

How Much Can I Make? Cross-Firm Pay Transparency's Effects On the US Labor Market*

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Abstract

Many major US jurisdictions have implemented pay transparency laws that require firms to advertise wage offers in vacancy postings. Using data on the near universe of job postings and representative survey data in a difference-in-differences framework, we find that these laws increased the fraction of postings with wage information by 24.7 percentage points. This translated to average real wage increases of 3.2%-4.4% in Colorado and 0.6% - 1.3% in California and Washington. We further consistently find significant positive effects on real wages for workers who are male, have a college-degree, or are over forty years old.

Keywords: pay transparency, job postings, incomplete information

JEL classification: D83, J31, M52

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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 understanding 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 firms are required to publicly disclose a wage offer in their vacancy advertisements.¹

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 24.7 percentage points in treated states. We also create a quantifiable measure of wage transparency that is robust to the possibility of firms engaging in the practice of 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 increased by 0.6-1.3%. We also find only small, insignificant effects on 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. If men receive higher wages on average than women because they have stronger networks in the labor market resulting in better information about wage offers, then pay transparency laws should level the playing field by improving the access of wage information for women. Strikingly, we find that only men's wages consistently 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.

We also find that the laws' first stage effects were higher among occupations and industries with higher skill requirements, such as years of experience and education. Thus, we may also expect to these uncover heterogeneous wage effects across educational background and age. We find that in Colorado, the law caused workers with a bachelor's degree or higher to experience a significantly higher wage increase of 5.2% while for lower-educated workers, it was 2.0% (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](#);

¹We use the terms 'cross-firm pay transparency' and 'inter-firm pay transparency in job postings' interchangeably throughout the paper.

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 states 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 laws’ institutional setting 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 firms publicly disclose wage information in job postings. This wage information signals a firm’s willingness to pay to hire applicants for a particular position. To incentivize compliance, firms are given the flexibility to post salary ranges with no restrictions on their width. Non-compliance (due to omission of wage information) is punishable by fines.

After Colorado, several jurisdictions with large labor markets also enacted cross-firm pay transparency. These laws are practically identical; they only differed 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.² In this paper, we focus our analysis on Colorado, California, and Washington state. We omit other states that also enacted cross-firm pay transparency laws from our analysis due to insufficient post-treatment data on real labor outcomes.

To provide 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 1.³ Given how only approximately 20% of all online postings contained any wage information before any such laws were enacted,

²We 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.

³Hershbein and Kahn (2018) show that while online job postings are broadly representative of labor demand in the US labor market, they tend to over-represent higher-skilled occupations.

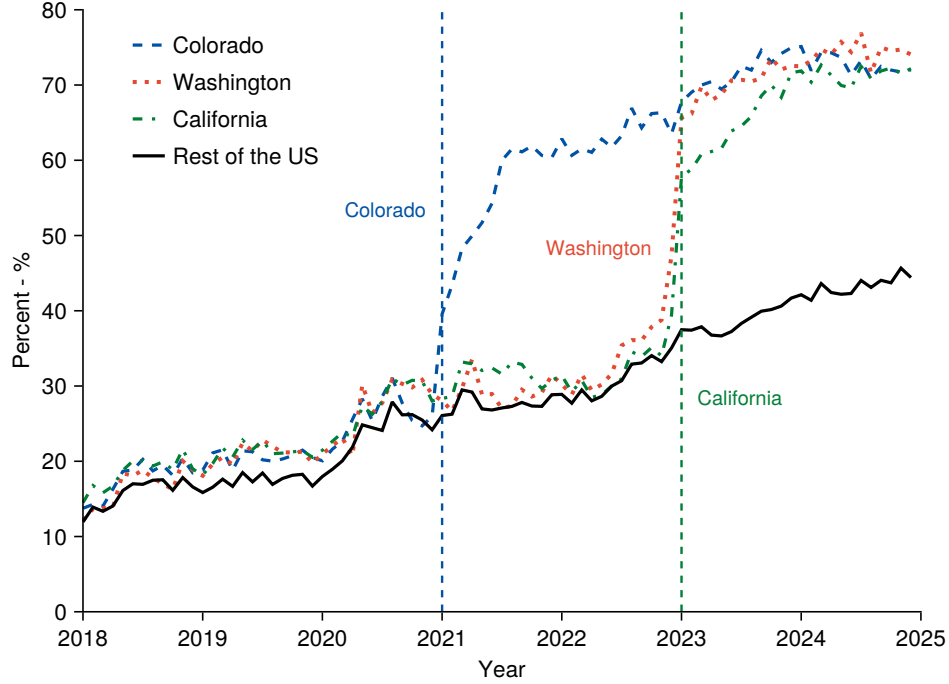


Figure 1: Share of All Online Vacancy Postings With Any Wage Information

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.

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 state, indicating compliance with the law. After the initial jump, these shares grew gradually until converging to approximately 70%.

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. They use machine-learning algorithms 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, and employment status), 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 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 by Sun and Abraham (2021), which yields an unbiased estimate 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:

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

where y_{ist} refers to the outcome variable, λ_s denotes a set of state fixed effects, and θ_t denotes time fixed effects. $\mathbf{X}_{ist}$ includes occupation (SOC3 level) and industry (NAICS3 level)

⁴We 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.

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

⁶We 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>.

controls, along with demographic controls for gender, education, marital status, race, and age (quadratic). $\mathbb{I}(i \in E_s)$ 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 . Therefore, Equation 1 can be understood as a linear two-way fixed effects specification that interacts relative period indicators with cohort indicators. This overcomes the “forbidden comparisons” issue associated with the standard TWFE specification documented by Goodman-Bacon (2021). 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). 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. Overall, our control group consists of Connecticut, Delaware, Illinois, Maine, Massachusetts, Michigan, Minnesota, Nebraska, Nevada, New Hampshire, Oregon, and Vermont.⁷

Our treatment group consists of US states that both enacted cross-firm pay transparency, and for which we have at least two periods of post-treatment data. This leaves us with Colorado, California, and Washington state as our treatment group. Other states, such as New York State, Hawaii, Maryland and Illinois implemented the law in late 2023 or later, giving us insufficient post-treatment periods due to the ACS’s yearly frequency.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 and employment status. Lastly, we explore whether there were heterogeneous wage effects across key demographics of interest such as gender, educational attainment, and age.

IV.A Effects on Job Postings

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

⁷Our results are also robust to other criteria for defining our control group (refer to Table 1 in (Cullen, 2024)).

⁸We also exclude city or county-level jurisdictions such as Jersey City and New York City since we were unable to find an appropriate control group for those jurisdictions.

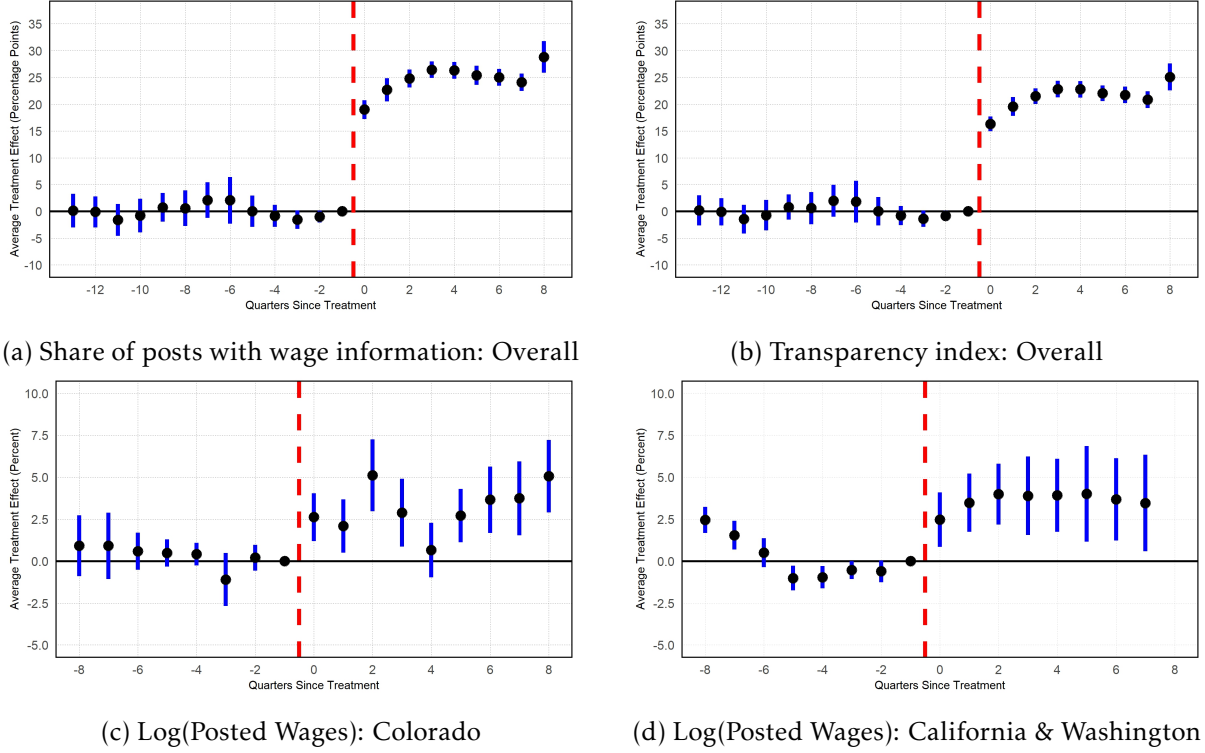


Figure 2: Event Studies for First-Stage Outcomes

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.

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 ATT estimate of the cross firm pay transparency's effect on the share of job postings with wage information is 24.7 percentage points ($SE = 0.68$) across all three treated states. In Colorado, our ATT estimate is 29.5 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 (19.9%), 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 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 was a significant decrease in the share of point wage offers

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

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 creating a novel index for measuring a posting's transparency 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.

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

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 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. This implies that even after accounting for the precision of wage information available in job postings, we find that workers obtain more wage information after the implementation of pay transparency laws.

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 increases 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-SOC3 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 advertised 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

¹⁰However, 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.

¹¹If an observation advertised a wage range, we use its mid-point.

¹²We make two further modifications from our baseline specification to facilitate this sample restriction. Firstly,

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 that the level of 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

Having established that the pay transparency reforms did indeed have a significant first-stage effect on the prevalence of wage information in job posting, we now analyze how they affected individuals' real wages and employment status in the treated states.

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 1.2% to 1.7%, which is in stark contrast to the often negative or insignificant wage effects found in the previous literature on pay transparency (Cullen and Pakzad-Hurson, 2023).

A potential explanation for this is that previous research has focused on intra-firm pay transparency laws, where workers only obtain information on their colleagues' 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 3.2% to 4.4%, while the increase in California and Washington was only 0.7% to 1.3%, albeit still statistically significant. Formally understanding the cause behind this discrepancy

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 eligibility criteria, we limit our sample to eight quarters before and after a treated state enacted its pay transparency law.

¹³We 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. This would lead to only the largest of firm-SOC3 groups to 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*** (0.256)	1.200*** (0.203)	1.741*** (0.305)	1.231*** (0.247)
Colorado	4.373*** (0.396)	3.532*** (0.381)	3.427*** (0.481)	3.166*** (0.393)
California + Washington	0.680** (0.313)	0.566** (0.260)	1.283*** (0.381)	0.703** (0.326)
N	3,522,674	3,522,674	2,391,580	2,948,979
Controls	No	Yes	Yes	Yes

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

Sample	Respondents (All)	Respondents (All)	Respondents (Prime Age)	Respondents (Private Sector)
Overall	-0.091 (0.119)	-0.094 (0.129)	0.051 (0.153)	-0.268 (0.180)
Colorado	0.317 (0.226)	-0.067 (0.251)	0.839** (0.310)	-0.202 (0.257)
California + Washington	-0.195 (0.116)	-0.100 (0.129)	-0.152 (0.144)	-0.284 (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, 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. * $\Rightarrow p < 0.1$, ** $\Rightarrow p < 0.05$, *** $\Rightarrow p < 0.01$.

Table 1: ATT Estimates For Real Labor Market Outcomes

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 groups 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



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.

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' employment status. Our ATT estimates on employment status are shown [Table 1b](#). For this, 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 1b](#) shows that the ATT on employment is often insignificant and highly variable across specifications.¹⁶

¹⁴According 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.

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

¹⁶For the analysis on employment, we exclude occupation and industry fixed effects from the regression speci-

Sample	Gender		Education		Age	
	Female	Male	BA Or Greater	Less Than BA	≤ 40 Years	> 40 Years
Overall	0.518 (0.377)	1.829*** (0.325)	1.924*** (0.354)	0.478 (0.318)	0.897* (0.451)	1.513*** (0.298)
Colorado	3.061*** (0.490)	3.847*** (0.359)	5.238*** (0.496)	2.038*** (0.498)	3.163*** (0.529)	3.912*** (0.310)
California & Washington	-0.169 (0.490)	1.279*** (0.369)	0.889** (0.400)	0.100 (0.417)	0.259 (0.613)	0.891** (0.386)
<i>N</i>	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. $*$ $\Rightarrow p < 0.1$, $**$ $\Rightarrow p < 0.05$, $***$ $\Rightarrow p < 0.01$.

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

IV.C Heterogeneity Analysis

An explicit aim of cross-firm pay transparency laws is to reduce wage inequality, particularly across gender. Thus, it is important to understand whether the policy had heterogeneous effects across gender and other demographic groups. We test for this by estimating (1) separately for the different demographic groups and report our ATT estimates in Table 2.

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, and not in California-Washington. For both treatment groups, the ATT was greater for men than for women. 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 the reader should not interpret them as causal.¹⁷

We can make stronger claims when we consider differences across educational background and age. As per Column 3 Table 2, we consistently 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%. This difference in magnitude can be attributed to differences in first-stage effects. We find that prior to the laws being implemented, occupations with high education requirements were significantly less likely to advertise wage

cation in Table 1b since this information is unavailable for non-employed individuals.

¹⁷In Section E.3 of the Online Appendix, we use a triple difference-in-difference specification to analyze the gender wage gap itself as an outcome. We find that we are unable to satisfy the parallel trends assumption, thereby preventing us from making causal statements.

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

offers whereas occupations with low education requirements were already posting wage offers despite not being required to.

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.¹⁹ We again argue that differences in first-stage 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 Table 2 is that inter-firm pay transparency did not result in a significant negative wage effect on wage effect for any demographic group. This is in contrast to intra-firm pay transparency, which has been found to have negative effects on men’s wages. Our findings are consistent with those 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 assess whether they achieve their policy goals of increasing wages and reducing wage inequality across genders.

We find that these laws were successful in increasing the amount of salary information available in job postings and the very wages that firms posted. This consequently led to an overall increase in realized wages for workers, albeit significantly more in Colorado than in California and Washington. We show that this heterogeneity in wage effects is driven by differences in prevailing labor market conditions across the two groups of states. Our staggered implementation setting — where Colorado and California-Washington adopted these laws at different times and under different labor market conditions — allowed us to study these heterogeneous effects directly. This highlights an important lesson for policy evaluation: the effects of pay transparency laws are not uniform, and future analyses should account for the labor market conditions under which such laws are enacted.

However, we do not find evidence that pay transparency laws successfully reduced wage inequality across genders. While we robustly find that men’s 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,

¹⁹We choose 40 years as the cutoff as it is the median age in the ACS.

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.

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