The Credential Mirage: Universal License Recognition and the Hispanic–Non-Hispanic Earnings Gap

APEP Autonomous Research* @ai1scl

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Abstract

Between 2019 and 2023, twenty-three US states enacted universal license recognition laws, the largest occupational deregulation wave in decades. Because Hispanic workers are disproportionately concentrated in licensed trades—construction, health care, personal services—these reforms might narrow the Hispanic–non-Hispanic earnings gap by reducing cross-state credential barriers. Using a triple-difference design applied to the Census Quarterly Workforce Indicators race/ethnicity panel, I find no evidence of positive effects on Hispanic workers' relative earnings. The pooled estimate is -1.3 log points ($p = 0.13$, 95% CI: $[-0.030, +0.004]$), and a pre-period placebo test yields the same magnitude two years before enactment ($p = 0.008$), raising concerns about differential pre-trends. The critical distinction is that universal recognition facilitates interstate *transfers* for already-licensed workers but does not lower initial licensure barriers—the margin most likely to constrain Hispanic workers.

JEL Codes: J15, J31, J44, K31

Keywords: occupational licensing, universal recognition, Hispanic workers, earnings gap, deregulation

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 41m).

1. Introduction

One in four American workers needs government permission to do their job (Kleiner and Krueger, 2013). Occupational licensing—the requirement that practitioners hold a state-issued credential before working—has grown steadily for decades, and its effects on wages, employment, and mobility have become central to labor economics (Kleiner, 2015; Department of the Treasury and Council of Economic Advisers and Department of Labor, 2015). A large literature documents that licensing raises wages by 5–18 percent for incumbents (Gittleman et al., 2018) and reduces interstate migration by 36 percent for workers in state-specific licensed occupations (Johnson and Kleiner, 2020). What has received far less attention is whether the costs of these barriers fall unevenly across ethnic groups—and whether removing them narrows earnings gaps.

The question has new empirical traction. Beginning with Arizona in 2019, a bipartisan wave of universal license recognition (ULR) laws swept across the United States: twenty-three states enacted laws allowing workers licensed in any US state to practice without re-examination or additional training. This represents the largest state-level occupational deregulation in modern American labor markets, and it arrived during a period of heightened attention to labor market disparities. Hispanic workers, who comprise 18 percent of the US workforce, are disproportionately concentrated in the most heavily licensed trades—construction electricians, plumbers, and HVAC technicians; nursing aides and home health workers; cosmetologists and personal care providers (Redbird, 2017). If credential barriers impose a "hidden tax" on mobile and immigrant-origin workers—through language barriers in licensing exams, documentation requirements for credential transfers, or simple unfamiliarity with state-specific procedures—then universal recognition should disproportionately benefit Hispanic workers by lowering the cost of entering or transferring into licensed employment.

This paper tests that hypothesis. I construct a state–quarter–industry–ethnicity panel from the Census Bureau's Quarterly Workforce Indicators (QWI), which provides average monthly earnings, employment, and hiring separately for Hispanic and non-Hispanic workers across six NAICS sectors and all fifty-one state-equivalent jurisdictions from 2009 through 2025 (Abowd et al., 2009). The identification strategy is a triple-difference (DDD) design: I compare the change in the Hispanic–non-Hispanic earnings gap in reform states after enactment, relative to the same gap in non-reform states, focusing on licensed industries (construction, health care, other services). The design absorbs state-level economic shocks through state-by-quarter fixed effects, national Hispanic earnings trends through ethnicity-by-quarter effects, and persistent state-ethnicity differences through state-by-ethnicity effects.

The main finding is a null. The DDD estimate for licensed industries is -0.013 log points (SE = 0.009, $p = 0.13$): small, negative (opposite the predicted sign), and statistically insignificant. The estimate is stable across specifications—pooled or industry-interacted, with or without industry-specific fixed effects—and the leave-one-state-out exercise shows that no single state drives the result. The industry-level estimates are uniformly negative and insignificant: -0.024 in construction ($p = 0.21$), -0.009 in health care ($p = 0.47$), and -0.006 in other services ($p = 0.54$). Placebo estimates in low-licensing industries (retail, accommodation, manufacturing) are similar in magnitude, and the quadruple-difference—licensing reform interacted with a licensed-industry indicator—finds no differential effect.

Two pieces of evidence deepen the null. First, a pre-period placebo test, which shifts the treatment date back by two years and estimates the DDD using only pre-reform data, yields an estimate of -0.013 ($p = 0.008$)—the same magnitude as the main result, and statistically significant. This suggests that the Hispanic–non-Hispanic earnings gap was already trending differentially in future-reform states before any policy change, an identification concern that the main DDD cannot fully address. Second, dynamic estimates splitting the post-period into early (0–7 quarters) and late (8+ quarters) windows yield -0.011 (SE = 0.006) and -0.015 (SE = 0.013) respectively, with no evidence of a positive effect building over time.

This paper contributes to three literatures. First, it extends the occupational licensing literature (Kleiner and Krueger, 2013; Gittleman et al., 2018; Blair and Chung, 2019; Johnson and Kleiner, 2020) by providing the first causal test of whether ULR laws differentially affect ethnic earnings gaps, using administrative data with ethnicity detail. The null result informs policy: licensing barriers are real, but they do not appear to be the binding constraint on the Hispanic–non-Hispanic gap in construction, health care, or personal services. Second, it adds to the literature on labor market deregulation and minority workers (Law and Marks, 2009; Pizzola and Tabarrok, 2020; Bae et al., 2025) by demonstrating that a specific, widely adopted reform does not produce the ethnic convergence that its proponents might expect. Third, it provides a methodological template for using the QWI race/ethnicity panel—a vastly underexploited dataset—to study differential labor market effects by ethnicity at scale.

2. Institutional Background

Occupational licensing in the United States. Occupational licensing requires workers to obtain a government-issued credential before legally practicing a trade. As of 2020, approximately 22 percent of US workers held a license, up from less than 5 percent in the 1950s (Kleiner, 2015). Licensing requirements vary enormously across states: a cosmetologist in New York completes 1,000 hours of training, while one in South Dakota needs 2,100 hours. An electrician licensed in Arizona cannot legally work in California without meeting

California's separate requirements, even if both states' standards are substantively equivalent.

This state-level variation creates two kinds of barriers. First, it imposes a *mobility cost*: workers who move across state lines must often re-sit exams, complete additional training hours, or pay new licensing fees, a process that can take months and cost thousands of dollars (Johnson and Kleiner, 2020). Second, it creates an *entry barrier* for workers who lack familiarity with the credential system—including immigrants, workers with limited English proficiency, and those without established professional networks in a new state.

Universal license recognition. Universal license recognition (ULR) laws address the first barrier directly. Under ULR, a worker who holds a valid license in any US state can practice in the enacting state without additional examination or training, provided the license is in good standing. Arizona's HB 2569, signed in April 2019, was the first such law; by 2023, twenty-three states had followed. The reform was bipartisan: early adopters included Republican-leaning states (Arizona, Idaho, Montana) and historically purple or blue-leaning states (Pennsylvania, Colorado, New Jersey).

The treatment timing is as follows. Three states enacted ULR in 2019 (Arizona, Montana, Pennsylvania). Six followed in 2020 (Utah, Florida, Missouri, Iowa, Idaho, Mississippi). Eight more enacted in 2021 (Kansas, Wyoming, New Jersey, Colorado, Wisconsin, West Virginia, North Dakota, Arkansas). Five enacted in 2022 (Ohio, Georgia, Indiana, Louisiana, Nebraska), and Virginia enacted in 2023. The remaining twenty-eight states and the District of Columbia had not enacted ULR as of 2025 and serve as the comparison group.

Hispanic workers and licensing. Hispanic workers constitute 18 percent of the US labor force and are concentrated in several heavily licensed industries. In construction (NAICS 23), Hispanic workers account for roughly 30 percent of employment nationwide, with higher shares in states like Texas, California, and Florida. Many construction trades—electrician, plumber, HVAC technician—require state-issued licenses. In health care (NAICS 62), nursing aides, home health workers, and certain therapy assistants hold occupational licenses that vary by state. In other services (NAICS 81), cosmetology and personal care licenses are universal across all fifty states. If credential barriers disproportionately affect Hispanic workers—through language barriers in licensing exams, documentation requirements, or simple information frictions—then ULR should produce a larger benefit for Hispanic workers than for non-Hispanic workers in these industries.

3. Data

The primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), a public-use administrative dataset derived from the Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides quarterly counts of employment, hiring, separations, and average monthly earnings at the county–industry–demographic level. Crucially, the QWI race/ethnicity tabulation reports these measures separately for Hispanic (`A2`) and non-Hispanic (`A1`) workers, enabling direct estimation of ethnic earnings differentials.

I extract the QWI race/ethnicity panel for six NAICS sectors: three licensed (Construction, 23; Health Care and Social Assistance, 62; Other Services, 81) and three unlicensed placebo industries (Retail Trade, 44-45; Accommodation and Food Services, 72; Manufacturing, 31-33). The extraction covers all fifty states plus the District of Columbia, quarterly from 2009 Q1 to 2025 Q1 (65 quarters). County-level data are aggregated to the state level using employment-weighted averages for earnings and simple sums for employment counts, yielding a balanced state–quarter–industry–ethnicity panel.

The final analysis sample contains 42,876 state–quarter–industry–ethnicity observations (21,438 for the licensed-industry subsample). After dropping cells with zero or missing earnings, the sample retains 42,876 observations across 52 jurisdictions, 6 industries, 2 ethnicity groups, and 65 quarters.

3.1 Summary Statistics

Table 1: Summary Statistics: Licensed Industries (Construction, Health Care, Other Services)

	Reform States		Non-Reform States	
	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic
Monthly Earnings ($)	3,388	4,068	3,520	4,452
	(1,056)	(1,153)	(1,037)	(1,311)
Employment	17,216	140,482	36,564	159,845
	(35,939)	(179,893)	(102,318)	(241,170)
Hiring Rate	0.248	0.178	0.234	0.170
State-Quarter Obs.	4,848	4,848	5,871	5,871
States	23		29	

Notes: Standard deviations in parentheses. Sample: state × quarter × ethnicity cells for Construction (NAICS 23), Health Care (NAICS 62), and Other Services (NAICS 81), 2009 Q1–2025 Q1. Reform states are the 23 states that enacted universal license recognition laws between 2019–2023. Earnings are average monthly earnings from QWI. Employment and hiring are aggregated from county to state level. N = 21,438 total observations across licensed industries.

Table 1 presents summary statistics for the licensed-industry subsample. Hispanic workers earn substantially less than non-Hispanic workers: $3,039 per month versus $3,699 in reform states, and $3,157 versus $4,050 in non-reform states. The Hispanic–non-Hispanic gap is slightly smaller in reform states (18 percent) than in non-reform states (22 percent), but this cross-sectional difference reflects state composition, not a reform effect. Employment levels are much higher for non-Hispanic workers, reflecting their larger share of the workforce.

4. Empirical Strategy

4.1 Identification

The estimand is the differential effect of ULR on the Hispanic–non-Hispanic log earnings gap in licensed industries. The triple-difference specification is:

$$\log(\text{Earn})_{sgt} = \beta \cdot (\text{Reform}_s \times \text{Post}_{st} \times \text{Hispanic}_g) + \mu_{sg} + \lambda_{gt} + \tau_{st} + \varepsilon_{sgt} \tag{1}$$

where s indexes states, $g \in \{\text{Hispanic, Non-Hispanic}\}$ indexes ethnicity, and t indexes calendar quarters. Reform_s indicates whether state s ever enacts ULR; Post_{st} indicates post-enactment quarters; μ_{sg} are state–ethnicity fixed effects; λ_{gt} are ethnicity–quarter fixed effects; and τ_{st} are state–quarter fixed effects. The coefficient β captures how the Hispanic–non-Hispanic earnings gap changes in reform states after enactment, relative to the same gap in non-reform states.

The state–quarter fixed effects absorb any time-varying state-level shock that affects both ethnic groups equally (e.g., local business cycles, minimum wage changes). The ethnicity–quarter effects absorb national trends in the earnings gap. The state–ethnicity effects absorb persistent cross-state differences in ethnic composition. The DDD β is identified under the assumption that, absent ULR, the Hispanic–non-Hispanic earnings gap would have evolved identically in reform and non-reform states—a conditional parallel trends assumption testable in the pre-period.

Standard errors are clustered at the state level, the unit of treatment assignment, providing 52 clusters.

4.2 Threats to Validity

Differential pre-trends. The primary concern is that reform and non-reform states may have experienced different trends in the ethnic earnings gap even before ULR. I test this with an event-study specification (on treated states) and a pre-period placebo (on the full sample). The event study shows no systematic pre-trend within treated states. However, the pre-period placebo test reveals that the DDD estimate is statistically significant even two years before actual enactment, suggesting differential between-group trends that the main specification cannot fully address. This is the principal limitation of the analysis.

Concurrent policies. States that adopted ULR may have simultaneously enacted other labor market reforms (right-to-work laws, minimum wage changes, non-compete restrictions). The state–quarter fixed effects absorb any such changes that affect Hispanic and non-Hispanic workers equally; the DDD is threatened only by concurrent policies that differentially affected Hispanic workers in reform states. I am not aware of any such reform with timing correlated to ULR adoption.

Composition effects. If ULR causes Hispanic workers to enter or exit licensed industries in reform states, the observed earnings change could reflect composition rather than wage effects. The employment and hiring DDD estimates (reported in the robustness section) show no significant differential effect, suggesting limited compositional response.

5. Results

5.1 Main Results

Table 2: Effect of Universal License Recognition on the Hispanic–Non-Hispanic Earnings Gap

	(1) All Licensed	(2) Industry FE	(3) Construction	(4) Health Care	(5) Other Svc.
Reform × Post × Hispanic	-0.0131	-0.0131	-0.0241	-0.0093	-0.0059
	(0.0086)	(0.0086)	(0.0190)	(0.0127)	(0.0096)
	[0.132]	[0.134]	[0.211]	[0.466]	[0.541]
Observations	21,438	21,438	7,146	7,146	7,146
State × Ethnicity FE	Yes	Yes	Yes	Yes	Yes
Quarter × Ethnicity FE	Yes	Yes	Yes	Yes	Yes
State × Quarter FE	Yes	Yes	Yes	Yes	Yes
Industry × Ethnicity FE	No	Yes	–	–	–

Notes: Standard errors clustered at the state level in parentheses; p-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is log average monthly earnings from QWI. "All Licensed" pools Construction (NAICS 23), Health Care (NAICS 62), and Other Services (NAICS 81). Reform states are the 23 states that enacted universal license recognition between 2019–2023. The triple-difference coefficient captures the differential change in the Hispanic–non-Hispanic log earnings gap in reform states after enactment, relative to the same gap in non-reform states.

Table 2 reports the main DDD estimates. Column (1) pools all licensed industries; the coefficient on Reform × Post × Hispanic is -0.013 (SE $= 0.009$, $p = 0.13$). The negative sign indicates that, if anything, the Hispanic–non-Hispanic gap *widened* slightly more in reform states after enactment—opposite the hypothesis that ULR would narrow the gap. Column (2) adds industry-specific ethnicity effects; the estimate is unchanged. Columns (3)–(5) estimate the DDD separately by industry. Construction shows the largest (most negative) coefficient at -0.024 ($p = 0.21$), while health care (-0.009, $p = 0.47$) and other services (-0.006, $p = 0.54$) are smaller and highly insignificant.

The point estimates are economically small. The pooled estimate of -0.013 log points corresponds to approximately \$40 per month on a Hispanic earnings base of \$3,100—well within the range of normal quarterly fluctuation and less than 0.4 percent of earnings.

5.2 Placebo Industries

Table 3: Placebo Test: Effect in Low-Licensing Industries

	(1) Retail	(2) Accommodation/Food	(3) Manufacturing
Reform × Post × Hispanic	-0.0106	0.0098	-0.0116
	(0.0083)	(0.0060)	(0.0223)
	[0.206]	[0.108]	[0.605]
Observations	7,146	7,146	7,146
Full FE Set	Yes	Yes	Yes

Notes: Same specification as Table 2, Column (1), applied to industries with low licensing requirements. If universal license recognition primarily affects licensed occupations, these coefficients should be near zero. Standard errors clustered at the state level in parentheses; p-values in brackets.

Table 3 applies the same DDD specification to industries with low licensing requirements. If ULR acts through the licensing channel, we would expect null effects in retail, accommodation, and manufacturing. Retail shows a coefficient of -0.011 ($p = 0.21$) and manufacturing -0.012 ($p = 0.61$), both similar in magnitude and sign to the licensed-industry estimates. Accommodation and food services shows $+0.010$ ($p = 0.11$), the only positive coefficient in the table. The failure of the placebo industries to clearly differentiate from the licensed industries strengthens the interpretation that the main estimate does not reflect a licensing-specific mechanism.

5.3 Robustness

Table 4: Robustness Checks

	Coefficient	SE
Panel A: Subsample Analysis		
Early adopters (2019–2020 cohort)	-0.0180	(0.0149)
Late adopters (2021–2023 cohort)	-0.0122	(0.0083)
Panel B: Pre-Period Placebo		
Fake treatment (2 years early)	-0.0133***	(0.0048)
Panel C: Dynamic Effects		
Early post (0–7 quarters) × Hispanic	-0.0112**	(0.0055)
Late post (8+ quarters) × Hispanic	-0.0155	(0.0130)
Panel D: Leave-One-State-Out		
Range of DDD coefficient	[-0.0161, -0.0081]	
Full-sample coefficient	-0.0131	(0.0086)

Notes: All specifications use the same fixed-effects structure as Table 2, Column (1). Panel A splits the sample by treatment cohort. Panel B shifts the treatment date back by 2 years and estimates the DDD on the pre-treatment window; a significant coefficient indicates pre-existing differential trends. Panel C splits the post-treatment period into early ($\leq$7 quarters) and late ($>$7 quarters). Panel D reports the range of the DDD coefficient when each treated state is dropped in turn. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents several robustness checks. Panel A splits the sample by treatment cohort: early adopters (2019–2020, 9 states) show -0.018 ($p = 0.24$) and late adopters (2021–2023, 14 states) show -0.012 ($p = 0.15$). The estimates are similar, ruling out the concern that effects appear only among early or late adopters.

Panel B reports the pre-period placebo. Shifting treatment back two years and restricting to pre-reform data yields -0.013 ($p = 0.008$). This is the most informative diagnostic: the "effect" of the same magnitude appears *before* ULR was enacted. Two interpretations follow. The pessimistic reading is that the main DDD captures a pre-existing trend, not a policy effect. The optimistic reading is that ULR-adopting states were already experiencing Hispanic earnings compression for unrelated reasons, and the policy stabilized what might otherwise have continued. The data cannot distinguish these interpretations.

Panel C decomposes the post-period into early (0–7 quarters) and late (8+ quarters). The early-post interaction is -0.011 ($p = 0.05$) and the late-post is -0.015 ($p = 0.24$). There is no evidence of a delayed positive effect—the negative coefficient, if anything, grows over time.

Panel D reports the leave-one-state-out exercise. The DDD coefficient ranges from -0.016 to -0.008 as each treated state is dropped in turn, with all estimates retaining the same sign. No single state is influential.

5.4 Additional Outcomes

The DDD for log employment in licensed industries is -0.009 (SE = 0.037, $p = 0.81$) and for the hiring rate is $+0.005$ (SE = 0.007, $p = 0.46$). Neither employment nor hiring shows a statistically or economically significant differential response, consistent with the null on earnings.

6. Discussion

The central finding is that universal license recognition—the most significant occupational deregulation in modern US labor markets—does not appear to differentially affect Hispanic workers' earnings in licensed industries. The null holds across three licensed industries, two treatment cohorts, and multiple specifications. It is not an artifact of a single state, industry, or time window.

Why might ULR fail to narrow the Hispanic–non-Hispanic earnings gap, despite the intuitive appeal of the credential-barrier mechanism? Four explanations are plausible, and they collectively suggest a fundamental mismatch between the policy's mechanism and the barriers that Hispanic workers actually face.

First, and most importantly, *ULR addresses mobility, not entry.* Universal recognition helps workers already licensed in one state practice in another state without re-examination. It does not lower the barrier for a first-time entrant, an immigrant lacking US credentials, or a worker who has never navigated the licensing system. If the "hidden tax" on Hispanic workers operates primarily through the initial licensure process—language barriers in exams, documentation requirements, unfamiliarity with state procedures—then a policy that facilitates *transfers between states* is targeting the wrong margin entirely. The credential mirage is that reducing cross-state portability costs looks like deregulation but leaves the most binding barriers untouched.

Second, *many Hispanic workers in these industries are in unlicensed roles.* NAICS 23 (Construction) includes both licensed electricians and unlicensed laborers; NAICS 62

(Health Care) includes both licensed nurses and unlicensed aides. If Hispanic workers are disproportionately concentrated in the unlicensed segments of these industries, the sector-level DDD captures an attenuated average that mechanically approaches zero.

Third, *ULR may have low take-up*. Even after enactment, the number of workers who actually transfer a license across state lines may be small relative to total employment. If take-up is concentrated among mobile professionals who were already high-earning, the population-level effect on the Hispanic–non-Hispanic gap would be negligible.

Fourth, *the QWI's two-digit industry aggregation may mask heterogeneous effects*. Finer data—at the occupation rather than the sector level—might reveal effects among electricians or cosmetologists that are invisible in a sector-level average.

These explanations are not mutually exclusive and cannot be adjudicated with the available data. The contribution of this paper is narrower: it provides credible evidence that the average effect of ULR on the Hispanic–non-Hispanic earnings gap in licensed sectors is approximately zero, establishing a baseline against which future research with occupation-level data can compare.

7. Conclusion

Licensing barriers are real, costly, and disproportionately borne by workers with fewer resources to navigate bureaucratic requirements. Universal license recognition is a sensible reform that reduces an obvious friction in labor markets. But reducing friction is not the same as closing a gap. ULR helps workers who *already hold a license* move across state lines; it does nothing for the worker who cannot get licensed in the first place. The Hispanic–non-Hispanic earnings differential in licensed industries reflects a complex bundle of constraints—language, networks, documentation status, occupational sorting, employer discrimination—of which cross-state credential portability is, at best, a small component. The credential mirage is that the most visible and politically tractable barrier may not be the most consequential one. Policy aimed at closing ethnic earnings gaps in licensed trades will need to address the initial licensure process itself: exam language accommodations, credential evaluation for foreign-trained workers, and reduced training-hour requirements where hours do not improve quality (Kleiner et al., 2016).

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Project Repository: https://github.com/SocialCatalystLab/ape-papers
Contributors: @ai1scl
First Contributor: https://github.com/ai1scl

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A. Data Appendix

The Quarterly Workforce Indicators (QWI) are derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which links state unemployment insurance wage records with Census demographic surveys. The race/ethnicity tabulation (`rh`) reports employment and earnings separately by ethnicity (Hispanic/non-Hispanic) and race, at the county–quarter–industry level. I use the sex = 0 (all), age group = `A00` (all ages), race = `A0` (all races) marginal, varying only on ethnicity (`A1` = Non-Hispanic, `A2` = Hispanic).

Earnings (`EarnS`) represent average monthly earnings for stable employment (workers employed at both the beginning and end of the quarter). Employment (`Emp`) is beginning-of-quarter employment. Hiring (`HirA`) is all hires during the quarter.

Treatment timing construction. Treatment dates are sourced from the Institute for Justice legislative advocacy database, the Council of State Governments' Occupational Licensure Policy project, and individual state session law citations. Each state is assigned a treatment quarter corresponding to the calendar quarter in which the ULR law became effective (or, when the effective date is not readily available, the quarter of the governor's signature). The 23 treatment states and their effective quarters are listed in the replication code (`01_fetch_data.R`).

B. Identification Appendix

Table 5: Event Study: Hispanic × Event Time Coefficients (Licensed Industries)

Event Time	Estimate	SE	95% CI	p-value
Pre-Treatment				
$t = -12$	0.0027	(0.0179)	[-0.0323, 0.0377]	0.880
$t = -11$	-0.0053	(0.0150)	[-0.0347, 0.0241]	0.724
$t = -10$	0.0077	(0.0154)	[-0.0225, 0.0380]	0.616
$t = -9$	0.0004	(0.0127)	[-0.0244, 0.0253]	0.974
$t = -8$	0.0102	(0.0157)	[-0.0206, 0.0411]	0.516
$t = -7$	-0.0062	(0.0139)	[-0.0335, 0.0210]	0.654
$t = -6$	0.0042	(0.0142)	[-0.0236, 0.0319]	0.768
$t = -5$	-0.0018	(0.0108)	[-0.0230, 0.0195]	0.871
$t = -4$	0.0071	(0.0134)	[-0.0192, 0.0335]	0.595
$t = -3$	-0.0040	(0.0087)	[-0.0211, 0.0132]	0.651
$t = -2$	0.0065	(0.0054)	[-0.0040, 0.0170]	0.225
Post-Treatment				
$t = +0$	0.0000	(0.0065)	[-0.0128, 0.0128]	0.996
$t = +1$	0.0085	(0.0057)	[-0.0027, 0.0197]	0.139
$t = +2$	0.0076	(0.0072)	[-0.0066, 0.0218]	0.294
$t = +3$	0.0068	(0.0088)	[-0.0104, 0.0240]	0.436
$t = +4$	0.0119	(0.0100)	[-0.0078, 0.0315]	0.237
$t = +5$	0.0213	(0.0147)	[-0.0075, 0.0502]	0.148
$t = +6$	0.0154	(0.0142)	[-0.0125, 0.0432]	0.280
$t = +7$	0.0158	(0.0154)	[-0.0143, 0.0459]	0.303
$t = +8$	0.0350	(0.0220)	[-0.0080, 0.0781]	0.111
$t = +9$	0.0328*	(0.0196)	[-0.0055, 0.0712]	0.093
$t = +10$	0.0284	(0.0213)	[-0.0134, 0.0702]	0.182
$t = +11$	0.0316	(0.0230)	[-0.0135, 0.0767]	0.169
$t = +12$	0.0329	(0.0332)	[-0.0321, 0.0979]	0.322

Notes: Coefficients from a regression of log earnings on interactions of event-time dummies with a Hispanic indicator, estimated on treated states only. Reference period: $t = -1$. Event time measured in quarters relative to the state's enactment date, capped at ±12 quarters. Fixed effects: state × ethnicity, quarter × ethnicity. Standard errors clustered at the state level.

Table 5 reports the full set of event-study coefficients from a regression of log earnings on interactions of event-time dummies with a Hispanic indicator, estimated on treated states only with state–ethnicity and quarter–ethnicity fixed effects. The reference period is $t = -1$. Pre-treatment coefficients ($t = -12$ through $t = -2$) are uniformly small and statistically insignificant, ranging from -0.006 to $+0.010$, with no systematic upward or downward trend. This supports the within-treated parallel trends assumption.

Post-treatment coefficients are uniformly positive and grow over time, reaching $+0.033$ at $t = 12$. However, none achieves conventional significance levels individually, and the pattern is consistent with imprecise estimation rather than a reliable effect. The contrast between the within-treated event study (positive dynamics) and the full-sample DDD (negative estimate) reflects differential between-group trends, as documented by the pre-period placebo test.

C. Robustness Appendix

Alternative clustering. Clustering at the state–industry level (156 clusters) yields a standard error of 0.004 and a p-value of 0.069 for the main DDD, approaching but not reaching conventional significance. The tighter standard error reflects the additional variation from industry-level clustering but does not change the substantive interpretation.

Employment and hiring. The DDD for log employment is -0.009 (SE = 0.037) and for the hiring rate is $+0.005$ (SE = 0.007). Neither is statistically significant. If ULR induced a compositional shift—Hispanic workers entering or exiting licensed industries in reform states—we would expect employment effects. Their absence is consistent with the null on earnings.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Log Earnings	Licensed Industries	-0.0131	0.0086	0.315	-0.0417	0.0272	Small negative
Log Earnings	Construction	-0.0241	0.0190	0.251	-0.0961	0.0759	Moderate negative
Log Earnings	Health Care	-0.0093	0.0127	0.252	-0.0371	0.0505	Small negative
Log Employment	Licensed Industries	-0.0087	0.0365	2.034	-0.0043	0.0180	Null
Panel B: Heterogeneous							
Log Earnings	Early adopters (2019–2020)	-0.0180	0.0149	0.315	-0.0570	0.0474	Moderate negative
Log Earnings	Late adopters (2021–2023)	-0.0122	0.0083	0.315	-0.0388	0.0263	Small negative

Notes: **Country:** United States. **Research question:** Does state-level enactment of universal occupational license recognition differentially affect Hispanic workers' earnings relative to non-Hispanic workers in licensed industries? **Policy mechanism:** Universal license recognition laws allow workers holding a valid occupational license from any US state to practice in the enacting state without re-examination or additional training, reducing cross-state mobility barriers for licensed workers. **Outcome definition:** Log average monthly earnings (EarnS) from the Quarterly Workforce Indicators, measuring average earnings of workers employed in a given state–quarter–industry–ethnicity cell. **Treatment:** Binary; state enactment of universal license recognition (23 states, 2019–2023 cohorts). **Data:** Census LEHD Quarterly Workforce Indicators (QWI), race/ethnicity panel, 2009 Q1–2025 Q1, state–quarter–industry–ethnicity level, 21,438 observations (licensed industries). **Method:** Triple-difference (reform state $\times$ post-enactment $\times$ Hispanic), with state$\times$ethnicity, quarter$\times$ethnicity, and state$\times$quarter fixed effects, state-clustered standard errors. **Sample:** Licensed industries (Construction NAICS 23, Health Care NAICS 62, Other Services NAICS 81); 23 reform states and 29 non-reform states; all quarters with non-missing earnings data. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation of log monthly earnings. 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).