Effects of Pay Transparency Laws on the Labor Market*

Raymond He†and Suryansh Upmanyu‡

University of Texas at Austin

February 9, 2026

Abstract

To narrow the gender pay gap, many major US jurisdictions have implemented pay transparency laws that require employers to post wage offers in all vacancy postings. We use data on the near universe of job postings and representative survey data in a difference-in-difference framework to analyze cross-firm pay transparency's effects on their local labor market. We find that the law caused the fraction of postings with wage information to increase by 24.67 percentage points and increased real wages by 1.2%-1.7% on average. In Colorado, men experienced higher wage increases than women, although the difference is not significant.

Keywords: pay transparency, job postings, incomplete information
JEL classification: D83, J31, M52

*We are deeply grateful to Sandra Black, Eric Chyn, Andres Drenik, Richard Murphy, Stephen Trejo, seminar attendees at Delhi School of Economics Winter School 2024, Eastern Economic Association Annual Meeting 2025, Royal Economic Society Annual Conference 2025, European Winter Meeting of the Econometric Society 2025, 2025 STATA Texas Empirical Micoreconomics Conference, Annual Conference on Economic Growth and Development 2025, and all participants at the U.T. Austin Macroeconomics writing workshop for invaluable feedback. We also thank Olivier Coibion, Andreas Mueller, and Ayşegül Şahin for financial support. Morgan Myhill provided excellent research assistance.

†E-mail: raymond.he@austin.utexas.edu
‡E-mail: suryansh_upmanyu@utexas.edu

Pay transparency laws are considered an important tool in addressing wage inequality. By providing workers - especially those from minority backgrounds - with information about the salary levels of their peers, workers could then negotiate better wages for themselves. In the last few years, multiple countries have implemented laws mandating various form of pay transparency despite a limited understandingh of their effects and unintended consequences.

One particular form of pay transparency, which the US has recently experimented with, is cross-firm pay transparency, where employers are required to disclose wage information in all vacancy postings.[1] Given the public nature of these advertisements, the disclosed information is available to both job seekers and other firms. Providing more wage information in job postings can reduce the search costs for job seekers. Moreover, it can allow firms to observe what competing firms pay for the same job, leading to reduced wage dispersion (Cullen and Perez-Truglia, 2022).

In addition to employer-side factors such as gender discrimination, the literature on the gender pay gap has identified many other behavioural factors that also contribute to this gap. For example, (Exley and Kessler, 2022) documents that men often to overstate their productivity while women understate theirs. Furthermore, women also tend to ask for lower wages than men with comparable characteristics (Roussille, 2024) and are less willing to negotiate (Hernandez-Arenaz and Iriberri, 2019).

Proponents of cross-firm pay transparency argue that forcing employers to publicly disclose wage offers alleviates some of these aforementioned behavioral gaps. It is hoped that by at least partially closing the information friction channel, cross firm pay transparency equips women with the information needed to negotiate more actively with firms and thus reduce the gender pay gap.

However, the marginal wage information that inter-firm pay transparency laws provide is non-rivalrous and equally accessible to both men and women. Thus, in order for the pay gap to shrink, women would implicitly need to utilize this extra information more effectively than men. Moreover, if men on average use this information more aggressively than women, cross-firm pay transparency may have the unintended consequence of widening the gender pay gap. Therefore, cross-firm pay transparency's effects on the gender pay gap are ex-ante ambiguous.

In this paper, we study inter-firm pay transparency's effects on the labor market by leveraging the recent implementation of such reforms across several major states with large labor markets across the US. Using a dataset containing the near universe of online job postings in the US, we find that cross-firm pay transparency increased the share of advertisements with salary information by 24.67 percentage points in treated jurisdictions. We also create a quantifiable measure of wage transparency that is robust to the possibility of firms engaging in malicious compliance by posting very broad wage ranges, and find that the laws caused the amount of salary information in job postings to nearly double on average.

We then consider these laws' impacts on various real labor market outcomes using data from the American Community Survey (ACS). Using the heterogeneity-robust difference-in-differences estimator proposed by Sun and Abraham (2021), we find that real wages in Colorado increased significantly by 3.2%-4.4%, while real wages in California and Washington

[1] Throughout this paper, we use the terms 'cross-firm pay transparency' and 'inter-firm pay transparency in job postings' interchangeably.

increased by 0.57-1.3%. We also find only small, insignificant effects on the usual number of hours worked per week, and the probability of being employed.

Lastly, we analyze whether cross-firm pay transparency helped close the gender pay gap as part of a broader analysis on its potentially heterogeneous wage effects across different demographic groups. Strikingly, we robustly find that men's wages increased significantly in all treated states, while women's only increased significantly in Colorado. Moreover, this increase was smaller than men's, although the difference is not statistically significant. These findings are suggestive of the law being ineffective at reducing the gap at best, and even widening it at worst. However, this should not be interpreted as a causal statement.

Regarding heterogeneous effects across educational background, we find that in Colorado, the law caused workers with a bachelor's degree or higher to experience a significantly higher wage increase of 5.24% while for lower-educated workers, it was 2.04% (albeit still statistically significant). Turning our attention to age, we report that the wage effect was higher among workers over 40, although the difference is not significant.

Our paper directly contributes to the growing literature on the various forms of pay transparency and their effects across many countries, such as in Austria (Böheim and Gust, 2021; Gulyas et al., 2023), Canada (Baker et al., 2023), Denmark (Bennedsen et al., 2022), Germany (Ahrens and Scheele, 2022; Brütt and Yuan, 2023; Seitz and Sinha, 2022), the UK (Blundell et al., 2025), and the US (Mas, 2017; Burn and Kettler, 2019; Obloj and Zenger, 2022; Sinha, 2022; Feng, 2024; Arnold et al., 2025). While these papers study horizontal (or within-firm) pay transparency, we instead analyze cross-firm pay transparency, which is relatively new and thus less studied.

The most closely related papers to ours are Arnold et al. (2025) and Feng (2024), who focus on the effects of the Equal Pay For Equal Work Act passed in January 2021 in Colorado. We extend their analysis in two important ways. Firstly, we extend our analysis to consider California and Washington state, two major jurisdictions with significantly larger labor markets than Colorado. This gives us a more holistic understanding of pay transparency's effects and new time periods, which addresses potential external validity concerns about the uniqueness of Colorado in 2021. Secondly, we also directly examine cross-firm pay transparency's effects on the gender pay gap as part of a broader analysis on heterogeneous effects across demographic groups. Our empirical findings differing across treatment groups reveals important heterogeneity in how cross-firm pay transparency affect labor markets.

The rest of the paper is organized as follows. First, we describe the institutional setting of the pay transparency laws in section I. We then describe our data and empirical strategy in section II and section III, respectively. Our empirical results are presented and discussed in section IV. We conclude in section V.

I Institutional Setting

In January 2021, Colorado amended its Equal Pay for Equal Work Act to require that all firms must publicly disclose wage information in job postings. To incentivize compliance, firms are free to post 'good-faith' salary ranges with no restrictions on its width. Non-compliance is punishable by fines, but they are rarely enforced in practice.

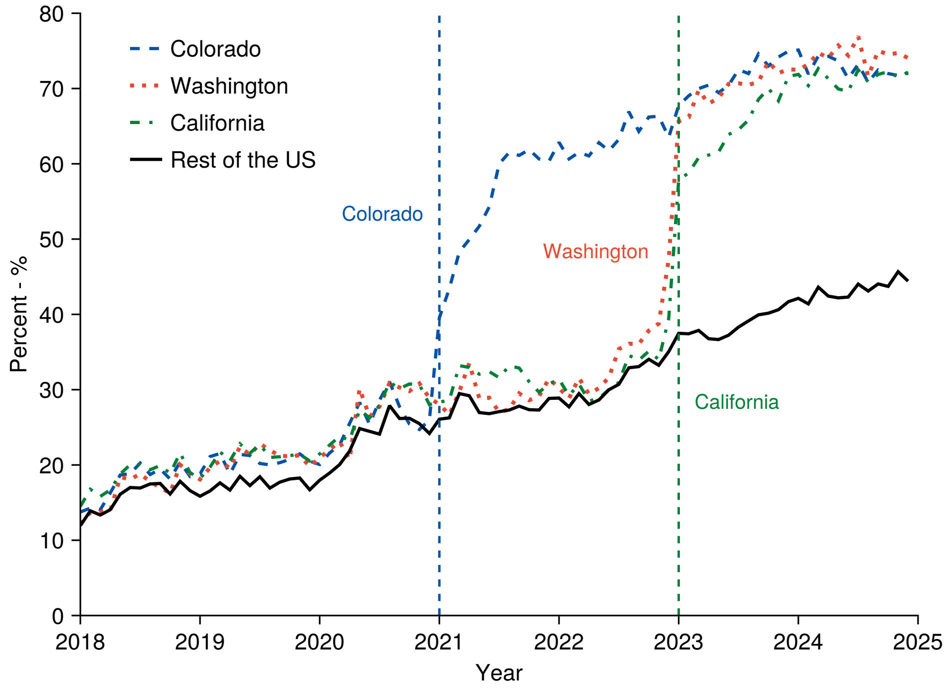


Figure 1: Share of All Online Vacancy Postings With Any Wage Information

Notes: This figure is created using Lightcast data on job postings aggregated up to the monthly frequency. The vertical lines denote the time of implementation of pay transparency laws in a given state. The rest of the US omits all jurisdictions that have implemented cross-firm pay transparency.

After Colorado, several jurisdictions with large labor markets also enforced cross-firm pay transparency. Specifically, New York City and Westchester county implemented their law in November 2022, while California and Washington's laws came into effect in January 2023.[2] These laws are practically identical; they only differ in the severity of penalties for non-compliance and the firm size thresholds for which they apply. Thus, we consider them all to be the same cross-firm pay transparency law in this paper.[3]

To provide some background context on the prevalence of wage information across the US before any transparency reforms, we plot monthly time series of the share of all online vacancy postings with any wage information across the US in Figure 1. Given how only approximately 20% of all online postings contained any wage information before any such laws were enacted, there was significant scope for increasing the amount of wage information that job applicants have when searching for jobs and negotiating wages.

Indeed, we observe an immediate and pronounced increase in this share after the law came into effect in each jurisdiction, indicating compliance with the law. After the initial jump, these shares grew gradually until converging to approximately 70%.

[2]Hawaii, Washington D.C., Maryland, and Illinois also enacted similar laws in January 2024, June 2024, October 2024, and January 2025, respectively. However, we do not have enough post-treatment data to analyze their effects.

[3]We provide a timeline of when each jurisdiction implemented their pay transparency law and details on each jurisdiction's specific eligibility criteria for employers under these laws in Online Appendix Section B.

II Data

II.A Lightcast

We use online job postings data from Lightcast (formerly Burning Glass Technologies) to analyze the effect of pay transparency laws on the prevalence of public wage information in the treated jurisdictions.

Lightcast is a private business analytics company that collects data on online job postings from over 160,000 unique sources from across the world each day. Machine-learning algorithms are then used on each posting to extract and infer several important details about each job posting such as its firm, location, occupation and industry classification, job requirements, and pay frequency. Most importantly for our purposes, we observe each posting's advertised wage (if one is posted at all). If a posting advertises a wage range offer instead of a point wage, we observe both its upper and lower bound.

Our dataset spans from 2018 until 2024,[4] giving us approximately 207 million unique online job postings to analyze after cleaning.[5] Importantly, our sample period enables us to study how cross-firm pay transparency laws affected firms' wage posting behavior not just in Colorado, but also in California and Washington state. This provides a more holistic understanding of these laws' effects on local labor markets.

II.B ACS

Additionally, we use survey data from the ACS between 2016 and 2024 to assess whether pay transparency laws have yielded any changes to real labor market outcomes.

Every year, the ACS collects extensive demographic information from a representative sample of approximately 3.5 million respondents across the entire US. In addition to the standard data on demographic controls (gender, marital status, education, race, and age), we also use data on respondents' labor market outcomes (wages, employment status, and hours worked), their reported state and county of work, and their occupation and industry (at the SOC-3 and NAICS-3 level, respectively).[6]

As is standard in the literature, we limit our analysis to individuals between 18 and 64 years of age, and remove those who are self-employed or report working in the military and unclassified occupations.

III Empirical Strategy

We use a difference-in-differences specification to estimate the average treatment effect of pay transparency laws on various labor market outcomes. Recent research on the difference-in-differences methodology finds that standard Two-Way Fixed Effects (TWFE) estimators may

[4] We do not use Lightcast's data prior to 2018 in our analysis because Lightcast implemented major changes to their web-scraping algorithms in 2018. We discuss this in Online Appendix Section A.2.

[5] We detail our data cleaning procedure in Online Appendix Section A.

[6] We use the following crosswalks from the Integrated Public Use Microdata Series to keep all NAICS and SOC codes consistent across the years in our sample: https://usa.ipums.org/usa/volii/occsoc18.shtml and https://usa.ipums.org/usa/volii/indnaics.shtml.

give biased estimates of the average treatment effect on the treated (ATT) in settings where the treatment was staggered across time (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). We address these concerns by using a stacked difference-in-differences estimator developed in Sun and Abraham (2021), which can give us unbiased estimates of the ATT.

To use the Sun and Abraham (2021) estimator, we first define E_s to denote groups of all US states that were treated at the same time, and C as our control group. Our baseline regression specification is given as follows:

$$y_{ist} = \lambda_s + \theta_t + \mathbf{\Gamma}\mathbf{X}_{ist} + \sum_{i \notin C} \sum_{\tau \neq -1} \delta_{E,\tau} \mathbb{I}(i \in E_s) \cdot \mathbb{I}(t - t_s = \tau) + \nu_{ist}, \tag{1}$$

where y_{ist} refers to the outcome variable, λ_s denotes a set of state fixed effects, and θ_t denotes time fixed effects. We also allow for occupation (SOC3 level) and industry (NAICS3 level) controls, denoted by $\mathbf{X}_{ist}$. $\mathbb{I}(i \in E_s)$ is an indicator for whether the sample observation is in a treated state in group E_S, and $\mathbb{I}(t - t_s = \tau)$ is an indicator variable for the time since treatment date t_s. Our object of interest is $\delta_{E,\tau}$, which gives us the ATT of pay transparency laws on the relevant outcomes.

Our identifying assumption is that in the absence of cross-firm pay transparency's implementation, the evolution of a given outcome variable of interest in the treated group would have been mirrored by the control group. Thus, in our baseline specification, our control group only consists of states that have similar labor laws to our treated states. Specifically, we use states that have passed Right of Workers to Talk laws which were studied in Cullen and Pakzad-Hurson (2023).[7] Intuitively, all the states in our analysis provide workers with the same level of wage bargaining power in the pre-treatment period. Therefore, any post-treatment differences in labor market outcomes between our treatment and control group can be attributed to the implementation of inter-firm pay transparency.

Our treatment group consists of US states that implemented inter-firm pay transparency (Colorado on January 1, 2021; California and Washington on January 1, 2023).[8]

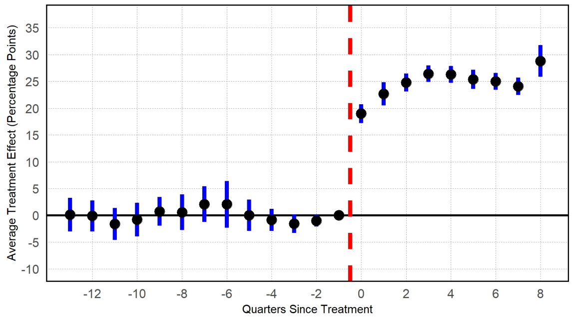
IV Results

We first analyze how pay transparency laws have affected firms' vacancy postings behavior. Specifically, we examine whether cross-firm pay transparency caused the amount of wage information advertised in job postings to increase, and whether the posted wages themselves changed. We then assess whether the law also caused changes to individuals' real labor market outcomes; namely their real wages, hours worked, and employment status. Lastly, we explore whether there were heterogenous wage effects across key demographics of interest such as gender, educational attainment, and age.

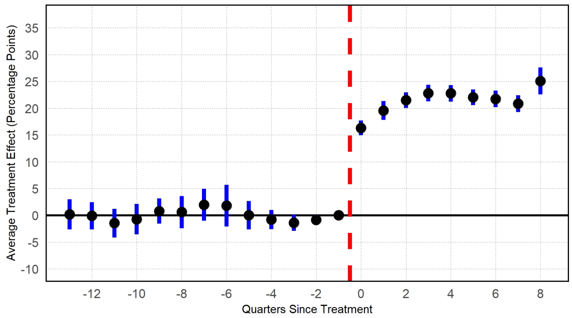
[7]Our control group consists of Connecticut, Delaware, Illinois, Maine, Massachusetts, Michigan, Minnesota, Nebraska, Nevada, New Hampshire, Oregon, and Vermont. Our results are also robust to other criteria for defining our control group (refer to Table 1 in Cullen (2024)).

[8]We omit Hawaii, Illinois, Maryland, and Washington D.C. from our analysis due to insufficient post-treatment data. We also exclude New Jersey and New York State from our analysis since we were unable to find an appropriate control group these jurisdictions.

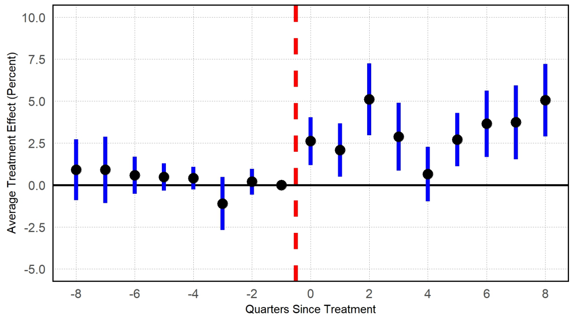
IV.A Effects on Job Postings



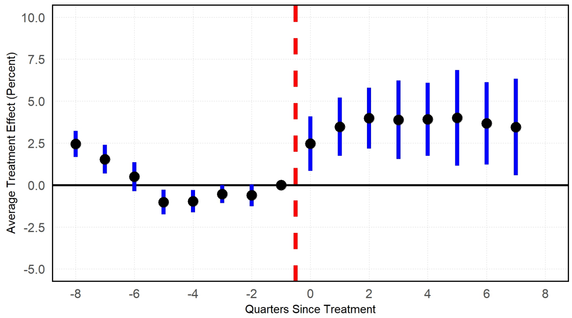
(a) Share of posts with wage information: Overall



(b) Transparency index: Overall



(c) Log(Posted Wages): Colorado



(d) Log(Posted Wages): California & Washington

Figure 2: Event Studies for First-Stage Outcomes

Notes: We use Lightcast data on job postings (2018-2024). For panels (a) and (b), the data is aggregated up to the state-SOC3-NAICS3-quarter level. For panels (c) and (d), the data is aggregated up to the state-SOC3-firm-quarter level. We use the number of observations within a group as weights. In the event studies, $t = 8$ ($t = -13$) actually represents the binned time period $t \geq 8$ ($t \leq -13$). The dashed red line denotes the time of implementation of pay transparency laws. The blue lines represent 95% confidence intervals. All standard errors are clustered at the state level.

In order to estimate the laws' effect on wage information, we first estimate (1) where y_{ist} is the share of postings with any wage information in SOC3-NAICS3 group i in state s at quarter t. Our estimates of $\delta_{E,\tau}$ are presented in Figure 2a. Given the parallel pre-trends and the the significant post-treatment increase, we can conclude that pay transparency laws significantly increased the share of job postings with wage information in treated states.

Aggregating across all post-treatment periods, our estimate of the ATT is 24.67 percentage points (SE = 0.68) across all three treated states. In Colorado, our ATT estimate is 29.48 percentage points (SE = 0.93), while in California and Washington together, it is 22.50 percentage points (SE = 0.81).[9] This means that relative to the pre-treatment share (19.85%), cross-firm pay transparency has more than doubled the share of job postings with wage information in treated states.

However, only considering the share of advertisements with any wage information may be misleading as posted wage offers can come in the form of either a point or a range. Since

[9] We show our ATT estimates for all first-stage outcomes in Table 4 of Online Appendix Section C.

none of the laws place any explicit restriction on the range's width, firms may comply with the law by advertising ranges so wide that they are still uninformative. Indeed, we find that after the laws' enactment, there is a significant decrease in the share of point wage offers among all postings with wage information.[10] Thus, a high compliance rate with the law does not necessarily mean that workers are receiving more precise wage information.

We formally account for this possibility by constructing a transparency index as per (2). As an advertisement's wage range widens (and becomes less precise), the index's value approaches 0, while point offers are nested as an index value of 1. The main advantage of this index is that it allows us to quantify wage transparency in a unified manner across all job postings, irrespective of whether job postings have a point offer, a range offer of any width, or no information at all.

$$\text{Index} = \begin{cases} 0, & \text{if no wage offered} \\ 1 - \frac{\text{upper bound - midpoint}}{\text{midpoint}} \in (0,1], & \text{if wage offered} \end{cases} \tag{2}$$

Figure 2b plots our estimates of $\delta_{E,\tau}$ from estimating (1) with the the median transparency index value within SOC3-NAICS3 group i in state s at quarter t as the outcome variable. Prior to the laws' implementation, the treated and control groups show similar trends in their transparency index, thereby satisfying the parallel pre-trends assumption. After the laws' implementation, we document that the index also jumps sharply. Our ATT estimate across all treated states is 21.36 percentage points (SE = 0.58), while for Colorado, and California and Washington together, the ATT estimates are 25.81 percentage points (SE = 0.80) and 19.35 percentage points (SE = 0.72), respectively.

Thus, our initial finding that the law significantly increased the amount of wage information publicly available in treated states is robust to the possibility of firms engaging in malicious compliance.

Lastly, we examine whether the law caused firms to change their advertised wage offers by re-estimating (1) with the natural logarithm of average annual posted wages within a firm-by-SOC3-by-state group as the outcome variable.[11]

However, in order to obtain an unbiased and causal estimate of the laws' effect on firms' posted wages, we need to make significant sample restrictions. Our primary concern with using the full sample is endogeneity arising from non-random selection into posting wages. Since complying with the laws mechanically increase the number of firms posting wages, any change in average posted wages could reflect composition effects rather than actual changes in wage offers. For instance, if firms that only started advertising wages due to the law tended to offer higher salaries, then average posted wages would rise even without any firm changing its wage offers.

Thus, we only consider jobs (defined by firm by SOCC3 occupation combinations) that advertised wage information at least once in a two-year window both before and after the law's implementation, and in both treated and untreated states. For example, if firm A adver-

[10] However, conditional on posting a range, we do not find that the median range width within SOC3-NAICS3 groups widened significantly. See Online Appendix Section C for details.

[11] If an observation advertised a wage range, we use its midpoint.

tised wage offers in its vacancy postings for occupation B in both Colorado (or California and Washington) and an untreated state, both two years before and two years after the law was implemented, then all of firm A's job postings for occupation B containing wage information will be in our sample.[12]

Figure 2c and Figure 2d plot our estimates of $\delta_{E,\tau}$ for Colorado and California and Washington, respectively. After the laws' implementation, we find a that posted wages increase by 3.81% (SE = 0.96) in Colorado and by 3.59% (SE = 1.09) in California and Washington.[13]

IV.B Effects on Real Labor Market Outcomes

We now analyze how pay transparency affected individuals' real wages, hours worked, and employment status in Colorado, California, and Washington. We report our ATT estimates for these outcomes in Table 1, and the corresponding event-study graphs in Figure 3. In the first column of Table 1, we estimate (1) on all employed respondents in the ACS without any covariates. In column 2, we add demographic controls. Columns 3 and 4, we only consider prime-age individuals and private sector workers, respectively. All specifications include fixed effects for state, year, occupation (SOC3), and industry (NAICS3).

Our estimates of pay transparency's effect on real wages across different specifications are shown in Table 1a. There are two particularly noteworthy aspects of these results.

Firstly, we estimate an overall positive wage increase of 1.20% to 1.74%, which is in stark contrast to the often negative or insignificant wage effects found by the previous literature on pay transparency. A potential explanation for this is that previous research has focused on intra-firm pay transparency laws, where workers only obtain information on their coworkers' wages within the same firm. The pay transparency law we analyze is inter-firm, where a firm's wage offer information is available to workers both within and outside the firm, and also to the firm's competitors in the labor market.

When firms are legally forced to publicly disclose wage offers, workers can easily compare wage offers from competing firms and direct their job search towards employers offering higher wages. This results in a form of Bertrand competition among employers, as they can only attract workers now if they offer at least as much as their competitors. We find evidence for this in Figure 2, where posted wage offers increase when firms are required to reveal wage information. Additional support for this mechanism is found in Cullen et al. (2025), who find that employers ascribe importance to information about competing employers' wage offers, so that they can modify their own wage offers accordingly.

Secondly, we find that Colorado experienced significantly larger real wage increases of

[12] We make two further modifications from our baseline specification to facilitate this sample restriction. Firstly, we redefine our control group to include all untreated states since imposing our eligibility criteria for posted wages results in our sample size decreasing by over 75%. Using the same control group as our baseline specification would require a firm-SOC3 to be have posted wages in a much narrow set of control states. Secondly, to keep our sample consistent with our eligiblity criteria, we limit our sample to eight quarters before and after a treated state enacted its pay transparency law.

[13] We do not study effects for all treated states together. On top of the aforementioned sample restrictions, it would further require the firm-SOC3 job to have posted wages in all three treated states and in the control group. Only the largest of firm-SOC3 groups would remain in the sample.

(a) Log(Wages Earned In The Last 12 Months)

Sample	Employed (All)	Employed (All)	Employed (Prime Age)	Employed (Private Sector)
Overall	1.469***	1.200***	1.741***	1.231***
	(0.256)	(0.203)	(0.305)	(0.247)
Colorado	4.373***	3.532***	3.427***	3.166***
	(0.396)	(0.381)	(0.481)	(0.393)
California +	0.680**	0.566**	1.283***	0.703**
Washington	(0.313)	(0.260)	(0.381)	(0.326)
N	3,522,674	3,522,674	2,391,580	2,948,979
Controls	No	Yes	Yes	Yes

(b) Log(Usual Hours Worked Per Week)

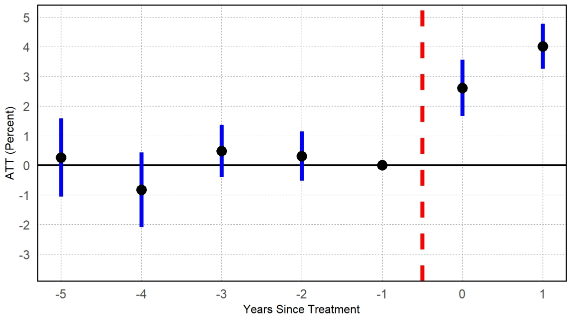
Sample	Employed (All)	Employed (All)	Employed (Prime Age)	Employed (Private Sector)
Overall	−0.073	−0.122	−0.047	−0.163
	(0.122)	(0.129)	(0.098)	(0.146)
Colorado	0.507***	0.306*	0.044	0.168
	(0.160)	(0.152)	(0.154)	(0.180)
California +	−0.231	−0.238	−0.071	−0.253
Washington	(0.151)	(0.160)	(0.132)	(0.181)
N	3,522,674	3,522,674	2,391,580	2,948,979
Controls	No	Yes	Yes	Yes

(c) Employment - Population Ratio (1 If Employed, 0 Otherwise)

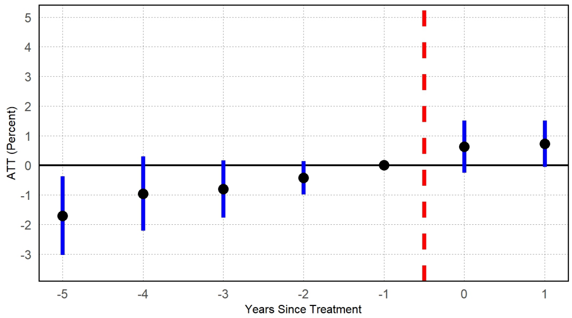
Sample	Respondents (All)	Respondents (All)	Respondents (Prime Age)	Respondents (Private Sector)
Overall	−0.091	−0.094	0.051	−0.268
	(0.119)	(0.129)	(0.153)	(0.180)
Colorado	0.317	−0.067	0.839**	−0.202
	(0.226)	(0.251)	(0.310)	(0.257)
California +	−0.195	−0.100	−0.152	−0.284
Washington	(0.116)	(0.129)	(0.144)	(0.189)
N	4,958,586	4,958,586	3,070,632	4,958,586
Controls	No	Yes	Yes	Yes

Notes: We use ACS data at the individual level. All values in Panels 1 and 2 are in percentage terms. All values in Panel 3 are in percentage points. All ATT estimates are obtained via the Sun and Abraham (2021) estimator. Prime age individuals are those between the age of 25 and 54 years. For wages and hours, the private sector estimates are obtained after limiting the focus only to individuals employed in the private sector. For estimating the effect on private sector employment, we redefine the outcome variable as 1 if employed in the private sector, and 0 otherwise (employed in the public sector or non-employed). Demographic controls include gender, race, education, marital status, and age (quadratic) variables. All standard errors (in parentheses) are clustered at the state level.

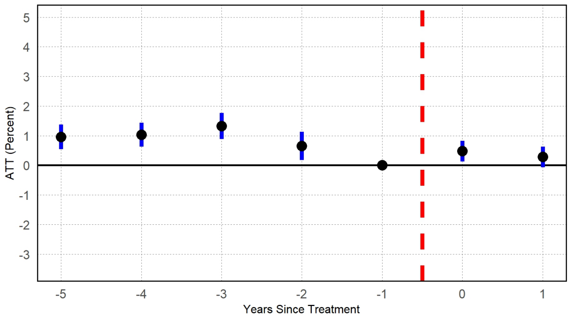
Table 1: ATT Estimates For Real Labor Market Outcomes



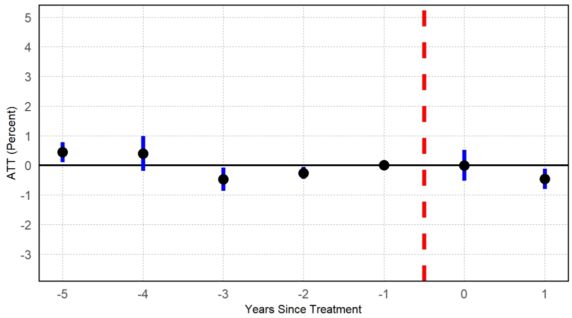
(a) Wages: Colorado



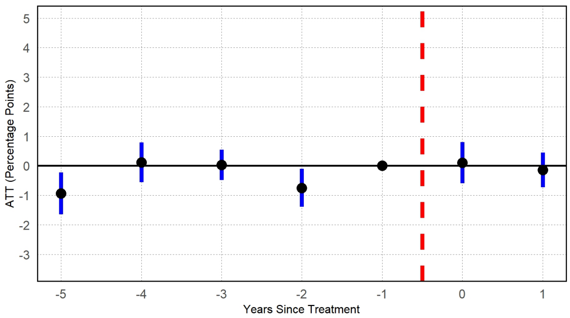
(b) Wages: California & Washington



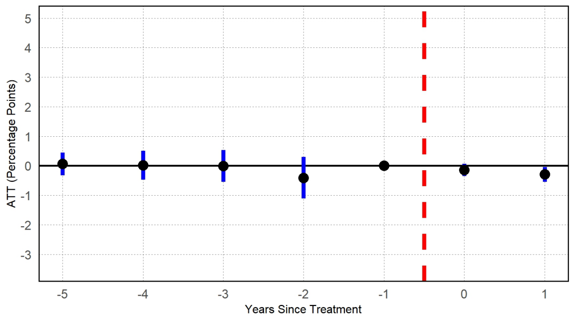
(c) Hours worked: Colorado



(d) Hours worked: California & Washington



(e) Employment: Colorado



(f) Employment: California & Washington

Figure 3: Event Studies for Second-Stage Outcomes

Notes: We use ACS data at the individual level. All ATT estimates are obtained via the Sun and Abraham (2021) estimator using the following demographic controls: gender, race, education, marital status, and age (quadratic). In the event studies, $t = 1$ actually represents the binned time period $t \geq 1$. The dashed red line denotes the time of implementation of pay transparency laws. The blue lines represent 95% confidence intervals. All standard errors are clustered at the state level.

3.17% to 4.37%, while the increase in California and Washington was only 0.68% to 1.28%, albeit still statistically significant.

Formally understanding the cause behind this discrepancy in magnitude is outside the scope of this paper. Nonetheless, we can rule out some potential explanations, which we fully discuss in Online Appendix Section F. For example, composition effects are unlikely to be the main driver, since we find that the industry and occupation composition across Colorado is similar to that of California and Washington. We can also discard the possibility of heterogeneous first-stage effects being the main cause, since both treated group experienced quantitatively similar first-stage effects in all first-stage outcomes.

We hypothesize that differences in labor market conditions at the time of implementation may play an important role in explaining the differing wage effects across treated groups. When Colorado implemented its pay transparency law in January 2021, the US labor market was beginning to recover from the covid-19 pandemic. In the following months, the US labor market was extremely tight, with real wages rising, and many workers finding new job offers.[14] Workers in Colorado may have benefitted even more from a tight labor market as they utilized the additional wage information in job postings to obtain better wage offers.

In contrast, when California and Washington implemented their pay transparency laws in January 2023, the labor market had begun cooling and has remained slack throughout 2024, the last year of our ACS data. Since employers in 2023 were not hiring as much as they were in 2021-2022,[15] workers in California and Washington may have been less able to utilize the additional wage information in job postings to obtain better wage offers for themselves.

We finalize our analysis by assessing inter-firm pay transparency's effect on workers' hours worked and employment status. Our ATT estimates on hours worked and employment status are shown in Table 1b and Table 1c, respectively. Regarding hours worked, we do not find consistent and significant effects. While we consistently find positive effects in Colorado across specifications, they are not statistically robust. Moreover, we find negative, albeit similarly noisy, effects in California and Washington.

For employment status, we estimate (1) where the outcome variable y_{ist} is the indicator variable for whether individual i in state s at time t is employed. Table 1c shows that the ATT on employment is often insigifcant and highly variable across specifications.[16]

IV.C Heterogeneity Analysis

Since cross-firm pay transparency laws explicitly aims to reduce wage inequality, especially across gender, it is important to understand whether their wage effects were heterogeneous across different demographics groups. We test for this by estimating (1) separately for the different demographic groups and report our ATT estimates in Table 2.

[14] According to the Jobs Openings and Labor Turnover Survey (JOLTS), the average number of job openings in a month throughout 2019-2020 was 6,751. This started increasing in 2021 and peaked in March 2022 at 12,134 openings, which represents an 79.7% increase.

[15] Similarly, throughout the 2024-25 period, the monthly average number of job openings was 7,637, which is a 37.1% decrease from the peak.

[16] For the analysis on employment, we exclude occupation and industry fixed effects from the regression specification in Table 1c since this information is unavailable for non-employed individuals.

	Gender		Education		Age	
Sample	Female	Male	BA Or Greater	Less Than BA	≤ 40 Years	> 40 Years
Overall	0.518	1.829***	1.924***	0.478	0.897*	1.513***
	(0.377)	(0.325)	(0.354)	(0.318)	(0.451)	(0.298)
Colorado	3.061***	3.847***	5.238***	2.038***	3.163***	3.912***
	(0.490)	(0.359)	(0.496)	(0.498)	(0.529)	(0.310)
California &	−0.169	1.279***	0.889**	0.100	0.259	0.891**
Washington	(0.490)	(0.369)	(0.400)	(0.417)	(0.613)	(0.386)
N	1,716,090	1,806,583	1,472,699	2,049,974	1,739,401	1,783,272

Notes: We use ACS data at the individual level. All values are in percentage terms. All cells contain ATT estimates on wages obtained by using the Sun and Abraham (2021) estimator with demographic covariates. Row 1 contains ATT estimates when all treated groups are included. Rows 2 and 3 give ATT estimates for Colorado, and California and Washington together, respectively. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. Gender is excluded when the ATT is estimated for males and females. Education is excluded when the ATT is estimated for BA or greater, and Less than BA subgroups. All standard errors (in parentheses) are clustered at the state level.

Table 2: ATT Estimates For Wage Effects Across Different Demographic Groups

From Columns 1 and 2 of Table 2, we consistently find that cross-firm pay transparency significantly increased men's wages, where the effect is strongest in Colorado again. In contrast, we find that women's wages only increased significantly in Colorado, although this increase was smaller in magnitude than men's. Moreover, we actually find negative effects for women in California and Washington, although this estimate is very noisy. Considered all together, these results would be consistent with the gender pay gap widening to women's detriment. However, it is very important to note that our results are only suggestive and to not interpret them as causal.[17]

We can make stronger claims when we consider differences across educational background and age. As per Columns 3 Table 2, we consisistently find that college-educated workers enjoyed significant wage increases due to pay transparency.[18] For non-college educated workers, we only find statistically significant wages increases in Colorado, where such workers experienced a 2.1% real wage increase as opposed to 5.2%. Indeed, this finding is consistent with our finding that before the laws were implemented, occupations with high education requirements were unlikely to publicly advertise wage offers whereas occupations with low education requirements were already posting wage offers despite not being required to. Thus, this difference in magnitude can be attributed to differences in first-stage effects; the law had more room to increase wage transparency among occupations requiring a college degree.

Lastly, we report the wage effects for workers above and below 40 years of age in Columns 5 and 6 of Table 2 and robustly find that workers over the age of 40 experienced wage increases that were larger in magnitude than those experienced by younger workers, although this difference was not statistically significant. We again argue that differences in first-stage

[17] We find similar patterns using a triple difference-in-differences specification in Online Appendix Section E.2.

[18] We present our event studies for this analysis in Online Appendix Section D.4, which show that the parallel pretrends assumption is visually satisfied.

effects can explain this finding. In addition to education, we further find that there was a highly significant negative correlation between the occupation's required years of experience and its likelihood of posting wage offers before the laws were implemented. Thus, it is likely that older workers were more likely to be in occupations that did not post wage offers before the laws were implemented, and thus had more room to benefit from the increased wage transparency.

Overall, a striking observation from the results in Table 2 is that no group on average experienced a significant, negative effect on wages due to the pay transparency laws. This is consistent with the findings of Cortés et al. (2024), which in a randomized experiment setting, found that providing information to respondents did not lead to negative outcomes in any way.

V Conclusion

In this paper, we study the effects of cross-firm pay transparency laws on local labor markets and assessed whether they achieved their policy goals of increasing wages and reducing wage inequality across genders.

We find that these laws were successful in significantly increasing the amount of salary information available in job postings and the very wages that firms posted. This consequently also led to an overall increase in realized wages for workers, albeit significantly more in Colorado than in California and Washington. However, we do not find evidence that it successfully reduced wage inequality across genders. While we robustly find that mens' wages increased due to the law, we can only document a significant, positive effect for women in Colorado. Unfortunately, our data does not allow us to fully explain why cross-firm pay transparency laws have not reduced the gender pay gap. Nevertheless, we highlight some potential explanations.

Firstly, the marginal wage information from cross-firm pay transparency is equally accessible to both men and women. Secondly, as Biasi and Sarsons (2022) and Roussille (2024) find, women may demand lower wages than men with comparable abilities. Since the majority of the wage information in job postings is in the form of wage ranges rather than point offers, it may be that female workers on average ask for wages at the lower or middle end of the wage range, while male workers ask for wages at the upper end of the range. We leave this as an area for future research.

References

Ahrens, Petra and Alexandra Scheele, "Game-Changers for Gender Equality in Germany's Labour Market? Corporate Board Quotas, Pay Transparency and Temporary Part-Time Work," *German Politics*, January 2022, *31* (1), 157–176.

Arnold, David, Simon Quach, and Bledi Taska, "The Impact of Pay Transparency in Job Postings on the Labor Market," November 2025.

Baker, Michael, Yosh Halberstam, Kory Kroft, Alexandre Mas, and Derek Messacar, "Pay Transparency and the Gender Gap," *American Economic Journal: Applied Economics*, April 2023, *15* (2), 157–183.

Bennedsen, Morten, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon, "Do Firms Respond to Gender Pay Gap Transparency?," *The Journal of Finance*, 2022, *77* (4), 2051–2091.

Biasi, Barbara and Heather Sarsons, "Flexible Wages, Bargaining, and the Gender Gap*," *The Quarterly Journal of Economics*, February 2022, *137* (1), 215–266.

Blundell, Jack, Emma Duchini, Ştefania Simion, and Arthur Turrell, "Pay Transparency and Gender Equality," *American Economic Journal: Economic Policy*, May 2025, *17* (2), 418–445.

Böheim, René and Sarah Gust, "The Austrian Pay Transparency Law and the Gender Wage Gap," Working Paper 14206, IZA Discussion Papers 2021.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting Event-Study Designs: Robust and Efficient Estimation," *The Review of Economic Studies*, November 2024, *91* (6), 3253–3285.

Brütt, Katharina and Huaiping Yuan, "Pitfalls of Pay Transparency: Evidence from the Lab and the Field," November 2023.

Burn, Ian and Kyle Kettler, "The More You Know, the Better You're Paid? Evidence from Pay Secrecy Bans for Managers," *Labour Economics*, August 2019, *59*, 92–109.

Callaway, Brantly and Pedro H. C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, December 2021, *225* (2), 200–230.

Cortés, Patricia, Jacob French, Jessica Pan, and Basit Zafar, "Gender Differences in Negotiations and Labor Market Outcomes: Evidence from an Information Intervention with College Students," February 2024.

Cullen, Zoë, "Is Pay Transparency Good?," *Journal of Economic Perspectives*, February 2024, *38* (1), 153–180.

— **and Ricardo Perez-Truglia**, "How Much Does Your Boss Make? The Effects of Salary Comparisons," *Journal of Political Economy*, March 2022.

Cullen, Zoë B. and Bobak Pakzad-Hurson, "Equilibrium Effects of Pay Transparency," *Econometrica*, 2023, *91* (3), 765–802.

Cullen, Zoë, Shengwu Li, and Ricardo Perez-Truglia, "What's My Employee Worth? The Effects of Salary Benchmarking," *The Review of Economic Studies*, October 2025, p. rdaf083.

de Chaisemartin, Clément and Xavier D'Haultfœuille, "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, September 2020, *110* (9), 2964–2996.

Exley, Christine L and Judd B Kessler, "The Gender Gap in Self-Promotion*," *The Quarterly Journal of Economics*, August 2022, *137* (3), 1345–1381.

Feng, Kexin, "Pay Transparency and Gender Differences in Job Search," October 2024.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, December 2021, *225* (2), 254–277.

Gulyas, Andreas, Sebastian Seitz, and Sourav Sinha, "Does Pay Transparency Affect the Gender Wage Gap? Evidence from Austria," *American Economic Journal: Economic Policy*, May 2023, *15* (2), 236–255.

Hernandez-Arenaz, Iñigo and Nagore Iriberri, "A Review of Gender Differences in Negotiation," in "Oxford Research Encyclopedia of Economics and Finance" December 2019.

Mas, Alexandre, "Does Transparency Lead to Pay Compression?," *Journal of Political Economy*, October 2017.

Obloj, Tomasz and Todd Zenger, "The Influence of Pay Transparency on (Gender) Inequity, Inequality and the Performance Basis of Pay," *Nature Human Behaviour*, May 2022, *6* (5), 646–655.

Roussille, Nina, "The Role of the Ask Gap in Gender Pay Inequality," *The Quarterly Journal of Economics*, August 2024, *139* (3), 1557–1610.

Seitz, Sebastian and Sourav Sinha, "Pay Transparency, Workplace Norms, and Gender Pay Gap: Early Evidence from Germany," February 2022.

Sinha, Sourav, "Salary History Bans: Strategic Disclosure by Job Applicants and the Gender Pay Gap," January 2022.

Sun, Liyang and Sarah Abraham, "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects," *Journal of Econometrics*, December 2021, *225* (2), 175–199.