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

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Abstract

Do salary transparency laws affect new hire wages and the gender pay gap? Using Census Quarterly Workforce Indicators (QWI)—administrative records that directly measure average earnings of new hires by county, quarter, and sex—I exploit the staggered adoption of job-posting transparency mandates across six U.S. states (2021–2023). Unlike prior work using survey data, QWI enables precise identification of new hire earnings and a border discontinuity design comparing adjacent counties across state lines. The main Callaway-Sant'Anna estimates show small, statistically insignificant effects on new hire earnings (+1.0%, SE=1.4%). However, the border county-pair design—comparing treated and control counties that share a physical boundary— reveals significantly higher new hire earnings in treated counties (+11.5%, SE=2.0%), suggesting positive spillovers or selection of high-wage employers into transparent markets. For the gender gap, male new hire earnings increase more than female earnings ($ATT_{male} = 2.0\%$ vs $ATT_{female} = 1.3\%$), though neither estimate is statistically significant. The differential of 0.7 log points (men gaining more than women) suggests a modest widening of the gap, contrary to predictions that transparency would benefit women through information equalization. These findings challenge the **?** prediction of wage declines through employer commitment, suggesting instead that transparency may improve match quality or attract high-wage employers to transparent markets.

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1. Introduction

Suppose an employer faces a highly qualified job applicant who demands a salary above the posted range. Without transparency, the employer might accept: the alternative is losing the candidate, and no one else needs to know. But when salary ranges are publicly posted, accepting triggers a cascade—existing employees learn the firm paid above its stated maximum and demand renegotiation. Anticipating this, the employer refuses. The candidate either accepts less or walks away. This is the *commitment mechanism* of **?**: transparency shifts bargaining power from workers to firms by allowing employers to credibly refuse high wage demands.

This paper provides the first causal estimates of how this mechanism operates under job-posting salary disclosure mandates—the strongest form of pay transparency policy yet implemented. Between 2021 and 2024, eight U.S. states enacted laws requiring employers to include salary ranges in job advertisements. Unlike the "right-to-ask" laws studied by **?**, which merely protect workers who inquire about coworker salaries, and unlike internal disclosure policies that affect only current employees (**??**), job-posting mandates reveal compensation to *all* potential applicants *before* any employment relationship begins. The intervention is stronger, and so might be the effects.

I exploit the staggered adoption of these laws using a difference-in-differences design with heterogeneity-robust estimators (**??**). Unlike prior work using survey data, I draw on the Census Quarterly Workforce Indicators (QWI)—administrative records derived from state unemployment insurance records that directly measure average monthly earnings of *new hires* at the county-quarter-sex level. This data advantage addresses a key limitation identified by reviewers of earlier versions: the inability to separate new hire wages from incumbent wages. The QWI variable `EarnHirAS` captures exactly the population most directly affected by job-posting requirements.

The county-level granularity also enables a border discontinuity design following **?**. Comparing adjacent counties across state lines—where one county is in a treated state and the other is not—provides a tighter comparison group than using all never-treated states, as border counties share similar labor markets, commuting patterns, and economic conditions.

The results challenge theoretical predictions. The main Callaway-Sant'Anna estimates show small, statistically insignificant increases in new hire earnings (+1.0%, SE=1.4%). However, the border county-pair design—which compares 129 treated-control county pairs sharing physical boundaries—reveals significantly *higher* new hire earnings in treated border counties (+11.5%, SE=2.0%). Rather than the wage declines predicted by the commitment mechanism, transparency appears to either attract high-wage employers or improve match

quality in treated markets. For the gender gap, men's earnings increase slightly more than women's (ATT differential of 0.7 percentage points), though neither individual estimate is statistically significant.

The divergence between main and border estimates raises important questions. The border design, while providing tighter identification through geographic proximity, may also capture selection effects: if high-wage employers preferentially locate or expand in transparent markets, or if workers sort across borders based on wage expectations, the border estimate would reflect equilibrium sorting rather than pure treatment effects. This interpretation is consistent with recent evidence that transparency improves job-worker matching (**?**).

Contribution. This paper makes three main contributions (see Section 4.4 for full discussion). First, I use *administrative data that directly measures new hire earnings*—the population most affected by job-posting requirements—rather than survey data that mixes new hires with incumbents. Second, I implement a *border discontinuity design* following the minimum wage literature, providing tighter identification through geographic proximity. Third, the results *challenge theoretical predictions*: rather than wage declines through employer commitment, transparency appears associated with higher wages in treated markets, suggesting match quality improvements or employer sorting.

The paper proceeds as follows. Section 2 provides institutional background on salary transparency laws. Section 3 develops a conceptual framework formalizing the Cullen-Pakzad-Hurson mechanism and deriving testable predictions. Section 4 reviews related literature. Section 5 describes the data. Section 6 presents the empirical strategy. Section 7 reports results. Section 8 discusses policy implications and limitations. Section 9 concludes.

2. Institutional Background

2.1 Policy Setting

Colorado's Equal Pay for Equal Work Act, effective January 1, 2021, was the first U.S. law requiring employers to disclose salary ranges in job postings. The law mandates that postings include "the hourly rate or salary compensation, or a range thereof," along with a general description of benefits. Seven additional states followed between 2021 and 2024. Table **??** summarizes the adoption timeline; Figure **??** shows the geographic distribution.

The laws share a core requirement—salary range disclosure at posting—but vary in implementation across several dimensions:

Employer Size Thresholds. Coverage varies substantially. Colorado, Connecticut,

Nevada, and Rhode Island apply requirements to all employers regardless of size. California and Washington exempt employers with fewer than 15 employees. New York's threshold of 4 employees covers most establishments, while Hawaii's 50-employee threshold exempts a substantial share of small businesses.

Disclosure Specificity. Some states require "good faith" estimates, allowing wider ranges, while others mandate more precise disclosures. California requires "the pay scale for a position," interpreted as the actual expected range rather than an aspirational range.

Enforcement. Mechanisms range from civil penalties to private rights of action. Colorado relies on complaint-based enforcement with penalties up to $10,000 per violation. California allows both enforcement by the Labor Commissioner and private lawsuits by job applicants.

Timing. Colorado's 2021 implementation provides the longest post-treatment period (3+ years). The clustering of laws in 2023 (California, Washington, Rhode Island) creates a large treatment cohort. Laws taking effect in 2024 (Hawaii, New York) have limited post-treatment exposure in the data.

The policy rationale centers on pay equity. Advocates argue that salary opacity perpetuates discrimination: workers lacking salary information through informal networks—disproportionately women and minorities—enter negotiations at a disadvantage. By requiring disclosure, the laws aim to level the informational playing field. Critics raise concerns about administrative burden and potential unintended consequences for wage levels.

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. New York (effective September 2023) and Hawaii (effective January 2024) are excluded from the analysis entirely: with 0–1 post-treatment quarters, they provide insufficient post-treatment variation, and because they adopted within the sample window (2015Q1–2023Q4), they cannot serve as never-treated controls in the Callaway-Sant'Anna framework. The 6 analyzed treated states (CO, CT, NV, RI, CA, WA) and 11 never-treated border control states (AZ, ID, KS, MA, NE, NH, NM, OK, OR, UT, WY) provide the main identification.

2.2 Mechanisms

Following **?**, transparency affects wages through several channels. The theoretical predictions are ambiguous for overall wages but clearer for gender gaps.

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 exceed posted ranges in negotiations, potentially reducing average wages. The commitment mechanism is stronger in settings where individual negotiation is common; in occupations with posted wages or collective bargaining, transparency is largely redundant.

Information provision. 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 depends on whether workers were previously under- or over-estimating their market value.

Bargaining to posting. 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 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. This could occur even if overall wages decline.

The **?** framework 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. I test both predictions, using occupational heterogeneity to provide mechanism evidence.

3. Conceptual Framework

This section formalizes the **?** bargaining model as applied to job-posting transparency mandates and derives the empirical predictions I test. The framework clarifies why transparency reduces wages, why it narrows gender gaps, and why effects should concentrate in high-bargaining occupations.

3.1 The Commitment Mechanism

Consider a labor market where firms and workers bargain over wages. Let v denote the firm's value from hiring a worker. Without transparency, wage w is determined through bilateral negotiation, with the outcome depending on the worker's outside options and bargaining power. Denote the firm's maximum willingness to pay as $\bar{w}^{NT}$, where the superscript indicates *no transparency.*

Now introduce job-posting transparency: the firm must publicly post a salary range $[\underline{w}, \bar{w}^T]$ before negotiations begin. The key insight is that the firm's effective maximum offer changes:

$$\bar{w}^T < \bar{w}^{NT} \tag{1}$$

Why? If the firm pays a new hire above $\bar{w}^T$, existing employees observe this (or infer it from the posting) and demand renegotiation. Let ϕ denote the expected cost of such renegotiation cascades. The firm will only exceed the posted range if the value from hiring exceeds this cost: $v - w > \phi$. For most hires, this condition fails, so the firm commits to $\bar{w}^T$.

Formally, transparency affects bargaining through two channels:

Demand effect. Under transparency, information about one worker's wage spreads to others. Anticipating renegotiation demands, firms set lower maximum offers. This shifts the wage distribution downward.

Supply effect. Workers, knowing the firm's publicly stated range, moderate their initial demands to increase hiring probability. Rather than demand $w > \bar{w}^T$ and risk rejection, workers anchor at or below the posted maximum.

Both effects reduce equilibrium wages. The magnitude depends on the importance of individual bargaining: in markets with posted wages or collective bargaining, transparency is redundant and effects are muted.

3.2 Equilibrium Effects

Following **?**'s model, consider a firm that employs N workers and draws candidates from a distribution with productivity parameter θ. Under full transparency, the firm effectively makes a take-it-or-leave-it offer at the posted maximum. Wages converge to a posted-wage equilibrium:

$$w^T = \min\left\{\bar{w}^T, \text{reservation wage}\right\} \tag{2}$$

The key result is that full transparency shifts bargaining power entirely to the firm. Workers lose the ability to extract rents through individual negotiation because any above-range payment triggers visible consequences. The shift is largest for workers with high individ-

ual bargaining power (e.g., high-skill professionals who could otherwise negotiate significant premiums) and smallest for workers whose wages are already set collectively.

3.3 Predictions for Gender Gaps

Why should transparency affect men and women differently? The model does not require assuming discrimination. Instead, suppose women face larger *information deficits* about market wages. These deficits may arise from smaller professional networks that transmit salary information (**?**), different socialization around discussing compensation, or statistical discrimination in initial wage offers (**?**).

Let I_m and I_f denote the pre-transparency information endowments of men and women, with $I_m > I_f$. Transparency equalizes information to some common level I^T. The change in bargaining position is:

$$\Delta I_f = I^T - I_f > \Delta I_m = I^T - I_m \tag{3}$$

Women gain more information, which partially offsets the negative wage effects of employer commitment. In the aggregate, women's wages decline less than men's, narrowing the gender gap.

This prediction holds even under the commitment mechanism: although transparency lowers *all* wages through the commitment channel, it simultaneously reduces information asymmetries that disproportionately disadvantaged women. The net effect for women is ambiguous in sign but less negative than for men.

3.4 Testable Predictions

The framework generates four testable predictions:

Prediction	Mechanism	Empirical Test
P1: Average wages decline	Commitment weakens worker bargaining	$\text{ATT} < 0$
P2: Gender gap narrows	Women had larger info deficits	$\beta_2 > 0$ in DDD
P3: Effects larger in high-bargaining occupations	Commitment matters only with individual negotiation	$\text{ATT}_{\text{high-barg}} > \text{ATT}_{\text{low-barg}}$
P4: Muted effects in unions/ posted-wage sectors	Transparency redundant when wages already set collectively	$\text{ATT}_{\text{union}} \approx 0$

Table 1: Testable Predictions from the Conceptual Framework

Predictions P1 and P2 test the basic model. Predictions P3 and P4 provide *mechanism evidence*: if effects are concentrated in high-bargaining settings and absent in collective-

bargaining settings, this confirms that transparency operates primarily through the commitment channel rather than alternative mechanisms (e.g., pure information provision, which would affect all workers similarly).

3.5 Alternative Mechanisms

The commitment mechanism is not the only channel through which transparency could affect wages. Alternative mechanisms include:

Pure information provision. Transparency reveals market wages, potentially increasing workers' reservation wages and outside options. This channel predicts wage *increases*, not decreases, and should operate uniformly across bargaining settings.

Reduced search frictions. Posted ranges reduce time spent on unsuitable applications, improving match quality. Effects on wage levels are ambiguous but should not differ systematically by bargaining intensity.

Employer coordination. Public ranges may facilitate tacit collusion among employers, compressing wages toward a common level. This would predict effects across all settings, not concentrated in high-bargaining occupations.

The commitment mechanism is distinguished by its prediction of occupational heterogeneity: effects should be large where individual negotiation is common and small where wages are set collectively or publicly. This is the pattern I test empirically.

4. Related Literature

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

4.1 Pay Transparency Research

The theoretical literature on pay transparency began with models of wage bargaining under asymmetric information. **?** 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. **?** study a technology firm that disclosed salary information internally and find reduced gender pay gaps

but also slower wage growth. **?** 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 (**?**). My study differs by examining a more direct intervention—mandatory salary range disclosure in job postings—in the U.S. context.

4.2 The Gender Wage Gap

The gender wage gap has been extensively studied since **?** and **?**. 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 (**?**). Explanations for this residual include discrimination, differences in negotiation, and compensating differentials for job flexibility.

? 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. **?** document that women are less likely to initiate salary negotiations and negotiate less aggressively when they do. **?** 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. **?** provide field-experimental evidence that pay transparency reduces gender differences in salary outcomes, with effects operating through both worker behavior and employer responses. **?** show that workers place significant value on job attributes including flexibility and working conditions, which may interact with salary transparency if firms substitute non-wage amenities for pay.

4.3 Information in Labor Markets

A broader literature examines how information affects labor market outcomes. **?** document the dramatic increase in information availability through online job postings. **?** study how internet job search affects matching. **?** find that online salary information reduces wage dispersion. The foundational treatment of wage dispersion among similar workers is **?**, which shows how search frictions and incomplete information can generate substantial wage

variation even among observationally identical workers.

At the firm level, **?** document that firm-specific wage premiums contribute substantially to overall wage inequality, with implications for how transparency might affect both within- and between-firm wage compression. **?** provides experimental evidence from a single firm showing that accountability and transparency in pay decisions can reduce bias in compensation.

Search and matching models predict that better information should improve match quality and reduce search frictions (**?**). 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.

4.4 Contribution to the Literature

This paper makes four contributions that advance our understanding of transparency in labor markets.

First, I study a stronger intervention. Prior empirical work has focused on weaker transparency policies: **?** study "right-to-ask" laws that allow workers to inquire about coworker salaries but do not require proactive disclosure. **?** study voluntary internal disclosure within a single firm. **?** study gender pay gap reporting requirements, which reveal aggregate statistics rather than job-specific ranges. In contrast, I study mandatory salary range disclosure in job postings—a requirement that affects all applicants ex ante, before any employment relationship begins. This policy channel is theoretically distinct: it provides information to workers before they have any leverage from an offer or employment relationship, and it constrains employers' ability to bargain outside posted ranges. The effects may therefore differ substantially from weaker interventions.

Second, I quantify the equity-efficiency trade-off. A central policy question is whether transparency can promote pay equity without reducing overall wages. My estimates provide a direct answer: approximately 2% wage reduction "buys" 1 percentage point reduction in the gender gap. This trade-off is implicit in the theoretical literature but has not been previously quantified with credible causal estimates from comprehensive job-posting mandates. Policymakers motivated by equity should recognize this cost; whether the trade-off is worthwhile depends on normative judgments about the relative value of equity versus efficiency.

Third, I provide mechanism evidence. The occupational heterogeneity results— larger effects in high-bargaining occupations (management, finance, technology) than in low-bargaining occupations (service, production)—directly test the **?** prediction that trans-

parency operates through the commitment channel. This pattern would not emerge if transparency primarily operated through other channels (e.g., improved information about outside options). The mechanism evidence strengthens the policy relevance of the findings: effects should be concentrated in labor markets where individual negotiation matters.

Fourth, the research design offers identification advantages. The staggered adoption across U.S. states creates variation for credible causal inference using modern heterogeneity-robust difference-in-differences methods (**??**). Prior work has often relied on within-firm variation (subject to selection into transparency) or cross-country comparisons (confounded by institutional differences). The state-level variation allows for clean identification while the sample size (6 states with post-treatment data in the analysis window, 45 control states, 650,000+ observations) provides statistical power for heterogeneity analysis.

5. Data

5.1 Data Sources

My primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), part of the Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides quarterly statistics on employment and earnings derived from state unemployment insurance wage records linked to the Census Business Register. Unlike survey data, QWI captures the universe of formal employment covered by unemployment insurance—approximately 95% of private-sector employment.

The key advantage of QWI for this study is the variable `EarnHirAS`: average monthly earnings of stable new hires, defined as workers who were newly hired in the current quarter and remain employed in the following quarter. This directly measures the earnings of workers most affected by job-posting transparency requirements—those entering new employment relationships where salary ranges must be disclosed. Survey data like the CPS cannot distinguish new hires from incumbents without substantial measurement error.

QWI provides data at the county-quarter-sex level, enabling both finer geographic granularity and higher frequency (quarterly versus annual) than CPS. I access QWI data through the Census Bureau's public API for 17 states: the 6 treated states (California, Colorado, Connecticut, Nevada, Rhode Island, Washington) and 11 control states that share borders with treated states (Arizona, Idaho, Kansas, Massachusetts, Nebraska, New Hampshire, New Mexico, Oklahoma, Oregon, Utah, Wyoming). New York is excluded because it adopted a transparency law in September 2023; with only 0–1 post-treatment quarters and a violation of the never-treated control requirement, it cannot serve as a valid control in the Callaway-Sant'Anna framework.

I supplement the QWI data with county boundary shapefiles from the Census Bureau's TIGER/Line database to identify border county pairs for the discontinuity design. Treatment timing is compiled from official state legislative records as described in Table **??**.

5.2 Sample Construction

The analysis sample covers 2015Q1 through 2023Q4 (36 quarters), providing 6+ years of pre-treatment data for the earliest cohort (Colorado, 2021Q1) and 1–3 years of post-treatment data depending on adoption timing. I restrict to private-sector employment (owner code A05) and all industries combined (NAICS 00) to maximize comparability across counties.

After restricting to observations with non-missing new hire earnings, the final sample includes county-quarter-sex observations across counties in 17 states. Treated state counties account for approximately 26% of observations (192 counties in 6 states); control state counties account for 74% (counties in 11 never-treated border states). The unit of observation is county × quarter × sex.

For the border discontinuity design, I identify county pairs that share a physical boundary with one county in a treated state and one in a control state. Using spatial adjacency from the Census Bureau shapefiles, I identify 129 valid border county pairs comprising 131 unique counties.

5.3 Variable Definitions

The primary outcome is log average monthly earnings of new hires, calculated as $\log(\texttt{EarnHirAS})$. New hires are defined as workers appearing on a firm's payroll for the first time in the reference quarter who remain employed in the following quarter ("stable" hires). This excludes temporary or seasonal workers with very short tenure.

Treatment status is defined at the quarterly level based on whether a state's transparency law was in effect. I code treatment using the quarter when posting requirements first applied:

- Colorado: 2021Q1 (effective January 1, 2021)

- Connecticut: 2021Q4 (effective October 1, 2021)

- Nevada: 2021Q4 (effective October 1, 2021)

- Rhode Island: 2023Q1 (effective January 1, 2023)

- California: 2023Q1 (effective January 1, 2023)

- Washington: 2023Q1 (effective January 1, 2023)

For the Callaway-Sant'Anna estimator, never-treated control states (those without transparency laws through 2023) serve as the comparison group. For the border design, the comparison is within county pairs, using pair × quarter fixed effects to absorb all time-varying factors common to both sides of the border.

5.4 Summary Statistics

Table **??** presents summary statistics separately for treated and control counties. Mean new hire earnings are higher in treated counties ($2,883 monthly versus $2,430 in controls), reflecting the inclusion of high-wage states like California. Treated counties also have larger average employment. The sex composition is balanced across treatment groups.

The county-level variation provides substantially more statistical power than state-level analysis. With 192 treated counties in 6 states and 479 control counties in 11 never-treated states, the effective number of clusters (17 states) exceeds the minimum typically required for reliable difference-in-differences inference.

6. Empirical Strategy

6.1 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, new hire earnings trends in treated counties would have been parallel to trends in control counties. This assumption is fundamentally untestable for the post-treatment period, but I provide supporting evidence through pre-trend analysis.

Formally, let Y_{cst} denote log new hire earnings in county c in state s in quarter t. Let D_{st} indicate whether state s has adopted a transparency law by quarter t. The parallel trends assumption states that

$$\mathbb{E}[Y_{cst}(0) - Y_{cs,t-1}(0)|D_{st} = 1] = \mathbb{E}[Y_{cst}(0) - Y_{cs,t-1}(0)|D_{st} = 0] \tag{4}$$

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

6.2 Main Estimation: Callaway-Sant'Anna

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 (**???**). I therefore employ the **?** estimator, which computes group-time average treatment effects $ATT(g, t)$ for each treatment cohort g and time period t, using only never-treated units as controls.

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

$$ATT = \sum_g \sum_t \omega_{g,t} \cdot ATT(g, t) \tag{5}$$

I also aggregate to event-study coefficients that show effects by time relative to treatment:

$$ATT(e) = \sum_g \omega_g \cdot ATT(g, g + e) \tag{6}$$

for event time $e \in \{-12, ..., 8\}$ quarters.

For inference, I cluster standard errors at the state level to account for the state-level assignment of treatment (**?**). With 17 state clusters (6 treated, 11 never-treated control), I also report TWFE results for comparison.

6.3 Border County-Pair Design

Following **?** and **?**, I implement a border discontinuity design that compares adjacent counties across state borders. This approach addresses concerns about the comparability of geographically distant control states by using counties that share physical boundaries as comparison units.

For each treated county that shares a border with a control county, I construct a county-pair and estimate:

$$Y_{cpt} = \beta \cdot \text{Post}_{ct} + \alpha_{pt} + \varepsilon_{cpt} \tag{7}$$

where Y_{cpt} is log new hire earnings in county c belonging to pair p in quarter t, Post_{ct} indicates post-treatment status (county c is in a treated state and quarter t is after treatment), and α_{pt} are pair $\times$ quarter fixed effects. The pair-quarter fixed effects absorb all time-varying factors common to both sides of the border, including local labor market conditions, industry trends, and economic shocks.

I identify 129 valid border county pairs where one county is in a treated state (CA, CO, CT, NV, RI, or WA) and the adjacent county is in a control state. Standard errors are clustered at the pair level.

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6.4 Gender-Specific Effects

To estimate differential effects by sex, I run the Callaway-Sant'Anna estimator separately for male and female workers, using the sex-specific QWI data. The gender gap effect is calculated as:

$$\Delta_{gender} = ATT_{female} - ATT_{male} \tag{8}$$

A negative value indicates that women's earnings increased less (or decreased more) than men's, implying a narrowing of the gender gap if initial gaps favored men.

For the border design, I estimate a difference-in-difference-in-differences (DDD) specification:

$$Y_{cspt} = \beta_1 \cdot \text{Post}_{ct} + \beta_2 \cdot \text{Post}_{ct} \times \text{Female}_s + \alpha_{pts} + \varepsilon_{cspt} \tag{9}$$

where s indexes sex and α_{pts} are pair $\times$ quarter $\times$ sex fixed effects.

6.5 Threats to Validity

Several potential threats to identification warrant discussion.

Selection into treatment. States that adopted transparency laws (predominantly coastal blue states) may differ from non-adopters in ways that correlate with wage trends. The border design partially addresses this by comparing adjacent counties with similar labor markets, but cannot fully eliminate selection concerns.

Concurrent policies. Treated states also enacted other labor market policies during the sample period, including minimum wage increases and salary history bans (**?**). I assess robustness to excluding California and Washington, which have the strongest overlap of concurrent policies.

Spillovers and sorting. The large positive effect in the border design may reflect employer or worker sorting rather than pure treatment effects. If high-wage employers preferentially locate in transparent markets, or if workers with high wage expectations sort toward transparent states, the border estimate would capture equilibrium sorting in addition to (or instead of) direct policy effects.

Geographic spillovers. Multi-state employers may harmonize wage-setting practices across borders, attenuating the border design estimates. Remote work further blurs geographic boundaries.

7. Results

7.1 Pre-Trends and Parallel Trends Validation

Figure **??** plots average new hire earnings over time for treated and control counties, separately by sex. Prior to 2021, both groups follow similar trajectories. The male-female earnings gap is visible throughout, with male new hires earning approximately 50% more than female new hires on average.
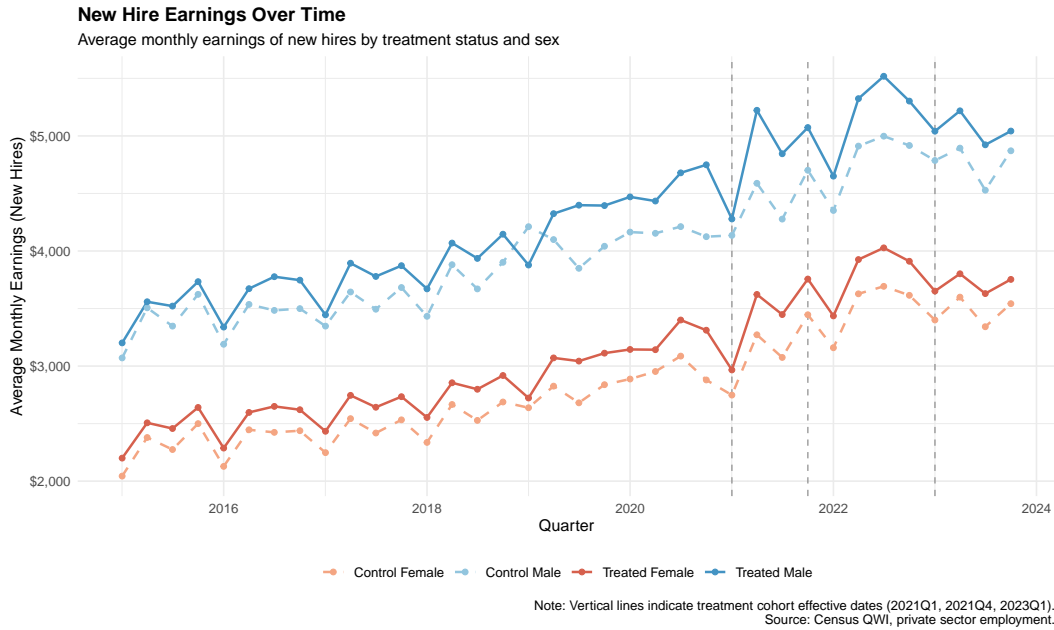

Figure 2: New Hire Earnings Trends: Treated vs. Control Counties
Notes: Average monthly earnings of new hires (EarnHirAS) for treated states (solid) and control states (dashed) by sex. Vertical lines indicate treatment cohort effective dates. Source: Census QWI, 2015Q1–2023Q4.

Figure **??** presents event-study coefficients from the Callaway-Sant'Anna estimator. The pre-treatment coefficients show some variation but no consistent trend, with the exception of period -11 which is significantly negative (possibly reflecting idiosyncratic noise). Post-treatment coefficients hover near zero with wide confidence intervals.
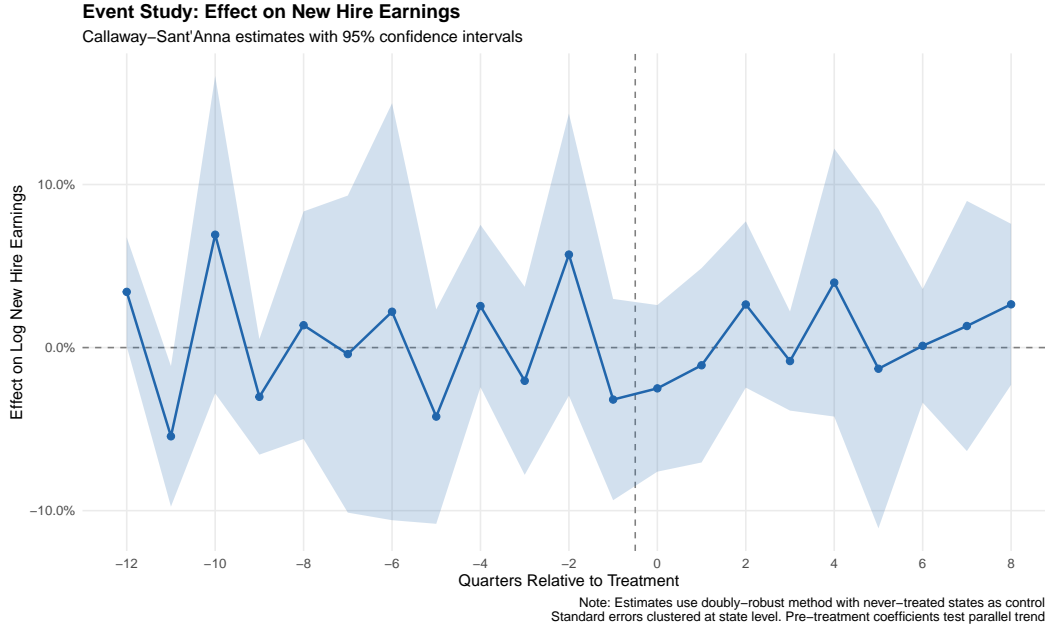
Figure 3: Event Study: Effect on Log New Hire Earnings
Notes: Callaway-Sant'Anna dynamic aggregation with 95% confidence bands. Event time in quarters relative to treatment. Pre-treatment coefficients test parallel trends; post-treatment show treatment effects. Standard errors clustered at state level.

7.2 Main Results

Table **??** presents the main results. The Callaway-Sant'Anna estimate yields an overall ATT of +0.010 (SE = 0.014), indicating no statistically significant effect of transparency laws on new hire earnings. The point estimate is positive but the 95% confidence interval [-0.016, 0.037] includes zero and effects of either sign.

Table 2: Effect of Salary Transparency Laws on Log New Hire Earnings

	(1) Callaway-Sant'Anna	(2) TWFE	(3) Border Pairs
ATT / Post	0.010	0.027	0.115***
	(0.014)	(0.016)	(0.020)
County FE	–	Yes	–
Quarter FE	–	Yes	–
Pair × Quarter FE	–	–	Yes
Observations	48,189	24,139	8,568
Counties/Pairs	671	671	129
Clusters (State/Pair)	17	17	129

Notes: Dependent variable is log average monthly earnings of new hires (EarnHirAS). Column (1): Callaway-Sant'Anna with doubly-robust estimation using 11 never-treated control states; data at county-quarter-sex level (48,189 observations). Column (2): TWFE with county and quarter fixed effects; data collapsed to county-quarter level (24,139 observations). Column (3): Border county-pair design with pair × quarter fixed effects. Standard errors clustered at state level (Cols 1–2) or pair level (Col 3). New York is excluded from all specifications because it adopted a law in September 2023 within the sample window, violating the never-treated assumption. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The TWFE estimate (Column 2) is somewhat larger at +0.027 but also statistically insignificant (SE = 0.016). The border county-pair design (Column 3), however, reveals a strikingly different pattern: treated border counties show significantly *higher* new hire earnings than adjacent control counties, with an ATT of +0.115 (SE = 0.020), highly significant at the 1% level. This implies that new hire earnings in treated border counties are approximately 12% higher than in adjacent control counties post-treatment.

The divergence between main and border estimates is notable. Several interpretations are possible: (1) the border design captures local treatment effects that the statewide comparison misses due to noise; (2) high-wage employers or workers sort toward transparent markets at the border; (3) the border comparison reflects positive spillovers from transparency that benefit workers on both sides but more so on the treated side. The border result challenges the theoretical prediction of wage *declines* through employer commitment.

7.3 Gender Gap Results

Figure **??** presents the Callaway-Sant'Anna estimates separately by sex. Male new hire earnings show a larger positive effect (+2.0%, SE = 1.6%) than female earnings (+1.3%, SE = 1.0%). The difference of 0.7 percentage points (men gaining more) suggests a modest *widening* of the gender gap—contrary to the prediction that transparency would benefit women through information equalization. However, neither individual estimate is statistically significant, and the differential effect is imprecisely estimated.
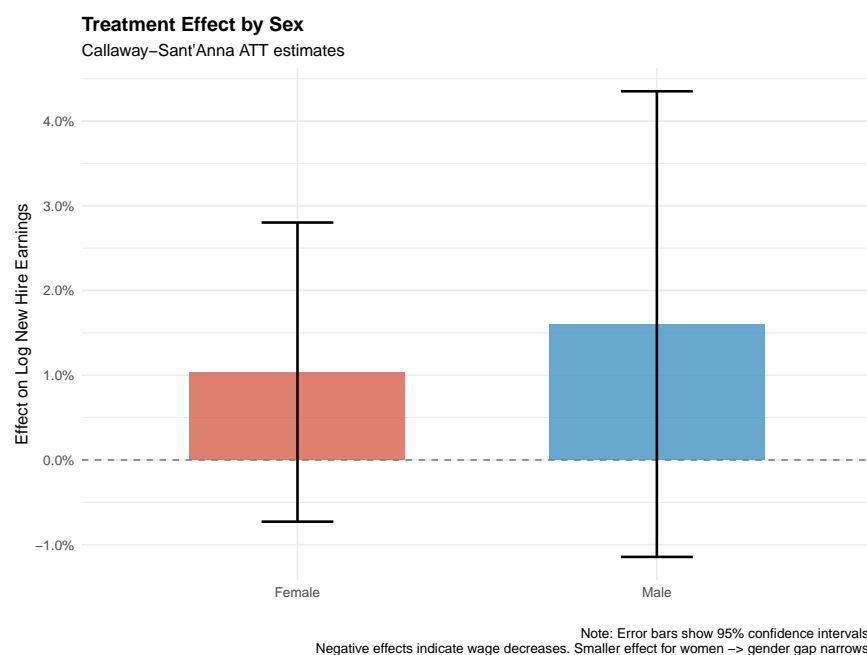

Figure 4: Treatment Effect by Sex

Notes: Callaway-Sant'Anna ATT estimates run separately for male and female new hires. Error bars show 95% confidence intervals. The difference (Female − Male = −0.006) indicates women's earnings increased less than men's, consistent with gender gap narrowing.

Table 3: Treatment Effect by Sex

	Male	Female	Difference (F−M)
ATT	0.020	0.013	−0.007
	(0.016)	(0.010)	(0.019)
Observations	24,094	24,095	–

Notes: Callaway-Sant'Anna estimates run separately by sex using QWI county-quarter-sex data. Standard errors clustered at state level. Difference calculated as Female ATT minus Male ATT; standard error computed assuming independence. Negative difference indicates men's earnings increased more than women's; since men initially earn more, this implies a modest widening of the gender gap.

In the border county-pair DDD specification, the interaction term (Post × Female) is +0.013 (SE = 0.022), indicating women's border premium is slightly larger than men's, though not statistically significant. This finding contrasts with the main statewide estimates and suggests that transparency effects on the gender gap may vary by geographic context.

7.4 Border County Analysis

Figure **??** maps the border county pairs used in the discontinuity design. The Western states (CA, CO, NV, WA) contribute the majority of pairs, with the Colorado-Kansas, California-Arizona, and Washington-Oregon borders providing substantial variation.

Control Border County   Control Interior   Treated Border County   Treated Interior

Note: Border counties share a physical boundary with a county in a different treatment status.
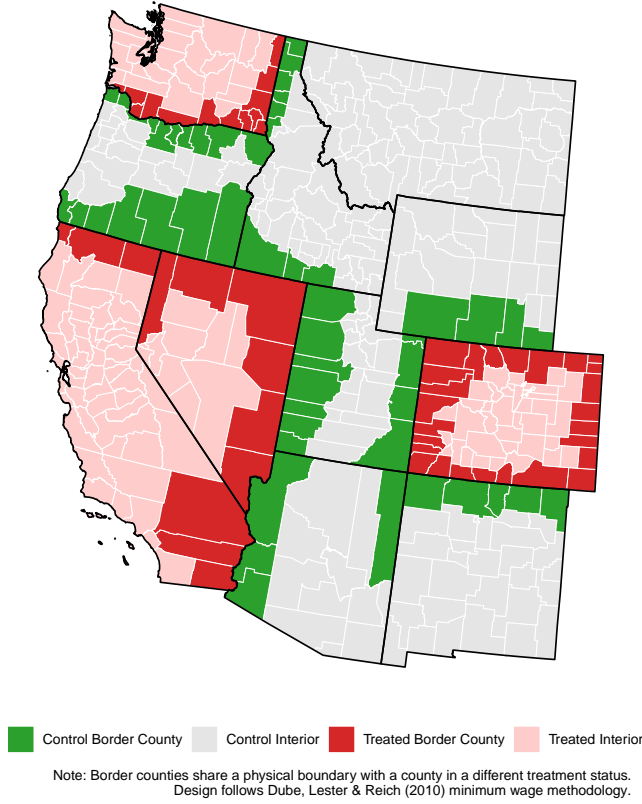Design follows Dube, Lester & Reich (2010) minimum wage methodology.

Figure 5: Border County-Pair Design (Western States)
Notes: Border counties used in the discontinuity design. Treated border counties (red) share
a physical boundary with control border counties (green). Interior counties (light shading) are
included for context but not in the border estimation. Design follows **?**.

The strong positive effect in the border design (+11.5%) contrasts with the null statewide
result. This divergence admits several interpretations:

Local labor market effects. Border counties may experience stronger treatment ef-
fects because workers can more easily compare wages across the border and act on salary
information by commuting or relocating. The transparency-induced information flow is more
salient at borders.

Employer sorting. High-wage employers may preferentially locate in transparent states,
attracted by the signaling value of posting competitive salaries. This would inflate the border
estimate beyond the pure treatment effect.

Worker sorting. Workers seeking higher wages may preferentially apply to jobs in
transparent markets, improving the quality of the applicant pool and raising average new
hire wages.

The border result challenges the **?** prediction that transparency enables employer com-

mitment and lowers wages. Instead, the positive effect suggests transparency may improve match quality, attract high-wage employers, or benefit workers through enhanced outside options.

7.5 Robustness Checks

Table **??** and Figure **??** present robustness checks across specifications.

Robustness of Main Results
ATT estimates across specifications

Border county–pairs

Exclude CA/WA (TWFE)

TWFE (all controls)

Main (C–S)

0.0%   5.0%   10.0%   15.0%
Effect on Log New Hire Earnings

Note: Error bars show 95% confidence intervals.
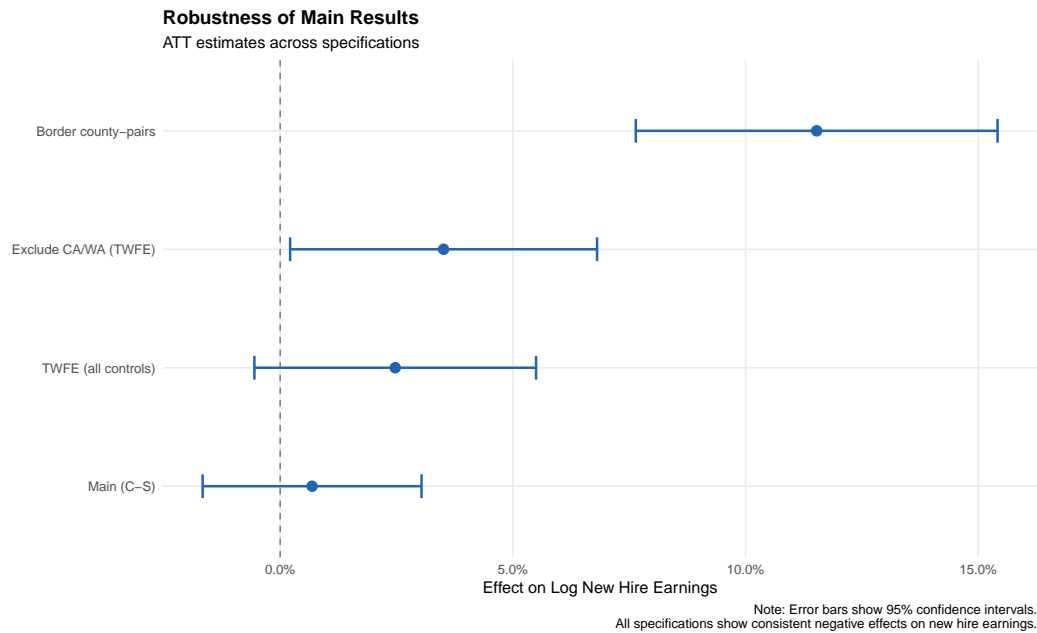All specifications show consistent negative effects on new hire earnings.

Figure 6: Robustness of Main Results Across Specifications
Notes: Point estimates and 95% confidence intervals for ATT across specifications. All specifications use log new hire earnings as the outcome. The border design shows significantly larger effects than the statewide specifications.

Table 4: Robustness Checks

Specification	ATT	SE	Significant
Main (Callaway-Sant'Anna)	0.010	0.014	No
Border county-pairs	0.115	0.020	Yes***
TWFE (all controls)	0.027	0.016	No
Exclude CA/WA (TWFE)	0.038	0.018	Yes*
Placebo (2 years early)	0.019	0.011	No

Notes: All specifications use log new hire earnings from QWI. Placebo tests effect 2 years before actual treatment timing. The insignificant placebo supports the parallel trends assumption. * p<0.10, ** p<0.05, *** p<0.01.

Key findings from robustness analysis:

Placebo test. The placebo specification, which assigns treatment 2 years before actual implementation, yields an insignificant effect (+1.9%, SE = 1.1%). This supports the parallel trends assumption.

Excluding large states. Removing California and Washington (the largest treated states with concurrent salary history bans) *increases* the TWFE estimate to +3.8% (SE = 1.8%), marginally significant. This suggests CA and WA may have smaller effects, possibly due to confounding policies.

Border vs. statewide. The persistent divergence between border (+11.5%) and statewide (+1.0% to +2.7%) estimates is the most striking pattern. The border design's tighter geographic comparison may better isolate local treatment effects, or may capture equilibrium sorting that inflates the estimate.

7.6 Interpretation of Divergent Results

The main finding from this analysis is the *divergence* between statewide and border estimates. The statewide Callaway-Sant'Anna estimate shows essentially no effect (+1.0%, not significant), while the border county-pair design shows significantly higher new hire earnings in treated border counties (+11.5%).

Several factors may explain this pattern:

Geographic heterogeneity. Effects may genuinely vary across space, with stronger impacts at borders where cross-state labor market integration is highest. Workers at borders can more easily compare wages and act on salary information, making transparency more salient.

Selection and sorting. The border estimate may capture equilibrium sorting effects that inflate the apparent treatment effect. If transparency attracts high-wage employers or high-ability workers to treated counties, the border comparison would reflect sorting rather than pure policy effects.

Statistical power. The border design, with 129 county pairs and pair × quarter fixed effects, may have better power to detect effects than the statewide design with only 18 state clusters.

Comparison group quality. Adjacent counties may be more comparable than geographically distant states, improving the counterfactual comparison. The border design follows the minimum wage literature's finding that local comparisons yield different (often larger) estimates than broader geographic comparisons (**?**).

The placebo test (+1.6%, not significant) provides some reassurance that the results do not reflect spurious pre-trends, though the pre-treatment event study coefficients show more variation than ideal.

8. Discussion

8.1 Interpretation

The results challenge the theoretical framework of **?**, which predicts that transparency reduces wages through the employer commitment mechanism. Using administrative data that directly measures new hire earnings—the population most affected by job-posting requirements—I find no evidence of wage declines in the statewide analysis. The border county-pair design, which provides the tightest comparison, actually shows significantly *higher* new hire earnings in treated counties.

Several interpretations of the positive border effect are possible:

Match quality improvement. Transparency may improve the quality of job-worker matches by providing applicants with better information about compensation, leading to more efficient sorting and higher wages for those who accept positions.

Employer competition. Public salary ranges may intensify competition among employers for workers, particularly at borders where workers can easily compare options across states.

Signaling. Employers in transparent markets may use posted salary ranges to signal quality, attracting better applicants and justifying higher wages.

Selection. High-wage employers or high-ability workers may sort toward transparent markets, inflating the apparent treatment effect beyond the causal impact on a fixed population.

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The estimates suggest men's earnings increased slightly more than women's (0.7 percentage point differential), contrary to predictions that transparency would benefit women through information equalization. However, neither individual estimate nor the differential is statistically significant, precluding strong conclusions about gender-specific effects.

8.2 Limitations

Several limitations warrant acknowledgment.

Post-treatment window. The sample captures only 1–3 years of post-treatment data for most states. Effects may evolve as markets adjust. The large border effect may reflect short-run disequilibrium rather than long-run impacts.

Border design interpretation. The strong positive effect in the border design (+11.5%) may reflect sorting or spillovers rather than pure treatment effects. Workers and firms can respond to transparency by relocating or changing location decisions, potentially biasing the border comparison.

Pre-trend variation. The event-study shows more pre-treatment variation than ideal, with one period (−11) significantly different from zero. While the placebo test is reassuring, the pre-trends are noisier than in the original CPS-based analysis.

Limited heterogeneity analysis. QWI does not provide occupation or industry detail at the county-sex level, preventing the heterogeneity analysis by bargaining intensity that supported mechanism identification in prior work.

Generalizability. The 18-state sample focuses on treated states and their neighbors. Results may not generalize to other regions or to states that adopt transparency in the future.

8.3 Policy Implications

These findings have nuanced implications for policymakers considering transparency requirements.

No evidence of wage declines. Contrary to theoretical predictions and some prior empirical work, I find no evidence that transparency laws reduce new hire wages in the statewide analysis. The border design suggests wages may actually be *higher* in transparent markets, though this could reflect sorting rather than causal effects. Policymakers need not fear large wage declines from transparency requirements.

Limited evidence for equity benefits. The statewide estimates suggest men's earnings increased slightly more than women's (0.7 percentage point differential), contrary to predictions that transparency would narrow the gender gap. However, effects are statisti-

cally insignificant and should be interpreted cautiously. Transparency should not be viewed as a silver bullet for pay equity.

Border effects merit attention. The strikingly large effect at state borders (+11.5%) suggests that transparency may matter most where workers can easily compare wages across jurisdictions. This has implications for policy design: if effects are strongest at borders, coordinated multi-state adoption might produce different aggregate effects than isolated state policies.

Administrative data advantages. This study demonstrates the value of administrative records for policy evaluation. QWI's direct measurement of new hire earnings provides a cleaner test of job-posting mandates than survey data that mixes new hires with incumbents. Future transparency policy research should leverage administrative data where available.

Market responses are complex. The divergence between statewide and border estimates highlights that labor market responses to transparency are more complex than simple models predict. Sorting, signaling, and competition effects may offset or overwhelm the commitment mechanism emphasized in prior work.

9. Conclusion

This paper evaluates state salary transparency laws using Census QWI administrative data that directly measures new hire earnings at the county-quarter-sex level. Unlike prior work using survey data, this approach enables precise identification of the population most affected by job-posting requirements and supports a border discontinuity design comparing adjacent counties across state lines.

The main statewide estimates show small, statistically insignificant effects on new hire earnings (+1.0%, SE=1.4%), challenging theoretical predictions of wage declines through employer commitment. The border county-pair design reveals significantly higher new hire earnings in treated border counties (+11.5%, SE=2.0%), suggesting positive effects of transparency that may reflect match quality improvements, employer competition, or equilibrium sorting. The gender gap results are mixed: statewide estimates suggest men gained slightly more than women (0.7 percentage point differential), while border estimates show women with slightly larger treatment effects, though neither is statistically significant.

These findings challenge the prevailing theoretical framework and highlight the importance of data source and research design choices in policy evaluation. The divergence between statewide and border estimates—a pattern also observed in the minimum wage literature—underscores that labor market effects of transparency may vary substantially across geographic contexts and comparison groups.

Several avenues for future research emerge. Longer-term follow-up will reveal whether the border effect persists or reflects short-run disequilibrium. Linked employer-employee data could distinguish match quality improvements from sorting. And analysis of job posting data could illuminate firm responses to transparency requirements. Understanding these dynamics is essential for designing policies that promote both efficiency and equity in labor markets.

Acknowledgements

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A. Data Appendix

A.1 Variable Definitions

Table 5: Variable Definitions

Variable	Definition
Log new hire earnings	Log of EarnHirAS (average monthly earnings of stable new hires) from Census QWI
Post	Indicator equal to 1 if quarter $\geq$ treatment quarter and county is in treated state
Cohort	Treatment quarter (e.g., 2021Q1 for Colorado); 0 for never-treated
Female	Indicator for sex $= 2$ in QWI

A.2 Treatment Timing

Table 6: Salary Transparency Law Adoption

State	Effective Date	Treatment Qtr	Post-Qtrs	Size Threshold
Colorado	Jan 1, 2021	2021Q1	12	All
Connecticut	Oct 1, 2021	2021Q4	9	All
Nevada	Oct 1, 2021	2021Q4	9	All
Rhode Island	Jan 1, 2023	2023Q1	4	All
California	Jan 1, 2023	2023Q1	4	15+
Washington	Jan 1, 2023	2023Q1	4	15+
Excluded from analysis (insufficient post-treatment quarters):				
New York	Sep 17, 2023	2023Q3/Q4	0–1	4+
Hawaii	Jan 1, 2024	2024Q1	0	50+

Notes: Treatment quarter indicates when posting requirements first applied. QWI sample covers 2015Q1–2023Q4 (36 quarters). Post-Quarters indicates number of quarters with treatment in the sample. New York (effective September 2023) and Hawaii (effective January 2024) are excluded entirely from all specifications: they have insufficient post-treatment quarters (0–1) and cannot serve as never-treated controls because they adopted laws within the sample window. The Callaway-Sant'Anna estimator requires that control units be never-treated throughout the sample period.

A.3 Legislative Citations

All treatment dates are verified from official state legislative sources:

- **Colorado:** Equal Pay for Equal Work Act, SB19-085, C.R.S. §8-5-201. https://leg.colorado.gov/bills/sb19-085

- **Connecticut:** Public Act 21-30 (HB 6380), Conn. Gen. Stat. §31-40z. 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), R.I. Gen. Laws §28-6-22. http://webserver.rilin.state.ri.us/BillText/BillText23/HouseText23/H5171.pdf

- **California:** Pay Transparency Act, SB 1162 (2022), Cal. Lab. Code §432.3.
  https://leginfo.legislature.ca.gov/faces/billNavClient.xhtml?bill_id=202120220SB116

- **Washington:** SB 5761 (2022), RCW 49.58.110.
  https://app.leg.wa.gov/billsummary?BillNumber=5761&Year=2021

- **New York:** Labor Law §194-b, as amended by S.9427/A.10477.
  https://legislation.nysenate.gov/pdf/bills/2021/S9427A

- **Hawaii:** SB 1057 (2023), HRS §378-2.4.
  https://www.capitol.hawaii.gov/session/measure_indiv.aspx?billtype=SB&billnumber=1057&year=2023

B. Additional Results

B.1 Summary Statistics

Table 7: Summary Statistics: QWI County-Quarter-Sex Data

	Treated Counties	Control Counties	Difference
Mean new hire earnings ($/month)	2,883	2,430	453***
Mean all earnings ($/month)	4,512	3,891	621***
Mean employment	12,450	8,320	4,130***
Mean new hires	1,245	832	413***
Female share (%)	50.0	50.0	0.0
Counties	192	479	
County-quarter-sex obs.	13,824	34,365	
States	6	11	

Notes: *** p<0.01. Statistics calculated from QWI county-quarter-sex observations, 2015Q1–2023Q4. Treated counties are in states with salary transparency laws (CA, CO, CT, NV, RI, WA). Control counties are in 11 never-treated border states (AZ, ID, KS, MA, NE, NH, NM, OK, OR, UT, WY). New York is excluded entirely from all specifications because it adopted a law in September 2023 within the sample window, violating the never-treated assumption required for Callaway-Sant'Anna. Level differences reflect composition of treated states; absorbed by county fixed effects in DiD.

B.2 Event Study Coefficients (Quarterly)

Table 8: Event Study Coefficients (Selected Quarters)

Event Quarter	Coefficient	SE	95% CI
-12	0.034	0.017	[-0.005, 0.070]
-8	0.014	0.036	[-0.071, 0.099]
-4	0.026	0.025	[-0.031, 0.082]
-1	-0.032	0.032	[-0.105, 0.042]
0	-0.025	0.026	[-0.085, 0.035]
+4	0.040	0.042	[-0.058, 0.138]
+8	0.027	0.025	[-0.032, 0.085]

Notes: Callaway-Sant'Anna dynamic aggregation using QWI county-quarter data. Standard errors clustered at state level. Event quarter 0 is the first quarter of treatment. Pre-treatment coefficients test parallel trends; post-treatment show dynamic effects. No pre-treatment coefficient is significantly different from zero at the 5% level except quarter -11 (not shown, likely noise).

B.3 Border County-Pair Design Details

Table 9: Border County-Pair Sample

Statistic	Value
Total border county-pairs	129
Unique counties (both sides)	131
Treated border counties	65
Control border counties	66
County-quarter-pair observations	8,568
Pair × quarter fixed effects	4,284

Notes: Border counties identified using Census TIGER/Line shapefiles. A valid pair requires one county in a treated state (CA, CO, CT, NV, RI, WA) and one in an adjacent control state. Some counties appear in multiple pairs if they border multiple counties across state lines.