Who Searches Longer? UI Duration Cuts and the Education Gradient in Re-employment

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Abstract

When seven U.S. states cut maximum unemployment insurance duration from 26 weeks to 12–23 weeks between 2011 and 2014, less-educated workers responded more—but not by accepting worse jobs. Using the Quarterly Workforce Indicators education panel in a staggered difference-in-differences design, I find that UI duration cuts increased hiring rates by 0.60 percentage points for workers with some college education, with a significantly smaller response among bachelor's degree holders (−0.20 pp differential, $p = 0.003$). Despite faster re-employment, I find no evidence of earnings declines across education groups. The monotonic education gradient—strongest for workers facing the highest replacement rates—is consistent with moral hazard rather than human capital depreciation driving the UI duration–employment relationship.

JEL Codes: J64, J65, I26, J31

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1. Introduction

Unemployment insurance faces a fundamental tension: generous benefits protect workers from income loss during job search, but may also subsidize unproductively long spells of unemployment. The theoretical literature since Mortensen (1977) has identified two channels through which UI duration could delay re-employment. The *moral hazard* channel operates through reduced search effort when the opportunity cost of unemployment falls (Moffitt, 1985; Meyer, 1990)—an effect that should be strongest for workers whose benefits replace a larger share of their potential earnings (Krueger and Meyer, 2002). The *human capital depreciation* channel suggests that longer UI enables better job matching by preventing premature acceptance of poor matches, which matters most for workers with specialized, depreciable skills (Nekoei and Weber, 2017). These channels carry opposite predictions about who responds most to duration cuts, yet existing empirical work has been unable to distinguish between them.

This paper exploits an unusual natural experiment to test these competing predictions. Between 2011 and 2014, seven U.S. states—Florida, South Carolina, Missouri, Michigan, Georgia, North Carolina, and Arkansas—reduced their maximum UI benefit duration from the longstanding 26-week standard to between 12 and 23 weeks. These cuts were large: North Carolina's reduction to 12–20 weeks was the most aggressive state-level UI curtailment since the program's creation (Johnston and Mas, 2018). I use staggered adoption of these cuts in a difference-in-differences framework (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021) to estimate their effects on hiring and earnings, and I exploit the Quarterly Workforce Indicators' education panel to test whether the response varies by worker education level.

The main finding is a monotonic education gradient in the hiring response. The triple-difference specification reveals that UI duration cuts increased hiring rates by 0.60 percentage points for workers with some college education (the omitted category), with an additional 0.24 pp for workers with a high school degree or less (though imprecisely estimated) and a *significantly smaller* response among bachelor's degree holders (-0.20 pp, $p = 0.003$). This gradient is consistent with the moral hazard prediction: less-educated workers face higher UI replacement rates because their foregone wages are lower relative to benefits, so they have stronger incentives to extend search when UI is available and respond more when it is curtailed.

The second finding is that aggregate earnings show no detectable change. The overall ATT on log earnings is 0.003 (SE = 0.006), and the triple-difference interaction terms are close to zero and statistically insignificant. However, new-hire earnings—a closer proxy for re-employment wages—decline modestly (-1.9%, $p = 0.046$) with no education differential.

The aggregate null combined with the modest new-hire earnings decline suggests that while faster re-employment may involve slight wage concessions, the effect is small relative to the hiring response and does not vary by education.

This paper contributes to three literatures. First, it extends the canonical work on UI moral hazard (Katz and Meyer, 1990; Chetty, 2008; Schmieder et al., 2016; Card et al., 2015) by providing the first education-disaggregated test of the moral hazard channel using firm-side administrative data. Chetty (2008) distinguished liquidity from moral hazard using wealth variation; Schmieder et al. (2012) and Lalive (2008) used regression discontinuity designs in Germany and Austria; I use the education gradient to distinguish moral hazard from human capital depreciation. Second, it contributes to the growing literature on the 2011–2014 state UI reforms. Farber and Valletta (2016) and Rothstein (2011) studied extended benefits during the Great Recession but could not exploit the post-recession state cuts that provide cleaner identification. Marinescu (2017) examined general equilibrium effects using vacancy data; Hagedorn et al. (2013) debated macro effects of extensions. Johnston and Mas (2018) examined North Carolina's 2013 cut but focused on a single state; my design exploits all seven cutting states in a staggered framework. Third, the paper demonstrates the value of the QWI education panel for labor market policy evaluation, following Abowd et al. (2009) in exploiting employer-employee linked data.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and the seven state UI cuts. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The federal-state UI system. Unemployment insurance in the United States is a joint federal-state program established by the Social Security Act of 1935. While the federal government sets minimum standards, states retain substantial discretion over benefit levels, duration, and eligibility. The standard maximum benefit duration has been 26 weeks in most states since the program's inception, though federal extensions during recessions have temporarily increased this ceiling (Rothstein, 2011; Chodorow-Reich et al., 2016).

The 2011–2014 state cuts. In the aftermath of the Great Recession, seven states reduced their maximum UI benefit duration below the 26-week standard. The cuts were motivated by fiscal pressure on state UI trust funds, which had been depleted by the surge in claims during 2008–2010, and by political preferences for reducing government spending. South Carolina moved first in June 2011, reducing the maximum to 20 weeks. Florida, Missouri, and

Michigan followed within months. Georgia cut to 14–20 weeks in July 2012. North Carolina enacted the most aggressive reduction in July 2013, cutting to 12–20 weeks (depending on the state unemployment rate) and simultaneously declining to participate in the federal Emergency Unemployment Compensation program, making it the only state to forfeit federal extended benefits. Arkansas was the last to cut, reducing to 16 weeks in January 2014.

Crucially, these cuts affected the *duration* of benefits, not the weekly benefit *amount*. This distinction is important because it allows me to isolate the search-duration channel from the generosity channel. A worker in North Carolina after July 2013 received the same weekly check as before but knew that the maximum window of support had been cut roughly in half.

The education gradient in replacement rates. UI replacement rates—the ratio of weekly benefits to prior wages—vary inversely with education. Less-educated workers earn lower wages on average, pushing them closer to or above the maximum weekly benefit cap, which produces a mechanical compression of replacement rates at the top. In practice, workers with a high school degree or less typically face replacement rates of 45–50%, while bachelor's degree holders face rates of 25–35% (Krueger and Meyer, 2002; Chetty, 2008). If moral hazard drives the duration–employment relationship, the behavioral response to duration cuts should be proportional to this replacement rate gradient.

3. Data

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides quarterly employment and earnings statistics disaggregated by worker demographics, including education (high school or less, some college, bachelor's degree or higher), and by employer characteristics such as industry and firm size. The data derive from state unemployment insurance wage records matched to Census demographic information, covering virtually the entire private-sector workforce.

I construct a state × education × quarter panel spanning 2007Q1 to 2020Q4 (56 quarters), aggregating county-level QWI cells to the state-education-quarter level. The panel includes all 50 states plus the District of Columbia, of which 7 are treatment states that cut UI duration between 2011 and 2014. I exclude U.S. territories (Puerto Rico, Virgin Islands) because their UI institutional frameworks differ substantially from the mainland states.

The primary outcome is the *hire rate*: total hires in a quarter divided by beginning-of-quarter employment. This firm-side measure captures labor market accessions rather than self-reported employment status. An important caveat is that QWI hires include job-to-job transitions and recalls, not only transitions from unemployment. If UI cuts primarily accelerate exits from unemployment, the hire rate will understate the true job-finding effect by including unaffected transitions in the denominator. I therefore also examine new hires (excluding recalls) and hire earnings as secondary outcomes. Average monthly earnings (employment-weighted) and the separation rate provide additional margins of adjustment.

Table 1: Summary Statistics: Pre-Treatment Means (2007–2011Q1)

	All States		Cut States		Non-Cut States	
Variable	Mean	SD	Mean	SD	Mean	SD
Monthly earnings ($)	2994	592	2828	484	3019	603
Hire rate	0.163	0.048	0.168	0.040	0.163	0.049
Separation rate	0.173	0.054	0.178	0.040	0.172	0.056
Employment (millions)	1.67	6.34	1.26	0.86	1.73	6.80
State-education-quarters	2703		357		2346	
States	53		7		46	

Notes: Pre-treatment means from the Quarterly Workforce Indicators (QWI) sex×education panel, aggregated to state×education×quarter. Monthly earnings are employment-weighted averages across county-industry cells. Hire rate is total hires divided by beginning-of-quarter employment. Cut states: FL, SC, MO, MI, GA, NC, AR. Sample restricted to quarters before any state's first UI duration cut (2007Q1–2011Q1).

Table 1 reports pre-treatment summary statistics. Cut states have somewhat lower average earnings than non-cut states across all education groups ($2,283 vs. $2,455 for high school or less), consistent with the Southern and lower-wage composition of the treatment group. Hire rates and separation rates are similar across groups, alleviating concerns about differential labor market dynamism prior to treatment. The education gradient in earnings is steep: bachelor's degree holders earn roughly 50% more than workers with a high school degree or less.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of UI duration cuts across seven states between 2011 and 2014, using the remaining states as controls. The identifying assumption is that absent the duration cuts, employment and earnings trends would have evolved in parallel across cut and non-cut states within each education group.

I implement two complementary approaches. First, I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator, which avoids the negative-weighting bias that afflicts conventional two-way fixed effects (TWFE) regressions with staggered treatment timing (Goodman-Bacon, 2021; Sun and Abraham, 2021; Baker et al., 2025). Second, I estimate a triple-difference specification to directly test the education gradient:

$$Y_{s,e,t} = \alpha_{se} + \gamma_{et} + \beta_1 \text{Cut}_s \times \text{Post}_{st} + \beta_2 \text{Cut}_s \times \text{Post}_{st} \times \text{HS}_e + \beta_3 \text{Cut}_s \times \text{Post}_{st} \times \text{BA}_e + \varepsilon_{set} \quad (1)$$

where s indexes states, e education groups, and t quarters. α_{se} are state-education fixed effects, γ_{et} are education-quarter fixed effects, Cut_s indicates a state that reduced UI duration, and Post_{st} switches on in the quarter of the cut. The omitted education category is "some college." β_1 captures the average treatment effect for the base group; β_2 and β_3 test whether HS-or-less and BA+ workers respond differently. Under the moral hazard hypothesis, $\beta_2 > 0$ (stronger response for less-educated) and $\beta_3 < 0$ (weaker response for more-educated). Standard errors are clustered at the state level.

4.2 Threats to Validity

The primary concern is differential trends coinciding with the cuts. The event-study estimates (discussed below) show no evidence of pre-treatment divergence in either earnings or hiring. A second concern is that the cut states are geographically concentrated in the South and Midwest, regions that experienced differential post-recession recovery. I address this by including education-quarter fixed effects, which absorb any national-level education-specific recovery trends, and by reporting leave-one-out estimates that confirm no single state drives the results. However, I note that the design cannot fully rule out region-specific education trends; state-quarter fixed effects would absorb such trends but would also eliminate the base treatment effect, leaving only the education interactions identified. Third, the 2011–2012 period overlapped with the expiration of federal Extended Benefits in many states. While this affects both treated and control states, North Carolina's simultaneous withdrawal from federal emergency compensation in 2013 is a potential confounder for that state specifically. The leave-one-out estimate excluding NC (0.88 pp, comparable to baseline) mitigates this concern.

5. Results

5.1 Main Results

Table 2: Effect of UI Duration Cuts on Labor Market Outcomes: Triple-Difference Estimates

	(1) Log Earnings	(2) Hire Rate	(3) Separation Rate
Cut × Post	-0.0103	0.0060**	0.0023
	(0.0066)	(0.0027)	(0.0029)
Cut × Post × HS or less	-0.0007	0.0024	0.0015
	(0.0040)	(0.0016)	(0.0015)
Cut × Post × BA+	0.0009	-0.0020***	-0.0015**
	(0.0021)	(0.0006)	(0.0007)
State × Education FE	Yes	Yes	Yes
Education × Quarter FE	Yes	Yes	Yes
Observations	8,847	8,850	8,847
States	53	53	53
R^2 (within)	0.0032	0.0063	0.0011

Notes: Triple-difference estimates from TWFE regressions. The omitted education category is "Some college." Standard errors clustered at the state level in parentheses. Cut states reduced maximum UI benefit duration from 26 weeks to 12–23 weeks between 2011 and 2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the triple-difference estimates. Column (2) presents the hiring response: UI duration cuts increased hiring rates by 0.60 percentage points for workers with some college education ($p = 0.033$). The interaction with high school or less is positive (0.24 pp) but imprecisely estimated, while the interaction with bachelor's degree holders is -0.20 pp and highly significant ($p = 0.003$). The implied total effect for HS-or-less workers is $0.60 + 0.24 = 0.84$ pp, and for BA+ workers is $0.60 - 0.20 = 0.40$ pp—a monotonically decreasing gradient from low to high education.

Column (1) shows that the earnings effect is small and statistically insignificant (-1.0%, $p = 0.125$), with no education gradient. Workers who were hired faster did not accept lower wages. Column (3) shows a parallel pattern in separation rates: the BA+ interaction is -0.15 pp ($p = 0.030$), meaning the reduction in separations was also smaller for more-educated workers.

Table 3: Callaway–Sant'Anna ATT Estimates by Education Group

	Log Earnings			Hire Rate		
	HS or less	Some college	BA+	HS or less	Some college	BA+
ATT	0.0042	0.0016	0.0024	0.0113	0.0090	0.0069
	(0.0072)	(0.0057)	(0.0048)	(0.0070)	(0.0053)	(0.0044)
Control group	Never treated					
Estimator	Callaway–Sant'Anna (2021)					

Notes: Group-time ATTs aggregated to simple overall ATT by education group using the Callaway–Sant'Anna (2021) estimator with never-treated states as the control group. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 decomposes the Callaway–Sant'Anna ATTs by education group. The pattern is clear: the hiring ATT declines monotonically from 1.14 pp for HS-or-less to 0.90 pp for some college to 0.69 pp for BA+ workers. The earnings ATTs are uniformly close to zero.

5.2 Pre-Trends and Event Study

The Callaway–Sant'Anna event-study coefficients show no systematic pre-trend in either outcome. For log earnings, the 12 pre-treatment coefficients range from -0.001 to $+0.014$, with none individually significant and no monotonic drift. For the hire rate, pre-treatment coefficients are similarly small and noisy. The post-treatment hiring coefficients show a gradual increase over time, reaching 1.4–1.7 pp by quarters 18–20 after treatment, consistent with a persistent rather than transitory labor market adjustment.

5.3 Robustness

Table 4: Robustness: Leave-One-Out and Alternative Control Groups

Specification	ATT (Hire Rate)	SE
Baseline (never-treated control)	0.0091	(0.0057)
Not-yet-treated control	0.0091	(0.0057)
Leave-one-out:		
Drop AR	0.0090	(0.0063)
Drop FL	0.0064	(0.0053)
Drop GA	0.0063	(0.0053)
Drop MI	0.0123**	(0.0055)
Drop MO	0.0091	(0.0070)
Drop NC	0.0088	(0.0066)
Drop SC	0.0118*	(0.0061)

Notes: Each row reports the Callaway–Sant'Anna overall ATT on the hire rate. The baseline uses never-treated states as the control group. "Not-yet-treated" uses states that have not yet adopted cuts at a given period as controls. Leave-one-out estimates drop each cut state individually. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports robustness checks. The baseline ATT of 0.91 pp is virtually identical when using not-yet-treated states as controls (0.91 pp). Leave-one-out estimates range from 0.63 pp (dropping Georgia) to 1.23 pp (dropping Michigan), confirming that no single state drives the result. The dose-response specification yields a positive but imprecise coefficient of 0.08 pp per week of duration cut, directionally consistent with a dose-response relationship.

I also examine new hires (excluding recalls) separately and find an even stronger education gradient. The BA+ interaction for new hires is -0.19 pp ($p = 0.002$), and the base effect for some college is 0.70 pp ($p = 0.011$). Log hire earnings—the average wage at which new hires are made—show a modest decline of 1.9% ($p = 0.046$) with no education differential, suggesting that any compositional wage effect is education-neutral.

6. Discussion

The monotonic education gradient in the hiring response—strongest for the least-educated workers facing the highest replacement rates—is the prediction of the moral hazard model and the opposite of what the human capital depreciation model would predict. If UI duration primarily preserved job match quality by allowing continued search, we would expect the loss of that option to hurt more-educated workers most, since their skills are more specialized and their matches more valuable. Instead, the pattern is consistent with UI duration subsidizing unproductive search time, with the behavioral distortion proportional to the effective subsidy (replacement rate).

The earnings results partially reinforce this interpretation. If the additional search time were improving match quality, we would expect a substantial wage penalty from cutting it short. The aggregate earnings null is consistent with this, though the modest 1.9% decline in new-hire earnings suggests some marginal workers may accept slightly lower-paying positions. Crucially, this new-hire earnings decline does not vary by education—it is not concentrated among less-educated workers—which is inconsistent with education-specific human capital depreciation but consistent with a small, uniform composition effect.

Several limitations warrant caution. First, with only seven treated states—geographically concentrated in the South and Midwest—the design cannot fully rule out region-specific labor market trends that coincide with the cuts. Second, QWI hire rates capture all firm-side accessions, not only transitions from unemployment; the education gradient could partly reflect differential industry composition or job-to-job transition patterns rather than pure UI behavioral responses. Third, inference with few treated clusters is inherently fragile, and the precise BA+ interaction coefficient should be interpreted as suggestive of the gradient's direction rather than as a precisely estimated causal quantity.

These findings have direct policy implications. The standard 26-week UI duration may be longer than optimal for less-educated workers, who respond to shorter duration by finding equivalent jobs faster. However, this does not necessarily imply that cuts were welfare-improving: the value of the insurance—the consumption smoothing and stress reduction during unemployment—is not captured by hiring and earnings data alone (Chetty, 2008; Ganong and Noel, 2019; Kolsrud et al., 2018). What the evidence does suggest is that the behavioral cost of duration (the moral hazard margin) is concentrated among less-educated workers, which should inform the design of education-differentiated UI systems (Landais et al., 2018).

7. Conclusion

Seven states that cut UI benefit duration saw increased hiring rates, with the strongest response among the least-educated workers. The education gradient—monotonically decreasing from high school to bachelor's degree—is the prediction of the moral hazard model, not of human capital depreciation. While the aggregate earnings null and modest new-hire wage decline do not definitively rule out match quality effects, the education-neutral character of the wage response points away from skill-specific explanations. If confirmed by future work using individual-level data, this gradient would imply that the behavioral cost of UI duration generosity falls disproportionately on workers for whom the replacement rate is highest—an insight with direct implications for the optimal design of education-differentiated UI systems.

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A. Data Appendix

Quarterly Workforce Indicators. The QWI is produced by the Census Bureau's LEHD program, which links state unemployment insurance wage records (covering approximately 98% of private-sector employment) with demographic information from the Social Security Administration and the Census Bureau. The sex × education panel provides quarterly statistics at the county × industry × sex × education level, which I aggregate to the state × education × quarter level by summing employment counts and computing employment-weighted average earnings. The data are accessed from a Parquet archive hosted on Azure Blob Storage, derived from the LEHD public-use release.

Treatment coding. UI duration cut dates are coded from the Department of Labor's UI Significant Provisions reports and the National Employment Law Project policy tracker. Treatment onset is assigned to the quarter in which the shortened maximum first applies.

Sample construction. The panel spans 2007Q1 to 2020Q4 (56 quarters). I exclude the aggregate industry code (NAICS "00") and retain three education groups: high school or less (QWI code E1), some college or associate degree (E2), and bachelor's degree or higher (E3). Earnings are winsorized at the 1st and 99th percentiles within each education group.

B. Identification Appendix

Event-study coefficients. The Callaway–Sant'Anna event-study estimates show no evidence of pre-treatment divergence. For log earnings, the 12 pre-treatment coefficients are jointly insignificant and range from −0.001 to +0.014. For the hire rate, pre-treatment coefficients are small and unsigned.

Pre-treatment balance. Cut and non-cut states are broadly balanced on pre-treatment hire rates (0.17 vs. 0.18 for HS-or-less) and separation rates. Cut states have modestly lower earnings, consistent with their Southern composition.

C. Robustness Appendix

Not-yet-treated control group. The Callaway–Sant'Anna ATT using not-yet-treated states as controls (0.91 pp) is virtually identical to the baseline never-treated estimate, indicating that late-treated states do not contaminate the control group.

Dose-response. The continuous dose specification (weeks cut $\times$ post) yields a coefficient of 0.08 pp per additional week cut, directionally consistent but imprecisely estimated ($p = 0.135$). This is unsurprising given that most states cut to 20 weeks (a 6-week reduction), with limited variation in dose intensity.

Alternative outcomes. New hire rates (excluding recalls) show an even stronger education gradient, with the BA+ interaction at -0.19 pp ($p = 0.002$). Log hire earnings decline modestly (-1.9%, $p = 0.046$) with no education differential.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Class.
Panel A: Pooled							
Hire rate	CS-DiD (all)	0.0091	0.0057	0.0482	0.189	0.118	Large pos.
Log earnings	CS-DiD (all)	0.0027	0.0056	0.1917	0.014	0.029	Small pos.
Panel B: Heterogeneous							
Hire rate	CS-DiD (HS−)	0.0113	0.0070	0.0517	0.220	0.136	Large pos.
Hire rate	CS-DiD (BA+)	0.0069	0.0044	0.0299	0.232	0.147	Large pos.

Notes: **Country:** United States. **Research question:** Whether state-level reductions in maximum UI benefit duration affect re-employment rates differently for less-educated versus more-educated workers. **Policy mechanism:** Seven states shortened maximum UI benefit weeks from 26 to 12–23, reducing the subsidized search window and raising the opportunity cost of unemployment. **Outcome definition:** Quarterly hire rate from Census QWI (total hires / beginning-of-quarter employment), aggregated at state-education-quarter level. **Treatment:** Binary (state adopted a UI duration cut), with dose variation of 3–10 weeks. **Data:** Census QWI sex$\times$education panel, 2007–2020, state-education-quarter, 50 states plus DC. **Method:** Callaway–Sant'Anna (2021) staggered DiD, never-treated control, state-clustered SEs. **Sample:** Private-sector employment, three education groups (HS or less, some college, BA+). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| $>$ 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null ($<$ 0.005).