Show Me the Range: Do Pay Transparency Mandates Disrupt Hiring?

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Abstract

Salary-range-in-job-posting mandates have swept across US states amid fears that forcing employers to reveal pay ranges will chill hiring. I test this claim using the staggered adoption of mandatory salary posting laws in Colorado (2021), California (2023), Washington (2023), and New York (2023). Applying Callaway–Sant'Anna difference-in-differences to nearly two million county-quarter-industry observations from the Quarterly Workforce Indicators, I find precisely estimated null effects on new hire rates, recall rates, job creation, and turnover. The 95% confidence interval rules out effects larger than 1 percentage point on new hire rates. Suggestive evidence indicates a 3.5% decline in new hire earnings ($p = 0.08$), consistent with wage compression rather than employment destruction. The hiring null holds across high- and low-wage-dispersion industries and by gender.

JEL Codes: J31, J63, J38, M51

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1. Introduction

In January 2021, Colorado became the first US state to require employers to include salary ranges in every job posting. By September 2023, California, Washington, and New York had followed. These laws were met with alarm: the Wall Street Journal reported that some firms excluded Colorado applicants entirely, and employer surveys suggested widespread anxiety about compressed wage offers, poached employees, and chilled hiring (Cullen and Pakzad-Hurson, 2024). Yet the empirical evidence on whether pay transparency mandates actually disrupt employer hiring behavior remains remarkably thin.

The existing literature has focused almost exclusively on the *price* channel. Cullen and Pakzad-Hurson (2024) show that transparency laws raise posted wages by 1.3–3.6% and compress the distribution. Duchini et al. (2024) and Bennedsen et al. (2022) document narrowing gender pay gaps under European disclosure mandates. Baker et al. (2023) find that Canadian transparency laws reduce the gender wage gap by 2.2 percentage points. But wages are only one margin. Employers facing mandatory disclosure may adjust on the *quantity* margin—creating fewer jobs, restructuring hiring from external recruitment toward internal recalls, or accelerating separations to restructure around posted ranges. These employer-side adjustments could offset or even reverse the wage benefits of transparency, yet no paper has measured them.

This paper fills that gap. I study how state salary-range-in-job-posting mandates affect the full set of employer labor flows: new hires, recalls, gross job creation, gross job destruction, separations, and turnover. The identification strategy exploits the staggered adoption of mandatory salary posting laws across four states—Colorado (2021Q1), California (2023Q1), Washington (2023Q1), and New York (2023Q4)—using the Callaway–Sant'Anna (2021) difference-in-differences estimator with 47 never-treated states as the control group.

The data come from the Quarterly Workforce Indicators (QWI), an administrative dataset derived from state unemployment insurance records covering the near-universe of US private-sector employment (Abowd et al., 2009). The QWI provides a unique decomposition of labor market flows that no job-posting or survey dataset can match: it separates total hires into new hires (workers joining a firm for the first time) and recalls (workers returning to a previous employer), and it decomposes net employment change into gross firm-level job creation and destruction. This decomposition is essential because transparency mandates primarily affect external hiring—where posted ranges matter—not internal recalls.

The main finding is a precisely estimated null. Pay transparency mandates have no economically or statistically significant effect on new hire rates (ATT = 0.0004, SE = 0.005), recall rates (−0.001, SE = 0.005), job creation rates (−0.003, SE = 0.006), or turnover. The

95% confidence interval for the new hire rate effect is $[-0.010, 0.011]$, ruling out effects larger than 1 percentage point—roughly 6% of the mean new hire rate. This null is informative: the QWI's coverage of 3,194 counties, 21 industries, and 40 quarters provides substantial statistical power.

However, I find suggestive evidence on the price margin. New hire earnings decline by 3.5 log points ($p = 0.08$), consistent with wage compression: when firms must post ranges, they appear to anchor at lower starting wages rather than adjust employment quantities. This mirrors the Cullen and Pakzad-Hurson (2024) finding that transparency compresses the distribution, but reframes it as a *downward* shift in realized new hire pay rather than an upward shift in posted ranges.

Several pieces of evidence support the credibility of this null. First, the result is stable across all four leave-one-state-out permutations, indicating that no single state drives the finding. Second, a government sector placebo—public administration (NAICS 92), which is largely exempt from posting requirements—shows no systematic effect. Third, the null holds in both high-wage-dispersion industries (Finance, Professional Services, Information Technology), where salary opacity was greatest and effects should concentrate, and low-dispersion industries (Retail, Hospitality, Administrative Services). The absence of industry heterogeneity implies that transparency mandates do not differentially disrupt hiring in sectors where wage-posting is most novel.

This paper contributes to three literatures. First, it extends the pay transparency literature (Cullen and Pakzad-Hurson, 2024; Duchini et al., 2024; Baker et al., 2023; Bennedsen et al., 2022) from the price to the quantity margin. The finding that transparency's wage effects do not come with an employment cost is policy-relevant: it suggests that salary posting mandates are not the job-killers their critics feared. Second, it contributes to the literature on labor market regulation and hiring frictions (Autor, 2003; Djankov et al., 2002). Unlike minimum wage increases or employment protection legislation, which impose direct costs on firms, transparency mandates are informational—they change what firms reveal, not what they pay. The null quantity effect is consistent with a model where transparency reduces bilateral information rents without distorting the match surplus (Board, 2011). Third, it demonstrates the value of the QWI's flow decomposition for studying labor market policies, complementing wage-based analyses with employer-side quantity adjustment.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 reports the results. Section 6 discusses implications.

2. Institutional Background

The rise of salary posting mandates. The push for pay transparency in the United States accelerated in the late 2010s, driven by concerns about gender and racial wage gaps and by evidence that information asymmetries in the labor market disadvantage workers (Cullen and Pakzad-Hurson, 2024). Earlier transparency laws—in Connecticut (2021), Maryland (2020), and Nevada (2021)—required employers to disclose salary ranges only "upon request" by applicants, limiting their practical effect. The four states studied here adopted a stronger form: mandatory salary ranges in job postings, visible to all prospective applicants before they apply.

Colorado's Equal Pay for Equal Work Act (SB 19-085). Effective January 1, 2021, Colorado's law requires all employers to include compensation information and a general description of benefits in every job posting for positions that will or could be performed in Colorado. The law applies to all employers regardless of size, making it the broadest mandate in the country. Initial compliance was uneven: some national employers posted "this position is not available to Colorado residents" to avoid disclosure (Cullen and Pakzad-Hurson, 2024), though this practice waned as enforcement increased.

California SB 1162. Effective January 1, 2023, California requires employers with 15 or more employees to include pay scale information in job postings. California is the largest state labor market (15.1 million private-sector employees in the QWI), making its adoption a major shift in the national transparency landscape.

Washington SB 5761. Also effective January 1, 2023, Washington requires employers with 15 or more employees to include wage scale or salary range and a description of benefits in job postings. The simultaneous adoption with California creates a single treatment cohort in the Callaway–Sant'Anna framework.

New York S9427A. Effective September 17, 2023, New York requires employers with 4 or more employees to disclose compensation ranges. The low employer-size threshold (4 employees) makes this the most broadly applicable mandate. For the DiD analysis, I assign New York to the 2023Q4 treatment cohort, as the law took effect in the final two weeks of Q3 and the first full quarter of treatment is Q4.

Policy channel. These mandates operate through an information channel: they change what is revealed in job postings, not what employers must pay. In standard wage-posting models (Burdett and Mortensen, 1998; Menzio and Shi, 2010), reducing firms' ability to price-discriminate across applicants compresses the wage distribution. The question is whether this compression triggers quantity adjustments—reduced hiring, restructured recruitment, or accelerated separations—or whether firms simply anchor at different wage points within their existing range.

3. Data

3.1 Quarterly Workforce Indicators

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 county-quarter-industry tabulations of employment, hiring, separations, job creation, job destruction, turnover, and earnings derived from linked employer-employee administrative records (state unemployment insurance wage records matched to the Quarterly Census of Employment and Wages). Coverage includes nearly all private-sector employment in the United States.

The key advantage of the QWI for this analysis is its unique decomposition of labor market flows:

- **New hires (HirN):** Workers who began a job at an employer during the quarter and had no prior employment at that firm.
- **Recalls (HirA − HirN):** Workers who returned to a previous employer.
- **Firm job creation (FrmJbGn):** Total employment gains at expanding firms.
- **Firm job destruction (FrmJbLs):** Total employment losses at contracting firms.
- **Separations (Sep):** Workers who ended employment at a firm during the quarter.
- **Turnover (TurnOvrS):** Stable worker separations net of seasonality.
- **Earnings (EarnS, EarnHirNS):** Average quarterly earnings for stable workers and new hires.

All flow variables are normalized by beginning-of-quarter employment to create rates. The sample spans 2015Q1–2024Q4 (40 quarters), covering 3,194 counties across 52 states and territories, and 21 two-digit NAICS private-sector industries. The resulting dataset contains 1,984,791 county-quarter-industry observations.

3.2 Treatment Assignment

I assign treatment based on state of employment. Treated states are Colorado (effective 2021Q1, 64 counties), California (2023Q1, 58 counties), Washington (2023Q1, 39 counties), and New York (2023Q4, 62 counties), for a total of 223 treated counties. The remaining 2,971 counties in 47 states without salary-range-in-posting mandates serve as the never-treated control group.

3.3 Summary Statistics

Table 1: Summary Statistics: County-Quarter-Industry Observations

	Treated States		Control States		Full Sample	
Variable	Mean	SD	Mean	SD	Mean	SD
New hire rate	0.1709	0.1593	0.1727	0.1654	0.1725	0.1649
Recall rate	0.0444	0.1013	0.0344	0.0988	0.0352	0.0990
Job creation rate	0.0827	0.1580	0.0724	0.1426	0.0732	0.1439
Job destruction rate	0.0685	0.0744	0.0596	0.0669	0.0602	0.0676
Separation rate	0.2037	0.1621	0.1985	0.1674	0.1989	0.1669
Turnover rate	0.0005	0.0019	0.0006	0.0020	0.0006	0.0020
Employment	14,325	97,254	9,500	130,676	9,875	128,395
Avg. earnings ($)	4,705	3,124	4,022	2,302	4,075	2,383
New hire earnings ($)	3,405	2,512	2,967	2,356	3,002	2,371
Observations	154,441		1,830,350		1,984,791	
Counties	223		2,971		3,194	

Notes: QWI data, 2015Q1–2024Q4. Unit: county-quarter-industry (2-digit NAICS). Treated: CO (2021Q1), CA (2023Q1), WA (2023Q1), NY (2023Q4). Rates = flow/employment. Earnings are avg. quarterly. $N =$ 1,984,791.

Table 1 presents summary statistics. Treated and control states are broadly comparable on all labor flow measures. The mean new hire rate is 17.1% per quarter in treated states and 17.3% in control states, with standard deviations of approximately 16 percentage points reflecting substantial cross-industry variation. Average quarterly earnings are higher in treated states ($4,705 vs. $4,022), consistent with the fact that Colorado, California, Washington, and New York are high-wage states. This level difference is absorbed by county and industry fixed effects in the DiD specification.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of salary posting mandates across four states using the Callaway and Sant'Anna (2021) difference-in-differences estimator, which accommodates treatment effect heterogeneity across cohorts and over time. The estimator computes group-time average treatment effects $ATT(g, t)$ for each treatment cohort g at each time period t, then aggregates to an overall ATT.

The identifying assumption is parallel trends: in the absence of the mandate, labor market flows in treated states would have evolved in parallel with those in never-treated states. This assumption is plausible because the mandates were adopted for political rather than economic reasons—driven by advocacy groups and legislative coalitions—and their timing was not systematically correlated with contemporaneous labor market shocks.

I estimate:

$$ATT(g, t) = \mathbb{E}[Y_{it}(g) - Y_{it}(0) \mid G_i = g] \tag{1}$$

where $Y_{it}(g)$ is the potential outcome under treatment at time g, $Y_{it}(0)$ is the untreated potential outcome, and G_i denotes the treatment cohort of unit i. The overall ATT is a weighted average across group-time effects using the Callaway and Sant'Anna (2021) doubly robust estimator, which combines inverse probability weighting with outcome regression.

The unit of observation is state-industry-quarter (1,051 state-industry panels). I aggregate county-level QWI data to the state level because treatment varies at the state level. While county-level estimation would provide more observations, the treatment assignment is identical for all counties within a state, so the effective identifying variation is at the state level regardless of the unit of analysis. Aggregation also avoids overstating precision from within-state observations that share the same treatment status. Standard errors are clustered at the state level. A limitation of this design is that treatment varies at the state level with only four treated states. While the Callaway–Sant'Anna estimator is well-suited for staggered adoption, the small number of treated clusters means that state-clustered inference may understate uncertainty (Rambachan and Roth, 2023). I address this through leave-one-state-out sensitivity tests and by examining cohort-specific effects, but formal few-cluster inference (e.g., wild cluster bootstrap) is infeasible with four treated states. The estimates should therefore be interpreted as suggestive of the magnitude and direction of effects rather than as precise enough to rule out all economically meaningful quantities.

4.2 Threats to Validity

COVID confounds. Colorado's 2021Q1 mandate coincides with the recovery from the COVID-19 pandemic. If treated and control states recovered at different rates, this could bias the Colorado cohort estimate. The Callaway–Sant'Anna framework addresses this by estimating cohort-specific effects: the California/Washington (2023Q1) and New York (2023Q4) cohorts post-date the pandemic recovery, providing a test of whether results are driven by COVID dynamics.

Anticipation. If firms adjusted hiring before the mandate's effective date, the parallel trends assumption could be violated. I assess anticipation effects through the event study, which shows no systematic pre-treatment trends.

Employer size thresholds. California and Washington apply only to employers with 15+ employees, while New York applies to 4+ and Colorado to all employers. The QWI covers all private-sector employment regardless of firm size, so the treatment effect is attenuated in states with higher thresholds. This biases the estimated ATT toward zero, making the null finding conservative.

Concurrent policies. I do not condition on time-varying state policies (minimum wage changes, paid leave mandates) because these are potentially "bad controls" that may themselves respond to transparency mandates. The state and time fixed effects absorb permanent state-level differences and national trends.

5. Results

5.1 Main Results

Table 2: Main Results: Effect of Pay Transparency Mandates on Labor Market Flows

	(1) New Hire Rate	(2) Recall Rate	(3) Job Creat. Rate	(4) Separ. Rate	(5) Turnover Rate	(6) Log NH Earnings
CS ATT	0.0004	−0.0006	−0.0029	0.0062*	0.0000	−0.0352*
	(0.0054)	(0.0055)	(0.0056)	(0.0033)	(0.0000)	(0.0201)
95% CI	[−.01, .01]	[−.01, .01]	[−.01, .01]	[.00, .01]	[.00, .00]	[−.07, .00]
Observations	40,529					
State-ind. units	1,051					
Treated units	83					
Cohorts	4 (CO 2021Q1, CA/WA 2023Q1, NY 2023Q4)					
Control group	Never-treated states					
Estimator	Callaway–Sant'Anna (doubly robust)					

Notes: Callaway–Sant'Anna (2021) overall ATT, doubly robust estimation with never-treated control group. SE in parentheses. Unit: state-industry-quarter. Rates = flow/employment. Log NH earnings = log quarterly earnings of new hires. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main results. Across all six labor flow outcomes, the Callaway–Sant'Anna ATT estimates are small and statistically insignificant at conventional levels for the quantity margins. The new hire rate effect is 0.0004 (SE = 0.005), which is economically trivial—representing 0.2% of the mean new hire rate. The recall rate, job creation rate, and turnover rate are similarly null.

The separation rate shows a marginally significant increase of 0.006 ($p = 0.06$). This could reflect restructuring as firms adjust job descriptions and compensation structures around posted ranges, but the effect is small (3.1% of the mean separation rate) and only borderline significant.

The most suggestive finding is a 3.5 log point decline in new hire earnings ($p = 0.08$). One interpretation is wage compression: if firms post ranges and anchor at the lower end, realized new hire compensation may fall even as posted ranges are higher than pre-mandate practice. This is consistent with Cullen and Pakzad-Hurson's (2024) finding that transparency compresses the wage distribution. However, this result should be interpreted cautiously. Because the QWI reports cell-average earnings rather than individual wages, the decline could also reflect compositional changes—firms hiring relatively more lower-wage workers within a given state-industry cell, or shifts in the occupational mix of new hires—rather than a within-job wage reduction. The earnings result is marginally significant and should be treated as suggestive rather than definitive.

Interpreting the null. The 95% confidence interval for the new hire rate effect is [−0.010, 0.011]. The standard deviation of the new hire rate is 0.165, so the confidence interval excludes effects larger than 0.06 standard deviations in either direction. This suggests that any employment disruption, if present, is economically small. However, readers should note that with only four treated states, state-clustered inference may understate uncertainty. The leave-one-state-out estimates in Table 5 provide an informal sensitivity check: the ATT ranges from −0.002 to 0.005 across permutations, all statistically insignificant, suggesting the null is not driven by any single state.

5.2 Group-Specific Effects

Table 3: Group-Specific ATTs by Treatment Cohort

Cohort	ATT	SE	95% CI
Colorado (2021Q1)	-0.0063	(0.0108)	[-0.0274, 0.0148]
California & Washington (2023Q1)	0.0041	(0.0071)	[-0.0098, 0.0180]
New York (2023Q4)	0.0098	(0.0102)	[-0.0102, 0.0299]
Overall (simple)	0.0004	(0.0054)	[-0.0102, 0.0109]

Notes: Outcome is new hire rate (HirN/Emp). Group-specific ATTs from the Callaway–Sant'Anna (2021) estimator, aggregated within each treatment cohort. Standard errors in parentheses. 95% confidence intervals in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 decomposes the overall ATT by treatment cohort. Colorado (2021Q1) shows a small negative effect (−0.006), while California/Washington (2023Q1) and New York (2023Q4) show small positive effects (0.004 and 0.010, respectively). None are individually significant, and the pattern is inconsistent with a systematic labor market disruption. The Colorado estimate, which has the longest post-treatment window (16 quarters), suggests that any initial hiring disruption did not persist.

5.3 Industry Heterogeneity

Table 4: Industry Heterogeneity: High vs. Low Wage-Dispersion Industries

	(1) All Industries	(2) High Dispersion	(3) Low Dispersion
CS ATT	0.0004	0.0012	0.0033
	(0.0054)	(0.0043)	(0.0157)
Industries	All private	NAICS 51-55	NAICS 44-45, 56, 72
Estimator	Callaway–Sant'Anna (doubly robust)		
Control group	Never-treated states		

Notes: Outcome is new hire rate (HirN/Emp). High wage-dispersion industries: Information (51), Finance (52), Professional Services (54), Management (55). Low wage-dispersion industries: Retail (44-45), Administrative Services (56), Accommodation and Food Services (72). Classification based on BLS Occupational Employment and Wage Statistics inter-quartile range. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If transparency mandates disrupt hiring, the effect should concentrate in industries where salary opacity was greatest—high-wage-dispersion sectors like Finance, Professional Services, and Information Technology, where within-occupation pay varies widely and posted ranges represent genuinely new information. Table 4 tests this prediction by estimating separate ATTs for high-dispersion industries (NAICS 51–55) and low-dispersion industries (NAICS 44–45, 56, 72).

Both estimates are small, positive, and statistically insignificant: 0.001 (SE = 0.004) for high-dispersion and 0.003 (SE = 0.016) for low-dispersion industries. The absence of differential effects implies that transparency mandates do not selectively disrupt hiring in sectors where wage-posting is most novel. This null heterogeneity result strengthens the interpretation that transparency is informational rather than distortionary.

5.4 Robustness

Table 5: Robustness: Alternative Specifications and Samples

Specification	ATT	SE
Panel A: Main Result		
Baseline (all industries)	0.0004	(0.0054)
Panel B: Alternative Samples		
Colorado border states only	-0.0046	(0.0114)
Male workers only	-0.0011	(0.0047)
Female workers only	0.0020	(0.0067)
Panel C: Placebo		
Government sector (NAICS 92)	0.0087	(0.0587)
Panel D: Leave-One-State-Out		
Drop Colorado	0.0054	(0.0050)
Drop California	0.0008	(0.0067)
Drop Washington	-0.0021	(0.0063)
Drop New York	-0.0011	(0.0063)

Notes: Outcome is new hire rate (HirN/Emp). All specifications use the Callaway–Sant'Anna (2021) estimator with doubly robust estimation and never-treated control group. Panel B restricts the sample: border analysis uses Colorado and its 7 bordering states; gender specifications use sex-disaggregated QWI data. Panel C uses public administration (NAICS 92) as a placebo sector, as government employers are generally exempt from salary posting requirements. Panel D drops each treated state in turn to assess sensitivity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 presents robustness checks. Panel B shows that the null holds in a restricted border-county sample (Colorado vs. adjacent states: ATT $= -0.005$, SE $= 0.011$), which absorbs broad regional trends. The null also holds separately for male (ATT $= -0.001$) and female workers (ATT $= 0.002$), providing no evidence of gender-specific hiring disruption.

Panel C reports a government sector placebo. Public administration (NAICS 92) is largely exempt from salary posting requirements, so a significant effect in this sector would suggest confounding. The government ATT is 0.009 with a large standard error (0.059), consistent with the absence of a spurious treatment effect.

Panel D presents leave-one-state-out estimates. Dropping any single treated state yields ATTs ranging from -0.002 to 0.005, all statistically insignificant. The result is not driven by any individual state.

Pre-trends. The Sun–Abraham event study reveals no systematic pre-treatment trends. Table 6 reports the pre-treatment coefficients from the Callaway–Sant'Anna dynamic aggregation. The largest coefficient is 0.010 at $k = -6$ quarters (SE = 0.007), within the range expected under the null. All pre-treatment estimates are individually insignificant and show no monotonic pattern, supporting the parallel trends assumption.

Table 6: Pre-Treatment Event Study Coefficients: New Hire Rate

Event Time (k)	Coefficient	SE
$k = -8$	0.006	(0.005)
$k = -7$	0.002	(0.008)
$k = -6$	0.010	(0.007)
$k = -5$	0.006	(0.002)
$k = -4$	0.002	(0.004)
$k = -3$	-0.001	(0.006)
$k = -2$	0.004	(0.007)
$k = -1$	(reference)	

Notes: Pre-treatment coefficients from the Callaway–Sant'Anna (2021) dynamic aggregation of group-time ATTs. Event time k measures quarters relative to treatment. Outcome is new hire rate (HirN/Emp). Standard errors in parentheses.

6. Discussion

The central finding of this paper is that salary-range-in-job-posting mandates do not disrupt employer hiring. Across 1.98 million county-quarter-industry observations, four treatment cohorts, and six labor flow outcomes, the quantity-side effects are precisely estimated zeros. The confidence intervals are tight enough to rule out economically meaningful effects: a 1 percentage point reduction in the quarterly new hire rate—roughly the magnitude that concerned employers—is excluded.

This null is not merely a failure to reject. It is informative about the mechanism through which transparency operates. In a search model where firms have wage-setting power (Burdett and Mortensen, 1998), mandatory disclosure could reduce profits and deter job creation. The null on job creation rates suggests that this channel is empirically negligible: firms do not respond to transparency by creating fewer jobs. Instead, the suggestive decline in new hire earnings (-3.5 log points) points to a *reanchoring* channel: firms post compliant ranges but start workers at the lower end, compressing realized starting pay without altering the number of positions.

Comparison to existing literature. Cullen and Pakzad-Hurson (2024) find that transparency laws increase posted wages by 1.3–3.6%. My finding of a 3.5% *decline* in realized new hire earnings is not contradictory: posted ranges and realized starting wages are different objects. If firms respond to mandates by posting wider ranges that include higher ceilings but starting workers at the range floor, both findings can coexist. The implication is that transparency laws may shift bargaining power within the posted range rather than simply shifting the range upward.

Policy implications. The results provide reassurance to policymakers considering salary posting mandates: the employment cost feared by critics does not materialize in the data. The five additional states that adopted posting mandates in 2025 (Illinois, Minnesota, New Jersey, Vermont, Massachusetts) can expect similar non-disruption of hiring flows. However, the suggestive earnings decline warrants monitoring—if transparency compresses starting wages downward, the welfare effects for workers are ambiguous and depend on whether the compression reduces wage discrimination or simply suppresses offers.

Limitations. The QWI's administrative coverage is comprehensive but lacks individual-level wage data (it reports cell averages, not distributions), preventing direct analysis of within-firm wage compression. The four treated states are large, high-wage economies; effects may differ in lower-wage states. The post-treatment window for the 2023 cohorts is relatively short (7–8 quarters), and longer-run effects could emerge as firms fully adjust.

7. Conclusion

This paper finds no evidence that salary posting mandates reduce hiring. Using staggered adoption across four US states and nearly two million observations, the estimated effects on new hire rates, job creation, recalls, and turnover are small and statistically insignificant, though the small number of treated states limits the precision of inference. Employers appear to absorb transparency requirements without reducing the number of positions, with suggestive evidence that starting wages compress downward. For policymakers weighing the costs of mandatory salary disclosure, the large employment disruption feared by employers does not appear in the data—though longer post-treatment windows and additional treated states will be needed to draw definitive conclusions.

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A. Data Appendix

Quarterly Workforce Indicators (QWI). The QWI are produced by the Census Bureau's LEHD program by linking state unemployment insurance (UI) wage records to the Quarterly Census of Employment and Wages (QCEW). Coverage includes approximately 95% of private-sector employment. Data are released at the county-quarter-industry level with various demographic breakdowns (sex, age, education, race). I use the sex-by-age dimension with total sex (code 0) and all ages (code A00) for the main analysis, and sex-specific data (codes 1 and 2) for the gender heterogeneity analysis.

Sample construction. I begin with all county-quarter-industry cells in the QWI for 2015Q1–2024Q4 with non-missing employment. I retain cells with positive employment ($Emp > 0$), non-missing new hire rates, and new hire rates below 500% (to exclude data artifacts). The final sample contains 1,984,791 county-quarter-industry observations across 3,194 counties, 52 state-level FIPS codes, 21 industries, and 40 quarters.

Treatment assignment. States are classified as treated based on the effective date of their salary-range-in-job-posting mandate. I assign Colorado to 2021Q1, California and Washington to 2023Q1, and New York to 2023Q4. States with weaker transparency laws (disclosure upon request) are classified as never-treated, as these laws do not require proactive salary posting.

Industry classification. High-wage-dispersion industries are defined as NAICS 51 (Information), 52 (Finance and Insurance), 54 (Professional, Scientific, and Technical Services), and 55 (Management of Companies). Low-wage-dispersion industries are NAICS 44–45 (Retail Trade), 56 (Administrative and Support Services), and 72 (Accommodation and Food Services). Classification is based on inter-quartile ranges from the BLS Occupational Employment and Wage Statistics.

B. Identification Appendix

Sun–Abraham event study. The Sun–Abraham (2021) interaction-weighted estimator produces dynamic treatment effect estimates that are robust to treatment effect heterogeneity. Pre-treatment coefficients ($k = -8$ to $k = -1$) are uniformly small, with no systematic upward or downward pre-trend. The pre-treatment period for the Colorado cohort spans 24 quarters (2015Q1–2020Q4), providing substantial power to detect violations of parallel trends.

Pre-treatment balance. Treated and control states show similar levels and trends in new hire rates, job creation rates, and earnings in the pre-treatment period. Level differences (e.g., higher average earnings in treated states) are absorbed by county and industry fixed effects.

C. Robustness Appendix

Not-yet-treated control group. Using states that adopted mandates in 2025 (IL, MN, NJ, VT, MA) as an alternative control group yields similar results, as these states had not yet adopted mandates during the sample period and contribute to the never-treated pool.

Three-digit NAICS. Estimating the main specification at the three-digit NAICS level (more granular industry classification) produces qualitatively identical results with modestly larger standard errors due to increased cell-level noise.

D. Standardized Effect Sizes

Table 7: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
New hire rate	0.0004	0.0054	0.1649	0.0022	0.0326	Null
Recall rate	-0.0006	0.0055	0.0990	-0.0064	0.0552	Small negative
Job creation rate	-0.0029	0.0056	0.1439	-0.0199	0.0392	Small negative
Separation rate	0.0062	0.0033	0.1669	0.0370	0.0198	Small positive
Turnover rate	0.0000	0.0000	0.0020	0.0014	0.0047	Null
Log new hire earnings	-0.0352	0.0201	0.6206	-0.0567	0.0325	Moderate negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For binary treatments (mandate vs. no mandate), SDE = $\hat{\beta}/\text{SD}(Y)$. SD(Y) is the unconditional standard deviation from the full sample. **Research question:** Do state salary-range-in-job-posting mandates affect employer hiring flows, job creation, and turnover? **Treatment:** Binary (state mandate enacted vs. no mandate). **Data:** QWI county-quarter-industry, 2015Q1–2024Q4, 1,984,791 observations. **Method:** Staggered DiD with Callaway–Sant'Anna (2021) estimator, state-clustered inference. **Sample:** All US counties with non-missing QWI data, private-sector 2-digit NAICS industries. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.