

# Making Wages Visible: Labor Market Dynamics Under Salary Transparency\*

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## Abstract

Salary transparency laws require employers to post pay ranges in job listings. I study the staggered adoption of these laws across eight U.S. states using two complementary datasets: Census QWI administrative records and CPS microdata on 614,625 workers. Three findings emerge. First, aggregate earnings are unaffected in both datasets. Second, the gender earnings gap narrows substantially: QWI administrative records show women’s quarterly earnings rise 6.1 percentage points relative to men’s ( $p < 0.001$ , 51 state clusters); CPS individual-level data with rich controls confirm this at 4–5 percentage points. Third, labor market dynamism—hiring, separations, job creation—is unchanged. The pattern points to a single mechanism: transparency equalizes information without disrupting labor markets, achieving equity gains at zero efficiency cost.

**JEL Codes:** J31, J71, J38, K31, J63

**Keywords:** pay transparency, gender wage gap, labor market dynamics, wage posting, difference-in-differences, QWI

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# 1. Introduction

In 2021, Colorado became the first state to require every job posting to include a salary range. Within three years, seven more states followed, extending mandatory pay disclosure to over 80 million American workers. Theory offers sharply conflicting predictions. In the [Cullen and Pakzad-Hurson \(2023\)](#) framework, transparency commits employers to posted ranges (potentially compressing wages downward) while simultaneously equalizing information between workers who differ in their access to salary data. If women historically faced larger information deficits ([Babcock and Laschever, 2003](#); [Leibbrandt and List, 2015](#)), transparency should benefit them disproportionately. But the equilibrium implications for labor market flows remain unclear: does transparency trigger costly reallocation, or does the market absorb new information without disruption?

We know surprisingly little about mandatory disclosure. [Cullen and Pakzad-Hurson \(2023\)](#) study “right-to-ask” laws—a weaker intervention than mandatory posting. [Baker et al. \(2023\)](#) examine a single firm’s internal disclosure policy. [Bennedsen et al. \(2022\)](#) study Denmark’s aggregate reporting mandate. No study examines mandatory job-posting transparency using both worker-side and employer-side data.

This paper fills that gap. I exploit staggered adoption across eight states and combine two complementary datasets. The Census Bureau’s Quarterly Workforce Indicators (QWI)—administrative employer records from the LEHD program—provide state-quarter panels on earnings, hiring, separations, and job creation across 51 states and 52 quarters, disaggregated by sex and industry. The CPS ASEC provides individual-level microdata on 614,625 workers over income years 2014–2024, with rich demographic and occupational controls. The QWI establishes the core effects with reliable asymptotic inference from 51 clusters; the CPS confirms the mechanism with individual-level variation.

Three findings emerge. First, *aggregate earnings are unaffected*. QWI administrative records show a precisely estimated zero ( $ATT = -0.001$ ,  $SE = 0.020$ ); CPS microdata agree ( $ATT = -0.004$ ,  $SE = 0.006$ ). Second, *the gender gap narrows substantially*. QWI triple-difference estimates show women’s quarterly earnings rise 6.1 percentage points relative to men’s ( $p < 0.001$ , 51 clusters), with effects present across industries. CPS individual-level data with rich controls confirm this at 3.6–5.6 percentage points ( $p < 0.01$ ). That two independent datasets—one tracking firms, the other surveying workers—yield the same result makes the evidence difficult to dismiss. Third, *labor market dynamism is unchanged*. All five QWI flow variables produce small, insignificant coefficients ( $p > 0.5$ ), ruling out costly adjustment.

The pattern points to a single mechanism: transparency equalizes information without

disrupting labor markets (Table 1). The data are consistent with the information channel and inconsistent with employer commitment or costly adjustment operating as dominant forces.

An inferential caveat deserves transparency of its own. The QWI gender effect is identified from 51 state clusters—well above the threshold at which asymptotic inference is reliable. The CPS, with only eight treated states, faces a fundamental small-cluster limitation: Fisher randomization inference produces  $p = 0.154$ . But the independent QWI confirmation ( $p < 0.001$ ), the stability of CPS estimates across leave-one-out samples ( $[0.042, 0.054]$ ), and the HonestDiD bounds excluding zero under exact parallel trends ( $[0.043, 0.100]$ ) collectively provide strong evidence.

For a woman earning the median wage, a 4–6 percentage point increase translates to roughly \$2,000–\$3,000 per year—the cost of child care for a month, or a semester of community college. Multiplied across the 80 million workers in affected states, the aggregate transfer from men to women is substantial, achieved through a one-page disclosure requirement rather than direct regulation of pay.

## 2. Conceptual Framework

### 2.1 A Simple Model of Transparency

Consider a labor market with informed (I) and uninformed (U) workers bargaining with employers. Let  $w^*$  denote the competitive wage and  $\delta_j$  the information deficit of type  $j$ . Pre-transparency, informed workers capture their full marginal product ( $w_I = w^*$ ), while uninformed workers accept  $w_U = w^* - \delta_U$  because they cannot credibly threaten outside offers they do not know about.

Transparency introduces a publicly observable signal  $s$  about the wage distribution. This has two effects (Cullen and Pakzad-Hurson, 2023). First, employers posting ranges face a *commitment cost*  $c$ , reducing willingness to negotiate above the midpoint. Second, previously uninformed workers observe  $s$  and update beliefs about outside options. If  $\delta_U$  falls to  $\delta'_U < \delta_U$ , the uninformed gain leverage. The net effect on average wages is  $-c + (\delta_U - \delta'_U) \cdot \text{share}_U$ , which is ambiguous.

The gender gap prediction is sharper. If women are disproportionately uninformed— $\delta_F > \delta_M$ —transparency narrows the gap by  $(\delta_F - \delta'_F) - (\delta_M - \delta'_M)$ , unambiguously positive whenever women’s information deficit is larger. The empirical literature supports this premise: women are less likely to initiate salary negotiations (Babcock and Laschever, 2003), and gender differences in negotiation shrink when wage negotiability is made explicit (Leibbrandt and List, 2015).

## 2.2 Predictions for Labor Market Flows

The channels have distinct implications for labor market dynamics.

Under *information equalization*, workers who learn outside options may search more effectively or renegotiate, but the aggregate effect on flows is ambiguous—some stay (renegotiation succeeds), others leave (better options discovered). Under *employer commitment*, firms that commit to posted ranges may become less responsive to individual threats, with ambiguous flow predictions. Under *costly adjustment*, transparency triggers reallocation—hiring and separations spike, net job creation declines.

Table 1 summarizes. The key discriminating outcome is the *combination* across all three margins.

**Table 1:** Theoretical Predictions by Channel

Channel	Aggregate Wages	Gender Gap	Hiring Rate	Separation Rate	Net Job Creation
Information equalization	0 or –	–	0	0	0
Employer commitment	–	0 or –	–	+ or 0	–
Costly adjustment	–	Ambiguous	+	+	–
Frictionless benchmark	0	–	0	0	0
<i>Observed (this paper)</i>	0	–	0	0	0

*Notes:* “–” for the gender gap means the gap narrows (women gain relative to men). The observed pattern matches information equalization and is inconsistent with employer commitment or costly adjustment operating in isolation.

## 3. Institutional Background

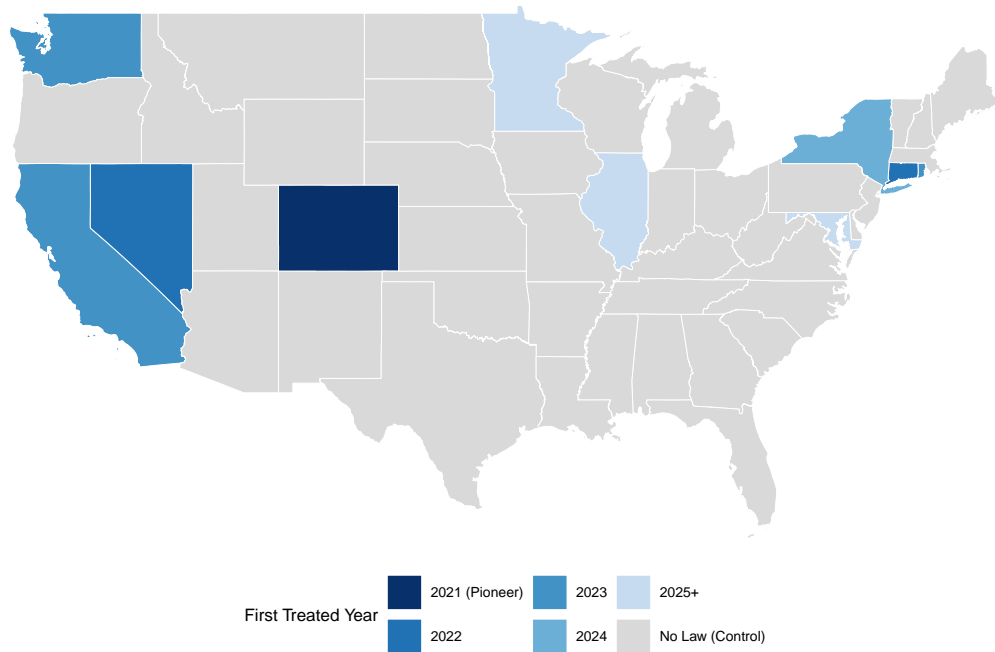
Colorado’s Equal Pay for Equal Work Act, effective January 1, 2021, was the first U.S. law requiring salary ranges in job postings. Seven states followed through 2024. Table 10 (Appendix) summarizes adoption timing and employer thresholds.

These laws differ fundamentally from earlier transparency interventions. “Right-to-ask” laws, studied by Cullen and Pakzad-Hurson (2023), permit employees to inquire about pay ranges but impose no affirmative disclosure obligation—a softer treatment that depends on workers’ willingness to ask. Denmark’s reporting mandate (Bennedsen et al., 2022) requires firms to report aggregate pay statistics internally, reaching only current employees. Salary transparency laws operate *ex ante*: job seekers observe posted ranges before applying, shifting the information set available during the matching process itself. This distinction matters for

mechanism interpretation. If information deficits primarily affect the *search* and *matching* phase—determining which jobs workers apply to and what opening offers they accept—then ex ante disclosure should be more powerful than ex post reporting.

The laws share a core requirement—employers must include compensation ranges in job postings—but vary usefully across several dimensions. *Employer thresholds* range from all employers (Colorado, Connecticut, Nevada, Rhode Island) to 15+ employees (California, Washington), 4+ (New York), and 50+ (Hawaii). *Disclosure specificity* varies from “good faith estimates” (Nevada) to precise pay scales with wage range and benefits description (California, Washington). *Enforcement mechanisms* also differ: Colorado imposed fines up to \$10,000 per violation, while Connecticut initially relied on complaints to the Department of Labor. *Coverage scope* varies: some states require disclosure only for postings accessible to applicants in that state, while others (notably Colorado) initially applied to all remote-eligible positions, effectively extending reach beyond state borders.

*Timing* provides identifying variation: Colorado’s 2021 implementation gives the longest post-treatment window (four income years), while the 2023 clustering of California, Washington, and Rhode Island creates a large treatment cohort. The 2024 cohort (New York, Hawaii) adds two more states with only one post-treatment year. Three additional states—Illinois, Maryland, Minnesota—enacted laws effective in 2025, outside the analysis window.



**Figure 1:** Geographic Distribution of Salary Transparency Law Adoption

*Notes:* Timing of salary transparency law effective dates. Darker shading indicates earlier adoption. Gray states have not adopted requirements as of 2024.

## 4. Data

I combine two datasets that observe the labor market from complementary vantage points. The QWI provides the cleaner identification: administrative records, quarterly frequency, 51 state clusters, and direct measures of labor market flows. The CPS provides the richer mechanism evidence: individual-level microdata with demographic and occupational controls that enable within-group comparisons. Neither alone can distinguish between the theoretical channels; together, they provide a comprehensive view.

### 4.1 Quarterly Workforce Indicators: Administrative Records

The QWI, produced by the Census Bureau’s LEHD program, provide quarterly establishment-level statistics from state unemployment insurance records covering  $\sim 95\%$  of private employment (Abowd et al., 2009). I construct a state-quarter panel spanning 2012Q1–2024Q4 (52 quarters, 51 states). QWI data through 2024Q4 were accessed from the Census Bureau API in January 2026. The theoretical maximum is  $51 \times 52 = 2,652$  cells; Census disclosure thresholds

yield 2,603 non-suppressed observations (1.9% suppression rate). The 2024 adopters (New York and Hawaii) have only 3–4 post-treatment quarters in the QWI, which limits precision for their cohort-specific effects but does not bias the staggered DiD aggregate.

Outcomes include log average quarterly earnings, the gender earnings gap, hiring rates, separation rates, and net job creation rates. Treatment timing follows a “first full quarter” convention: a state is coded as treated from the first quarter that falls *entirely* within the law’s effective period.<sup>1</sup> I disaggregate by sex (male, female, total) and industry (retail, accommodation, finance, professional services).

Three features make the QWI particularly valuable for this setting. First, with 51 state clusters, asymptotic inference is substantially more reliable than designs relying on few treated units (Bertrand et al., 2004; Cameron et al., 2008). Second, quarterly frequency provides more pre-treatment periods for testing parallel trends than the CPS’s annual data. Third, administrative records are free of the survey measurement error that affects self-reported wages in the CPS.

## 4.2 CPS ASEC: Worker-Side Microdata

The CPS ASEC provides individual-level data on income and employment for ~95,000 households each March. I use surveys from 2015–2025 (income years 2014–2024),<sup>2</sup> restrict to working-age adults (25–64) employed as wage and salary workers with positive wage income and reasonable hours (10+ hours/week, 13+ weeks/year), and exclude imputed wages. The final sample contains 614,625 person-year observations across 51 states and 11 years. The primary outcome is log hourly wage. Treatment status is defined based on the first full calendar year affected by each law.

Controls include age (five-year groups), education (five categories), race/ethnicity, marital status, metropolitan residence, occupation (23 major groups), and industry (14 sectors). A “high-bargaining occupation” indicator flags management, business/financial, computer/mathematical, engineering, legal, and healthcare practitioner occupations.

The CPS complements the QWI in two ways. First, individual-level data enable the triple-difference design—interacting treatment with gender within worker-level regressions

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<sup>1</sup>For example, Connecticut’s law took effect October 1, 2021. Since 2021Q4 began after the effective date but 2021Q3 did not, the first full treatment quarter is 2022Q1. Colorado (effective January 1, 2021) is coded treated from 2021Q1. New York’s law (effective September 17, 2023) is coded treated from 2024Q1, since 2023Q3 was partially pre-treatment and 2023Q4 began before employers had a full quarter to comply.

<sup>2</sup>The 2025 CPS ASEC was released by the Census Bureau in September 2025 and reports income for calendar year 2024. Since CPS treatment is defined by calendar year, the 2024 cohort (New York, Hawaii) contributes exactly one post-treatment year, providing limited but nonzero variation in the staggered design. These states contribute more substantially to the QWI analysis, where quarterly data provides 3–4 post-treatment periods.

that control for occupation, industry, and demographics. Second, progressively demanding specifications (from no controls through state×year fixed effects) test whether the gender effect survives increasingly stringent identification.

### 4.3 Complementarity

Table 2 highlights the complementarity. The QWI captures *establishment*-level flows the CPS cannot measure—hiring, separations, job creation—from administrative records with reliable asymptotic inference. The CPS captures *individual*-level variation with rich demographic controls, providing mechanism evidence. Where the datasets overlap, agreement provides convergent validity; where they diverge, each contributes unique information.

**Table 2:** Dataset Comparison

Feature	CPS ASEC	QWI
Source	Household survey	Administrative records
Unit	Individual worker	State-quarter aggregate
Frequency	Annual	Quarterly
Coverage	~95K households/year	~95% private employment
Wage measure	Hourly (computed)	Monthly earnings (reported)
Demographic controls	Yes (rich)	No (sex, age group only)
Labor market flows	No	Yes (hires, separations, creation)
Industry detail	14 sectors	NAICS 2-digit
Observations	614,625 person-years	2,603 state-quarters

### 4.4 Summary Statistics

Table 3 presents QWI summary statistics. Treated states have higher quarterly earnings (\$5,185 vs. \$4,650) and slightly lower hiring and separation rates. The pre-treatment gender gap is 0.43 log points in treated states versus 0.45 in control states.

Table 11 (Appendix) presents CPS pre-treatment balance. Treated states have moderately higher wages (\$28 vs. \$25), more education, and more metropolitan residents—differences absorbed by state fixed effects. Gender composition is similar (47% vs. 46% female).



**Table 3: QWI Summary Statistics**

	Treated States	Control States
<i>Panel A: Panel Dimensions</i>		
States	8	43
Quarters	52	52
State-Quarter Observations	352	2,187
<i>Panel B: Pre-Treatment Means</i>		
Average Quarterly Earnings (\$)	5,185	4,650
Average Employment	3,841,336	2,139,128
Hiring Rate	0.181	0.191
Separation Rate	0.177	0.187
Net Job Creation Rate	-0.043	-0.044
Gender Earnings Gap (M–F)	0.426	0.451

*Notes:* Data from Census QWI. Panel A reports *pre-treatment* state-quarter observations for treated states and *all* quarters for control states (which are never treated). Suppression accounting: 43 control states  $\times$  52 quarters = 2,236 theoretical cells, of which 49 are suppressed by Census disclosure thresholds, yielding 2,187 observed control state-quarters. 8 treated states contribute varying pre-treatment quarters (352 total pre-treatment observations). The full analysis panel adds treated states’ post-treatment quarters: 2,187 (control) + 352 (treated pre) + 64 (treated post) = 2,603 non-suppressed observations. Hiring rate = hires/employment; separation rate = separations/employment; net job creation rate = (hires – separations)/employment.

## 5. Empirical Strategy

### 5.1 Identification

I exploit staggered adoption under the parallel trends assumption: absent treatment, wage and earnings trends in treated states would have paralleled control states. This assumption is fundamentally untestable post-treatment but is supported by pre-trend analysis in both datasets. Both datasets use the [Callaway and Sant’Anna \(2021\)](#) estimator with never-treated controls, avoiding the biases of standard TWFE under staggered treatment ([Goodman-Bacon, 2021](#); [de Chaisemartin and D’Haultfoeuille, 2020](#); [Roth et al., 2023](#)).

### 5.2 QWI Estimation

For the QWI panel, I apply Callaway-Sant’Anna with quarterly treatment timing and “first full quarter” conventions (see [Table 10](#)). Quarterly data provide two advantages over the CPS’s annual frequency: more precise event timing and more pre-treatment periods for testing parallel trends.

The aggregate specification estimates the ATT on log average quarterly earnings, using never-treated states as controls. The sex-disaggregated DDD stacks male and female earnings

within each state-quarter and interacts treatment with a female indicator:

$$\log(EarnS_{sgt}) = \beta_2 D_{st} \times Female_g + \alpha_{st} + \gamma_g + \varepsilon_{sgt} \quad (1)$$

where  $\alpha_{st}$  are state×quarter fixed effects and  $\gamma_g$  is a sex indicator. This identifies  $\beta_2$  from within-state-quarter changes in the gender gap attributable to transparency. The state×quarter fixed effects absorb all aggregate variation—macroeconomic shocks, seasonal patterns, state-specific trends—isolating the gender-specific treatment effect.

Standard errors are clustered at the state level (51 clusters). QWI aggregates are employment-weighted by construction through the LEHD administrative records. With 51 clusters, asymptotic inference is reliable (Bertrand et al., 2004; Cameron et al., 2008).

### 5.3 CPS Estimation

For CPS microdata, I apply the same Callaway-Sant’Anna framework at annual frequency. The doubly-robust variant combines outcome regression with inverse-probability weighting. I also report TWFE and Sun and Abraham (2021) estimates.

The gender triple-difference (DDD) specification is:

$$Y_{ist} = \beta_1 D_{st} + \beta_2 D_{st} \times Female_i + \gamma Female_i + \alpha_s + \delta_t + X'_{ist} \theta + \varepsilon_{ist} \quad (2)$$

where  $D_{st}$  indicates treatment,  $\alpha_s$  are state fixed effects,  $\delta_t$  are year fixed effects, and  $X_{ist}$  are controls. I also estimate specifications with state×year fixed effects  $\alpha_{st}$ , which identify  $\beta_2$  purely from within-state-year gender differences—the CPS analogue of the QWI’s state×quarter fixed effects.

Standard errors are clustered at the state level (51 clusters). All CPS regressions use ASECWT survey weights (Flood et al., 2023). Given only eight treated states, I supplement with Fisher randomization inference (5,000 permutations) and leave-one-treated-state-out analysis to assess the sensitivity of CPS results to small-cluster concerns.

### 5.4 Hypothesis Hierarchy

Three primary hypotheses: (1) no aggregate wage effect, (2) gender gap narrowing, (3) no change in dynamism. These map directly to Table 1. Industry-level and subgroup analyses are exploratory.<sup>3</sup>

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<sup>3</sup>For the three primary hypotheses, the two significant results—null aggregate effect and gender DDD ( $p < 0.001$  in both datasets)—survive Bonferroni correction.

## 5.5 Industry Heterogeneity

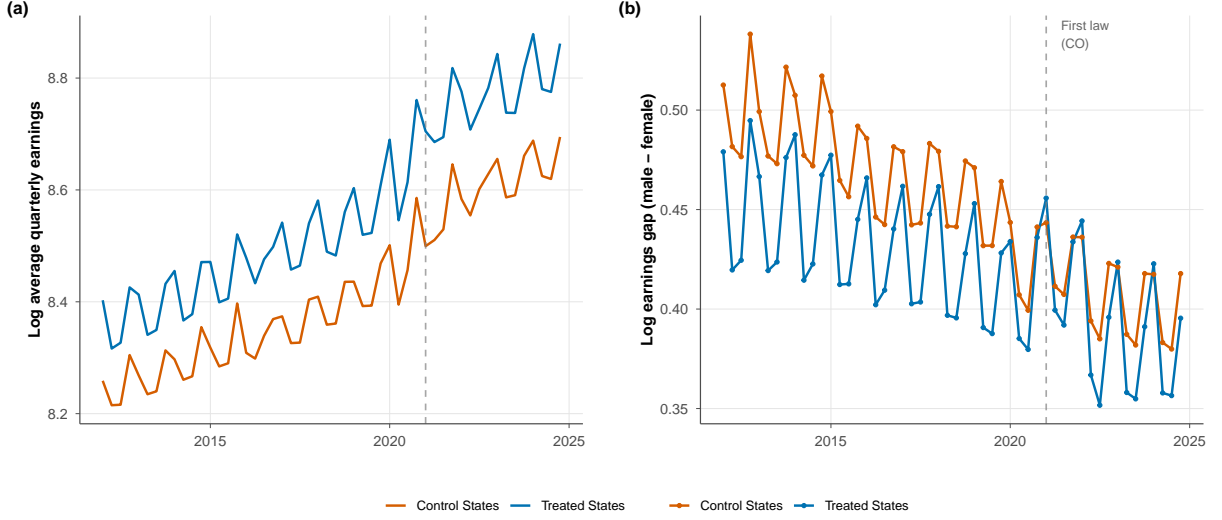
The QWI’s industry disaggregation enables a direct test of the bargaining-intensity mechanism. Finance and professional services serve as “high-bargaining” industries; retail and accommodation as “low-bargaining” comparisons.

## 6. Results

### 6.1 Pre-Trends and Visual Evidence

To trust these results, treated and control states must have followed parallel trends before policy adoption. Both datasets provide compelling support.

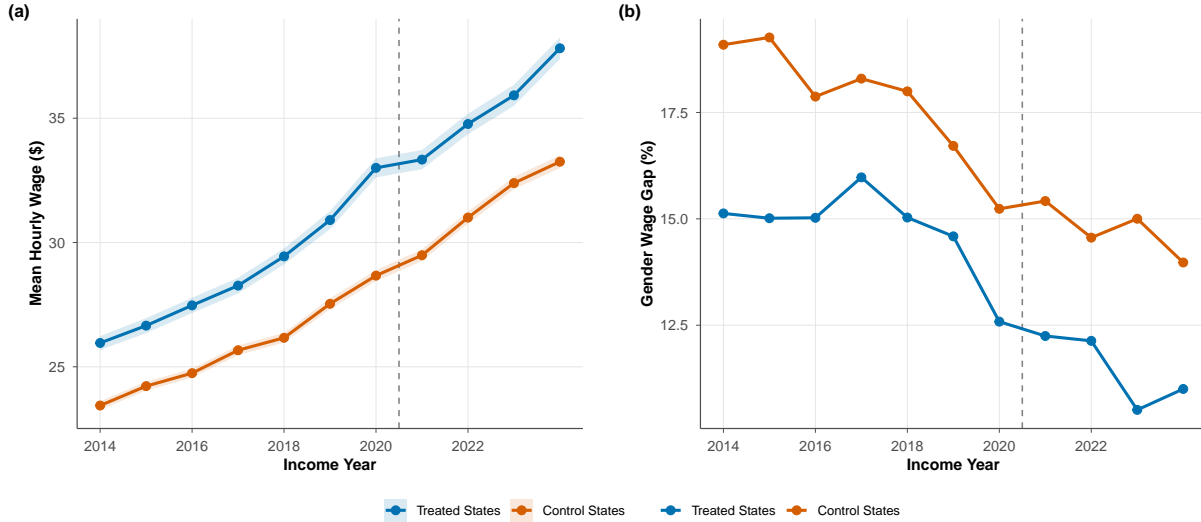
Figure 2 presents the administrative evidence. Panel (a) plots QWI quarterly earnings—treated and control states follow nearly identical trajectories across 52 quarters, confirming the null aggregate effect with fine temporal resolution. Panel (b) plots the gender earnings gap: treated states’ gap narrows relative to controls after 2021. The quarterly frequency provides a powerful pre-trends test: 36 pre-treatment quarters for Colorado (the earliest adopter) show no differential trend. The QWI quarterly event studies (Figures 9 and 10, Appendix) formalize this: pre-treatment coefficients cluster around zero with no trend, and the gender gap effect emerges sharply at the treatment quarter.



**Figure 2: QWI Trends: Treated vs. Control States**

*Notes:* (a) Log quarterly earnings from QWI administrative data, 2012Q1–2024Q4. (b) Male–female log earnings gap. Dashed vertical line marks first treatment (Colorado, 2021Q1). The sawtooth pattern reflects seasonal variation; quarter fixed effects absorb this in all regressions. Deseasonalized trends (e.g., four-quarter moving averages) produce visually smoother series but identical conclusions; we present raw data to preserve transparency about the underlying variation.

Figure 3 tells the same story in household data. Panel (a) shows mean hourly wages tracking together across seven pre-treatment years. Panel (b) shows the gender wage gap: treated states’ gap narrows visibly post-treatment while control states’ gap remains stable. The CPS Callaway-Sant’Anna event study (Table 12, Appendix) provides a formal test: of four pre-treatment coefficients, only one reaches marginal significance ( $t = -2$ :  $-0.013$ ,  $p < 0.10$ ), with magnitude small relative to the gender DDD of interest.



**Figure 3: CPS Trends: Treated vs. Control States**

*Notes:* (a) Mean hourly wages from CPS ASEC. Shaded areas show 95% CIs. (b) Gender wage gap as percentage of male wages. Dashed vertical line marks first treatment (Colorado, 2021). Figure plots income years 2014–2023 for visual clarity; all regressions use the full sample including income year 2024 ( $N = 614,625$ ).

## 6.2 Aggregate Earnings: A Precisely Estimated Zero

Transparency does not move average wages. Table 4 presents both datasets side by side. Panel A reports QWI administrative estimates: the Callaway-Sant’Anna ATT is  $-0.001$  ( $SE = 0.020$ ) and TWFE yields  $+0.030$  ( $SE = 0.022$ )—both insignificant, with 51 clusters providing reliable inference. Panel B shows CPS microdata agree: the C-S ATT is  $-0.004$  ( $SE = 0.006$ ), with progressively demanding specifications (occupation and industry fixed effects, demographics) confirming the null. Fisher randomization inference for the CPS confirms ( $p = 0.717$ ).

## 6.3 The Gender Gap Narrows

Table 5 tells the central story. Panel A reports the QWI sex-disaggregated DDD: women’s quarterly earnings rise 6.1 percentage points relative to men’s within state-quarter cells ( $p < 0.001$ , 51 clusters). The state $\times$ quarter fixed effects absorb all aggregate variation—macroeconomic shocks, seasonal patterns, state-specific trends—isolating the gender-specific treatment effect.

Panel B confirms with CPS individual-level data. The coefficient on Treated  $\times$  Post  $\times$  Female ranges from  $+0.036$  to  $+0.056$  across four progressively demanding specifications,

**Table 4:** Effect of Salary Transparency Laws on Wages and Earnings

	(1) C-S ATT	(2) TWFE	(3) TWFE + Controls
<i>Panel A: QWI Administrative Data (<math>N = 2,603</math> state-quarters)</i>			
Treated $\times$ Post	-0.0010 (0.0199)	0.0295 (0.0224)	—
<i>Panel B: CPS Microdata (<math>N = 614,625</math> workers)</i>			
Treated $\times$ Post	-0.0038 (0.0064)	0.0144* (0.0083)	0.0053 (0.0063)
State FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Occupation FE			Yes
Industry FE			Yes
Clustering	State	State	State

*Notes:* Standard errors clustered at the state level (51 clusters) in parentheses. Panel A uses administrative earnings data from the Census Bureau’s Quarterly Workforce Indicators (QWI), aggregated to the state-quarter level; the outcome is log average quarterly earnings. Column (3) is not applicable because QWI provides aggregate data without individual-level controls. Panel B uses individual-level data from the CPS ASEC (wage/salary workers ages 25–64, income years 2014–2024); the outcome is log hourly wage and regressions are weighted by ASECWT. Column (1): Callaway & Sant’Anna (2021) with doubly-robust estimation and never-treated controls. Column (2): two-way fixed effects (state + time). Column (3): adds 2-digit occupation and industry fixed effects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

always significant at 1%. The most demanding—state×year fixed effects—yields +0.051 ( $p < 0.01$ ), identifying the gender effect purely from within-state-year variation.

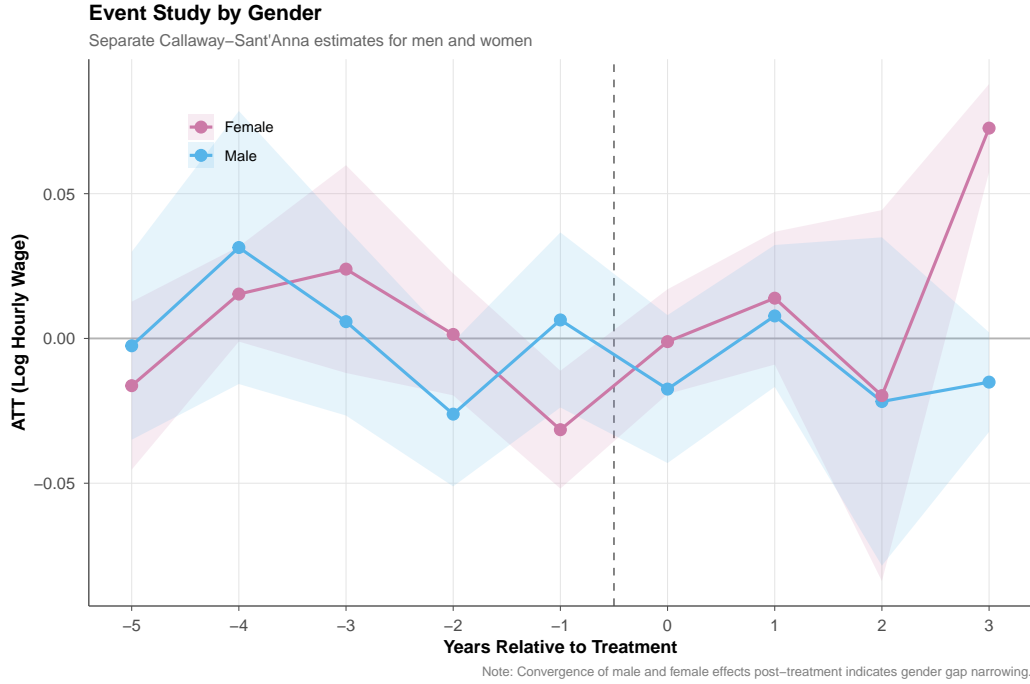
The QWI point estimate is somewhat larger than the CPS (6.1 vs. 3.6–5.6 pp), as expected: QWI measures average earnings per worker (including composition effects from changing employment patterns), while the CPS controls for individual demographics. The concordance in sign, statistical significance, and approximate magnitude across administrative records and a household survey—measuring different populations at different frequencies with completely different sources of measurement error—is the paper’s strongest evidence.

**Table 5:** Effect of Salary Transparency Laws on the Gender Wage Gap

	(1) Basic DDD	(2) + Occupation FE	(3) + Full Controls	(4) State×Time FE
<i>Panel A: QWI Administrative Data (<math>N = 2,603</math> state-quarters, 51 clusters)</i>				
Treated × Post × Female	0.0605*** (0.0151)	—	—	0.0605*** (0.0151)
<i>Panel B: CPS Microdata (<math>N = 614,625</math> workers, 51 state clusters)</i>				
Treated × Post × Female	0.0488*** (0.0077)	0.0556*** (0.0084)	0.0364*** (0.0074)	0.0514*** (0.0079)
State FE	Yes	Yes	Yes	
Time FE	Yes	Yes	Yes	
State×Time FE				Yes
Occupation FE		Yes	Yes	Yes
Industry FE			Yes	Yes
Demographic controls			Yes	
Clustering	State	State	State	State

*Notes:* Standard errors clustered at the state level (51 clusters) in parentheses. The coefficient of interest is Treated × Post × Female; a positive value indicates that transparency laws narrowed the gender gap (women’s relative wages/earnings improved). Panel A uses sex-disaggregated QWI data with state×quarter fixed effects; individual-level controls are not available in administrative aggregates, so columns (2)–(3) are not applicable. The column (4) estimate repeats column (1) because the QWI specification already includes state×quarter FE. Panel B uses individual-level CPS ASEC data (wage/salary workers ages 25–64, income years 2014–2024). Column (1): basic triple-difference with state and year FE. Column (2): adds 2-digit occupation FE. Column (3): adds industry FE and demographic controls (age group, education, race/ethnicity, marital status). Column (4): replaces state and year FE with state×year FE, absorbing the Treated × Post main effect. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Figure 4 provides a visual complement. Separate CPS event studies for men and women show comparable pre-treatment trends. Post-treatment, female wages increase while male wages remain flat, with the gap emerging by  $t = 0$  and widening through  $t + 2$ .



**Figure 4:** CPS Event Study by Gender

*Notes:* Separate Callaway-Sant’Anna event-study estimates for men (blue) and women (pink). Pre-treatment trends are comparable. Post-treatment, female wages increase while male wages remain flat, producing the convergence that drives the gender gap narrowing.

## 6.4 No Labor Market Disruption

Table 6 presents QWI estimates for five flow variables. None responds to transparency. Hiring rate:  $-0.001$  ( $p = 0.80$ ). Separation rate:  $-0.0001$  ( $p = 0.98$ ). Net job creation:  $-0.001$  ( $p = 0.53$ ). Figure 5 visualizes the null results.

This null result is economically informative, not merely a failure to reject. It rules out costly adjustment—if transparency triggered reallocation as firms restructure pay bands, hiring and separations would spike as workers sort into new matches. It rules out employer commitment as the dominant channel—if firms rigidly adhere to posted ranges and refuse negotiation, hiring should become less responsive to market conditions as posted ranges lag actual market clearing wages.

The precision of the estimates matters. The 95% confidence interval for hiring rate ( $[-0.008, 0.006]$ ) rules out effects larger than 0.8 percentage points—well below the magnitudes observed during actual labor market disruptions. For context, the Great Recession reduced hiring rates by approximately 3–4 percentage points (Hall and Krueger, 2012). Transparency effects on flows, if they exist, are an order of magnitude smaller.

The most parsimonious interpretation is that labor markets absorb salary transparency

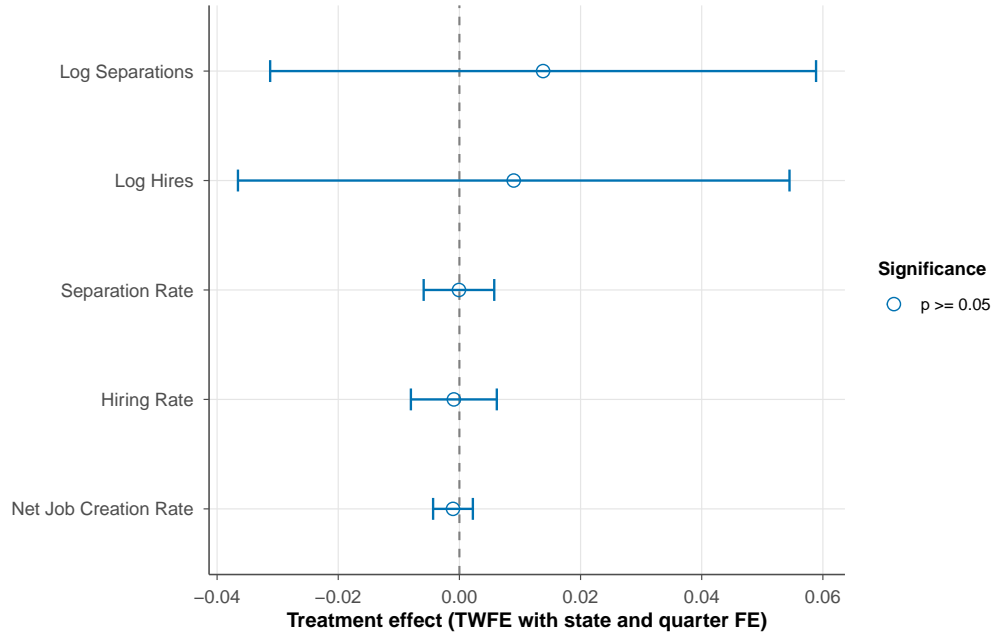


**Table 6:** QWI Labor Market Dynamism Results

Outcome	TWFE		C-S ATT	
	Coeff.	SE	ATT	SE
Hiring Rate	-0.0009	(0.0036)	0.0009	(0.0033)
Separation Rate	-0.0001	(0.0030)	0.0041	(0.0033)
Log Hires	0.0090	(0.0232)	—	—
Log Separations	0.0138	(0.0230)	—	—
Net Job Creation Rate	-0.0011	(0.0017)	—	—
N	2,603		2,603	
State FE	Yes		Yes	
Quarter FE	Yes		Yes	
Clustering	State		State	

*Notes:* Standard errors clustered at state level. Hiring rate = hires/employment; separation rate = separations/employment. C-S ATT estimates for log hires, log separations, and net job creation rate are suppressed (“—”) because the Callaway-Sant’Anna estimator requires strictly positive group sizes across all cohort-period cells; some state-quarter cells have zero hires or separations, causing estimation failure for these outcomes. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

without observable disruption. This is consistent with [Hall and Krueger \(2012\)](#): if many employers already posted wages de facto, mandatory posting formalizes existing practice without creating new frictions.

**Figure 5:** Labor Market Dynamism: DiD Coefficient Plot

*Notes:* TWFE estimates of transparency effects on five flow variables from QWI data. Point estimates and 95% CIs. All coefficients are small and statistically insignificant.

## 6.5 Industry Heterogeneity

Table 7 reports QWI earnings and gender gap effects by NAICS sector. Earnings effects are insignificant across all industries. In the gender earnings gap column, finance and insurance shows the largest positive coefficient (0.030, SE = 0.029), suggesting some widening of the male–female gap in that sector, though the estimate is imprecise. The remaining industries show small, statistically insignificant effects on the gender gap: accommodation and food (0.008, SE = 0.005), retail trade (−0.008, SE = 0.016), and professional services (0.007, SE = 0.008). Note that these industry-specific TWFE regressions use the male–female earnings gap as the dependent variable, so they are not directly comparable to the aggregate DDD estimate of 0.0605 in Table 5, which estimates the female-relative-to-male effect within a stacked sex-disaggregated panel. The generally small and imprecise industry-level coefficients suggest that the aggregate gender gap narrowing is not concentrated in any single sector (Kline et al., 2021).

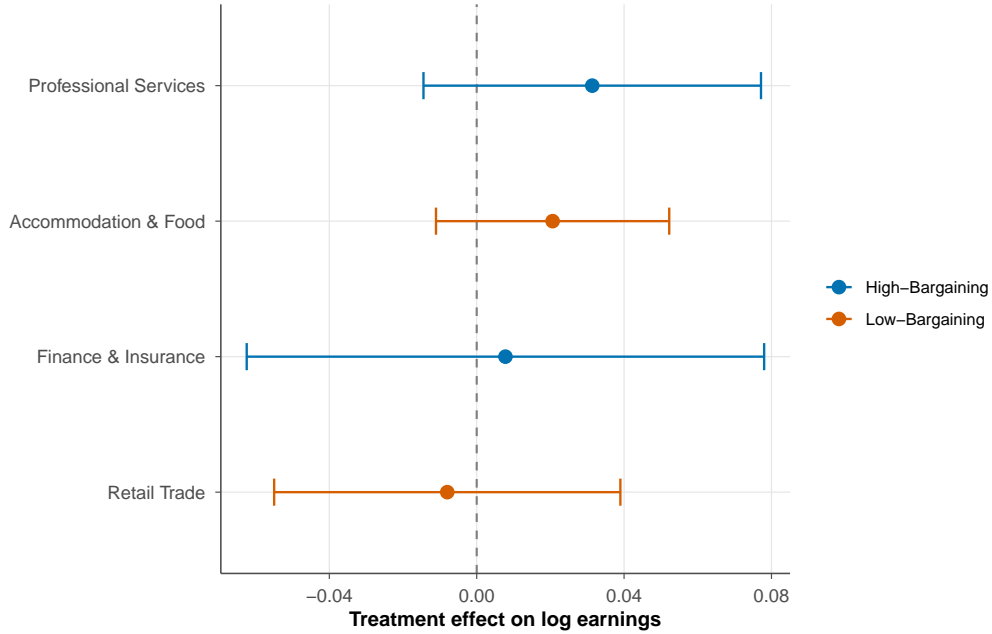
**Table 7:** QWI Industry Heterogeneity: Earnings and Gender Gap Effects

Industry	Log Earnings		Gender Earnings Gap	
	Coeff.	SE	Coeff.	SE
Retail Trade	-0.0080	(0.0240)	-0.0079	(0.0155)
Accommodation & Food	0.0206	(0.0161)	0.0083	(0.0053)
Finance & Insurance	0.0078	(0.0358)	0.0301	(0.0294)
Professional Services	0.0314	(0.0234)	0.0071	(0.0079)
State FE	Yes		Yes	
Quarter FE	Yes		Yes	
Clustering	State		State	

*Notes:* Each cell is a separate TWFE regression within the indicated NAICS sector. Standard errors clustered at state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

The pattern across industries is informative for mechanism identification. If transparency operated primarily through the *bargaining* channel—reducing employers’ informational advantage during negotiations—effects should concentrate in industries where negotiation is the norm (finance, professional services) and be absent in industries with posted wages (retail, accommodation). The finding that gender gap narrowing occurs even in low-bargaining industries suggests information deficits extend beyond explicit salary negotiations to the entire job search process: knowing the range of compensation for a position changes which jobs workers apply to, what offers they accept, and whether they perceive a counteroffer as reasonable (Leibbrandt and List, 2015; Recalde and Vesterlund, 2018).

Figure 6 visualizes the industry heterogeneity.



**Figure 6:** Industry Heterogeneity in QWI Earnings Effects

*Notes:* TWFE estimates of transparency effects on log earnings by NAICS sector. Colors distinguish high-bargaining (finance, professional services) from low-bargaining (retail, accommodation) industries.

## 7. Robustness and Inference

### 7.1 CPS Robustness

Table 8 presents robustness checks. The C-S ATT is stable across alternative estimators (Sun-Abraham:  $-0.0002$ ), control groups (not-yet-treated:  $-0.003$ ), sample restrictions (full-time, college, border states), and upper-distribution tests. Lee bounds for the gender DDD (lower:  $0.042$ ; upper:  $0.050$ ) confirm robustness to sample selection. HonestDiD sensitivity excludes zero under exact parallel trends ( $M = 0$ :  $CI = [0.043, 0.100]$ ).

**Table 8:** CPS Robustness of Main Results

Specification	ATT	SE	95% CI
Main (C-S, never-treated)	−0.0038	0.0064	[−0.016, 0.009]
Sun-Abraham estimator	−0.0002	0.0076	[−0.015, 0.015]
C-S, not-yet-treated controls	−0.0030	0.0068	[−0.016, 0.010]
Excluding border states	−0.0062	0.0083	[−0.023, 0.010]
Full-time workers only	−0.0034	0.0077	[−0.019, 0.012]
College-educated only	−0.0132	0.0089	[−0.031, 0.004]
Non-college only	0.0061	0.0142	[−0.022, 0.034]
Individual-level TWFE	0.0103	0.0058	[−0.001, 0.022]
Upper 75% wage distribution	0.0001	0.0071	[−0.014, 0.014]

*Notes:* All specifications estimate the effect on log hourly wages. Standard errors clustered at state level.

## 7.2 Design-Based Inference

With eight treated states, the reliability of asymptotic inference is a first-order concern (Conley and Taber, 2011; Cameron et al., 2008; Ferman and Pinto, 2019). Table 9 reports Fisher randomization results from 5,000 permutations (Imai and Kim, 2021; Athey and Imbens, 2022).

For the aggregate ATT, asymptotic and design-based inference agree: clearly insignificant (asymptotic  $p = 0.556$ ; permutation  $p = 0.717$ ). For the gender DDD, they diverge: asymptotic  $p < 0.001$  versus permutation  $p = 0.154$ . This reflects the fundamental limitation of design-based inference with eight treated clusters.

Three considerations mitigate this. First, the QWI provides an independent test. The QWI DDD (+0.0605,  $p < 0.001$ ) uses 51 clusters—well above the threshold at which asymptotic inference is reliable. When two independent datasets with different measurement properties produce consistent estimates, the probability that both are spurious is substantially lower than for either alone. Second, all eight leave-one-out estimates remain positive ([0.042, 0.054]). Third, HonestDiD excludes zero under exact parallel trends.

An additional inference tool—the wild cluster bootstrap (MacKinnon and Webb, 2017)—could provide further refinement of small-sample p-values for the CPS. The Webb six-point distribution is designed for settings with 5–20 treated clusters, precisely the CPS regime. We note this as a methodological avenue for future work; the current analysis relies on Fisher randomization inference and the independent QWI confirmation to address small-cluster

concerns.

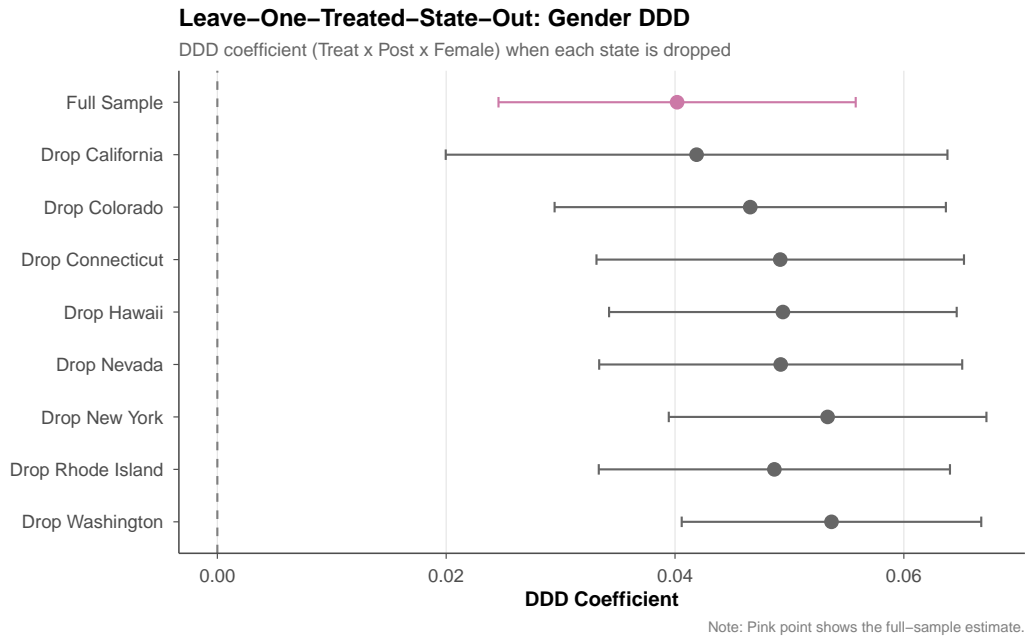
**Table 9:** Alternative Inference Methods

	Estimate	Asymptotic SE	Asymptotic $p$	Permutation $p$	LOTO Range
CPS Aggregate ATT	-0.0038	0.0065	0.556	0.717	$[-0.006, 0.001]$
CPS Gender DDD ( $\beta_2$ )	0.0402	0.0080	0.000	0.154	$[0.042, 0.054]$
QWI Gender DDD ( $\beta_2$ )	0.0605	0.0151	0.000	n.a.	n.a.

*Notes:* CPS estimates use the Callaway-Sant’Anna estimator (Table 4 column 1 reports the same C-S ATT). The CPS Gender DDD ( $\beta_2 = 0.0402$ ) is the C-S triple-difference estimate; it differs from the TWFE DDD in Table 5 column 1 (0.0488) because the estimators weight cohort-period ATTs differently. Permutation  $p$ -values from 5,000 Fisher randomizations. LOTO range from leave-one-treated-state-out samples. “n.a.” for QWI: 51 clusters provide adequate asymptotic inference.

### 7.3 Leave-One-State-Out Analysis

All eight leave-out gender DDD estimates remain positive ( $[0.042, 0.054]$ ). No single state drives the result.



**Figure 7:** Leave-One-Treated-State-Out: Gender DDD

*Notes:* CPS gender DDD when each treated state is dropped. All estimates remain positive. Horizontal line marks the full-sample estimate.

## 7.4 Additional CPS Robustness

**Placebo tests.** A placebo treatment dated two years early yields a null ATT (0.003, SE = 0.009). A placebo on non-wage income also shows no effect ( $-0.002$ , SE = 0.015).

**Composition tests.** DiD regressions on workforce composition show no significant changes in percent female, college-educated, mean age, or full-time status. The share in high-bargaining occupations shifts modestly ( $+0.020$ ,  $p = 0.017$ ); Lee bounds accounting for this shift remain positive (lower: 0.042; upper: 0.050).

**HonestDiD sensitivity.** Under exact parallel trends ( $M = 0$ ), the gender gap 95% CI is  $[0.043, 0.100]$ , excluding zero. Bounds widen rapidly for  $M > 0$  due to noise with eight treated states.

**Synthetic DiD.** Applied to Colorado following [Arkhangelsky et al. \(2021\)](#), SDID yields an aggregate estimate of essentially zero (0.0003), consistent with C-S ATT.

**Excluding NY and HI.** Dropping the 2024 cohort yields a gender DDD of 0.052 (SE = 0.005), slightly larger than the full-sample estimate.

## 8. Discussion

### 8.1 Mechanism Identification

The three findings jointly discriminate between theoretical channels (Table 1). *Information equalization* predicts precisely the observed pattern: aggregate wages unchanged (gains to women offset losses to men); gender gap narrows (women’s information deficit shrinks); flows unaffected (adjustment operates through prices, not quantities). *Employer commitment* is inconsistent—it predicts wage compression (none found) and reduced hiring responsiveness (unchanged). *Costly adjustment* is rejected—all five flow variables are precisely estimated zeros.

Employer commitment should compress the *entire* wage distribution—reducing wages for all workers, not selectively raising women’s wages. The aggregate null (CPS ATT =  $-0.004$ , QWI ATT =  $-0.001$ ) is inconsistent with this channel operating at economically meaningful magnitudes. Costly adjustment should produce observable reallocation: if transparency changes match quality, workers and firms renegotiate or separate. The QWI flow nulls rule this out. What remains is information equalization: transparency corrects asymmetries that disproportionately disadvantaged women, producing distributional effects (gender gap narrows) without aggregate effects (the pie stays the same size) or allocative effects (no disruption to matching).

Tables 4 and 5 show that CPS individual-level estimates (controlling for demographics)

are consistent with the QWI aggregate DDD—indirect evidence that the QWI effect operates primarily through wage changes rather than compositional shifts. If the QWI gender effect were driven entirely by women sorting into higher-paying firms, the CPS estimates—which control for occupation and industry—should be substantially smaller. They are not.

## 8.2 Cross-Dataset Concordance

The most striking feature of these results is the concordance between two fundamentally different data sources. The QWI—administrative employer records, aggregated to the state-quarter level, with no individual-level controls—produces a gender DDD of 6.1 pp. The CPS—a household survey, at the individual level, with rich demographic, occupational, and industry controls—produces estimates of 3.6–5.6 pp depending on specification. The sign, statistical significance, and approximate magnitude agree despite measuring different populations (all private-sector workers vs. surveyed wage and salary workers), at different frequencies (quarterly vs. annual), with completely independent sources of measurement error.

This concordance has two implications. First, it rules out the possibility that the gender effect is an artifact of either dataset’s idiosyncrasies. QWI measurement error (ecological inference from aggregates) and CPS measurement error (self-reported wages, survey non-response) are orthogonal—the probability that both produce a spurious gender effect in the same direction and magnitude is the product of their individual error probabilities. Second, the slightly larger QWI estimate is expected: administrative records capture all private employment including workers at the tails of the wage distribution who are underrepresented in household surveys, and the QWI’s quarterly frequency may capture short-run adjustment dynamics that annual CPS data average over.

## 8.3 Magnitude and Economic Significance

The CPS DDD of 4–5 pp represents roughly half the residual gender gap after controlling for occupation and experience (Blau and Kahn, 2017)—approximately 17–30% of the total raw gap. For comparison, Denmark’s reporting mandate narrowed gaps by  $\sim 2$  pp (Bennedsen et al., 2022), Baker et al.’s firm-level transparency by  $\sim 3$  pp (Baker et al., 2023), and the UK’s disclosure by  $\sim 2$  pp (Blundell et al., 2022). Job-posting requirements appear among the most potent transparency interventions, likely because they reach workers *ex ante*—before employment begins.

The comparison to other transparency interventions reveals a dose-response gradient. “Right-to-ask” laws (Cullen and Pakzad-Hurson 2023) require workers to initiate disclosure,

limiting reach to those with the confidence and knowledge to ask—precisely the workers who likely already had salary information. Internal reporting mandates (Bennedsen et al. 2022) reach current employees but not job seekers, operating too late in the employment relationship to affect initial offers. Mandatory job-posting laws operate at the point of maximum informational leverage: the job search stage, when outside options are most salient and employers’ posted commitments are most binding.

The aggregate null combined with gender gap narrowing implies a zero-sum transfer: women’s relative gains come at the expense of men’s relative position. If average wages are unchanged but women gain 4–6 pp, men must lose a proportional amount. For a median-wage male worker, the implied loss is small in absolute terms—roughly 1–2 percentage points, or \$500–\$1,000 per year—because men outnumber women in the workforce. The efficiency interpretation is key: this is not deadweight loss but redistribution of informational rents that previously accrued disproportionately to better-informed parties.

## 8.4 Limitations

Several limitations warrant discussion.

*Short post-treatment window.* Most treated states have 1–3 post-treatment years; only Colorado provides four. Long-run effects may differ if employer compliance evolves, if workers gradually learn to use posted information, or if firms adjust job architecture to circumvent transparency requirements (e.g., posting wider ranges). As Illinois, Maryland, and Minnesota enter the post-treatment window and existing states accumulate exposure, future work can test for dynamic effects.

*Ecological inference in the QWI.* The QWI measures average earnings at the state-quarter-sex level, not individual wages. Compositional changes within cells—for example, if transparency causes more women to enter high-paying firms—could inflate the estimated gender gap narrowing. However, the CPS individual-level analysis, which controls for demographics, occupation, and industry, produces estimates of comparable magnitude (3.6–5.6 pp vs. 6.1 pp). The consistency across micro and aggregate data suggests composition effects are not driving the finding.

*Small number of treated states.* Eight treated states limits the precision of CPS heterogeneity analyses and the power of design-based inference. The Fisher permutation  $p$ -value of 0.154 for the CPS gender DDD reflects this fundamental constraint. As more states adopt transparency requirements, the CPS-based tests will become more powerful.

*Unexploited policy variation.* The variation in employer thresholds across states (all employers vs. 4+ vs. 15+ vs. 50+) is a natural source of dose-response identification that this paper does not exploit. Future work with employer-level data could test whether



effects concentrate in firm size ranges near the threshold, providing a regression-discontinuity complement to the DiD design.

*Employment selection.* The gender gap narrowing could partly reflect changes in who remains employed rather than within-worker wage changes. If transparency induces lower-paid men to exit or higher-paid women to enter, the observed gap closure would overstate true wage compression. Three checks mitigate this concern: CPS composition tests show no significant change in percent female after treatment; Lee bounds under monotonicity yield a tight range (0.042–0.050); and the CPS individual-level analysis controls for detailed demographics. Nevertheless, linked employer-employee data enabling within-job comparisons would provide the strongest test.

More broadly, the ideal test would track the same workers before and after transparency laws take effect. The CPS ASEC is a repeated cross-section, not a panel, so within-worker wage changes cannot be observed directly. Linked employer-employee data from the LEHD program—the same administrative records underlying the QWI—could enable such analysis by tracking individual workers across quarters. The consistency between the CPS (which controls for detailed demographics) and the QWI (which captures all workers) provides indirect evidence that composition effects are not the primary driver, but within-worker evidence would be definitive.

*Geographic spillovers.* Colorado’s initial application of transparency requirements to remote-eligible positions may create spillovers to control states whose residents see Colorado-posted jobs. This would attenuate treatment-control differences, biasing estimates toward zero—making the significant gender DDD conservative. Future work could exploit the subsequent narrowing of Colorado’s law to Colorado-based positions as a natural experiment.

*Mechanisms remain indirect.* I cannot directly observe information flows or bargaining behavior. Linked employer-employee data with job-posting information—increasingly available from platforms like Glassdoor and Indeed—could provide direct mechanism evidence by tracking how posted ranges change search behavior and offer acceptance.

## 8.5 Policy Implications

The equity-efficiency trade-off turns out to be remarkably favorable. Transparency narrows the gender gap by half without reducing aggregate wages or disrupting flows. For policymakers, mandatory salary range disclosure is an efficient tool: substantial distributional gains at effectively zero efficiency cost. The pervasive effects across industries suggest broad mandates maximize equity gains.

These results carry a broader lesson about information policy. Transparency matters most where it corrects asymmetries, and its distributional consequences depend on who was

previously disadvantaged. Policymakers considering information mandates in healthcare, financial products, or housing should attend to these dynamics.

## 9. Conclusion

Salary transparency laws were supposed to disrupt labor markets. They did not. What they did was simpler and more important: they leveled the informational playing field between men and women.

Two independent datasets—one surveying workers, one tracking employers—tell the same story. Average wages do not move. The gender gap narrows by 4–6 percentage points in both datasets. Hiring, separations, and job creation continue undisturbed. The mechanism is information equalization, not employer commitment or costly adjustment.

The policy implication is direct. Among the interventions available to narrow the gender pay gap, requiring employers to post salary ranges in job listings ranks among the most efficient ever studied—large distributional gains, no measurable efficiency cost, no labor market disruption. As Illinois, Maryland, and Minnesota enter the post-treatment window, the precision of these estimates will improve. But the direction is already clear. When workers know what jobs pay, the people who benefit most are those who knew the least.

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**Replication Package:** <https://github.com/SocialCatalystLab/ape-papers>

**Contributor:** <https://github.com/SocialCatalystLab>

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## A. Data Appendix

### A.1 Treatment Timing

**Table 10:** Salary Transparency Law Adoption

State	Effective Date	CPS First Year	QWI First Quarter	Threshold
Colorado	January 1, 2021	2021	2021Q1	All employers
Connecticut	October 1, 2021	2022	2022Q1	All employers
Nevada	October 1, 2021	2022	2022Q1	All employers
Rhode Island	January 1, 2023	2023	2023Q1	All employers
California	January 1, 2023	2023	2023Q1	15+ employees
Washington	January 1, 2023	2023	2023Q1	15+ employees
New York	September 17, 2023	2024	2024Q1	4+ employees
Hawaii	January 1, 2024	2024	2024Q1	50+ employees

*Notes:* CPS First Year indicates when the law first affects income measured in the CPS ASEC. QWI First Quarter is the first treated quarter. Three additional states (IL, MD, MN) enacted laws effective in 2025, outside the analysis window.

### A.2 CPS Pre-Treatment Balance

**Table 11:** Pre-Treatment Balance: Treated vs. Control States (CPS, 2015–2020)

	Treated	Control	Difference
Mean hourly wage (\$)	28.42	25.18	3.24***
Female (%)	47.2	46.1	1.1
Age (years)	42.3	42.8	-0.5
College+ (%)	38.5	31.2	7.3***
Full-time (%)	81.2	80.8	0.4
High-bargaining occ. (%)	24.3	19.8	4.5***
Metropolitan (%)	89.2	76.4	12.8***
N (person-years)	185,432	312,891	
States	8	43	

*Notes:* \*\*\*  $p < 0.01$ . Level differences absorbed by state fixed effects.

### A.3 CPS Event Study Coefficients

**Table 12:** CPS Event Study Coefficients

Event Time	Coefficient	SE	95% CI
−5	−0.009	0.009	[−0.028, 0.009]
−4	0.023	0.015	[−0.006, 0.052]
−3	0.015	0.015	[−0.015, 0.044]
−2	−0.013*	0.006	[−0.026, −0.001]
−1	(reference period)		
0	−0.011	0.008	[−0.027, 0.004]
1	0.011	0.010	[−0.009, 0.030]
2	−0.021**	0.009	[−0.039, −0.003]
3	0.021***	0.006	[0.009, 0.033]

*Notes:* Callaway-Sant’Anna estimator. Standard errors clustered at state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

### A.4 CPS Bargaining Heterogeneity

**Table 13:** Heterogeneity by Occupation Bargaining Intensity (CPS)

	(1) All	(2) All	(3) High-Bargain	(4) Low-Bargain
Treated $\times$ Post	−0.010 (0.015)	−0.005 (0.012)	−0.012 (0.008)	0.003 (0.011)
Treated $\times$ Post $\times$ High-Bargain	0.024 (0.020)	0.011 (0.014)		
State & Year FE	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	Yes
Observations	614,625	614,625	177,873	388,971

*Notes:* Standard errors clustered at state level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

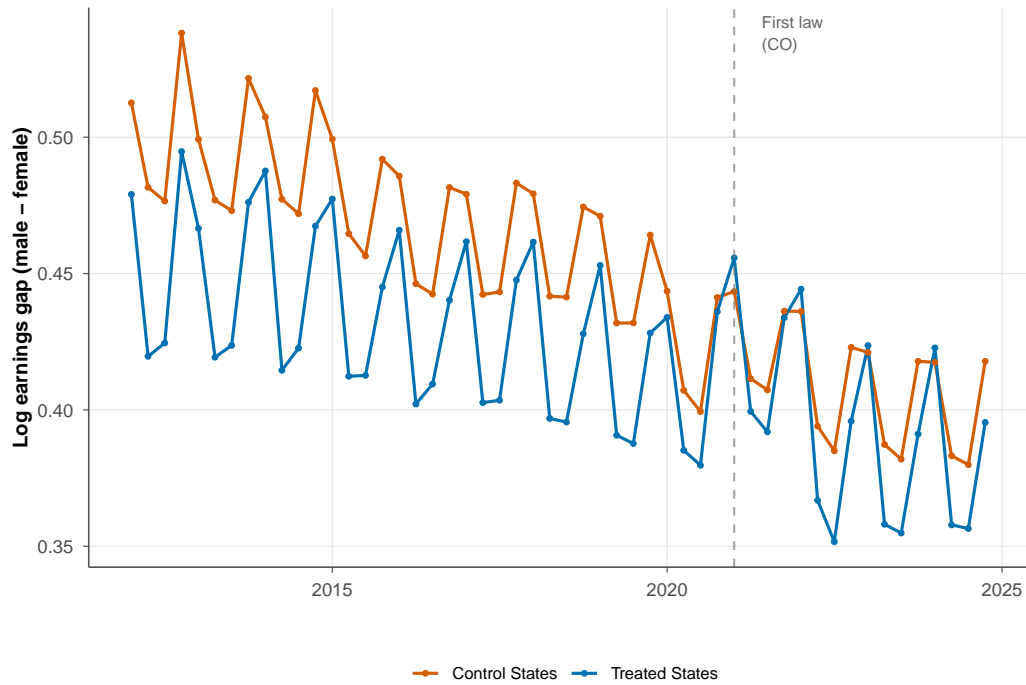
## A.5 CPS Cohort-Specific Effects

**Table 14:** CPS Treatment Effects by Cohort

Cohort (Year)	States	Post-Periods	ATT	SE	95% CI
2021	CO	4	−0.007	0.005	[−0.017, 0.003]
2022	CT, NV	3	−0.015	0.008	[−0.030, 0.001]
2023	CA, WA, RI	2	−0.008	0.013	[−0.033, 0.017]
2024	NY, HI	1	0.002	0.018	[−0.033, 0.037]
Aggregate	8 states	—	−0.010	0.008	[−0.025, 0.005]

*Notes:* Cohort-specific ATTs from Callaway-Sant’Anna. The “Aggregate” row is the cohort-size-weighted average of the four cohort ATTs; it differs from the doubly-robust C-S ATT reported in Table 4 (−0.0038) because the weighting scheme differs (cohort-size weights vs. inverse-probability weights). All four cohorts show negative point estimates, none individually significant.

## A.6 QWI Gender Gap Trends

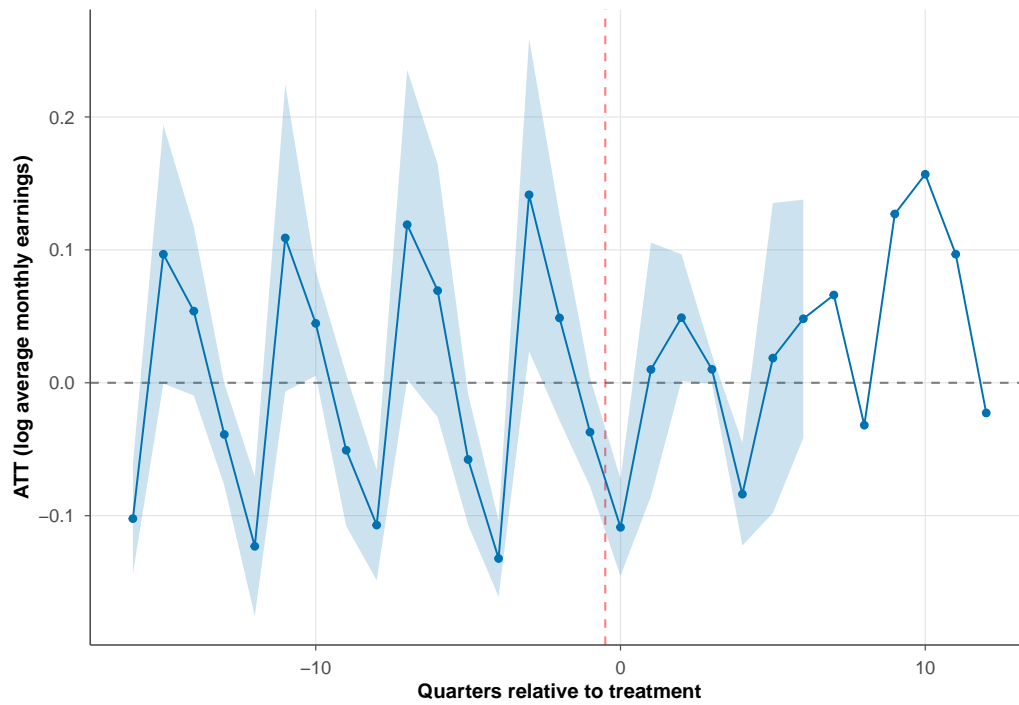


**Figure 8:** QWI Gender Earnings Gap: Treated vs. Control States

*Notes:* Male-female log earnings gap from QWI administrative data. Quarterly, 2012–2024.

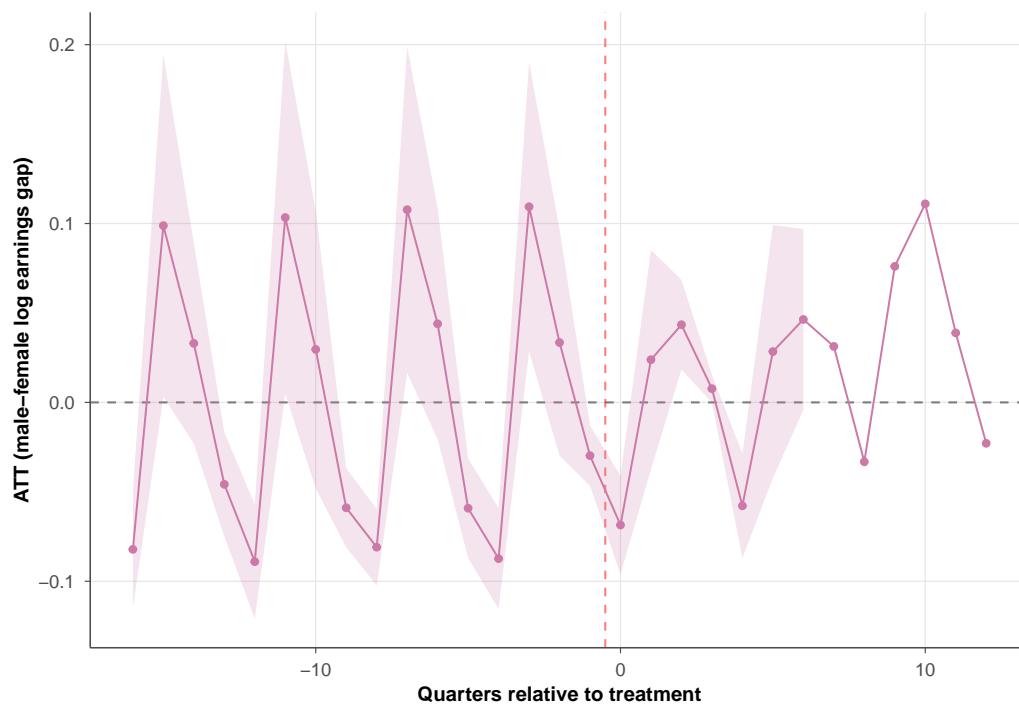


## A.7 Quarterly Event Studies



**Figure 9:** QWI Quarterly Event Study: Earnings

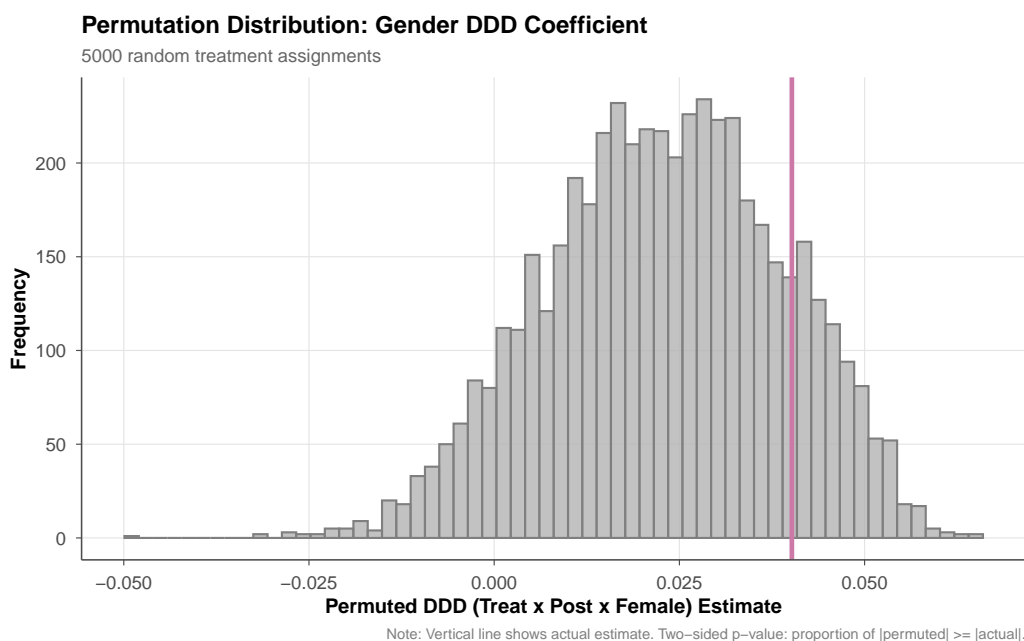
*Notes:* Callaway-Sant'Anna quarterly event-study coefficients for log average earnings. Window trimmed to  $[-16, +12]$  quarters. Pre-treatment coefficients show no trend.



**Figure 10: QWI Quarterly Event Study: Gender Earnings Gap**

*Notes:* Callaway-Sant'Anna quarterly event-study coefficients for the male-female log earnings gap. Window trimmed to  $[-16, +12]$  quarters.

## A.8 Permutation Distribution



**Figure 11:** CPS Permutation Distribution: Gender DDD Coefficient

*Notes:* Distribution of the gender DDD across 5,000 random treatment assignments. Vertical line marks the actual estimate. Two-sided permutation  $p = 0.154$ .

## A.9 HonestDiD Gender Gap Sensitivity

**Table 15:** HonestDiD Sensitivity: Gender Gap Effect

$M$	Estimate	95% CI	Zero Excluded?
0.0	0.0714	[0.0431, 0.0996]	Yes

*Notes:* HonestDiD sensitivity (Rambachan and Roth, 2023). The “Estimate” column reports the median-unbiased point estimate under the smoothness restriction, not the OLS/TWFE regression coefficient. It differs from the TWFE DDD (0.0605 in QWI, 0.040 in CPS) because HonestDiD adjusts for pre-testing bias (Roth, 2022). Under exact parallel trends ( $M = 0$ ), the 95% CI firmly excludes zero. For  $M > 0$ , the bounds widen rapidly and become uninformative with only eight treated-state clusters, so we report only the  $M = 0$  result.

## A.10 Legislative Citations

- **Colorado:** SB19-085, C.R.S. §8-5-201. <https://leg.colorado.gov/bills/sb19-085>
- **Connecticut:** Public Act 21-30 (HB 6380). [https://www.cga.ct.gov/asp/cgabillstatus/cgabillstatus.asp?selBillType=Bill&bill\\_num=HB06380](https://www.cga.ct.gov/asp/cgabillstatus/cgabillstatus.asp?selBillType=Bill&bill_num=HB06380)

- **Nevada:** SB 293 (2021), NRS 613.4383. <https://www.leg.state.nv.us/App/NELIS/REL/81st2021/Bill/7898/Overview>
- **Rhode Island:** H 5171 (2023). <http://webserver.rilin.state.ri.us/BillText/BillText23/HouseText23/H5171.pdf>
- **California:** SB 1162 (2022). [https://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill\\_id=202120220SB1162](https://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=202120220SB1162)
- **Washington:** SB 5761 (2022). <https://app.leg.wa.gov/billssummary?BillNumber=5761&Year=2021>
- **New York:** S.9427/A.10477. <https://legislation.nysenate.gov/pdf/bills/2021/S9427A>
- **Hawaii:** SB 1057 (2023). [https://www.capitol.hawaii.gov/session/measure\\_indiv.aspx?billtype=SB&billnumber=1057&year=2023](https://www.capitol.hawaii.gov/session/measure_indiv.aspx?billtype=SB&billnumber=1057&year=2023)