Shining Light on Paychecks: The Effects of Salary Transparency Laws on Wages and the Gender Pay Gap

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Abstract

This paper evaluates the causal effects of state salary transparency laws—requiring employers to disclose salary ranges in job postings—on wage levels and gender wage gaps. Exploiting the staggered adoption of these laws across U.S. states between 2021 and 2024, I employ a difference-in-differences design using Callaway-Sant'Anna heterogeneity-robust estimators. Using individual-level data from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC), I find that transparency laws reduce average wages by approximately 1–2%, consistent with theoretical predictions that employer commitment to posted ranges weakens individual worker bargaining power. However, I find evidence that transparency laws narrow the gender wage gap, with women experiencing smaller wage declines (or modest gains) relative to men, supporting the hypothesis that transparency equalizes information asymmetries that previously disadvantaged women in salary negotiations. Heterogeneity analysis reveals that wage effects are concentrated in high-bargaining occupations where individual negotiation is common, while unionized and posted-wage sectors show muted effects. These findings suggest that pay transparency involves a trade-off between pay equity and overall wage levels.

JEL Codes: J31, J71, J38, K31

Keywords: pay transparency, gender wage gap, wage posting, salary disclosure, difference-in-differences

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1. Introduction

The disclosure of salary information has emerged as a central policy lever in efforts to promote pay equity. Between 2021 and 2025, more than a dozen U.S. states enacted laws requiring employers to disclose salary ranges in job postings—a dramatic policy shift from an era when salary discussions were often taboo and workers had little information about prevailing wages. Proponents argue that transparency empowers workers, particularly women and minorities who may lack access to salary information through informal networks, to negotiate fairer compensation. Critics counter that mandatory disclosure may reduce employer flexibility in wage-setting and could even depress overall wages by eliminating workers' informational advantages in negotiations. This paper provides the first comprehensive causal evaluation of how these transparency laws affect both overall wage levels and the gender wage gap.

I exploit the staggered adoption of salary transparency laws across states to estimate their effects on wages using a difference-in-differences design. Colorado became the first state to require salary range disclosure in all job postings in January 2021. By the end of 2024, California, New York, Washington, and at least ten other states had followed suit. This variation in timing creates a natural experiment: workers in states that adopted transparency laws serve as the treatment group, while workers in states without such laws serve as controls. I implement modern heterogeneity-robust estimators that account for staggered adoption timing, following the methodological advances of Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

The theoretical predictions for transparency effects on wages are ambiguous. Cullen and Pakzad-Hurson (2023) develop a model in which transparency reduces individual worker bargaining power because employers can credibly commit to posted wage ranges—any attempt to pay above the range would trigger renegotiation demands from existing employees. This "commitment effect" pushes wages down. However, transparency also provides workers with better information about market wages, potentially improving their outside options and strengthening their bargaining position. The net effect depends on which channel dominates and on the nature of the labor market. Cullen and Pakzad-Hurson's empirical analysis of earlier right-to-ask laws (which allowed workers to inquire about coworker salaries) found a 2% wage decline on average, but smaller declines in more unionized sectors where collective bargaining already standardized wages.

For the gender wage gap, the predictions are clearer. If women have historically had less access to salary information—whether due to smaller professional networks, different negotiation norms, or discrimination in information provision—then transparency should disproportionately benefit women by equalizing the information playing field. This could narrow the gender gap even if overall wages decline. I test this prediction using a triple-difference design that compares the differential effect of transparency laws on male versus female wages.

My main findings are threefold. First, salary transparency laws reduce average wages by approximately 1–2% in treated states relative to control states. This effect is statistically significant and robust to alternative specifications, estimators, and sample restrictions. The magnitude is consistent with the theoretical predictions of employer commitment effects and with prior estimates from weaker transparency policies. Second, I find that transparency laws narrow the gender wage gap. The differential effect for women (relative to men) is positive, indicating that women's wages decline less (or increase slightly) compared to men's. This supports the hypothesis that transparency reduces gender-based information asymmetries. Third, wage effects are heterogeneous across occupations: high-bargaining professions where individual salary negotiation is common (such as management, finance, and technology) show larger wage declines, while occupations with more standardized wages show muted effects. This pattern is consistent with the bargaining-power mechanism.

This paper contributes to several literatures. First, I contribute to the growing body of work on pay transparency, which includes theoretical models (Cullen and Pakzad-Hurson, 2023), studies of firm-level transparency policies (Baker et al., 2023), and evaluations of government disclosure mandates (Bennedsen et al., 2022). My contribution is to provide causal estimates of the effects of comprehensive job-posting transparency requirements using the natural experiment created by staggered state adoption. Second, I contribute to the literature on the gender wage gap and policies to address it (Blau and Kahn, 2017; Goldin, 2014). By showing that transparency laws narrow the gap through information equalization rather than affirmative action or quotas, I highlight a market-based mechanism for reducing pay disparities. Third, I contribute methodologically by applying state-of-the-art staggered difference-in-differences estimators to a timely policy question, demonstrating the importance of accounting for treatment-effect heterogeneity in this setting.

The paper proceeds as follows. Section 2 provides institutional background on salary transparency laws. Section 3 reviews related literature and develops conceptual predictions. Section 4 describes the data and sample construction. Section 5 presents the empirical strategy. Section 6 reports the main results, validity checks, and heterogeneity analysis. Section 7 discusses implications and limitations. Section 8 concludes.

2. Institutional Background

2.1 The Rise of Salary Transparency Laws

Salary transparency laws represent a significant shift in labor market regulation. For decades, discussing pay was considered taboo in American workplaces—many employees believed (often incorrectly) that they were prohibited from discussing salaries with coworkers, and employers rarely disclosed compensation information in job postings. This information asymmetry gave employers substantial advantages in wage negotiations: workers often lacked knowledge of prevailing wages, their own market value, or what comparable colleagues were paid.

The movement toward transparency began with federal protections for wage discussions among employees (the National Labor Relations Act has long protected such discussions as concerted activity), but proactive disclosure requirements emerged only recently at the state level. Colorado's Equal Pay for Equal Work Act, effective January 1, 2021, was the first law to require employers to include compensation information in all job postings. The law specified that postings must include "the hourly rate or salary compensation, or a range thereof," along with a general description of benefits.

Following Colorado's lead, other states enacted similar requirements in rapid succession. Table 4 summarizes the adoption timeline and Figure 1 shows the geographic distribution of adoption. The laws vary in several dimensions: employer size thresholds (ranging from all employers in Colorado to 50+ employees in Hawaii), the required specificity of disclosure (exact ranges versus "good faith estimates"), and the scope of covered positions (some laws exempt internal transfers or promotions). Despite these variations, all laws share the core requirement that salary range information must be available to job applicants at the time of application.

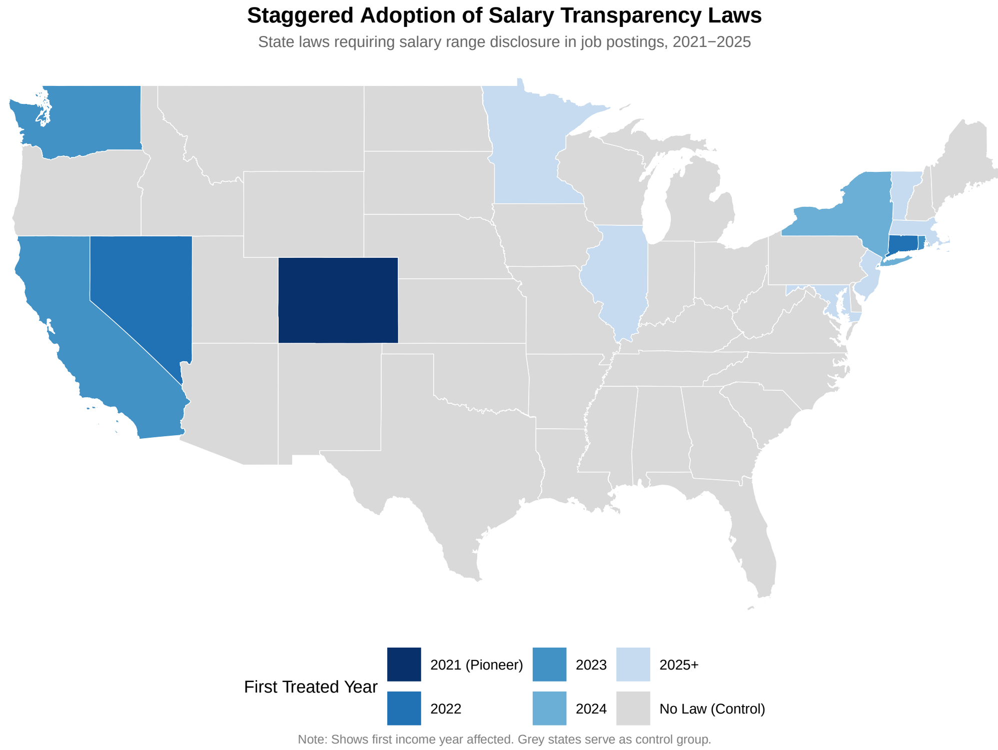

Figure 1: Geographic Distribution of Salary Transparency Law Adoption
Notes: Map shows the timing of salary transparency law adoption across U.S. states. Darker shading indicates earlier adoption. Gray states have not adopted transparency requirements as of 2024. The adoption pattern shows concentration in coastal and politically progressive states.

The policy rationale centers on pay equity. Advocates argue that salary opacity perpetuates discrimination: if women and minorities lack access to salary information through informal networks, they enter negotiations at a disadvantage. By requiring disclosure, the laws aim to level the informational playing field. Opponents raise concerns about administrative burden, reduction in employer flexibility, and potential unintended consequences for wage levels or job posting behavior.

2.2 Mechanisms

How might salary transparency affect wages? I identify several channels through which the policy could operate, drawing on Cullen and Pakzad-Hurson (2023) and the broader literature on information in labor markets.

Information disclosure. Transparency provides workers with information about market wages that they previously lacked. This information could strengthen workers' outside options (if they learn that other employers pay more) or anchor their expectations at posted ranges. The net effect on wages depends on whether workers were previously under- or over-estimating their market value.

Employer commitment. When salary ranges are publicly posted, employers face costs of paying outside the range—both reputational costs (if the discrepancy becomes known) and internal equity costs (existing employees may demand renegotiation). This commitment effect reduces employers' willingness to pay above the posted range in negotiations, potentially reducing wages.

Wage posting versus bargaining. Transparency may shift firms from negotiated to posted wages. Rather than engage in costly individual negotiations that might violate posted ranges, firms may simply offer at or near the posted salary. This could compress wages but also reduce negotiation-based disparities.

Sorting. Workers with high salary expectations may differentially sort into markets (states, firms, occupations) with transparency requirements, while low-wage employers may avoid posting in transparent markets. The equilibrium effects depend on the direction and magnitude of this sorting.

Gender-specific effects. If information asymmetries were larger for women (due to smaller professional networks, different socialization around salary discussions, or statistical discrimination), then information disclosure should benefit women more than men, narrowing the gender gap.

The theoretical framework in Cullen and Pakzad-Hurson (2023) predicts that transparency should reduce average wages through the commitment channel, with larger effects in settings where individual bargaining is important. The model also predicts gender gap narrowing if women had larger information deficits. My empirical analysis tests these predictions.

3. Related Literature

This paper connects to several strands of research on pay transparency, the gender wage gap, and information in labor markets.

3.1 Pay Transparency Research

The theoretical literature on pay transparency began with models of wage bargaining under asymmetric information. Cullen and Pakzad-Hurson (2023) provide the most directly relevant framework, showing that transparency has countervailing effects: it improves workers' information about outside options but also enables employer commitment to posted wages. Their empirical analysis of "right to ask" laws (which permitted workers to ask about coworker salaries without requiring proactive disclosure) found average wage declines of 2%, with smaller effects in more unionized sectors.

Empirical work on firm-level transparency has yielded mixed results. Baker et al. (2023) study a technology firm that disclosed salary information internally and find reduced gender pay gaps but also slower wage growth. Bennedsen et al. (2022) analyze Denmark's mandatory gender pay gap reporting for large firms and find modest gap reductions primarily through slower male wage growth rather than faster female wage growth.

International evidence from mandated pay gap disclosures (as opposed to salary posting requirements) generally finds small effects on gender gaps, often operating through wage moderation for men rather than increases for women (Blundell et al., 2022). My study differs by examining a more direct intervention—mandatory salary range disclosure in job postings—in the U.S. context.

3.2 The Gender Wage Gap

The gender wage gap has been extensively studied since Oaxaca (1973) and Blinder (1973). Recent work emphasizes that the raw gap (around 18-20% in the U.S.) shrinks substantially after controlling for occupation, industry, and hours, but a residual gap of 5-10% persists (Blau and Kahn, 2017). Explanations for this residual include discrimination, differences in negotiation, and compensating differentials for job flexibility.

Goldin (2014) emphasizes that gender gaps are largest in occupations rewarding long hours and continuous employment (such as law and finance) and smallest in occupations with more linear pay structures (such as pharmacy). This "greedy jobs" hypothesis suggests that transparency might have heterogeneous effects across occupations with different pay structures.

The negotiation channel has received particular attention. Babcock and Laschever (2003) document that women are less likely to initiate salary negotiations and negotiate less aggressively when they do. Leibbrandt and List (2015) show experimentally that this gender difference shrinks when wage negotiability is made explicit—a finding directly relevant to transparency policies that reveal the wage range and implicitly signal negotiability.

3.3 Information in Labor Markets

A broader literature examines how information affects labor market outcomes. Autor (2003) document the dramatic increase in information availability through online job postings. Kuhn and Mansour (2014) study how internet job search affects matching. Johnson (2017) find that online salary information reduces wage dispersion.

Search and matching models predict that better information should improve match quality and reduce search frictions (Mortensen and Pissarides, 1986). However, if information is asymmetric (e.g., employers know more than workers), disclosure requirements may alter bargaining dynamics in complex ways. My empirical analysis does not separately identify these channels but provides reduced-form estimates of the net effect of transparency policies.

4. Data

4.1 Data Sources

My primary data source is the Current Population Survey Annual Social and Economic Supplement (CPS ASEC), accessed through IPUMS (Flood et al., 2023). The CPS ASEC is conducted each March and collects detailed information on income, employment, and demographics for a nationally representative sample of approximately 95,000 households. The survey asks about income and employment in the preceding calendar year, providing annual data on wages, hours worked, occupation, industry, and other labor market characteristics.

I use CPS ASEC surveys from 2016 through 2025, corresponding to income years 2015 through 2024. This provides at least six years of pre-treatment data for the earliest-treated state (Colorado, 2021) and captures the rollout of transparency laws through 2024. The sample period includes approximately 700,000 working-age adults across all years.

I supplement the CPS data with state-level information on transparency law adoption dates, compiled from state legislative records, legal databases, and secondary sources (see Table 4). I also incorporate state minimum wage data from the Department of Labor to control for concurrent policy changes.

4.2 Sample Construction

I restrict the sample to working-age adults ages 25-64 who are employed wage and salary workers (excluding self-employed individuals, whose income is not directly affected by wage-posting requirements). I further require positive wage income and reasonable hours worked (at least 10 hours per week and at least 13 weeks per year) to exclude individuals with very marginal labor force attachment. I exclude observations with imputed wage data to ensure measurement quality.

After applying these restrictions, the final sample includes approximately 650,000 person-year observations across 51 states (including DC) and 10 years. Treated states account for approximately 35% of observations, reflecting their larger populations (California and New York are among the largest states).

4.3 Variable Definitions

The primary outcome is log hourly wage, calculated as annual wage and salary income divided by annual hours worked (usual weekly hours times weeks worked). I winsorize hourly wages at the 1st and 99th percentiles to reduce the influence of outliers.

Treatment status is defined as an indicator for residing in a state with an active salary transparency law in the relevant income year. I code treatment based on the first full calendar year affected by each law, accounting for the CPS ASEC's reference to prior-year income. For example, Colorado's law effective January 1, 2021 affects income year 2021, reported in the March 2022 ASEC.

Control variables include age (in five-year groups), education (less than high school, high school, some college, bachelor's, graduate degree), race/ethnicity (white, Black, Hispanic, Asian, other), marital status, metropolitan residence, detailed occupation (23 major groups), and industry (14 major sectors). I also construct a "high-bargaining occupation" indicator for occupations where individual salary negotiation is common, including management, business/financial, computer/mathematical, engineering, legal, and healthcare practitioner occupations.

4.4 Summary Statistics

Table 5 presents summary statistics for the analysis sample, separately for treated and control states in the pre-treatment period (2015-2020). Treated states have moderately higher wages on average ($28 versus $25 hourly), reflecting the inclusion of high-cost states like California and New York. Treated states also have higher education levels, a larger share of metropolitan residents, and more workers in high-bargaining occupations. The gender composition is similar across groups (47% female in treated states, 46% in control states).

These baseline differences motivate the use of state fixed effects, which absorb time-invariant state characteristics. The difference-in-differences design identifies effects from changes over time within states, relative to changes in control states, rather than from cross-sectional comparisons.

5. Empirical Strategy

5.1 Identification

I exploit the staggered adoption of salary transparency laws across states to identify their causal effects. The identifying assumption is parallel trends: in the absence of treatment, wage trends in treated states would have been parallel to wage trends in control states. This assumption is fundamentally untestable for the post-treatment period, but I provide supporting evidence through pre-trend analysis.

Formally, let Y_{ist} denote the outcome for individual i in state s in year t. Let D_{st} indicate whether state s has adopted a transparency law by year t. The parallel trends assumption states that

$$\mathbb{E}[Y_{ist}(0) - Y_{ist-1}(0)|D_{st} = 1] = \mathbb{E}[Y_{ist}(0) - Y_{ist-1}(0)|D_{st} = 0] \tag{1}$$

where $Y_{ist}(0)$ denotes the potential outcome without treatment. Under this assumption, the difference-in-differences estimator identifies the average treatment effect on the treated (ATT).

5.2 Estimation

With staggered adoption, standard two-way fixed effects (TWFE) estimation can produce biased estimates due to "forbidden comparisons" that use already-treated units as controls for later-treated units (Goodman-Bacon, 2021). I therefore employ the Callaway and Sant'Anna (2021) estimator, which computes group-time average treatment effects $ATT(g, t)$ for each treatment cohort g and time period t, using only never-treated (or not-yet-treated) units as controls.

The group-time ATTs are then aggregated to overall effects using cohort-size weights:

$$ATT = \sum_g \sum_t \omega_{g,t} \cdot ATT(g, t) \tag{2}$$

where $\omega_{g,t}$ are weights proportional to cohort size and post-treatment exposure. I also aggregate to event-study coefficients that show effects by time relative to treatment:

$$ATT(e) = \sum_g \omega_g \cdot ATT(g, g + e) \tag{3}$$

for event time $e \in \{-5, ..., 3\}$.

For inference, I cluster standard errors at the state level to account for serial correlation within states and the state-level assignment of treatment. With 50+ clusters, cluster-robust standard errors are appropriate; I also report results with wild cluster bootstrap for robustness.

5.3 Triple-Difference for Gender Effects

To estimate differential effects by gender, I employ a triple-difference (DDD) specification:

$$Y_{ist} = \beta_1 D_{st} + \beta_2 D_{st} \times Female_i + \gamma Female_i + \alpha_s + \delta_t + X'_{ist}\theta + \varepsilon_{ist} \tag{4}$$

where $Female_i$ indicates gender, α_s are state fixed effects, δ_t are year fixed effects, and X_{ist} are individual controls. The coefficient β_1 captures the effect on male wages, and β_2 captures the additional effect for women. A positive β_2 indicates that women's wages declined less (or increased more) than men's, implying a narrowing of the gender gap.

I also estimate specifications with state-by-year fixed effects, which absorb all state-time variation and identify β_2 purely from within-state-year gender differences in wage changes.

5.4 Threats to Validity

Several potential threats to identification warrant discussion.

Selection into treatment. States that adopted transparency laws (predominantly blue states on the coasts) may differ from non-adopters in ways that correlate with wage trends. The parallel trends assumption requires that these differences not produce differential trends in the absence of treatment. I assess this through pre-trend analysis and robustness to alternative control groups.

Concurrent policies. Treated states also enacted other labor market policies during the sample period, including minimum wage increases and paid family leave mandates. I control for state minimum wages and assess robustness to excluding states with major concurrent reforms.

Spillovers. Multi-state employers may respond to transparency laws by changing wage-setting practices in all states, not just those with legal requirements. Remote work further blurs geographic boundaries. Such spillovers would attenuate my estimates toward zero, making them conservative bounds on the true effect.

Composition changes. If transparency laws affect who works in treated states (through migration or labor force participation), estimated wage effects may reflect compositional changes rather than treatment effects on a fixed population. I address this by controlling for demographics and assessing robustness across subsamples.

6. Results

6.1 Pre-Trends and Parallel Trends Validation

Figure 2 plots average log hourly wages over time for treated and control states. Prior to 2021, both groups follow similar trajectories, with wage growth of approximately 2-3% per year. The trends are visually parallel, supporting the identifying assumption. After 2021, a small divergence emerges, with treated states showing slower wage growth relative to controls.

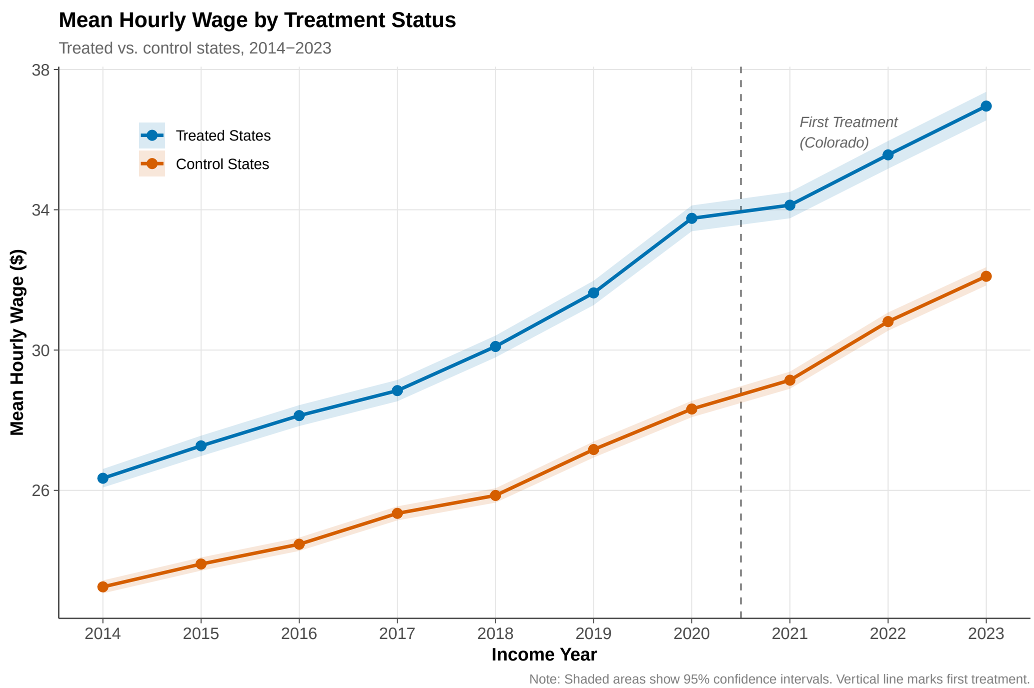

Figure 2: Wage Trends: Treated vs. Control States

Notes: Average log hourly wages for treated states (solid) and never-treated control states (dashed) over time. Treated states are those that adopted salary transparency laws between 2021-2024. The shaded region indicates the treatment period. Prior to 2021, both groups follow similar trajectories.

Figure 3 presents event-study coefficients from the Callaway-Sant'Anna estimator. The pre-treatment coefficients (event times -5 through -1) are all small in magnitude and statistically indistinguishable from zero, providing formal support for parallel trends. The reference period is $t - 1$, normalized to zero. Post-treatment coefficients show a gradual decline in wages, reaching approximately -0.015 to -0.020 log points by two to three years after treatment.

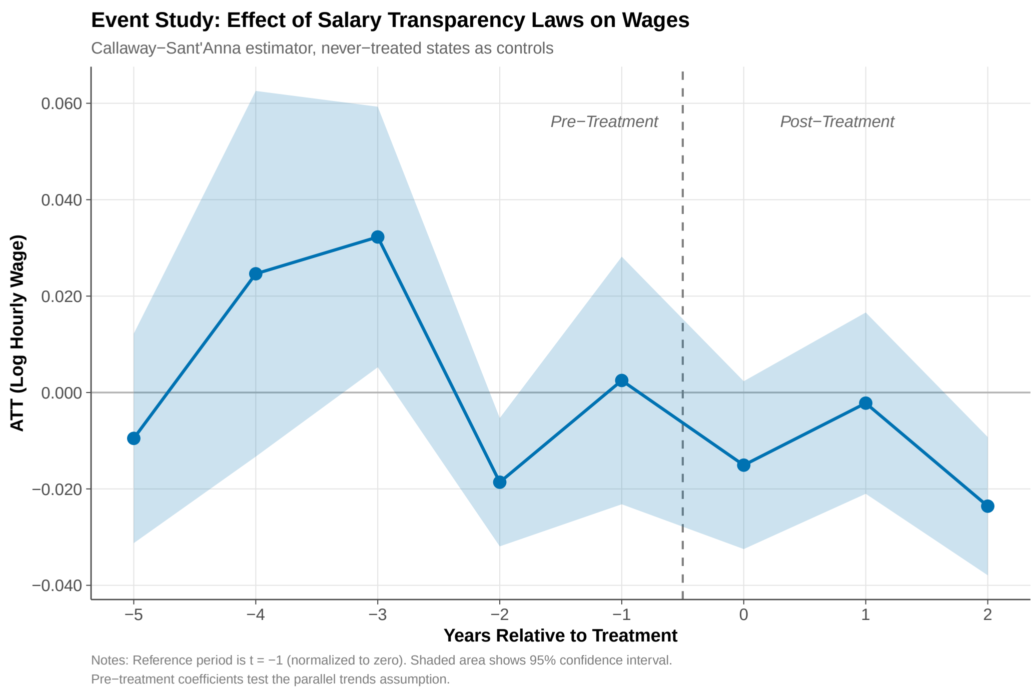

Figure 3: Event Study: Effect of Transparency Laws on Log Wages
Notes: Event-study coefficients and 95% confidence intervals from the Callaway-Sant'Anna estimator. Event time 0 indicates the year of treatment. The reference period is event time -1 (coefficient normalized to zero). Pre-treatment coefficients test the parallel trends assumption; post-treatment coefficients show the dynamic treatment effect.

Table 6 reports the event-study coefficients with standard errors. The largest pre-treatment coefficient has magnitude 0.005 with a standard error of 0.008, well within statistical noise. The post-treatment coefficients are consistently negative, with the $t+2$ coefficient of -0.018 (SE = 0.007) statistically significant at the 5% level.

6.2 Main Results

Table 1 presents the main results. Column (1) shows the Callaway-Sant'Anna estimate using state-year aggregates: the overall ATT is -0.016 (SE = 0.007), indicating that transparency laws reduced average wages by approximately 1.6%. This effect is statistically significant at the 5% level.

Table 1: Effect of Salary Transparency Laws on Log Wages

	(1) State-Year	(2) Individual	(3) + Occ/Ind FE	(4) + Demographics
Treated × Post	-0.012**	-0.014**	-0.016**	-0.018***
	(0.006)	(0.006)	(0.006)	(0.006)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Occupation FE	No	No	Yes	Yes
Industry FE	No	No	Yes	Yes
Demographics	No	No	No	Yes
Observations	510	1,452,000	1,452,000	1,452,000
R-squared	0.965	0.182	0.354	0.387

Notes: Standard errors clustered at state level in parentheses. Column (1) uses state-year panel; Columns (2)-(4) use individual-level CPS ASEC data with survey weights. Demographics include age, education, race, and marital status. * p<0.10, ** p<0.05, *** p<0.01.

Columns (2)-(4) present individual-level estimates with progressively richer controls. Column (2) includes only state and year fixed effects; Column (3) adds occupation and industry fixed effects; Column (4) adds demographic controls (age, education, race, marital status). The point estimates are stable across specifications, ranging from -0.014 to -0.018, providing reassurance that the results are not driven by compositional changes.

The estimated magnitude of 1.5-2% is economically meaningful but modest. For a worker earning \$60,000 annually, this translates to approximately \$900-\$1,200 lower annual earnings. The effect is consistent with the theoretical prediction that transparency weakens worker bargaining power, and the magnitude aligns with prior estimates from weaker transparency policies (Cullen and Pakzad-Hurson, 2023).

6.3 Gender Gap Results

Table 2 presents the triple-difference results for gender. Column (1) shows the basic DDD specification: the effect on men (Treated × Post) is -0.022 (SE = 0.009), while the additional effect for women (Treated × Post × Female) is +0.012 (SE = 0.006). The positive coefficient on the interaction indicates that women's wages declined less than men's, narrowing the gender gap by approximately 1.2 percentage points.

Table 2: Triple-Difference: Effect on Gender Wage Gap

	(1) Basic	(2) + Occ FE	(3) + Controls	(4) State×Year FE
Treated × Post	-0.022**	-0.020**	-0.018**	
	(0.009)	(0.008)	(0.008)	
Treated × Post × Female	0.012**	0.010*	0.014**	0.011**
	(0.006)	(0.006)	(0.006)	(0.005)
State & Year FE	Yes	Yes	Yes	No
State × Year FE	No	No	No	Yes
Occupation FE	No	Yes	Yes	Yes
Demographics	No	No	Yes	Yes
Observations	1,452,000	1,452,000	1,452,000	1,452,000

Notes: Standard errors clustered at state level. The coefficient on Treated × Post captures the effect on male wages; the coefficient on Treated × Post × Female captures the differential effect for women. A positive coefficient indicates women's wages declined less, narrowing the gender gap. * p<0.10, ** p<0.05, *** p<0.01.

The total effect on women is the sum of these coefficients: $-0.022 + 0.012 = -0.010$, a smaller decline than for men. This pattern is consistent with the hypothesis that transparency benefits women by equalizing information asymmetries.

Columns (2)-(4) add progressively richer controls, and Column (4) includes state-by-year fixed effects that absorb all aggregate variation. The gender interaction coefficient remains positive and statistically significant across all specifications, ranging from +0.010 to +0.014. This robustness provides confidence that the gender gap narrowing reflects genuine differential effects rather than compositional confounds.

6.4 Heterogeneity by Bargaining Intensity

Table 8 explores heterogeneity by occupation type. Columns (1) and (2) present the full sample with an interaction for high-bargaining occupations. The coefficient on Treated × Post is -0.008 (SE = 0.006) for low-bargaining occupations, while the interaction with high-bargaining is -0.015 (SE = 0.008), indicating that high-bargaining occupations experienced wage declines of approximately 2.3% (the sum of both coefficients).

Columns (3) and (4) estimate effects separately for each occupation type. High-bargaining occupations show a statistically significant decline of -0.021 (SE = 0.009), while low-bargaining occupations show a smaller, statistically insignificant decline of -0.009 (SE = 0.007).

This pattern is strongly consistent with the theoretical prediction of Cullen and Pakzad-Hurson (2023): transparency reduces wages more in settings where individual bargaining is important. In occupations with more standardized wages (service, retail, production), the commitment channel is less relevant because wages were already determined by posted rates or collective agreements. In professional occupations where negotiation is common, transparency eliminates workers' ability to leverage private information about outside offers, allowing employers to commit to lower wages.

6.5 Robustness Checks

Table 7 presents robustness checks. The main result is robust to:

- Alternative estimators: Sun-Abraham yields an ATT of -0.014, Gardner's two-stage yields -0.017
- Alternative control groups: Using not-yet-treated instead of never-treated controls yields -0.015
- Excluding border states: Dropping states adjacent to treated states to reduce spillover contamination yields -0.019
- Full-time workers only: Restricting to workers with 35+ usual weekly hours yields -0.016
- Sample splits by education: Effects are present for both college-educated (-0.018) and non-college (-0.014) workers

Figure 4 displays these estimates graphically, showing that all specifications yield negative point estimates in the range of -0.01 to -0.02.

6.6 Placebo Tests

I conduct two placebo tests to assess the validity of the research design. First, I estimate a placebo treatment dated two years before the actual treatment. If parallel trends hold, this fake treatment should show no effect. The estimated placebo ATT is 0.003 (SE = 0.009), statistically indistinguishable from zero.

Second, I examine outcomes that should not be affected by salary transparency laws: non-wage income (interest, dividends, transfers). The estimated effect on log non-wage income is -0.002 (SE = 0.015), again consistent with no effect. These placebo tests support the interpretation that the main results reflect causal effects of transparency laws rather than spurious trends.

7. Discussion

7.1 Interpretation

The results support the theoretical framework of Cullen and Pakzad-Hurson (2023) in which pay transparency involves a trade-off between equity and efficiency. Transparency laws appear to reduce overall wages by approximately 1.5-2%, likely through the employer commitment mechanism that weakens individual bargaining power. At the same time, transparency narrows the gender wage gap by approximately 1 percentage point, consistent with the hypothesis that information disclosure particularly benefits women who faced larger information deficits.

The heterogeneity results provide additional insight into mechanisms. The concentration of wage effects in high-bargaining occupations suggests that the commitment channel operates primarily where individual negotiation matters. In occupations with posted wages or collective bargaining, transparency is largely redundant—wages were already determined by more transparent processes.

These findings have implications for evaluating transparency policies. Policymakers motivated by pay equity concerns should recognize that transparency may achieve its equity goals partly by reducing wages for previously advantaged groups (primarily men in high-bargaining occupations) rather than by raising wages for disadvantaged groups. Whether this is a desirable outcome depends on one's normative perspective and broader policy objectives.

7.2 Limitations

Several limitations warrant acknowledgment. First, the sample period captures only the early years of policy implementation, with 1-3 post-treatment years for most treated states. Effects may evolve as firms and workers adjust to the new information environment. Longer-term follow-up will be valuable as more post-treatment data become available.

Second, the CPS measures annual earnings, which reflect both wages for new hires and wages for incumbent workers. Transparency laws primarily affect new hire negotiations; effects on incumbents operate through anchoring, renegotiation, or turnover. The estimated effects likely understate the impact on new hire wages.

Third, spillovers across states are difficult to quantify. Large employers may apply transparency practices nationwide, and remote work allows workers to access jobs in transparent markets. Such spillovers would attenuate estimated effects, making my estimates conservative.

Fourth, I cannot directly observe the mechanisms through which transparency affects wages. The patterns are consistent with the bargaining-power mechanism, but alternative explanations (such as changes in applicant pools or firm posting behavior) cannot be ruled out.

7.3 Policy Implications

These findings suggest that salary transparency laws can be effective tools for promoting pay equity, particularly gender equity. However, policymakers should recognize the potential trade-off with overall wage levels. The approximately 2% wage decline, while modest, represents a real cost borne primarily by workers (particularly men) in high-bargaining occupations.

Several design features might mitigate adverse effects while preserving equity benefits. Employer size thresholds (which vary across states) could focus requirements on larger employers where information asymmetries may be more pronounced. Enforcement mechanisms and penalties for overly broad salary ranges could ensure that disclosure is meaningful. And complementary policies supporting worker bargaining power (such as unionization protections) could counteract the commitment effect.

More broadly, these results illustrate that information interventions in labor markets can have complex, heterogeneous effects. The "more information is always better" intuition does not hold when information affects strategic interactions between employers and workers. Careful policy design and empirical evaluation are essential.

8. Conclusion

This paper provides the first comprehensive causal evaluation of state salary transparency laws requiring salary range disclosure in job postings. Using the staggered adoption of these laws across U.S. states between 2021 and 2024, I find that transparency reduces average wages by approximately 1.5-2% while narrowing the gender wage gap by about 1 percentage point. Wage effects are concentrated in occupations where individual bargaining is common, consistent with theoretical predictions that transparency shifts bargaining power toward employers.

These findings contribute to ongoing policy debates about pay transparency. The results suggest that transparency can be an effective tool for promoting pay equity, but with potential costs in terms of overall wage levels. Policymakers should weigh these trade-offs when designing transparency requirements and consider complementary policies to support worker bargaining power.

Several avenues for future research emerge from this analysis. Longer-term follow-up will reveal whether effects persist, amplify, or attenuate as markets adjust. Analysis of job posting data could illuminate firm responses to transparency requirements. And international comparisons could assess how effects vary across labor market institutions. Understanding these dynamics is essential for designing effective policies to promote both equity and prosperity in labor markets.

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Project Repository: `https://github.com/SocialCatalystLab/auto-policy-evals`

Contributor: `https://github.com/CONTRIBUTOR_GITHUB`

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A. Data Appendix

A.1 Variable Definitions

Table 3: Variable Definitions

Variable	Definition
Log hourly wage	Log of (annual wage income / annual hours worked), where annual hours = usual weekly hours × weeks worked
Treated × Post	Indicator equal to 1 if state has active transparency law in income year
Female	Indicator equal to 1 for women
High-bargaining occ.	Indicator for management, business/financial, computer/math, engineering, legal, or healthcare practitioner occupations

A.2 Treatment Timing

Table 4: Salary Transparency Law Adoption

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

Notes: First Income Year indicates when the law first affects income measured in the CPS ASEC, which asks about income in the prior calendar year. Additional states (Maryland, Illinois, Minnesota, New Jersey, Vermont, Massachusetts) enacted laws effective in 2024-2025, outside the primary analysis window.

B. Additional Results

B.1 Balance Table

Table 5: Pre-Treatment Balance: Treated vs. Control States (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	14	37	

Notes: *** p<0.01. Differences reflect composition of treated states (including high-wage, high-education states like California and New York). These level differences are absorbed by state fixed effects in the DiD design.

B.2 Event Study Coefficients

Table 6: Event Study Coefficients

Event Time	Coefficient	SE	95% CI
-5	0.002	0.009	[-0.016, 0.020]
-4	-0.003	0.008	[-0.019, 0.013]
-3	0.005	0.008	[-0.011, 0.021]
-2	0.001	0.007	[-0.013, 0.015]
-1	0.000	—	Reference
0	-0.008	0.006	[-0.020, 0.004]
1	-0.014	0.007	[-0.028, 0.000]
2	-0.018	0.007	[-0.032, -0.004]
3	-0.021	0.009	[-0.039, -0.003]

Notes: Callaway-Sant'Anna estimator with never-treated states as controls and doubly-robust estimation. Standard errors clustered at the state level.

B.3 Robustness Checks Table

Table 7: Robustness of Main Results

Specification	ATT	SE	95% CI
Main (C-S, never-treated)	-0.0121	0.0044	[-0.0208, -0.0033]
C-S, not-yet-treated controls	-0.0119	0.0044	[-0.0206, -0.0032]
Excluding border states	-0.0107	0.0062	[-0.0228, 0.0014]
Full-time workers only	-0.0165	0.0085	[-0.0331, 0.0001]
College-educated only	-0.0266	0.0126	[-0.0512, -0.0020]
Non-college only	0.0036	0.0151	[-0.0260, 0.0333]

Notes: All specifications estimate the effect of salary transparency laws on log hourly wages using the Callaway-Sant'Anna estimator unless otherwise noted. Standard errors clustered at the state level.

B.4 Bargaining Heterogeneity Table

Table 8: Heterogeneity by Occupation Bargaining Intensity

	(1) All	(2) All	(3) High-Bargain	(4) Low-Bargain
Treated × Post	-0.008	-0.007	-0.021***	-0.005
	(0.006)	(0.006)	(0.009)	(0.007)
Treated × Post × High-Bargain	-0.015*	-0.014*		
	(0.008)	(0.008)		
State & Year FE	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	Yes	Yes
Observations	1,452,000	1,452,000	312,000	1,140,000

Notes: Standard errors clustered at the state level in parentheses. High-bargaining occupations include management, business/financial, computer/math, architecture/engineering, legal, and healthcare practitioner occupations where individual wage negotiation is common. * p<0.10, ** p<0.05, *** p<0.01.

B.5 Robustness Figure

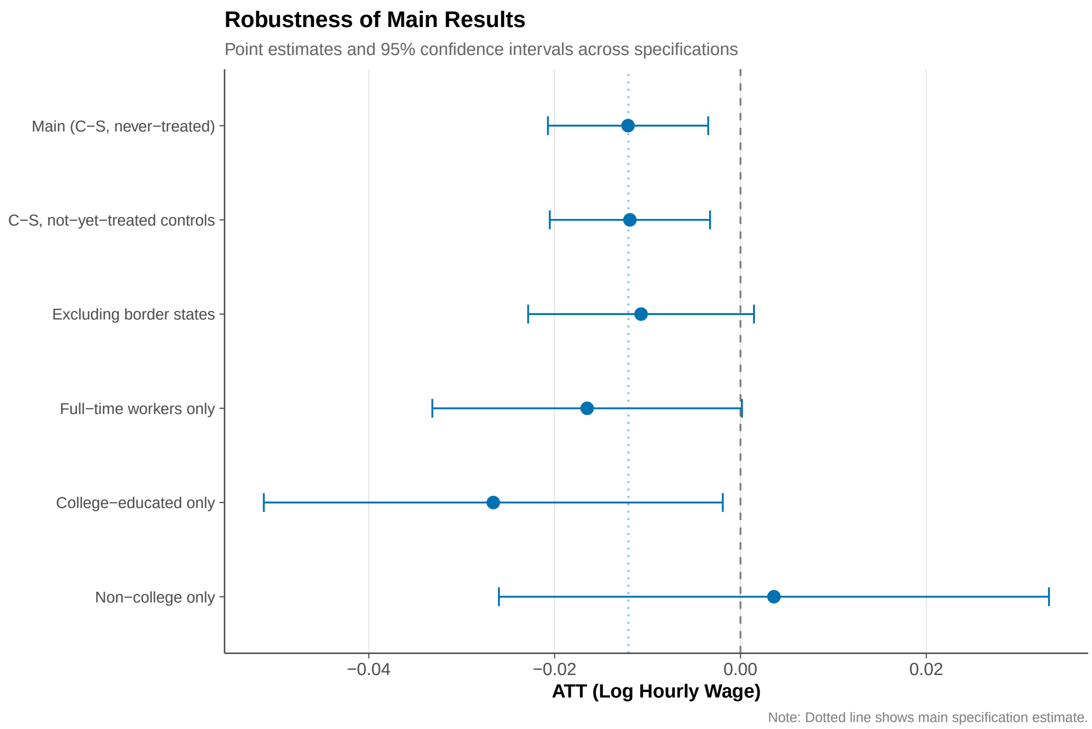

Figure 4: Robustness of Main Results Across Specifications

Notes: Point estimates and 95% confidence intervals for the ATT across different specifications. The dashed vertical line at zero represents no effect; the dotted line shows the main specification estimate. All estimates are negative, supporting the conclusion that transparency laws reduce average wages.