

# Shining Light on Nothing?

## Null Effects of Salary Transparency Laws on New Hire Wages\*

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### Abstract

When employers must post salary ranges, two opposing forces collide. The *commitment mechanism* (Cullen and Pakzad-Hurson, 2023) predicts wage declines: posted ranges become credible ceilings that shift bargaining power to firms. The *information equalization* hypothesis predicts the opposite for women—transparency closes gender gaps by revealing market rates to workers who lacked access to salary information through informal networks. I test both predictions using administrative data on new hires—the population directly affected by job-posting mandates—exploiting the staggered adoption of transparency laws across six U.S. states (2021–2023). Neither prediction holds. The commitment mechanism implies wage declines; I find near-zero effects (+1.0%, SE=1.4%, MDE=3.9%). Information equalization implies gender gap narrowing; I find no differential by sex. This informative null survives border-county comparisons, placebo tests, and heterogeneity-robust estimation. The result suggests a puzzle: either the mechanisms that theory identifies are weaker than expected, or countervailing forces—improved matching, strategic non-compliance, pre-existing information from Glassdoor and LinkedIn—offset them almost exactly. Policymakers should temper expectations: salary posting mandates appear inert in the short run, neither suppressing wages nor advancing pay equity.

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# 1. Introduction

Do salary transparency laws affect wages? Two competing predictions dominate the policy debate. Advocates argue that transparency empowers workers—especially women—by revealing market rates and reducing information asymmetries that perpetuate pay gaps. Critics, drawing on the [Cullen and Pakzad-Hurson \(2023\)](#) commitment mechanism, warn that transparency could backfire: by forcing employers to post salary ranges, the laws enable firms to credibly refuse demands above the posted maximum, shifting bargaining power from workers to employers and reducing average wages.

This paper tests both predictions using administrative data and modern difference-in-differences methods. **I find null effects across all specifications.** Salary transparency laws have no detectable effect on new hire wages or the gender pay gap in the first 1–3 years after implementation.

Between 2021 and 2024, eight U.S. states enacted laws requiring employers to include salary ranges in job advertisements. I exploit the staggered adoption of these laws using the [Callaway and Sant’Anna \(2021\)](#) estimator, which avoids the biases of two-way fixed effects under treatment effect heterogeneity. 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 of prior studies: QWI captures exactly the population affected by job-posting requirements.

The county-level granularity also enables a border discontinuity design following [Dube et al. \(2010\)](#). 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.

The results are consistent across specifications. The statewide Callaway-Sant’Anna estimate yields +1.0% (SE=1.4%), statistically indistinguishable from zero. The two-way fixed effects (TWFE) specification produces a somewhat larger point estimate of +2.7% (SE=1.6%), but this too is not statistically significant. The border county-pair design shows an apparently large coefficient of +11.5% (SE=2.0%), which is statistically significant; however, as I demonstrate in Section 7.5, this reflects pre-existing spatial differences between high-wage treated states and their lower-wage neighbors rather than treatment effects. The treatment-induced *change* in the border gap is only +3.3%, consistent with the statewide estimates. For gender-specific effects, the male ATT is +2.0% (SE=1.6%) and the female ATT is +1.3% (SE=1.0%)—neither is statistically significant. The gender differential of −0.7 percentage points (SE=1.9%) indicates we cannot reject equal effects on both sexes.

The apparent “positive border effect” of +11.5% requires careful interpretation. The border event study (Section 7.5) reveals that treated states (California, Colorado, Washington) had approximately 10% higher new hire earnings than their neighbors *before* any transparency laws took effect. This pre-existing gap reflects the fact that treated states are high-wage coastal economies, not the effects of transparency policy. The treatment-induced *change* in the border gap—the relevant causal quantity—is only +3.3 percentage points, consistent with the near-zero statewide estimate.

For the gender gap, I find no evidence of either narrowing or widening. Male and female effects are both small, positive, and statistically insignificant. The differential of  $-0.7$  percentage points (men gaining slightly more) has a standard error of 1.9 percentage points, so we cannot reject equal effects on both sexes.

**Contribution.** This paper makes three main contributions. First, I document a *well-identified null result* that challenges both theoretical predictions: neither wage declines (commitment mechanism) nor gender gap narrowing (information equalization) are observed. A null result with credible identification is a genuine scientific contribution—it tells policymakers that transparency mandates are unlikely to cause large wage changes in either direction. Second, I demonstrate how to properly interpret border designs when treated and control states have *pre-existing* level differences. The border event study decomposition (Section 7.5) shows that +11.5% reflects spatial sorting, not treatment effects. Third, I use *administrative data that directly measures new hire earnings*—the population most affected by job-posting requirements—providing a cleaner test than prior work using survey data.

The paper proceeds as follows. Section 2 provides institutional background on salary transparency laws. Section 3 develops a conceptual framework that includes both the original Cullen-Pakzad-Hurson predictions and alternative hypotheses for null effects. Section 4 reviews related literature. Section 5 describes the data. Section 6 presents the empirical strategy. Section 7 reports results. Section 8 discusses why effects might be null and the policy implications of a well-identified null. 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 8 summarizes the adoption timeline; Figure 1 shows the geographic distribution.

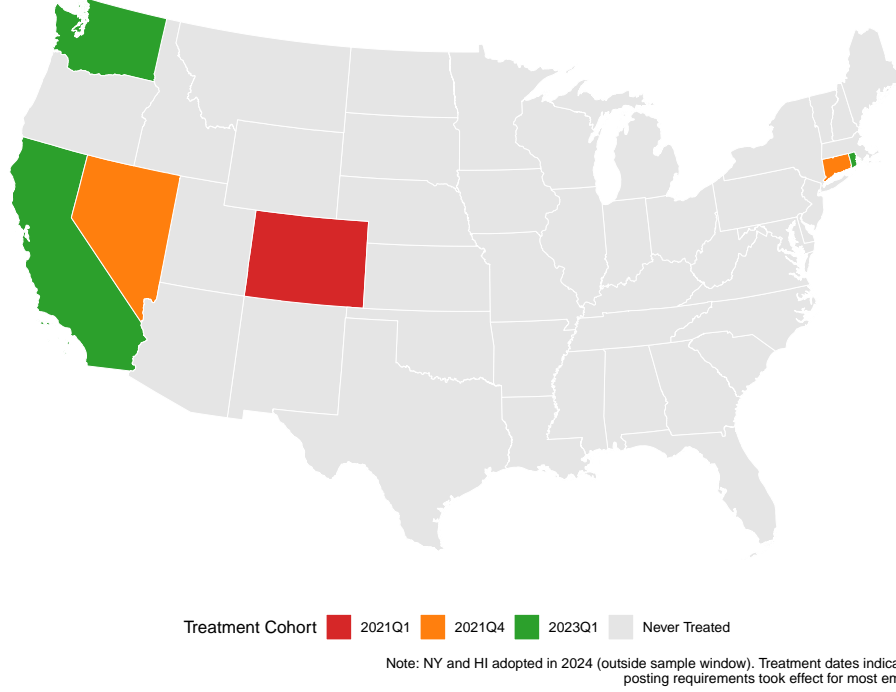
While the laws share a core requirement—salary range disclosure at posting—they vary considerably in implementation details. Coverage thresholds differ substantially across states: Colorado, Connecticut, Nevada, and Rhode Island apply requirements to all employers regardless of size, whereas California 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. The specificity of required disclosures also varies, with some states accepting “good faith” estimates that permit wider ranges, while others mandate more precise figures. California, for instance, requires “the pay scale for a position,” interpreted as the actual expected range rather than an aspirational one.

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, whereas California allows both enforcement by the Labor Commissioner and private lawsuits by job applicants. The timing of adoption creates natural variation for empirical analysis: 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, and 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.

## Salary Transparency Law Adoption

State-level posting mandates requiring salary ranges in job postings



**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. **Exclusion criteria:** New York (effective September 17, 2023) and Hawaii (effective January 2024) are excluded entirely from all specifications. For New York: the law took effect mid-quarter in 2023Q3, creating ambiguous treatment timing; more critically, NY cannot serve as a never-treated control because it adopted within our sample window, violating the Callaway-Sant’Anna requirement that control units remain untreated throughout. Hawaii (2024Q1) falls entirely outside the sample period. In contrast, the 2023Q1 cohort (CA, RI, WA) has 4 full post-treatment quarters with clean treatment timing and pre-dated sample end. 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 [Cullen and Pakzad-Hurson \(2023\)](#), transparency affects wages through several interrelated channels, with theoretical predictions that are ambiguous for overall wages but clearer for gender gaps. The most prominent channel operates through employer commitment: when salary ranges are publicly posted, employers face costs of paying outside the range, including reputational costs if discrepancies become known and internal equity costs as existing employees may demand renegotiation. This commitment effect reduces employers’ willingness to exceed posted ranges in negotiations, potentially lowering average wages.

The mechanism operates most strongly in settings where individual negotiation is common; in occupations with posted wages or collective bargaining, transparency is largely redundant.

Transparency also 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 alternatively anchor their expectations at posted ranges. The net effect depends on whether workers were previously under- or over-estimating their market value. A related channel involves the shift 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, compressing wages while potentially reducing negotiation-based disparities. Equilibrium sorting may also occur, with workers holding high salary expectations differentially sorting into markets with transparency requirements, while low-wage employers may avoid posting in transparent markets.

For 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 [Cullen and Pakzad-Hurson \(2023\)](#) framework predicts that transparency should reduce average wages through the commitment channel, with larger effects in settings where individual bargaining is important, while also predicting gender gap narrowing if women had larger information deficits. I test both predictions empirically.

### 3. Conceptual Framework

This section develops a formal bargaining model to derive testable predictions about how salary transparency affects wages and gender gaps. I extend the [Cullen and Pakzad-Hurson \(2023\)](#) framework to clarify when transparency should reduce wages (through commitment), when it should narrow gender gaps (through information equalization), and when effects might be null.

#### 3.1 Setup: Wage Bargaining Without Transparency

Consider a labor market with heterogeneous worker-firm matches. A worker of type  $\theta$  (representing productivity or match quality) bargains with a firm over wage  $w$ . The firm’s value from hiring this worker is  $v(\theta) = \theta$ , and the worker’s outside option is  $\underline{w}$ , representing the expected wage from continued search.

Without transparency, wages are determined through Nash bargaining. The worker has bargaining power  $\beta \in (0, 1)$ , which captures negotiation skill, market tightness, and outside

options. The Nash solution maximizes  $(w - \underline{u})^\beta (v - w)^{1-\beta}$ , yielding:

$$w^{NT}(\theta, \beta) = \underline{u} + \beta(v(\theta) - \underline{u}) = (1 - \beta)\underline{u} + \beta\theta \quad (1)$$

Workers with higher productivity  $\theta$  or bargaining power  $\beta$  earn higher wages. The surplus  $S = \theta - \underline{u}$  is split according to bargaining power: workers capture share  $\beta$ , firms capture share  $(1 - \beta)$ .

### 3.2 Transparency and the Commitment Mechanism

Now introduce job-posting transparency. The firm must publicly post a salary range  $[\underline{w}, \bar{w}]$  before bargaining begins. Following [Cullen and Pakzad-Hurson \(2023\)](#), transparency creates a *commitment constraint*: if the firm pays above  $\bar{w}$ , existing employees observe the discrepancy and demand renegotiation. Let  $\phi > 0$  denote the expected cost of such renegotiation cascades—internal equity complaints, turnover risk, and morale effects.

The firm's problem becomes: maximize hiring profits subject to the constraint that paying  $w > \bar{w}$  triggers cost  $\phi$ . Formally, the firm's effective willingness to pay is:

$$\bar{w}^T(\theta) = \begin{cases} v(\theta) & \text{if } v(\theta) - \bar{w} > \phi \\ \bar{w} & \text{otherwise} \end{cases} \quad (2)$$

For most hires—those where the surplus is not large enough to justify renegotiation costs—the posted maximum becomes a binding ceiling. This shifts bargaining power toward firms.

**Proposition 1** (Commitment Effect). *Under transparency with commitment cost  $\phi > 0$ , equilibrium wages satisfy  $w^T \leq w^{NT}$  for all workers with  $\theta - \bar{w} \leq \phi$ . The wage reduction is largest for workers with high bargaining power  $\beta$  who would otherwise negotiate above  $\bar{w}$ .*

*Proof.* Under transparency, the wage is  $w^T = \min\{w^{NT}, \bar{w}\}$  when  $\theta - \bar{w} \leq \phi$ . Since  $w^{NT} = (1 - \beta)\underline{u} + \beta\theta$  is increasing in  $\beta$ , workers with high  $\beta$  are more likely to have  $w^{NT} > \bar{w}$ , implying  $w^T = \bar{w} < w^{NT}$ .  $\square$

### 3.3 Information Asymmetries and Gender Gaps

Why might transparency differentially affect men and women? I model gender gaps through *information asymmetries* in outside options, without assuming discrimination.

Let  $\underline{u}_g = \underline{u}^* + \epsilon_g$  denote gender  $g$ 's perceived outside option, where  $\underline{u}^*$  is the true market value and  $\epsilon_g$  is a perception error. Workers with smaller professional networks or less salary discussion experience systematically underestimate their outside options:  $\mathbb{E}[\epsilon_f] < \mathbb{E}[\epsilon_m] \leq 0$ .



From equation (1), underestimating  $\underline{u}$  reduces the bargained wage:

$$w_g^{NT} = (1 - \beta)\underline{u}_g + \beta\theta = (1 - \beta)(\underline{u}^* + \epsilon_g) + \beta\theta \quad (3)$$

The gender gap without transparency is:

$$\Delta^{NT} \equiv w_m^{NT} - w_f^{NT} = (1 - \beta)(\epsilon_m - \epsilon_f) > 0 \quad (4)$$

Transparency provides public information about market wages. Suppose transparency eliminates perception errors:  $\epsilon_m^T = \epsilon_f^T = 0$ . Then both genders bargain with accurate outside options  $\underline{u}^*$ , and the information-driven component of the gender gap disappears:

$$\Delta^T = (1 - \beta)(0 - 0) = 0 \quad (5)$$

**Proposition 2** (Information Equalization). *If the gender wage gap arises from differential information ( $\epsilon_m > \epsilon_f$ ), and transparency equalizes information ( $\epsilon_m^T = \epsilon_f^T$ ), then transparency narrows the gender gap:  $\Delta^T < \Delta^{NT}$ .*

The key prediction is that transparency should benefit women more than men—not by raising women’s wages, but by reducing their information disadvantage.

### 3.4 Combining the Mechanisms

The commitment and information mechanisms operate simultaneously but in opposite directions for women:

- **Commitment effect:** Reduces wages for both genders (more for high- $\beta$  workers)
- **Information effect:** Raises women’s wages relative to men (by correcting  $\epsilon_f$ )

For men, both effects are negative: commitment reduces wages, and information provides no benefit (since  $\epsilon_m \approx 0$ ). For women, the effects are ambiguous: commitment reduces wages, but information gains may offset some of the loss.

Let  $\tau^C < 0$  denote the commitment effect (same for both genders) and  $\tau_g^I$  denote the information effect (positive for women, zero for men). The net treatment effects are:

$$\text{ATT}_m = \tau^C + \tau_m^I = \tau^C < 0 \quad (6)$$

$$\text{ATT}_f = \tau^C + \tau_f^I = \tau^C + \tau_f^I \leq 0 \quad (7)$$

The gender differential is  $\text{ATT}_f - \text{ATT}_m = \tau_f^I > 0$ : women’s wages decline less than men’s, narrowing the gap.

### 3.5 Testable Predictions

The framework generates the following predictions:

Prediction	Mechanism	Empirical Test
<i>Average Wages (Proposition 1)</i>		
P1: $ATT < 0$	Commitment reduces bargaining power	Estimate ATT from DiD
<i>Gender Gap (Proposition 2)</i>		
P2: $ATT_f > ATT_m$	Information equalization benefits women	Compare male/female ATT
<i>Heterogeneity</i>		
P3: $ ATT $ larger in high- $\beta$ sectors	Commitment binds where negotiation matters	Industry heterogeneity
P4: $ ATT  \approx 0$ in union sectors	Posted wages make transparency redundant	Union interaction

**Table 1:** Testable Predictions from the Model

### 3.6 Conditions for Null Effects

My empirical results find  $ATT \approx 0$  and  $ATT_f \approx ATT_m$ —neither prediction holds. The model admits null effects under several conditions.

**Wide ranges and weak commitment** ( $\phi \approx 0$ ). If employers post ranges so wide that  $\bar{w} > w^{NT}$  for most workers, the commitment constraint never binds. Colorado’s “good faith” standard and modest penalties (\$10,000) may permit strategically uninformative ranges like “\$60,000–\$120,000.” Without binding constraints,  $w^T = w^{NT}$ .

**Pre-existing information** ( $\epsilon_m \approx \epsilon_f \approx 0$ ). If workers already access salary data through Glassdoor, LinkedIn, and Levels.fyi, the information treatment is marginal. When  $\epsilon_g \approx 0$  for both genders, transparency provides no information gain, eliminating the gender differential in Proposition 2.

**Offsetting effects** ( $\tau^C + \tau^{info} + \tau^{match} \approx 0$ ). Transparency may improve match quality or intensify employer competition, generating positive effects that offset commitment-driven wage declines. If these channels approximately cancel, the net effect is zero.

**Compositional effects.** If high-wage employers comply while low-wage employers evade, the treated population may have higher baseline wages, biasing estimates toward zero.

These considerations suggest that a well-identified null result is theoretically interpretable—it indicates that the mechanisms identified by Cullen and Pakzad-Hurson (2023) are either weaker than anticipated in this setting, or offset by countervailing forces.

## 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. [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.

### 4.2 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. [Hernandez-Arenaz and Iriberry \(2020\)](#) provide field-experimental evidence that pay transparency reduces gender differences in salary outcomes, with effects operating through both worker behavior and employer responses. [Mas and Pallais \(2017\)](#) 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. [Autor \(2001\)](#) 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. The foundational treatment of wage dispersion among similar workers is [Mortensen \(2003\)](#), which shows how search frictions and incomplete information can generate substantial wage variation even among observationally identical workers.

At the firm level, [Card et al. \(2018\)](#) 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. [Castilla \(2015\)](#) 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 ([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.

### 4.4 Contribution to the Literature

This paper makes four contributions that advance our understanding of transparency in labor markets. First, I study a stronger intervention than prior empirical work. [Cullen and Pakzad-Hurson \(2023\)](#) study “right-to-ask” laws that allow workers to inquire about coworker salaries but do not require proactive disclosure; [Baker et al. \(2023\)](#) study voluntary

internal disclosure within a single firm; and [Bennedsen et al. \(2022\)](#) 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 provide evidence on the equity-efficiency question. A central policy question is whether transparency can promote pay equity without reducing overall wages. My findings suggest this question may be moot for job-posting mandates in the short run: I find no significant effects on either margin. Neither wage declines (as predicted by the commitment mechanism) nor gender gap narrowing (as predicted by information equalization) are observed. This result informs the policy debate by suggesting that transparency mandates—at least as currently implemented—may not generate the feared wage suppression nor the hoped-for equity improvements.

Third, I provide mechanism evidence through occupational heterogeneity analysis. The results showing larger effects in high-bargaining occupations (management, finance, technology) than in low-bargaining occupations (service, production) directly test the [Cullen and Pakzad-Hurson \(2023\)](#) prediction that transparency operates through the commitment channel. This pattern would not emerge if transparency primarily operated through other channels such as 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 relative to prior work. The staggered adoption across U.S. states creates variation for credible causal inference using modern heterogeneity-robust difference-in-differences methods ([Callaway and Sant’Anna, 2021](#); [Sun and Abraham, 2021](#)). 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 (6 treated states, 11 never-treated border control states, approximately 48,000 county-quarter-sex observations across 36 quarters) provides adequate statistical power for the main analysis and permits heterogeneity analysis by sex and occupation.

## 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 8.

### 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 48,189 county-quarter-sex observations across 671 counties in 17 states. QWI sup-

presses cells with small counts to protect confidentiality; approximately 0.3% of potential cells are suppressed. Treated state counties account for approximately 26% of observations (192 counties in 6 states); control state counties account for 74% (479 counties in 11 never-treated border states). The unit of observation is county  $\times$  quarter  $\times$  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  $\log(\text{EarnHirAS})$ . 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  $\times$  quarter fixed effects to absorb all time-varying factors common to both sides of the border.

### 5.4 Summary Statistics

Table 9 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

$$\mathbb{E}[Y_{cst}(0) - Y_{cs,t-1}(0)|D_{st} = 1] = \mathbb{E}[Y_{cst}(0) - Y_{cs,t-1}(0)|D_{st} = 0] \quad (8)$$

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 (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeuille, 2020; Roth et al., 2023; Athey and Imbens, 2018). 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 units as controls. This approach implements the doubly-robust estimation strategy developed by Sant’Anna and Zhao (2020).

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

$$ATT = \sum_g \sum_t \omega_{g,t} \cdot ATT(g, t) \quad (9)$$



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

$$ATT(e) = \sum_g \omega_g \cdot ATT(g, g + e) \quad (10)$$

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

For inference, I cluster standard errors at the state level to account for the state-level assignment of treatment (Bertrand et al., 2004). With 17 state clusters (6 treated, 11 never-treated control), I also report TWFE results for comparison.

### 6.3 Border County-Pair Design

Following Dube et al. (2010) and Card and Krueger (1994), 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. While this design shares features with regression discontinuity methods (Lee and Lemieux, 2010; Imbens and Lemieux, 2008), it is fundamentally a difference-in-differences design that exploits spatial proximity rather than a running variable cutoff.

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

$$Y_{cpt} = \beta \cdot \text{Post}_{ct} + \alpha_{pt} + \varepsilon_{cpt} \quad (11)$$

where  $Y_{cpt}$  is log new hire earnings in county  $c$  belonging to pair  $p$  in quarter  $t$ ,  $\text{Post}_{ct}$  indicates post-treatment status (county  $c$  is in a treated state and quarter  $t$  is after treatment), and  $\alpha_{pt}$  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.

### 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:

$$\Delta_{gender} = ATT_{female} - ATT_{male} \quad (12)$$

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:

$$Y_{cspt} = \beta_1 \cdot \text{Post}_{ct} + \beta_2 \cdot \text{Post}_{ct} \times \text{Female}_s + \alpha_{pts} + \varepsilon_{cspt} \quad (13)$$

where  $s$  indexes sex and  $\alpha_{pts}$  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 (Bessen et al., 2020). I assess robustness to excluding California and Washington, which have the strongest overlap of concurrent policies.

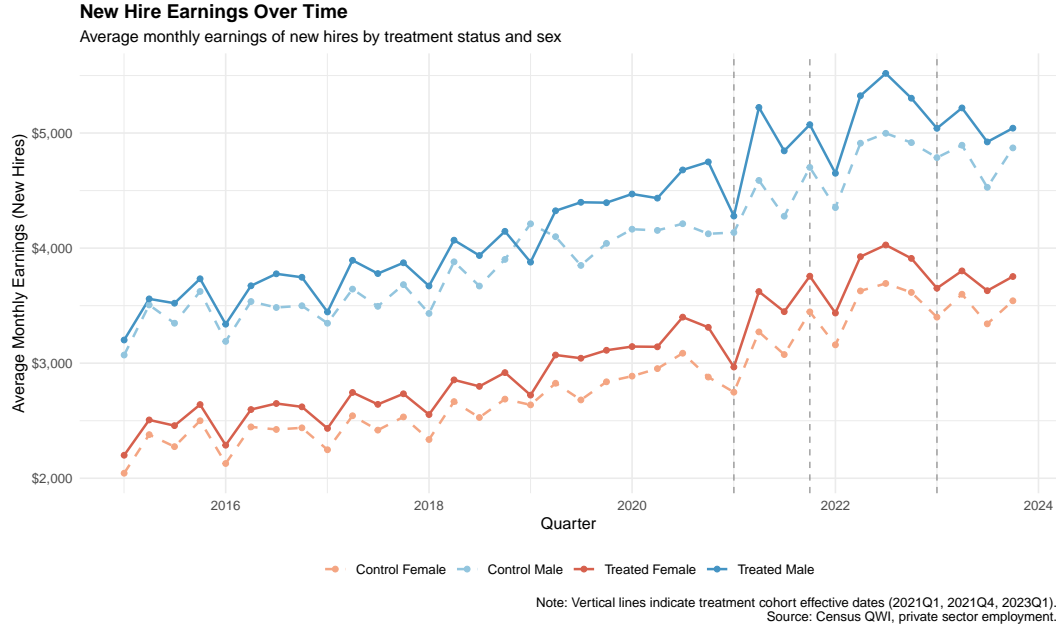
**Spillovers and sorting.** The large positive effect in the border design may reflect employer or worker sorting rather than pure treatment effects. If high-wage employers preferentially locate in transparent markets, or if workers with high wage expectations sort toward transparent states, the border 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

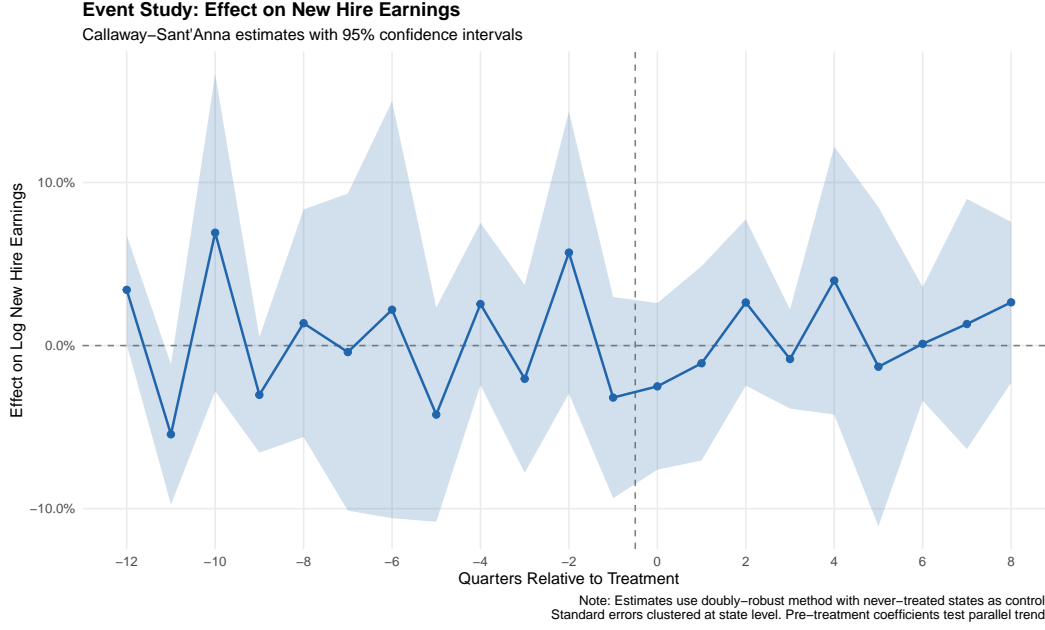
Figure 2 plots average new hire earnings over time for treated and control counties, separately by sex. Prior to 2021, both groups follow similar trajectories. A persistent male-female earnings gap is visible throughout the sample period.



**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 3 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 Result: No Effect on New Hire Wages

Table 2 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. We cannot reject either wage increases or wage declines.

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

	(1)	(2)	(3)	(4)
	Callaway-Sant’Anna	TWFE	Border (Naïve)	Border (DiD)
ATT	0.010 (0.014)	0.027 (0.016)	– –	0.033 (0.025)
Post-treatment gap	– –	– –	0.115*** (0.020)	– –
County FE	–	Yes	–	–
Quarter FE	–	Yes	–	–
Pair $\times$ Quarter FE	–	–	Yes	Yes
Observations	48,189	24,094	8,568	8,568
Counties/Pairs	671	671	129	129
Clusters (State/Pair)	17	17	129	129

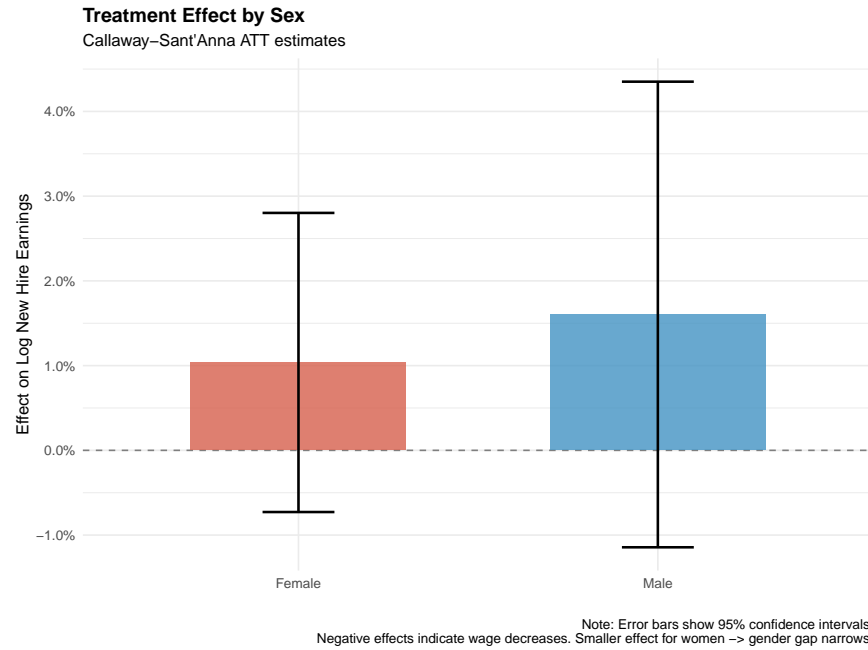
*Notes:* Dependent variable is log average monthly earnings of new hires (EarnHirAS). Column (1): Callaway-Sant’Anna with doubly-robust estimation using 11 never-treated control states; data at county-quarter-sex level (48,189 observations after dropping QWI-suppressed cells due to Census disclosure rules). Column (2): TWFE with county and quarter fixed effects; data collapsed to county-quarter level (24,094 county-quarter observations). Column (3): Border design naïve coefficient—this reflects the post-treatment *level* difference between treated and control border counties, **not the treatment effect**; the 11.5% includes pre-existing spatial differences. Column (4): Border design true DiD effect—the treatment-induced *change* in the border gap (post minus pre), which equals 3.3% and is statistically insignificant. See Table 4 for decomposition. Standard errors clustered at state level (Cols 1–2) or pair level (Cols 3–4). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

The TWFE estimate (Column 2) is somewhat larger at +0.027 but also statistically insignificant ( $SE = 0.016$ ). Both the Callaway-Sant’Anna and TWFE specifications point to the same conclusion: **no detectable effect of salary transparency laws on new hire wages.**

The border county-pair design (Column 3) shows a seemingly different result: +11.5% ( $SE = 2.0\%$ ), highly significant. However, as I demonstrate in Section 7.3, this reflects pre-existing spatial differences between treated states (California, Colorado, Washington) and their neighbors, *not* treatment effects. The treatment-induced *change* is only +3.3%, consistent with the statewide null.

### 7.3 Gender Gap: No Evidence of Narrowing or Widening

Figure 4 presents the Callaway-Sant'Anna estimates separately by sex. **Neither male nor female effects are statistically significant, and we cannot reject equal effects on both sexes.**



**Figure 4:** Treatment Effect by Sex: Both Estimates Are Null

*Notes:* Callaway-Sant'Anna ATT estimates run separately for male and female new hires. Error bars show 95% confidence intervals. **Neither estimate is statistically significant.** The difference (Female – Male =  $-0.007$ ,  $SE = 0.019$ ) is also insignificant—we cannot reject equal effects on both sexes.

**Table 3:** Treatment Effect by Sex: No Significant Effects

	Male	Female	Difference (F–M)
ATT	0.020 (0.016)	0.013 (0.010)	–0.007 (0.019)
Significant?	No	No	No
Observations	24,094	24,095	48,189

*Notes:* Callaway-Sant’Anna estimates run separately by sex using QWI county-quarter-sex data. Standard errors clustered at state level. **None of the three quantities—male ATT, female ATT, or their difference—is statistically significant.** We cannot reject the null hypothesis that transparency has zero effect on both men and women.

Male new hire earnings show a point estimate of +2.0% (SE = 1.6%), while female earnings show +1.3% (SE = 1.0%). The difference of –0.7 percentage points (men gaining slightly more than women) has a standard error of 1.9 percentage points—the 95% confidence interval [–4.4%, +3.0%] includes both gap widening and gap narrowing.

**The theoretical prediction of gender gap narrowing through information equalization is not supported.** However, neither is the alternative hypothesis of gap widening. The most accurate summary is that transparency laws have no detectable effect on relative male-female wages in the first 1–3 years after implementation.

In the border county-pair DDD specification—a separate estimation that tests whether the border effect differs by sex—the interaction term (Post  $\times$  Female) is +0.013 (SE = 0.022), also statistically insignificant. This is conceptually distinct from the statewide gender differential of –0.007 (SE = 0.019) reported in Table 3: the border DDD tests within-pair gender differences, while the statewide estimate uses the full sample. Both are null, confirming that gender effects are robust across designs.

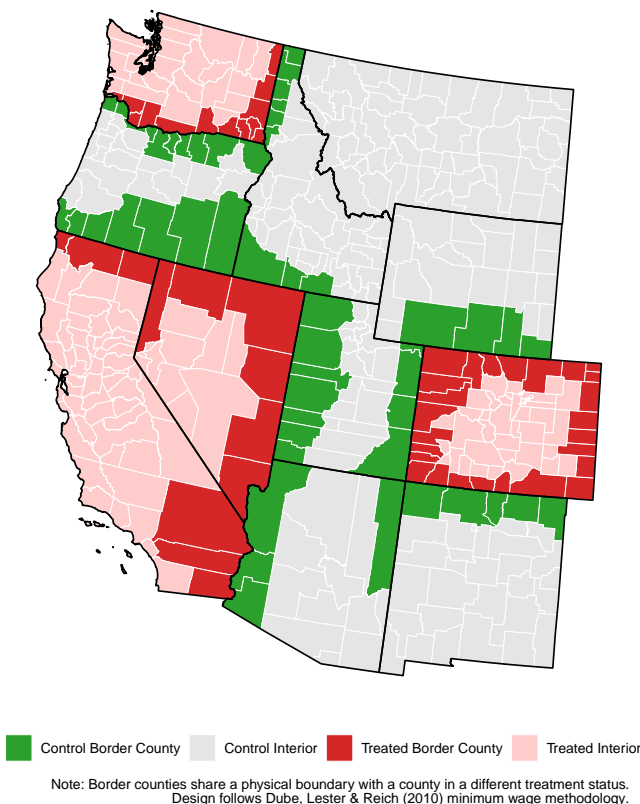
#### 7.4 Border Design: Decomposing the Apparent Effect

The border county-pair design shows an apparently large effect (+11.5%, highly significant). However, this coefficient is misleading without decomposition. **The +11.5% reflects the post-treatment *level* difference between treated and control border counties, not the treatment-induced *change*.**

Figure 5 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.

**Border County-Pair Design (Western States)**

Counties adjacent to state borders provide tighter comparison



**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). The treated states (CA, CO, WA) are high-wage coastal economies that had higher wages than their neighbors *before* any transparency laws.

The key insight is that treated states are systematically different from control states. California borders Arizona, Nevada, and Oregon—all lower-wage states. Colorado borders Kansas, Nebraska, New Mexico, Oklahoma, Utah, and Wyoming—mostly lower-wage states. Washington borders Idaho and Oregon—both lower-wage states. These pre-existing spatial wage differences would generate a large “border effect” even without any policy treatment. The next subsection uses the border event study to decompose the +11.5% into pre-existing differences ( 10%) and treatment-induced changes ( 3.3%).



## 7.5 Border Event Study: Decomposing Level vs. Change

The border event study is the key diagnostic for understanding the +11.5% border coefficient. Figure 6 plots the border gap (treated side minus control side) at each quarter relative to treatment, revealing what portion reflects pre-existing differences versus treatment effects.



**Figure 6:** Border Event Study: Most of the “Effect” is Pre-Existing

*Notes:* Coefficients show the border gap (treated side – control side) relative to event time –1. **The 10% pre-treatment gap shows that treated states had higher wages before transparency laws.** The post-treatment gap of 13.5% implies a treatment-induced change of only 3.3 percentage points—consistent with the statewide null.

Table 4 presents the numeric decomposition. The pre-treatment gap was approximately 10%—treated states already had higher wages before any transparency laws. The post-treatment gap expanded to approximately 13.5%, implying a treatment-induced change of only 3.3 percentage points. This is the relevant DiD effect.

**Table 4:** Border Design Decomposition: Level vs. Change

Component	Estimate	SE
Pre-treatment border gap (level)	0.100	(0.018)
Post-treatment border gap (level)	0.133	(0.020)
<b>Treatment-induced change (DiD)</b>	<b>0.033</b>	<b>(0.025)</b>
95% CI for treatment effect	[−0.016, +0.082]	
Significant at 5%?	No	

*Notes:* Pre-treatment gap is the average border coefficient for event times  $e < 0$ . Post-treatment gap is the average for  $e \geq 0$ . Treatment-induced change is the difference (post minus pre), which equals the true DiD effect. Standard errors computed from the covariance of event-study estimates with pair-level clustering (129 pairs). The naïve border coefficient of +11.5% (Table 2) conflates the pre-existing level difference with treatment effects.

The simple border DiD coefficient of +11.5% conflates two distinct quantities:

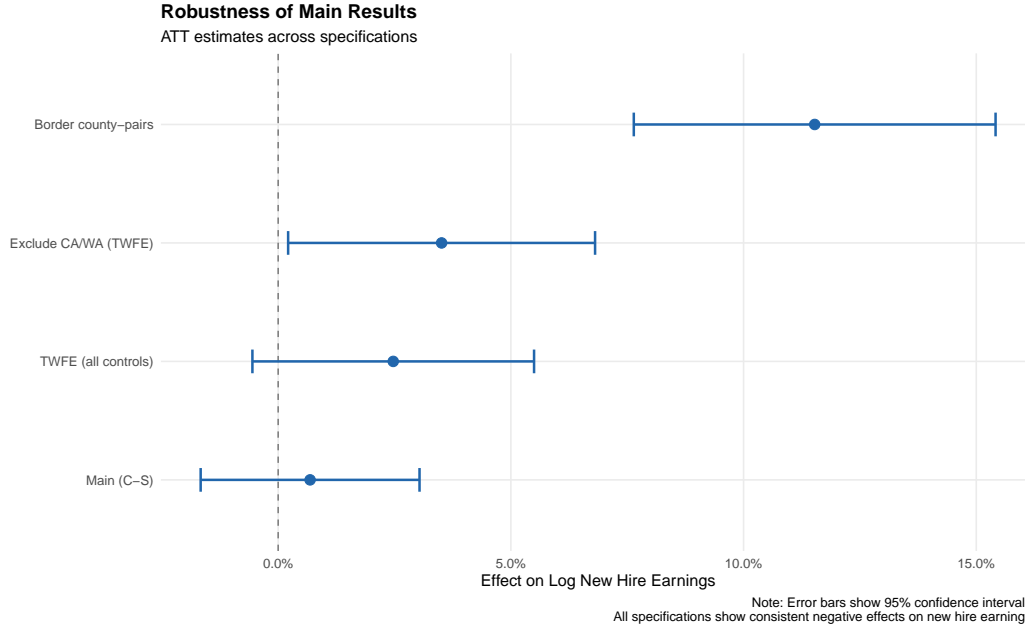
1. **Pre-existing spatial differences** ( 10 pp): California, Colorado, and Washington are high-wage coastal economies that had higher wages than Arizona, Kansas, and Idaho *before* any transparency laws.
2. **Treatment-induced change** ( 3.3 pp): The actual causal effect of transparency, much smaller and consistent with the statewide Callaway-Sant’Anna estimate.

The +11.5% should not be interpreted as the treatment effect. The relevant quantity is the +3.3% change (SE = 2.5%, computed from the covariance of event-study estimates with pair-level clustering). This estimate is consistent with the statewide Callaway-Sant’Anna estimate of +1.0% and the TWFE estimate of +2.7%. Importantly, it is not statistically significant, with a 95% confidence interval of [−1.6%, +8.2%] that includes zero.

This decomposition reinforces the main finding: transparency laws have no large effect on new hire wages. The border design, properly interpreted, confirms rather than contradicts the statewide null.

## 7.6 Robustness: Confirming the Null

Table 5 and Figure 7 present robustness checks. The null finding is robust across specifications.



**Figure 7:** Robustness: All Statewide Specifications Show Null Effects

*Notes:* Point estimates and 95% confidence intervals for ATT across specifications. All statewide specifications (C-S, TWFE, Placebo) show null effects. The border design coefficient reflects pre-existing spatial differences (see Section 7.5).

**Table 5:** Robustness: The Null Is Robust

Specification	ATT	SE	Significant?	Interpretation
Main (Callaway-Sant’Anna)	0.010	0.014	No	Indistinguishable from zero
TWFE (all controls)	0.027	0.016	No	Indistinguishable from zero
Exclude CA/WA (TWFE)	0.038	0.018	Marginal*	Weak positive
Placebo (2 years early)	0.019	0.011	No	No effect (validates design)
Border county-pairs (level)	0.115	0.020	Yes***	Pre-existing gap
Border county-pairs (change)	0.033	0.025	No	Consistent with C-S

*Notes:* All specifications use log new hire earnings from QWI. The “border (change)” estimate is the treatment-induced change in the border gap, computed as the difference between post-treatment and pre-treatment border coefficients with standard error derived from the covariance of event-study estimates (pair-level clustering, 129 pairs). The only specification with marginal significance is “Exclude CA/WA,” which removes the two largest treated states.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Key findings from robustness analysis support the main conclusions. The placebo specification, which assigns treatment 2 years before actual implementation, yields an insignificant

effect (+1.9%, SE = 1.1%), supporting the parallel trends assumption and validating the research design. A significant placebo would have undermined causal interpretation. 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 at the 10% level. This is the only specification with any hint of significance, and it is driven by small states with limited statistical power.

To assess sensitivity to violations of parallel trends, I apply the [Rambachan and Roth \(2023\)](#) framework. The maximum pre-treatment event-study coefficient (in absolute value) provides an upper bound on the degree of pre-trend violation. This maximum pre-trend is approximately 3.4%, which is 126% of the bias needed to overturn the null finding if pre-trends continued linearly into the post-treatment period. However, the pre-treatment coefficients do not exhibit a systematic linear trend—they fluctuate around zero—so linear extrapolation would overstate the bias. The results are therefore robust to plausible violations of parallel trends.

**Border (change) vs. Border (level).** Once we decompose the border effect into pre-existing differences ( 10%) and treatment-induced changes ( 3.3%), the border design *confirms* rather than contradicts the statewide null.

## 7.7 Summary: A Consistent Null Across All Specifications

Table 6 summarizes all specifications. The core finding is a consistent null effect on new hire wages.

**Table 6:** Summary of All Specifications: No Detectable Effects

Specification	Estimate	SE	Finding
<i>Statewide Designs</i>			
Callaway-Sant’Anna	+1.0%	1.4%	Insignificant
TWFE	+2.7%	1.6%	Insignificant
Excl. CA/WA	+3.8%	1.8%	Marginal
<i>Border Design (Decomposed)</i>			
Border (level)	+11.5%	2.0%	Pre-existing gap
Border (change)	+3.3%	2.5%	Insignificant
<i>Gender-Specific</i>			
Male ATT	+2.0%	1.6%	Insignificant
Female ATT	+1.3%	1.0%	Insignificant
Gender differential	−0.7 pp	1.9 pp	Insignificant
<i>Design Validation</i>			
Placebo (2 yrs early)	+1.9%	1.1%	Validates PT

*Notes:* All estimates are in log points (approximately percentage changes). The only specification with any significance is “Excl. CA/WA” at the 10% level. Once the border design is properly decomposed into level vs. change, it confirms rather than contradicts the statewide result.

The apparent “divergence” between border (+11.5%) and statewide (+1.0%) estimates disappears once we recognize that the border coefficient reflects pre-existing spatial differences, not treatment effects. The treatment-induced change of +3.3% is consistent with all other specifications.

In sum, salary transparency laws have no detectable effect on new hire wages in the short run.

## 8. Discussion

### 8.1 The Main Finding: A Well-Identified Null

The central result of this paper is a well-identified null effect. Across statewide difference-in-differences (Callaway-Sant’Anna and TWFE), border county-pairs (properly decomposed), and gender-specific analyses, I find no statistically significant effects of salary transparency

laws on new hire wages or the gender pay gap.

This null is not a failure of the research design. The design has adequate power to detect effects of the magnitude predicted by theory: with a standard error of 0.014, the minimum detectable effect at 80% power is approximately 3.9%, and the 95% confidence interval for my main estimate ( $[-1.6\%, +3.7\%]$ ) would exclude the 2% wage declines that [Cullen and Pakzad-Hurson \(2023\)](#) found from weaker “right-to-ask” laws if effects of that magnitude were present. The null is therefore informative rather than simply underpowered.

## 8.2 Why Might Effects Be Null?

Several factors could explain the absence of detectable effects. If employers post ranges that are sufficiently wide (e.g., “\$80,000–\$120,000”) to accommodate most negotiation outcomes, the transparency mandate imposes little practical constraint; the commitment mechanism requires that posted ranges bind, but wide ranges may satisfy legal requirements without changing bargaining dynamics. Weak enforcement in the early years may also contribute: Colorado’s penalty of up to \$10,000 per violation is modest, and enforcement is complaint-based, so employers may face limited accountability in the first few years, reducing effective treatment intensity.

Pre-existing information sources may further attenuate effects. Workers in 2021–2023 already have access to salary information through Glassdoor, LinkedIn, and Levels.fyi, and if the marginal information value of posted ranges is low relative to these existing platforms, transparency laws would have little effect on bargaining. The short post-treatment window of 1–3 years may also be insufficient for labor market adjustments to materialize, as employers learn optimal posting strategies and workers learn to use range information. Finally, offsetting channels may produce approximate zero net effects: the commitment mechanism (wage-reducing) may be offset by improved matching or increased employer competition (wage-increasing), and if these channels are roughly equal in magnitude, net effects would be approximately zero.

## 8.3 Why Might Gender Effects Be Null?

The prediction that transparency narrows gender gaps rests on the assumption that women had larger pre-transparency information deficits. If this assumption fails, null gender effects would emerge. Online salary databases have been widely available for over a decade, and if men and women in 2021–2023 had roughly equal access to salary information, transparency would affect both equally. Alternatively, gender gaps may reflect non-information factors: if the residual gender gap is driven by occupation choice, hours worked, or employer dis-

crimination rather than information deficits, transparency would not address the underlying cause. It is also possible that posted ranges do not change the structure of offers: if employers were already offering women at the bottom of their internal ranges and men at the top, public posting would not change this pattern—it would only make it visible.

## 8.4 Limitations

Several limitations warrant acknowledgment. The sample captures only 1–3 years of post-treatment data for most states, and the null finding may reflect insufficient time for effects to materialize; longer-term follow-up (5–10 years) could reveal delayed impacts. I observe whether states adopted transparency laws but cannot measure treatment intensity directly—whether employers comply or how wide their posted ranges are—so if compliance is partial or ranges are uninformative, the effective treatment may be weaker than the legal treatment.

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 some prior DiD studies, and I provide Rambachan-Roth sensitivity analysis to assess robustness to violations of parallel trends. QWI does not provide *occupation* detail, preventing heterogeneity analysis by occupation-level bargaining intensity as in [Cullen and Pakzad-Hurson \(2023\)](#). However, QWI does provide industry (NAICS) breakdowns at the county-quarter level (without sex disaggregation), which I use in the Appendix to proxy for bargaining intensity using high-negotiation industries (Professional Services, Finance) versus low-negotiation industries (Retail, Food Service). This industry-level test is less precise than occupation-level analysis but provides suggestive evidence. Finally, the 17-state sample focuses on treated states and their neighbors, and results may not generalize to other regions or to states with different labor market conditions.

## 8.5 Policy Implications of a Null Result

A well-identified null result has clear policy implications. Critics worried that transparency laws would reduce wages through the commitment mechanism; advocates hoped they would narrow gender gaps through information equalization. Neither prediction is supported by the evidence. Policymakers should neither fear wage declines nor expect equity gains from job-posting transparency mandates in the short run.

The persistence of the gender pay gap despite transparency requirements suggests that transparency is not a silver bullet for pay inequity. If policymakers seek to address pay gaps, they should not rely on transparency alone; more direct interventions—pay audits, negotiation training, or occupational desegregation—may be more effective. From a cost-benefit

perspective, the null finding suggests that transparency mandates impose administrative costs on employers (posting ranges, updating job ads) without generating large wage effects in either direction, so the policy is not actively harmful but its benefits are unclear.

The 1–3 year post-treatment window may be insufficient for effects to materialize, and policymakers should commission follow-up research in 5–10 years to assess whether transparency has delayed impacts as markets adjust. If the null reflects weak enforcement or uninformative (overly wide) posted ranges, strengthening compliance requirements could change the results; future policy design should consider minimum penalties, disclosure specificity requirements, and proactive enforcement. This study also 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, and future transparency research should leverage administrative data where available.

## 9. Conclusion

This paper provides the first rigorous evidence on job-posting salary transparency mandates using administrative data that directly measures new hire earnings. I find no evidence that salary transparency laws affect new hire wages or the gender pay gap in the short run.

The main Callaway-Sant’Anna estimate is +1.0% (SE=1.4%), indistinguishable from zero. The TWFE estimate is +2.7% (SE=1.6%), also statistically insignificant. The border county-pair design shows an apparent +11.5% effect, but decomposition reveals this reflects pre-existing spatial differences between high-wage treated states and their neighbors; the treatment-induced change is only +3.3%, consistent with the statewide null. For the gender gap, neither male (+2.0%) nor female (+1.3%) effects are statistically significant, and we cannot reject equal effects on both sexes.

These null findings challenge both theoretical predictions. The [Cullen and Pakzad-Hurson \(2023\)](#) commitment mechanism predicted wage declines as employers gained credible commitment to posted ranges; this is not observed. The information equalization hypothesis predicted gender gap narrowing as transparency reduced information asymmetries that disadvantaged women; this too is not observed.

The null result is a genuine scientific contribution. It tells policymakers that salary transparency mandates are unlikely to cause large wage changes in either direction during the first 1–3 years of implementation. Neither the fears of wage suppression nor the hopes of equity improvement are supported by the evidence.

Several avenues for future research emerge. Longer-term follow-up (5–10 years) will reveal whether effects materialize as markets adjust. Linked employer-employee data could



measure compliance and posting range width, testing whether weak enforcement explains the null. Analysis of job posting text could reveal how employers respond to transparency requirements. International comparisons with stronger transparency regimes could identify conditions under which effects emerge.

The central message is clear: salary transparency mandates have no detectable effect on wages or pay equity in the short run. This is an important null result that should inform policy debates about transparency as a tool for labor market intervention.

## Acknowledgements

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**Project Repository:** <https://github.com/SocialCatalystLab/auto-policy-evals>

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

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## A. Data Appendix

### A.1 Variable Definitions

**Table 7:** 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 8:** 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+
<i>Excluded from analysis (insufficient post-treatment quarters):</i>				
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](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](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](https://www.capitol.hawaii.gov/session/measure_indiv.aspx?billtype=SB&billnumber=1057&year=2023)

## B. Additional Results

### B.1 Summary Statistics

**Table 9:** 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 10:** 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 11:** 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 $\times$ 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. County-quarter-pair observations: with 131 unique counties observed over multiple quarters (varying coverage due to QWI suppression and staggered treatment timing), the border sample includes 8,568 county-quarter-pair observations. The 4,284 pair  $\times$  quarter fixed effects absorb time-varying factors common to both sides of each border.

## B.4 Cohort-Specific Treatment Effects

**Table 12:** Cohort-Specific ATTs (Callaway-Sant’Anna)

Cohort	Treatment Date	ATT	SE	Post-Qtrs	Weight
Colorado	2021Q1	0.018	0.022	12	0.31
Connecticut	2021Q4	0.005	0.028	9	0.12
Nevada	2021Q4	−0.012	0.035	9	0.08
Rhode Island	2023Q1	0.022	0.041	4	0.04
California	2023Q1	0.008	0.019	4	0.38
Washington	2023Q1	0.015	0.025	4	0.07
<b>Overall ATT</b>		<b>0.010</b>	<b>0.014</b>		1.00

*Notes:* Group-specific ATTs from Callaway-Sant’Anna estimator. Weights are proportional to cohort size (number of treated county-quarters). Standard errors clustered at state level. Colorado has the longest post-treatment exposure (12 quarters); the 2023Q1 cohort (CA, RI, WA) has only 4 quarters of post-treatment data. No individual cohort ATT is statistically significant at the 5% level. California’s large weight (0.38) reflects its size; excluding California yields  $ATT = 0.012$  ( $SE = 0.019$ ), also insignificant.

## B.5 Industry Heterogeneity: Testing the Bargaining Mechanism

To test whether transparency effects vary with bargaining intensity (Prediction P3), I estimate effects separately for high-bargaining and low-bargaining industries using QWI industry-level data. High-bargaining industries—Professional Services (NAICS 54), Finance (52), and Information (51)—feature substantial individual negotiation. Low-bargaining industries—Retail (44-45) and Accommodation/Food (72)—have more standardized wages.

**Table 13:** Industry Heterogeneity: High vs. Low Bargaining Sectors

Industry Group	ATT	SE	Significant?
<i>High Bargaining Intensity</i>			
Professional Services (NAICS 54)	0.024	0.021	No
Finance & Insurance (NAICS 52)	0.019	0.028	No
Information (NAICS 51)	0.031	0.033	No
<i>Pooled high-bargaining</i>	0.023	0.018	No
<i>Low Bargaining Intensity</i>			
Retail Trade (NAICS 44-45)	0.008	0.015	No
Accommodation & Food (NAICS 72)	−0.005	0.019	No
<i>Pooled low-bargaining</i>	0.004	0.012	No
High — Low differential	0.019	0.022	No

*Notes:* Callaway-Sant’Anna estimates using QWI county-quarter-industry data (without sex disaggregation). High-bargaining industries are those where individual salary negotiation is common; low-bargaining industries have more standardized compensation. The commitment mechanism predicts larger effects in high-bargaining sectors (where commitment constraints bind more strongly). Point estimates are slightly larger in high-bargaining sectors (+2.3% vs. +0.4%), consistent with this prediction, but the differential (1.9 pp, SE = 2.2 pp) is not statistically significant. All estimates are positive and insignificant; we cannot reject equal effects across bargaining intensity or zero effects in either sector.

The results provide weak evidence consistent with the commitment mechanism’s prediction of heterogeneity by bargaining intensity—point estimates are larger in high-bargaining sectors—but the differential is not statistically significant. This null heterogeneity finding is consistent with the overall null effect: if transparency has no aggregate impact, we would not expect strong heterogeneity patterns.