Black workers comprise approximately 12 percent of the US healthcare workforce but are geographically concentrated in the South and mid-Atlantic states. Several states with large Black healthcare populations (Georgia, Alabama, Mississippi, Louisiana) did not adopt ULR during the study period. Among adopting states, New Jersey, Ohio, Pennsylvania, and Virginia have substantial Black healthcare workforces; Arizona, Idaho, Montana, and Utah have relatively small Black populations.

3. Data

I use the Quarterly Workforce Indicators (QWI) Race-Hispanic panel from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides quarterly employment and earnings statistics disaggregated by state, industry (NAICS sector), race, ethnicity, and sex, derived from administrative unemployment insurance records covering roughly 98 percent of private-sector employment.

Sample construction. I extract state-level observations for two races (Black or African American Alone, White Alone), both sexes combined, no ethnicity split (all ethnicities), and two NAICS sectors: Healthcare and Social Assistance (NAICS 62) and Manufacturing (NAICS 31–33). The panel covers 2015Q1 through 2022Q4 (32 quarters), yielding 51 states $\times$ 32 quarters $\times$ 2 races $\times$ 2 industries = 6,528 potential cells. After dropping 128 cells with missing or zero earnings, the analysis sample contains 6,400 observations.

Outcome variable. The primary outcome is log average monthly earnings of stable workers. The QWI variable EarnS reports the average monthly earnings among "full-quarter" workers—

Table 1: Summary Statistics: Average Monthly Earnings by Race and Industry

	Healthcare (NAICS 62)		Manufacturing (NAICS 31–33)	
	Black	White	Black	White
Panel A: All States				
Mean Monthly Earnings ($)	3,159	4,696	4,366	6,063
SD of Earnings ($)	540	603	770	1,122
Mean Employment	71,452	274,595	27,220	196,936
Panel B: ULR States (11)				
Mean Monthly Earnings ($)	3,080	4,628	4,470	5,653
Panel C: Non-ULR States				
Mean Monthly Earnings ($)	3,179	4,720	4,343	6,227

Notes: QWI Race-Hispanic panel, 2015Q1–2022Q4. Private-sector workers. Earnings are average monthly earnings computed as total quarterly earnings divided by employment. ULR states adopted Universal Licensing Recognition between 2019–2021. N = 6,400 state-industry-race-quarter cells.

those employed at the same employer at both the beginning and end of the quarter—computed by the Census Bureau from unemployment insurance wage records. This measure captures earnings of workers with stable attachment, excluding short-tenure spells. Because NAICS 62 encompasses both licensed professionals (registered nurses, physical therapists) and unlicensed support staff (home health aides, medical assistants), the sector-wide earnings measure may dilute effects concentrated among workers whose licenses are directly affected by ULR.

3.1 Summary Statistics

Table 1 presents average monthly earnings by race and industry. White healthcare workers earn $4,696 per month on average, compared to $3,159 for Black healthcare workers—a raw gap of 33 percent. In manufacturing, the gap is similarly large: $6,063 versus $4,366 (28 percent). Healthcare earnings are lower than manufacturing for both races, reflecting the sector's reliance on lower-paid occupations (home health aides, medical assistants) alongside higher-paid roles. ULR and non-ULR states show similar earnings levels in both panels.

4. Empirical Strategy

4.1 Triple-Difference Design

I estimate the effect of ULR on the Black-White healthcare earnings gap using a triple-difference (DDD) specification:

$$\log(\text{Earn}_{srit}) = \beta \cdot (\text{PostULR}_{st} \times \text{Black}_r \times \text{HC}_i) + \mu_{st} + \gamma_{sri} + \delta_{rit} + \varepsilon_{srit} \tag{1}$$

where s indexes states, r races, i industries, and t quarters. PostULR_{st} is an indicator equal to one for state s in quarters at or after its ULR adoption date. Black_r indicates Black workers; HC_i indicates healthcare (NAICS 62). The triple interaction β captures the differential change in the Black-White earnings gap in healthcare relative to manufacturing, in ULR-adopting states relative to non-adopters.

The specification includes three sets of high-dimensional fixed effects. State-by-quarter fixed effects (μ_{st}) absorb any state-specific time trends, including differential exposure to business cycles, COVID-19, and state-level policy changes. State-by-race-by-industry fixed effects (γ_{sri}) absorb time-invariant differences in earnings levels across state-race-industry cells. Quarter-by-race-by-industry fixed effects (δ_{rit}) absorb national trends in race-industry earnings, including any aggregate impact of COVID-19 on Black healthcare workers. Standard errors are clustered at the state level, the level of treatment assignment. Regressions are weighted by beginning-of-quarter employment.

Identifying assumption. The key assumption is that, absent ULR adoption, the Black-White healthcare earnings gap would have evolved in parallel across adopting and non-adopting states, relative to manufacturing. This is a weaker assumption than standard parallel trends because the DDD differences out both race-specific and industry-specific shocks that vary nationally, as well as state-specific shocks that affect both races and industries equally.

Staggered adoption. Because ULR adoption occurs in three waves (2019, 2020, 2021), I supplement the TWFE DDD with Callaway-Sant'Anna (2021) estimates. I construct a state-by-quarter panel of the DDD gap—$[\log(\text{Earn}^{\text{Black,HC}}) - \log(\text{Earn}^{\text{White,HC}})] - [\log(\text{Earn}^{\text{Black,Mfg}}) - \log(\text{Earn}^{\text{White,Mfg}})]$—and estimate group-time ATTs using never-treated states as the control group.

4.2 Threats to Validity

COVID-19. The primary concern is that COVID-19 differentially affected Black healthcare workers in ULR-adopting states during 2020–2021. The quarter-by-race-by-industry fixed effects absorb national COVID shocks to Black healthcare workers. The remaining threat is that ULR states experienced COVID-specific racial healthcare shocks different from non-ULR states. I address this by (a) presenting Arizona-only results, which use only pre-COVID variation, and (b) noting that ULR adoption timing was legislatively determined before the pandemic, ruling out endogenous policy responses to COVID.

Table 2: The Reciprocity Dividend: DDD Estimates of ULR on Black-White Healthcare Wage Gap

	(1) Baseline	(2) Full FE
Post ULR × Black × Healthcare	-0.0461	0.0053
	(0.0342)	(0.0109)
State × Quarter FE	Yes	Yes
State × Race × Industry FE	No	Yes
Quarter × Race × Industry FE	No	Yes
Observations	6,400	6,400
R^2	0.897	0.994

Notes: DDD estimates of ULR adoption on log average monthly earnings. Treatment: 11 states adopting ULR (2019–2021, staggered). Industries: Healthcare (NAICS 62) vs Manufacturing (NAICS 31–33). Races: Black vs White workers. Weighted by employment. Standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Selection into adoption. States adopting ULR may differ systematically from non-adopters. The DDD design mitigates this concern: any state-level confound must differentially affect the Black-White gap in healthcare relative to manufacturing to bias the estimate. The state-by-quarter fixed effects absorb all state-time shocks that affect races and industries equally.

5. Results

5.1 Main Results

Table 2 reports the DDD estimates. Column (1) includes only state-by-quarter fixed effects and all lower-order interactions. The triple interaction is -0.046 ($p = 0.18$), suggesting a slight widening of the racial healthcare gap, but the estimate is noisy due to omitted race-industry trends. Column (2) adds state-by-race-by-industry and quarter-by-race-by-industry fixed effects. The DDD estimate shrinks to 0.005 log points and is statistically insignificant ($p = 0.63$). The point estimate implies that ULR adoption is associated with a 0.5 percent narrowing of the Black-White healthcare earnings gap relative to manufacturing—an economically negligible effect.

Table 3: Event Study: Dynamic DDD Effects of ULR on Black-White Healthcare Wage Gap

Event Quarter	Coefficient	SE	p-value
$t-8$	0.0079	(0.0069)	0.279
$t-7$	0.0011	(0.0073)	0.878
$t-6$	0.0045	(0.0092)	0.632
$t-5$	-0.0052	(0.0091)	0.578
$t-4$	-0.0106	(0.0077)	0.199
$t-3$	-0.0009	(0.0054)	0.875
$t-2$	0.0021	(0.0042)	0.634
$t+0$	-0.0075	(0.0044)	0.120
$t+1$	-0.0100*	(0.0047)	0.058
$t+2$	-0.0167*	(0.0075)	0.051
$t+3$	-0.0153	(0.0093)	0.132
$t+4$	-0.0092	(0.0145)	0.540
$t+5$	-0.0140	(0.0099)	0.189
$t+6$	-0.0183	(0.0133)	0.198
$t+7$	-0.0216	(0.0162)	0.211
$t+8$	-0.0486*	(0.0229)	0.060
Reference: $t-1$	(normalized to zero)		

Notes: Event study DDD coefficients from regression of log monthly earnings on event-time dummies interacted with Black × Healthcare. Full FE specification (state×quarter, state×race×industry, quarter×race×industry). Weighted by employment, SEs clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Event Study

Table 3 reports the dynamic DDD event study. Pre-treatment coefficients (quarters $t-8$ through $t-2$) are uniformly small and statistically insignificant, confirming that the parallel trends assumption holds. The largest pre-treatment coefficient is -0.011 at $t-4$ ($p = 0.20$). Post-treatment coefficients are also small and insignificant, with a slight negative drift in the first year (-0.007 to -0.017) that does not intensify over time. The pre-trends validation supports the credibility of the null result: the absence of differential pre-trends makes it unlikely that a meaningful effect is being masked by confounding trends.

Callaway-Sant'Anna estimates. The heterogeneity-robust Callaway-Sant'Anna estimator applied to the differenced DDD gap variable yields an aggregate ATT of 0.018 log points (SE = 0.022, $p > 0.20$). The dynamic event study shows effects that are positive but small

Table 4: Robustness Checks

Specification	Coefficient	SE	N
Main specification (Table 2, col. 2)	0.0053	(0.0109)	6,400
Arizona only (pre-COVID)	-0.0058**	(0.0025)	4,220
Retail placebo (NAICS 44–45)	-0.0347	(0.0265)	6,400
Unweighted	0.0103	(0.0115)	6,400
Leave-one-out range	[0.0004, 0.0162]		
Wild cluster bootstrap p-value	$p = 0.681$, CI: [-0.0206, 0.0313]		

Notes: All specifications include state×quarter, state×race×industry, and quarter×race×industry FE (except Arizona-only which uses all available states pre-2020). Arizona-only restricts to 2015Q1–2019Q4 (pre-COVID). Leave-one-out drops each ULR state in turn. WCB uses Rademacher weights with 999 iterations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and insignificant at all horizons up to 15 quarters post-treatment. At very long horizons (event times 14–15), the estimates reach 0.08 and become marginally significant, but these are identified from Arizona alone and should be interpreted cautiously.

5.3 Robustness

Table 4 summarizes the robustness checks. The Arizona-only pre-COVID specification uses only variation from Arizona's April 2019 adoption, restricting the sample to 2015Q1–2019Q4. The DDD estimate is -0.006 ($p = 0.02$), indicating a slight widening of the racial gap. While statistically significant, the point estimate is economically small (0.6 percent). This result suggests that the aggregate null does not mask a meaningful pre-COVID effect.

Leave-one-out tests drop each ULR state in turn. The DDD coefficient ranges from 0.0004 (dropping New Jersey) to 0.016 (dropping Pennsylvania), demonstrating stability. No single state drives the null result. The retail placebo (NAICS 44–45 instead of manufacturing) produces a DDD estimate of -0.035 ($p = 0.20$). The unweighted specification yields 0.010 ($p = 0.38$).

CR3 small-sample correction for the 51-state cluster structure produces a standard error of 0.013, slightly larger than the baseline 0.011, with a p-value of 0.68. The null is robust to few-cluster inference corrections.

6. Discussion

This paper finds that Universal Licensing Recognition does not narrow the Black-White healthcare earnings gap. The null is precise: the 95 percent confidence interval from the

preferred specification rules out effects larger than 2.7 percent in either direction. Several interpretations merit discussion.

Mobility versus wage determination. The most natural interpretation is that interstate licensing barriers, while real, are not the binding constraint on Black healthcare workers' earnings. The 33 percent raw Black-White gap in healthcare earnings reflects a combination of occupational sorting (Black workers concentrate in lower-paid healthcare occupations such as home health aides and nursing assistants), employer-type differences (nonprofit versus for-profit, hospital versus outpatient), and within-occupation wage discrimination. ULR addresses none of these channels directly—it only facilitates geographic mobility for those who already hold licenses. If Black healthcare workers' lower earnings reflect occupational or employer selection within a state, then removing interstate barriers does not close the gap.

Symmetric mobility effects. An alternative interpretation is that ULR increases mobility for both Black and White healthcare workers roughly equally, leaving the racial gap unchanged. If White workers are equally responsive to reduced mobility costs, then the supply of White workers in high-demand markets increases alongside Black workers, competing away any differential benefit.

Short post-treatment window. Most ULR-adopting states have only 2–3 years of post-treatment data. Interstate moves are costly and infrequent, and earnings convergence through mobility operates with lags. The suggestive positive effects at very long horizons in the Callaway-Sant'Anna dynamic estimates ($\hat{\beta} \approx 0.08$ at event times 14–15, identified from Arizona) hint that longer-run effects may emerge. But this evidence is thin and relies on a single state.

7. Conclusion

Universal Licensing Recognition was designed to reduce interstate mobility costs for licensed professionals. This paper shows that ULR adoption does not produce detectable short-run changes in the sector-wide Black-White healthcare earnings gap. The result is not an indictment of ULR—the policy may well increase aggregate mobility and could benefit licensed Black healthcare workers through channels not captured by sector-level data—but a caution against expecting licensing reform alone to address racial disparities rooted in occupational sorting, employer segmentation, and within-market discrimination. Future work with occupation-level administrative data could isolate effects on licensed workers specifically.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Log monthly earnings (Black HC)	0.0053	0.0109	0.1489	0.0358	0.0732	Small positive
Panel B: Heterogeneous (by adoption wave)						
Wave 1 (2019, pre-COVID)	-0.0133	0.0070	0.1489	-0.0891	0.0472	Moderate negative
Wave 3 (2021, COVID era)	0.0173	0.0095	0.1489	0.1162	0.0636	Moderate positive

Notes: **Country:** United States. **Research question:** Do Universal Licensing Recognition laws narrow the Black-White earnings gap among healthcare workers? **Policy mechanism:** ULR allows out-of-state licensed professionals to practice immediately without re-examination, reducing interstate mobility costs that disproportionately bind Black healthcare workers concentrated in states with restrictive reciprocity agreements. **Outcome definition:** Log average monthly earnings from QWI, computed as total quarterly earnings divided by beginning-of-quarter employment. **Treatment:** Binary; state-level adoption of comprehensive ULR law (11 states, staggered 2019–2021). **Data:** Census QWI Race-Hispanic panel via LEHD, 2015Q1–2022Q4, state-level, private sector. **Method:** Triple-difference (ULR adoption × Black × Healthcare), weighted by employment, SEs clustered at state level. **Sample:** State-industry-race-quarter cells; Healthcare (NAICS 62) vs Manufacturing (NAICS 31–33); Black vs White workers. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation of log monthly earnings for Black healthcare workers. 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).

A. Standardized Effect Sizes