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

Online Appendix

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Appendix A contains our detailed discussion of our data sources and cleaning procedures. We provide a comprehensive summary of the cross-firm pay transparency laws that have been implemented in the U.S. in Appendix B. Additional results related to our first stage and second stage analysis are shown in Appendix C and Appendix D. Appendix E focuses on results derived using the Two-Way Fixed Effects specification. We conclude by discussing various potential reasons why we observe particularly strong wage effects in Colorado in Appendix F.

A Data

A.1 Lightcast

In this section, we document our cleaning process for the Lightcast data. We removed observations that had missing information or that would add noise to our analysis, as outlined in Table 1. While most steps are standard in the literature, we make two noteworthy changes. Firstly, we remove job postings that are posted *exclusively* to the website Craigslist.org, which is a classified advertisements website where casual and one-off jobs are often posted. These jobs are fundamentally different from 'standard' jobs. We only remove the observation if Lightcast reports Craigslist as its only job source, to mitigate the risk of incorrectly removing a 'standard' job.[1]

Next, we apply several rounds of cleaning to address measurement issues in the prevalence of wage information. First, we address the issue of job postings that contain imputed or estimated wages by following Lafontaine et al. (2025) and reclassifying jobs that contain specific strings, such as "estimated wage", as not having any wage information.

Secondly, we modify job postings whose remuneration structure follows a schedule. In the US, the wages of several public sector jobs, such as teachers and police officers, are determined from a pay schedule that is publicly available and pre-specified. These pay schedules outline how the successful applicants' pay varies across dimensions, such as their experience and education level.

[1] As part of Lightcast's de-duplication algorithm, the newest vintage of Lightcast data allows us to observe every unique job board that hosted the same job posting.

	Number of Observations	% of All
All	270,299,816	100.00%
Remove internships	266,894,368	98.74%
Remove missing & unclassified	210,022,304	77.70%
Remove postings from Craigslist	209,260,427	77.42%
Remove military occupations	209,194,773	77.39%
Remove irrecoverable firm names	207,023,882	76.59%

Notes: The second column displays the total number of observations after cumulatively applying each successive layer of data cleaning. The third column displays the percentage of remaining observations. We delete an observation if it is missing a job title, the date of posting, its source, a company name, or important location information such as its state or county.

Table 1: Number of Job Postings in Lightcast (2018 - 2024)

Employers for these jobs cannot specify any salary information in the job posting since the salary they will pay depends on the characteristics of the successful applicant, which they do not know ex-ante. Lightcast classifies these observations as not having any wage information since there is none to extract from the posting despite wage information being there in reality. Hence, we reclassify jobs that are highly likely to follow a pay schedule as containing wage information, specifically, a point-wage offer.[2]

Lastly, we find that Lightcast's algorithm incorrectly classifies a small share of observations as not having any wage information. We know of at least two possible reasons for this. First, irrespective of the original posting's stated pay frequency (hourly, weekly, monthly, etc.), Lightcast converts all wage offers to a standardized annual salary. Yet there are some niche occupations where reporting an annual salary is difficult or outright or outright impossible. For example, truck drivers are often paid on a per-mile basis. Without knowing the expected number of miles driven ex-ante, it is impossible to convert this pay structure into a conventional annual salary. We reclassify such jobs as containing wage information, but we do not use them in our analysis on the actual range of values contained in a wage offer, since we cannot credibly extract this information from these job postings.

A second possible reason behind the presence of false negatives is that the way pay information is presented is not standardized across job boards which introduced significant difficulties for Lightcast's wage extraction algorithm.[3] Resolving this issue in a way that does not introduce false positives (incorrectly extracting wage information where there is none) is non-trivial, as there are many non-wage related reasons why a job posting's body would contain numbers or strings like '$', such as referral bonuses, or a company's annual profits.

We address this problem by creating our own pattern recognition-based text analysis algorithm that is able to extract wage information from an additional 8.54 million job postings

[2]We make this modification for observations that contain the strings 'salary schedule', 'pay schedule', or 'wage schedule' in the body of the text.

[3]For example, after manually inspecting the body of a job posting, we fpimd that abbreviations like '50K-60K' instead of '$50,000-$60,000' tend to confound Lightcast's algorithm.

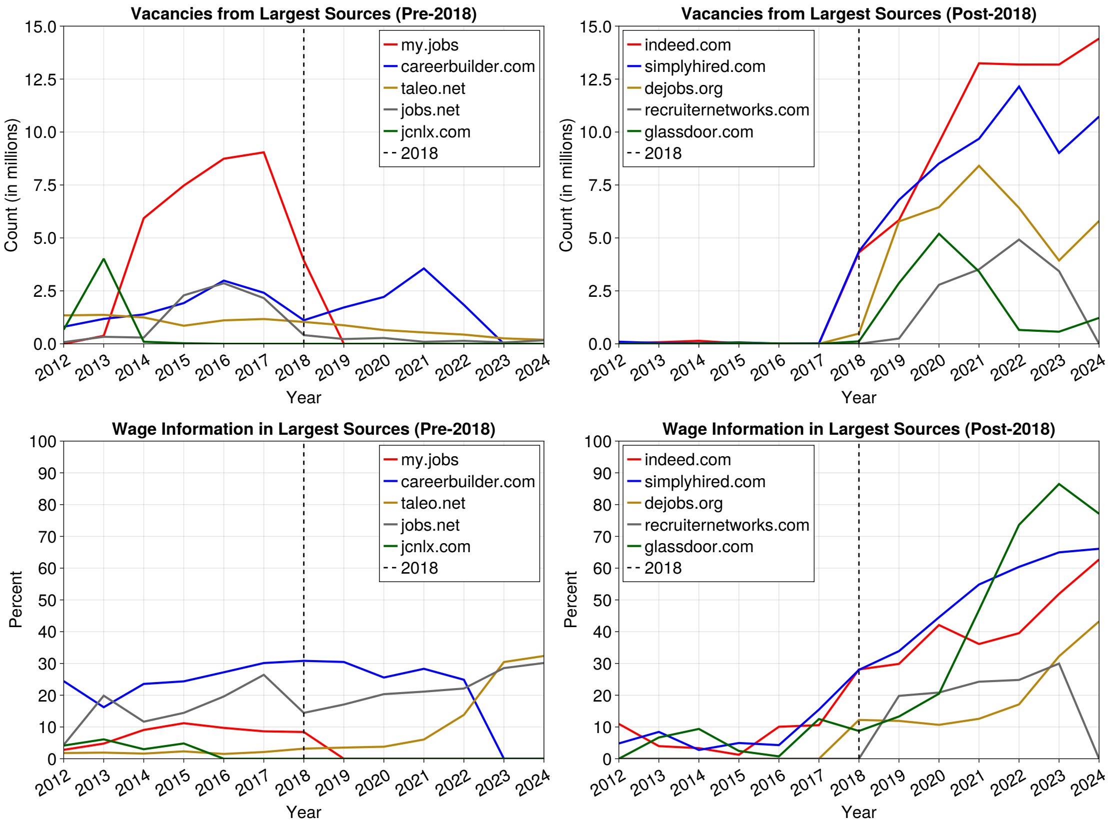


Figure 1: Change in Composition Of Job Sources And Wage Information Post-2018

Notes: This figure uses Lightcast data on job postings aggregated at the yearly level (2012-2023). Panels 1 and 3 focus on job sources with the most number of job postings pre-2018, and Panels 2 and 4 focus on job sources with the most number of job postings post-2018.

out of approximately 114 million that Lightcast classified as not having any wage information, implying that the false negative error rate is approximately 7.49%. Due to the number of observations and aforementioned confounding factors, achieving a true 0% error rate is not feasible. However, we believe that we have addressed this measurement issue enough such that the magnitude of the remaining error is small enough to be insignificant.

A.2 Addressing Concerns About Wage Information In Lightcast Data

Batra et al. (2023) use Lightcast data from 2012-2017 to show that only 13.5% of postings had any wage information. However, they document that the 2018 Lightcast data has double the share of job postings with any wage information.

Batra et al. (2023) attributes this increase to the emergence of job boards that include their own estimates of wage offers in job postings. Callaci et al. (2024) and Lafontaine et al. (2025) provide evidence for this by identifying job postings with imputed wages in Lightcast data if they have substrings like “estimated” or “similar jobs pay” in their text. They report that there were 0.2% imputed wage offers in 2017, but this number rose to 58.2% in 2018. Based on our discussion with Lightcast and the authors of Batra et al. (2023), we now discuss why these observations do not create an issue for our analysis.

Lightcast regularly updates its job postings dataset to include data on the latest job postings, but it also makes retroactive changes when necessary. These retroactive changes can take the form of adding new variables or improving the quality of data extracted from the job postings texts (company name cleaning or classification of occupations). Our version of the Lightcast dataset includes details on the sources for a particular job posting (such as job boards or company websites), whereas the version used in Batra et al. (2023) did not.

Figure 1 shows a marked shift in the composition of the most common sources found in Lightcast around 2018. This was the time when Lightcast started using data from major job boards like Indeed and Simply Hired. Panel 2 of Figure 1 also shows that these sources contain more wage information.[4] Our correspondence with Batra et al. (2023) revealed that the authors used a dataset version that did not include data on job sources. Therefore, they could not come to the same conclusion as we did.

Nevertheless, the wage information found in the data post-2018 may be useless if most of it is imputed and not provided by employers. We follow the strategy of Callaci et al. (2024) and Lafontaine et al. (2025) and manually check multiple job postings that contain the word "estimated" or "similar jobs pay". We find that in postings where both a posted wage and an imputed wage are present, the Lightcast algorithm correctly identifies the actual posted wage. In our data cleaning process, we keep such postings, but reclassify other postings as having no wage information if they only have an imputed wage but no separate posted wage. Panel 2 of Figure 1 shows the proportion of wage postings for each major job source that contains wage information after we address the imputed wages issue.

These findings suggest that the increase in the number of job postings and the increase in wage information post-2018 are driven by the inclusion of data from websites like Indeed and Simply Hired. We agree with Batra et al. (2023)'s conclusion that Lightcast data pre-2018 does not contain enough wage information. Therefore, we limit our analysis to job postings data from 2018 onwards. Following Callaci et al. (2024) and Lafontaine et al. (2025), we remove postings with imputed wages, but we find that the spike in the wage information in job postings after 2018 remains.

[4] A job posting can have multiple sources. To plot the number of job postings for each job source, we count all postings that refer to a particular website as a source. On the other hand, to plot the wage information for each job source, we only consider job postings coming from a single job source.

A.3 American Community Survey

Variable	**2016**	**2017**	**2018**	**2019**	**2020**	**2021**	**2022**	**2023**	**2024**
Female	0.51	0.51	0.51	0.51	0.51	0.51	0.51	0.51	0.51
Married	0.47	0.48	0.47	0.47	0.48	0.48	0.48	0.48	0.47
Age									
18-24	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
25-34	0.23	0.23	0.23	0.23	0.23	0.23	0.23	0.23	0.23
35-44	0.20	0.20	0.20	0.21	0.21	0.21	0.21	0.22	0.22
45-54	0.21	0.20	0.20	0.20	0.20	0.20	0.19	0.20	0.19
55-64	0.20	0.20	0.20	0.21	0.21	0.21	0.20	0.20	0.20
Race									
White	0.72	0.71	0.71	0.71	0.62	0.60	0.60	0.59	0.58
Black	0.14	0.14	0.14	0.14	0.13	0.13	0.13	0.13	0.13
Other	0.15	0.15	0.15	0.15	0.25	0.27	0.27	0.28	0.29
Education									
<= High School	0.71	0.70	0.70	0.69	0.67	0.67	0.66	0.66	0.65
>= Bachelors	0.29	0.30	0.30	0.31	0.33	0.33	0.34	0.34	0.35
Labor Market									
Employed	0.70	0.71	0.72	0.72	0.70	0.71	0.73	0.74	0.74
Private Sector	0.85	0.85	0.85	0.85	0.83	0.84	0.84	0.84	0.84
40+ Weeks	0.90	0.91	0.91	0.92	0.90	0.89	0.91	0.92	0.92
35+ Hours	0.82	0.82	0.83	0.82	0.83	0.83	0.84	0.83	0.83
Total (In Millions)	1.71	1.73	1.73	1.73	1.41	1.71	1.76	1.77	1.77

Notes: This table provides summary statistics for our ACS sample for the period 2016-2024, restricted to respondents between the age of 18-64 years. Each value denotes the proportion of the sample in a year with that particular characteristic. The shares are weighted by the person weights given by ACS, and all values are rounded up to 2 decimal points. The Total at the bottom is unweighted. <= High School and >= Bachelors denote the maximum educational attainment of an individual. 40+ Weeks and 35+ Hours are the number of weeks and usual hours worked by an individual in the previous year. Private Sector, 40+ Weeks, and 35+ Hours shares are calculated conditional on being employed.

Table 2: Summary Statistics For American Community Survey Sample (2016-2024)

Table 2 shows that between 2016 and 2024, the composition of our sample remains stable across multiple dimensions, such as gender, marital status, and age. The share of Black individuals is also consistent over time. However, the share of White individuals declines post-covid. The share of college-educated individuals has increased over time. The share of employed people also increases gradually. Among employed individuals, the shares of people in the private sector, working 40+ weeks in a year, or working 35+ hours per week remained stable.

B Further Details on Pay Transparency Laws

Jurisdiction	Implemented	Employer Coverage
Colorado	Jan 1, 2021	All employers with at least one employee in Colorado.
Jersey City (New Jersey)	Apr 13, 2022	Employers with five or more employees in Jersey City and a place of business there.
Ithaca (New York)	Sep 1, 2022	Employers in the City of Ithaca with four or more employees.
New York City (New York)	Nov 1, 2022	Employers with four or more employees where at least one of them is in New York City.
Westchester County (New York)	Nov 6, 2022	Any employer posting a job that can be performed in Westchester County.
California	Jan 1, 2023	Employers with more than 15 employees, with at least 1 based in California.
Washington	Jan 1, 2023	Employers with 15 or more employees who engage in business in Washington or recruit for jobs that could be filled by Washington-based employees.
Albany (New York)	Mar 9, 2023	All Employers
New York State	Sep 17, 2023	All private New York employers with four or more employees (note that the law doesn't specify whether this is total employees or employees in New York State).
Hawaii	Jan 1, 2024	Employers with at least 50 employees, except for public employee positions.
Washington, D.C.	Jun 30, 2024	Private employers with more than 1 employee.
Maryland	Oct 1, 2024	All Employers.
Illinois	Jan 1, 2025	Employers with at least 15 employees.

Table 3: Timeline For Implementation Of Pay Transparency Laws In Different Regions

C First Stage Analysis: Additional Outcomes

	Wage Information Share		Point Offers Share		Median Range Size		Wage Information Index		Log(Job Postings)	
	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA
Overall	24.914***	24.674***	-6.933***	-7.289***	0.205	0.113	21.577***	21.361***	-6.145	-6.482*
	(1.521)	(0.683)	(0.836)	(0.758)	(0.340)	(0.302)	(1.392)	(0.583)	(3.919)	(3.306)
Colorado		29.476***		-9.443***		-0.731*		25.806***		-5.797***
		(0.929)		(1.142)		(0.392)		(0.799)		(1.308)
California +		22.501***		-6.369***		0.458		19.350***		-7.085
Washington		(0.807)		(0.910)		(0.455)		(0.716)		(5.842)
N (millions)	1.58	1.58	1.05	1.05	0.90	0.90	1.58	1.58	1.58	1.58

Notes: This table is based on empirical analysis using Lightcast data (2018-2024). All ATT estimates are in percentage points, except for Log(Job Postings), where the ATT estimates are in percentages. The unit of observation is at the State x SOC3 x NAICS3 group level. Except for Log(Job Postings), regressions for all the other outcomes use the number of observations within a group as weights. TWFE = Two-Way Fixed Effects, SA = Sun and Abraham, CA = California, WA = Washington, N = Number of observations. All standard errors (in parentheses) are clustered at the state level. $^{*} \Rightarrow p < 0.1,^{**} \Rightarrow p < 0.05,^{***} \Rightarrow p < 0.01$.

Table 4: The Effect of Pay Transparency Laws On The Wage Information In Job Postings

In this section, we provide details for our analysis on how cross-firm pay transparency affected firms' vacancy posting behavior. We further investigate the precision of wage information provided by focusing on the share of point wage offers and the width of wage range offers. We also assess whether pay transparency affected the number of postings made by firms. Our ATT estimates are summarized in Table 4.

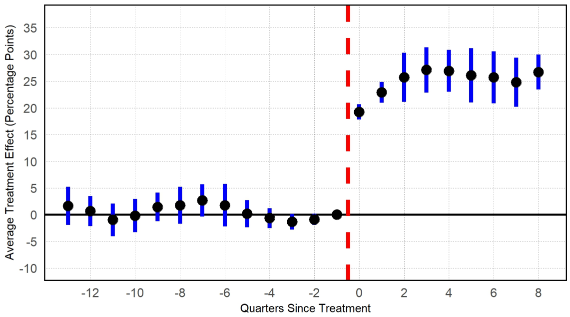
A few observations to take away from Table 4 are as follows. First, our results are invariant to the choice of estimator used. For each outcome, the Two-Way Fixed Effects estimator and the Sun and Abraham (2021) estimator give similar results. Although not reported here, our results are also broadly robust across the following dimensions: weighted vs unweighted, state vs county level geography, and granularity of occupation and industry classifications (for example, using 4-digit SOC and NAICS codes for occupations and industries respectively, instead of 3-digit codes).

Second, Colorado and California-Washington appear to be impacted similarly across the various first-stage outcomes we analyze. This rules out heterogeneous first stage effects as an explanation for why Colorado's wage increases were larger than the California-Washington group's.

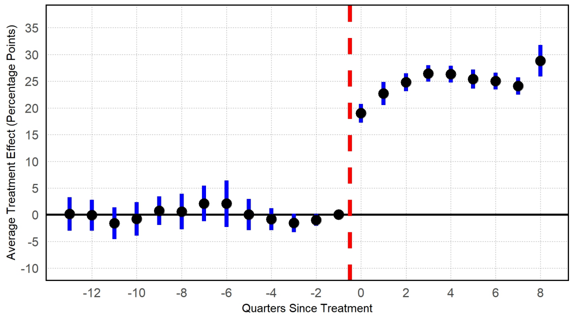
In the remainder of this section, we provide further details on each outcome and present event study plots for each estimate in Table 4. Apart from Log(Job Postings), we are able to report satisfactory pretrends that give visual support to the argument that changes in the respective outcome can be causally attributed to the pay transparency laws.

C.1 Share Of Job Postings With Wage Information

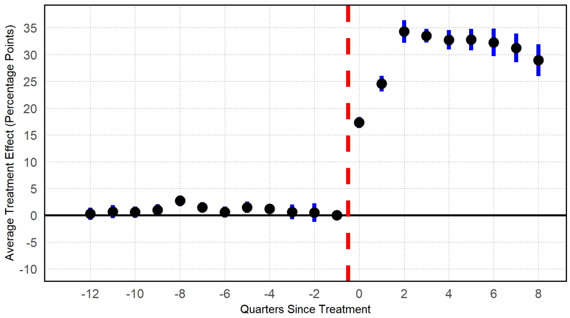
The share of job postings with wage information is defined as the proportion of all job postings within a State x SOC3 x NAICS3 group that have any kind of wage information (point wage



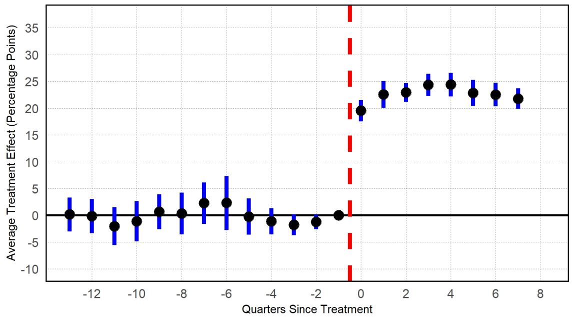
(a) Overall: Two-Way Fixed Effects



(b) Overall: Sun And Abraham



(c) Colorado: Sun And Abraham



(d) California-Washington: Sun And Abraham

Figure 2: Event Studies For The Share Of Wage Information

Notes: We use Lightcast data on job postings (2018-2024) aggregated up to the state-SOC3-NAICS3-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.

offer or wage range offer). Looking at Figure 2, we find that wage information in job postings increases overall, and in Colorado and the California-Washington group separately as well. We can interpret the impact on this outcome as follows: On average, a job seeker in treated states finds wage information in 22–29 percentage point more job postings post-treatment relative to the control group.

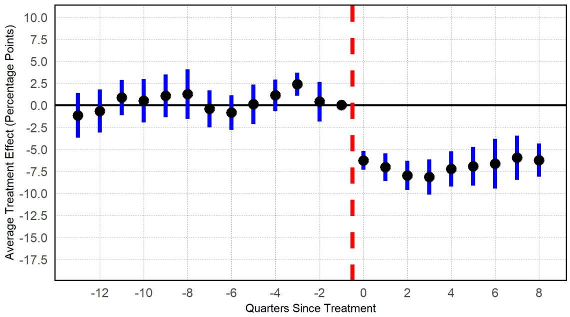
C.2 Share Of Wage Offers That Are Point Wage Offers

A job posting has a point wage offer if it contains a single wage amount.[5] For this analysis, we only focus on postings containing wage information. The outcome is calculated as the proportion of wage offers in a State x SOC3 x NAICS3 group that are in terms of a point wage.[6]

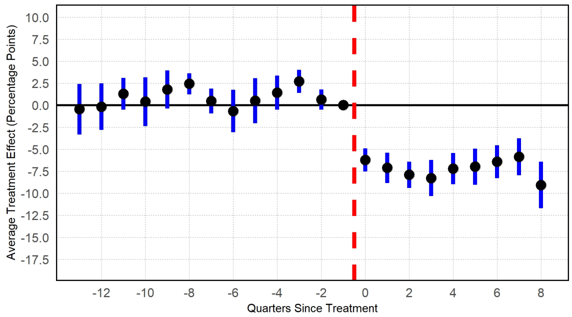
We can interpret the impact on this outcome as follows: On average, within the set of all wage offers, a job seeker in the treated states finds 6-9 p.p. fewer point wage offers post-

[5]For example, a posting with advertising \$30,000 per year is considered to contain a point wage offer. If it offered \$28,000 – \$32,000 per year, then it would be considered to contain a wage range offer.

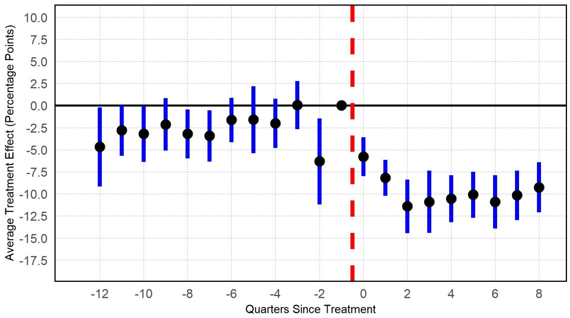
[6]By definition, the share of wage range offers = 100 - the share of point wage offers.



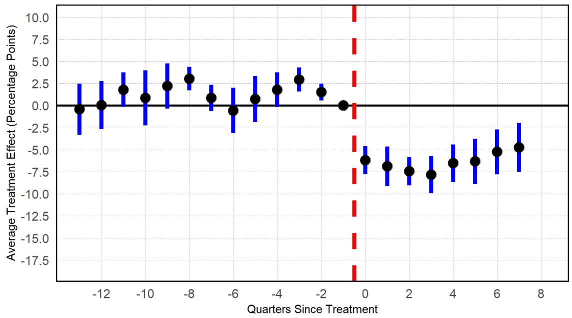
(a) Overall: Two-Way Fixed Effects



(b) Overall: Sun And Abraham



(c) Colorado: Sun And Abraham



(d) California-Washington: Sun And Abraham

Figure 3: Event Studies For The Share Of Point Wage Offers Among All Wage Offers

Notes: We use Lightcast data on job postings (2018-2024) aggregated up to the state-SOC3-NAICS3-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.

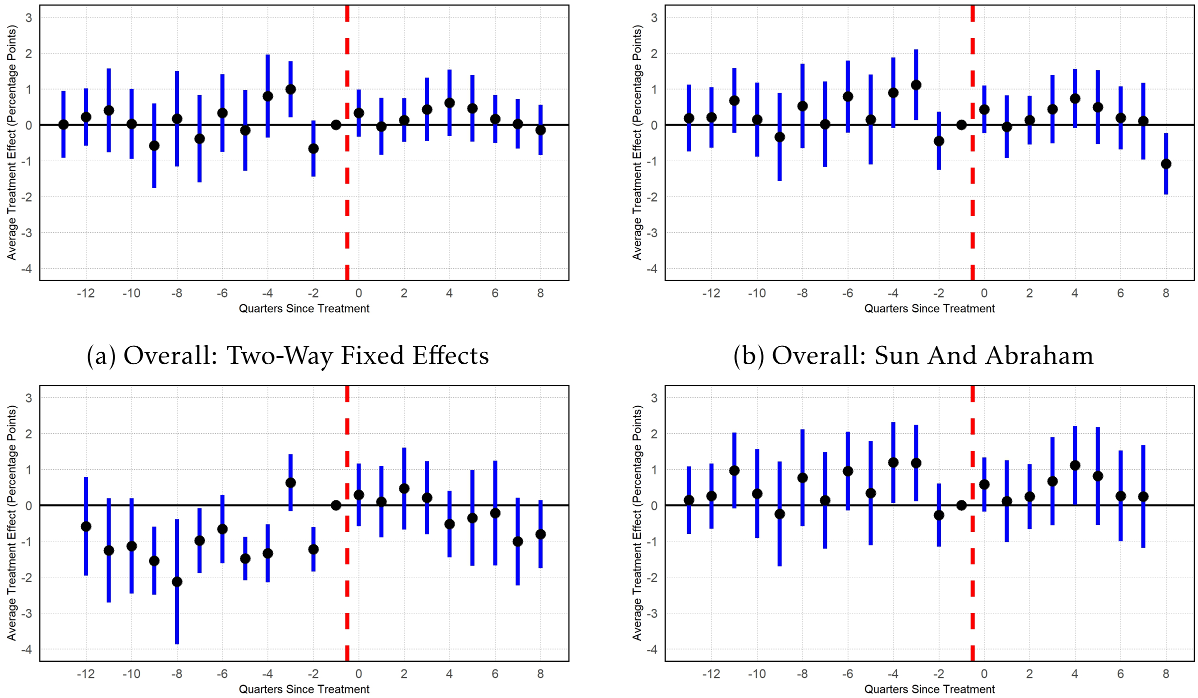
treatment relative to the control group. In other words, the share of wage range offers increases by 6-9 p.p. as well.

The increase in wage range offers suggests that employers generally prefer not to provide precise wage information. This may be due to adverse selection, as employers prefer flexibility in making wage offers when worker ability is unknown. Another reason may be to prevent competitors from perfectly observing their willingness to pay for a worker, since competition for workers among employers can drive up wage offers, raising labor costs.

C.3 Width Of Wage Range Offers

Here, we only focus on job postings containing wage range offers; i.e. observations with no wage information, or containing point wage offers are excluded. For each individual observation, we first calculate the width of its range as per (1) and (2). Then, we compute the median range width for each State x SOC3 x NAICS3 group. In order to reduce the influence of outliers within small groups, we use each group's median width instead of mean width as our outcome variable.

$$\text{Width} = \frac{\text{Upperbound} - \text{Lowerbound}}{\text{Midpoint}} \tag{1}$$



(a) Overall: Two-Way Fixed Effects

(b) Overall: Sun And Abraham

(c) Colorado: Sun And Abraham

(d) California-Washington: Sun And Abraham

Figure 4: Event Studies For The Median Width Of Wage Range Offers

Notes: We use Lightcast data on job postings (2018-2024) aggregated up to the state-SOC3-NAICS3-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.

where

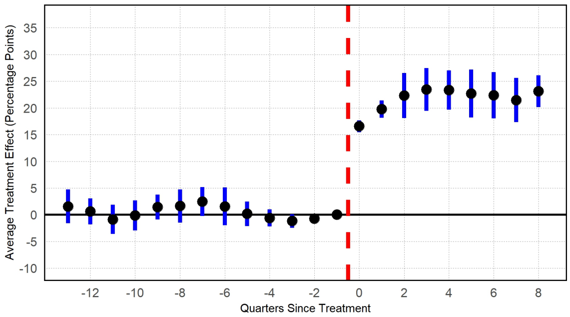
$$\text{Midpoint} = \frac{\text{Upperbound} + \text{Lowerbound}}{2} \tag{2}$$

While do find observe instances of extremely wide wage range offers that would not provide any meaningful information to job seekers, such instances are extremely rare. Indeed, Figure 4 shows that there does not seem to be any significant widening of wage ranges after cross-firm pay transparency was implemented.

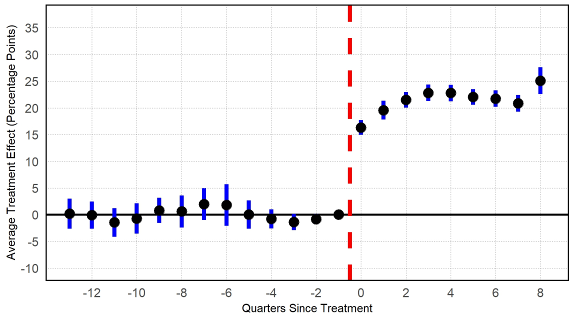
C.4 Wage Information Index

To analyze the impact on our wage information index, we first use Equation (2) in the main text to calculate the index value for each job posting. Then, we obtain the mean index value among job postings within a State x SOC3 x NAICS3 group, and use this as the outcome variable in our regressions.

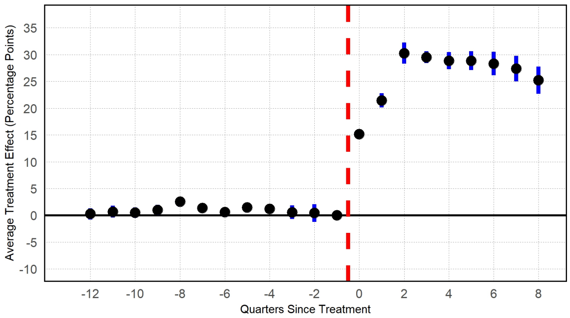
Note that Table 4 shows that the ATT for the wage information index is lower than the ATT for the share of wage information. This happens because the index also considers the precision of wage information, which falls because the share of point wage offers declines post-treatment, as shown in Table 4 and Figure 3. Figure 5 shows that the event studies satisfy the



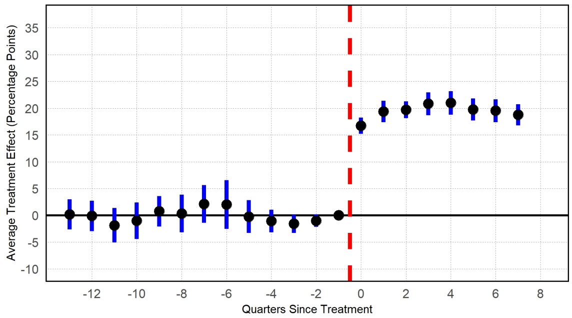
(a) Overall: Two-Way Fixed Effects



(b) Overall: Sun And Abraham



(c) Colorado: Sun And Abraham



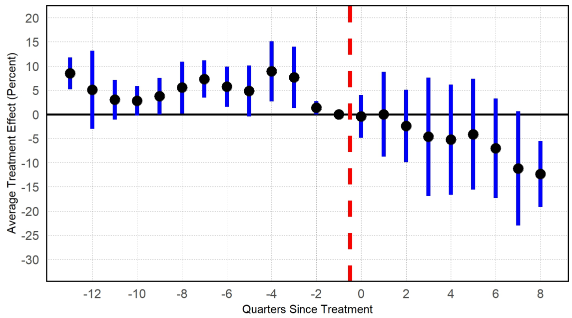
(d) California-Washington: Sun And Abraham

Figure 5: Event Studies For Our Wage Information Index

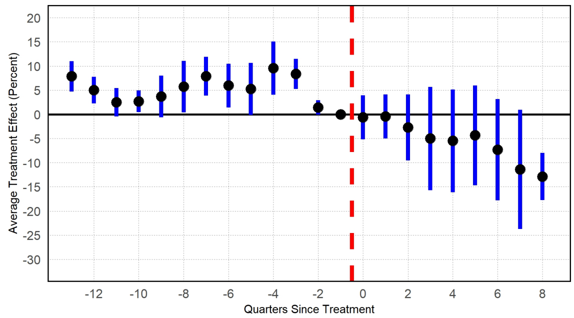
Notes: We use Lightcast data on job postings (2018-2024) aggregated up to the state-SOC3-NAICS3-quarter level. We use the number of observations within a group as weights. In the event studies, $t = 8$ ($t = -13$) actually represents the binned time period $t \geq 8$ ($t \leq -13$). The dashed red line 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.

parallel pretrends assumption. Therefore, even after accounting for the imprecise information in wage ranges, we can say that wage information in job postings increases significantly in the treated states.

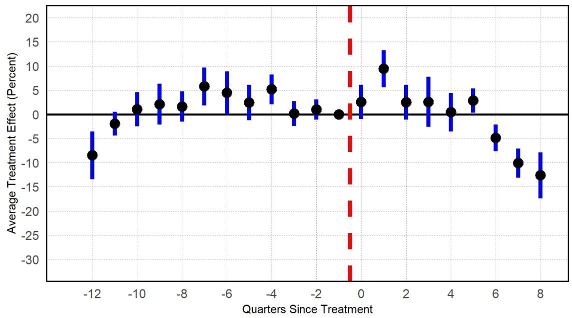
C.5 Number Of Online Vacancy Postings



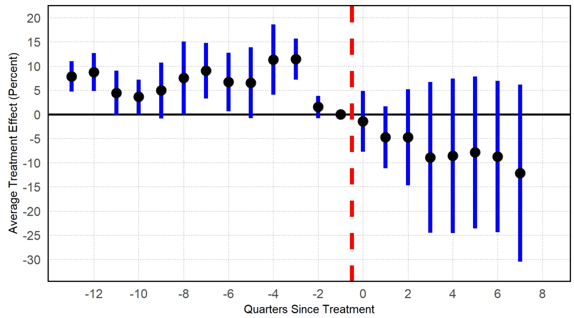
(a) Overall: Two-Way Fixed Effects



(b) Overall: Sun And Abraham



(c) Colorado: Sun And Abraham



(d) California-Washington: Sun And Abraham

Figure 6: Event Studies For Online Vacancy Postings

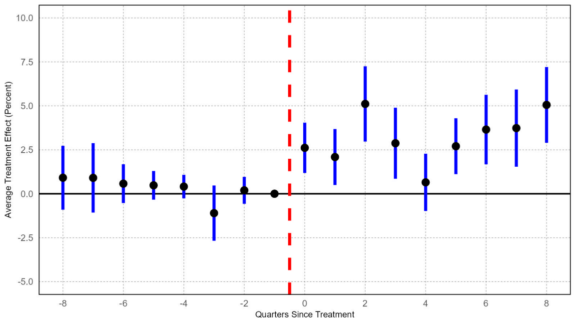
Notes: We use Lightcast data on job postings (2018-2024) aggregated up to the state-SOC3-NAICS3-quarter level. In the event studies, $t = 8$ ($t = -13$) actually represents the binned time period $t \geq 8$ ($t \leq -13$). The dashed red line denotes the time of implementation of pay transparency laws.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

Table 4 suggests that there is a negative effect on the number of online vacancy postings. One reason why we may observe a decrease in job postings is if revealing wage offer information is costly for employers. Post-treatment, firms may react by using alternative channels to recruit workers, such as referrals, that are not covered by inter-firm pay transparency.

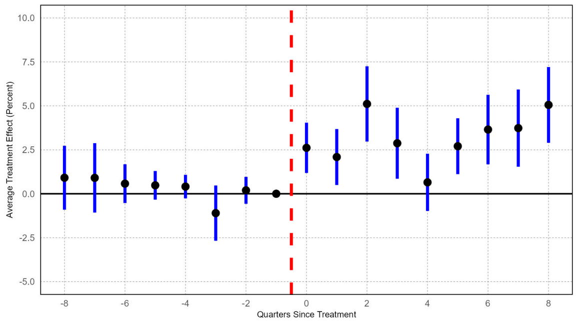
However, the event study plots in Figure 6 suggest that we should be careful with interpreting this result. The negative effect in Colorado appears to start 6 quarters after its pay transparency law was implemented. In California and Washington, the decline in the number of job postings seems to start a few quarters before their pay transparency laws were implemented.

One possible factor behind this behavior in job postings could be labor market conditions. Given that Colorado implemented its law in January 2021, and California and Washington implemented their laws in January 2023, the decline in job postings recorded in the event studies appears to happen during 2022. As Figure 14 shows, this was also when the US labor market started cooling down. Accounting for labor market conditions, Figure 6 suggests that our treated states experienced a more rapid cooling of their labor markets compared to other states. However, a formal analysis of this hypothesis is beyond the scope of this paper.

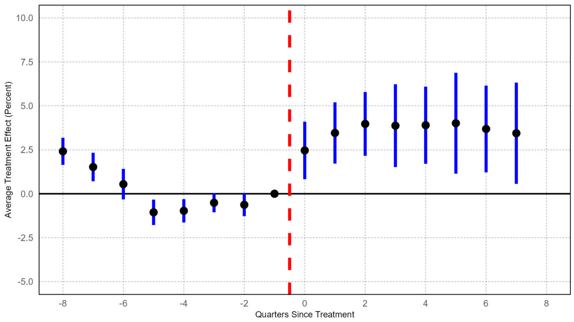
C.6 Posted Wage Offers



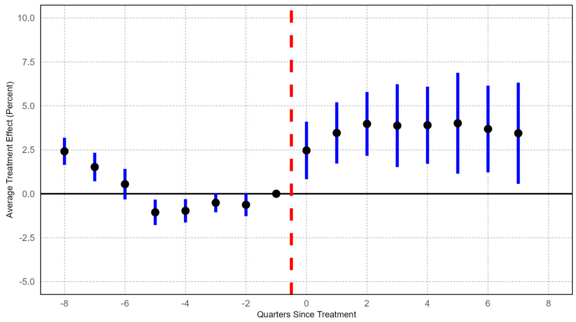
(a) Colorado: Two-Way Fixed Effects



(b) Colorado: Sun And Abraham



(c) California-Washington: Two-Way Fixed Effects



(d) California-Washington: Sun And Abraham

Figure 7: Event Studies For The Level Of Posted Wage Offers

Notes: We use Lightcast data on job postings (2018-2024) 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.

Figure 7 and Table 5 show that posted wages increased in Colorado by 3.81% (SE = 0.96), and in California and Washington by 3.59% (SE = 1.09).[7] This is significant because the law only requires employers to provide wage information, and there is no requirement for firms to change the level of their wage offers.

Our proposed reason for why wage offers are increasing is that employers compete for workers in the labor market, and they can use the information about their competitors' wage offers to offer even higher wages to attract workers. This form of Bertrand competition in the labor market turns out to be an unintended consequence of pay transparency laws, leading to higher posted wage offers.

[7] Note that the TWFE and SA estimates are similar, which should be the case when the treatment is not staggered and we have only one treated unit in our sample, as the TWFE and SA estimators are identical in this case.

	Colorado		California + Washington	
	TWFE	SA	TWFE	SA
ATT	3.812***	3.812***	3.586***	3.586***
	(0.955)	(0.955)	(1.091)	(1.091)
N (millions)	2.15	2.15	4.59	4.59

Notes: This table uses Lightcast data (2018-2024). All ATT estimates are in percentages. The unit of observation is at the State x SOC3 x firm group level. Number of observations is used as weights. TWFE = Two-Way Fixed Effects, SA = Sun and Abraham, CA = California, WA = Washington, N = Number of observations. All standard errors (in parentheses) are clustered at the state level.

Table 5: The Effect of Pay Transparency Laws On Log(Posted Wages)

D Event Studies For The Second Stage Analysis

In this section, we present the event study plots and ATT estimates for a broader class of regression specifications than we have in the main text. Based on the results in this section, we conclude that across different specifications, we find a positive and statistically significant effect on wages, and that there is minimal effect on the employment status of individuals.

In our analysis in the main text, we showed the event studies for the specification using the Sun and Abraham (2021) estimator, including demographic controls, where we keep all employed individuals in our sample. Here, we keep using the Sun and Abraham (2021) estimator, with some modifications. First, we show that our results do not depend on including demographic controls or not. Next, we restrict our attention to prime-age workers (those who are employed and between the age of 25 and 54 years) and we show that our results do not change. We also present results focusing on private sector workers. For each of these modifications, we also present the ATT estimates when we use all untreated states in our control group in Table 6.

We also try different ways of selecting states for our refined control group. First, we keep those states that have Right Of Workers To Talk (ROWTT) laws (Cullen and Pakzad-Hurson, 2023) but do not have pay transparency laws for job postings. This is the control group for which we show the ATT estimates and event studies in this paper. Call this control group A. Second, we modify control group A to keep only those states that implemented their ROWTT laws before 2016, because these states will have an unchanged policy environment in our sample. Third, we modify control group A by also including those states that implemented salary history ban laws. Finally, we keep only those states that have ROWTT laws and implemented a salary history ban law before 2018. For this exercise, we use the information from Table 1 in Cullen (2024) , which lists the time and location when different forms of pay transparency laws were implemented in the world. We present the list of states in each control group in the code files in our replication folder.

Finally, in Section D.4, we show the event studies when we focus on the effect of pay transparency laws on wages for different demographic groups, such as gender, education, and age. All the groups that we consider seem to experience weakly positive effects on their wages. We interpret this evidence as suggestive, since in some cases we do not obtain good pretrends.

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

Sample	All Employed		All Employed		Prime Age Employed		Private Sector Employed	
Control Group	Refined	All Untreated	Refined	All Untreated	Refined	All Untreated	Refined	All Untreated
Overall	1.469***	1.465***	1.200***	1.188***	1.741***	1.666***	1.231***	1.281***
	(0.256)	(0.165)	(0.203)	(0.141)	(0.305)	(0.215)	(0.247)	(0.161)
Colorado	4.373***	3.601***	3.532***	2.824***	3.427***	2.935***	3.166***	2.588***
	(0.396)	(0.316)	(0.381)	(0.292)	(0.481)	(0.315)	(0.393)	(0.304)
California +	0.680**	0.885***	0.566**	0.744***	1.283***	1.321***	0.703**	0.924***
Washington	(0.313)	(0.177)	(0.260)	(0.150)	(0.381)	(0.247)	(0.326)	(0.173)
N (in millions)	3.52	9.47	3.52	9.47	2.39	6.33	2.95	7.89
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes

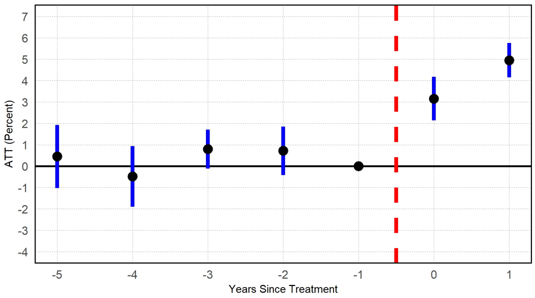
(b) Employment - Population Ratio (1 If Employed, 0 Otherwise)

Sample	All Respondents		All Respondents		Prime Age Respondents		Private Sector	
Control Group	Refined	All Untreated	Refined	All Untreated	Refined	All Untreated	Refined	All Untreated
Overall	-0.091	-0.178***	-0.094	-0.166***	0.051	0.083	-0.268	-0.478***
	(0.119)	(0.059)	(0.129)	(0.055)	(0.153)	(0.083)	(0.180)	(0.074)
Colorado	0.317	0.471***	-0.067	0.140	0.839**	0.909***	-0.202	-0.071
	(0.226)	(0.093)	(0.251)	(0.102)	(0.310)	(0.118)	(0.257)	(0.111)
California +	-0.195	-0.342***	-0.100	-0.244***	-0.152	-0.129	-0.284	-0.581***
Washington	(0.116)	(0.067)	(0.129)	(0.061)	(0.144)	(0.103)	(0.189)	(0.085)
N (in millions)	4.96	13.55	4.96	13.55	3.07	8.25	4.96	13.55
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes

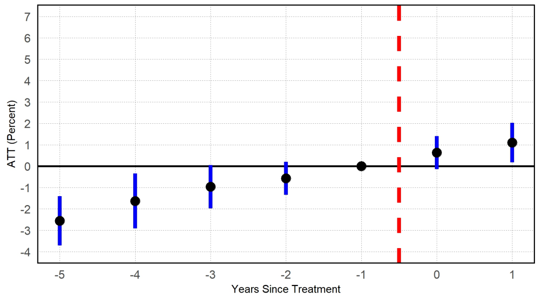
Notes: We use ACS data at the individual level. All values in Table 6a are in percentage terms. All values in Table 6b are in percentage points. All ATT estimates are obtained via the Sun and Abraham (2021) estimator. The refined control group consists of states that have similar labor laws as our treated states. The 'all untreated' control group consists of all states that do not implement pay transparency laws in job postings during our sample period. 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.

Table 6: ATT Estimates For Real Labor Market Outcomes

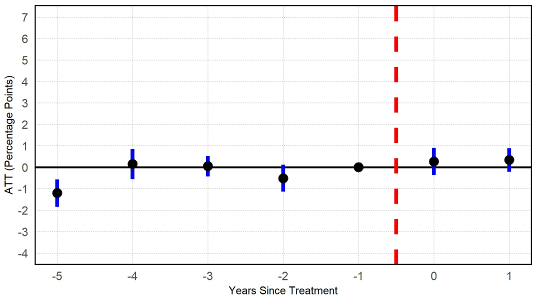
D.1 All Respondents + Without Controls



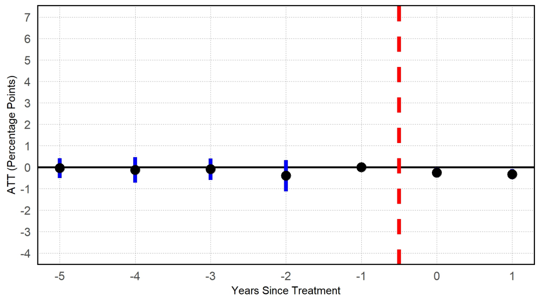
(a) Wages: Colorado



(b) Wages: California-Washington



(c) Employment: Colorado

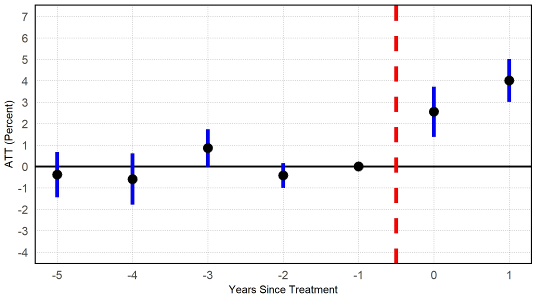


(d) Employment: California-Washington

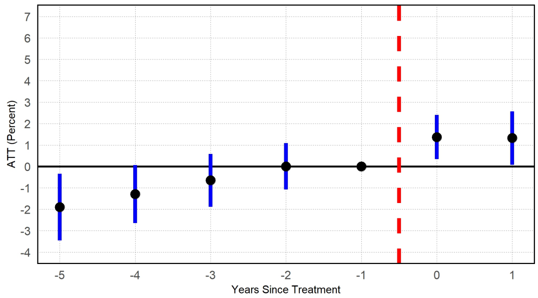
Figure 8: Event Studies for Second-Stage Outcomes (Without Controls)

Notes: We use ACS data at the individual level. All ATT estimates are obtained via the Sun and Abraham (2021) estimator. 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.

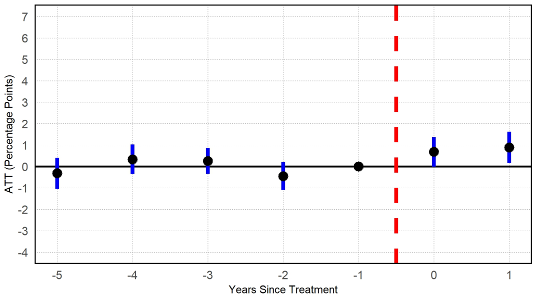
D.2 All Prime Age Individuals + With Controls



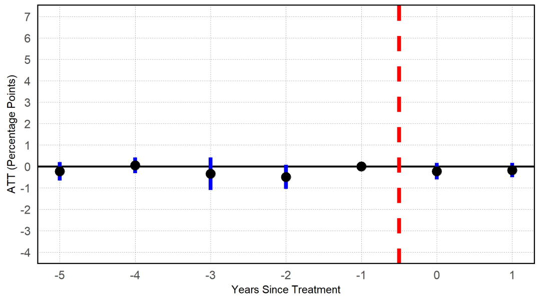
(a) Wages: Colorado



(b) Wages: California-Washington



(c) Employment: Colorado

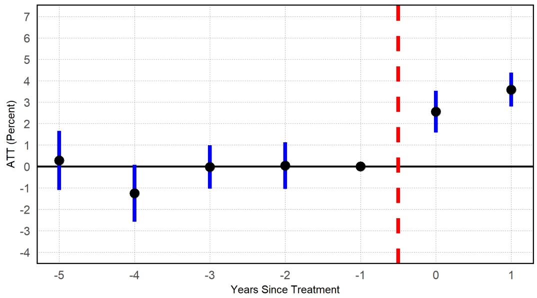


(d) Employment: California-Washington

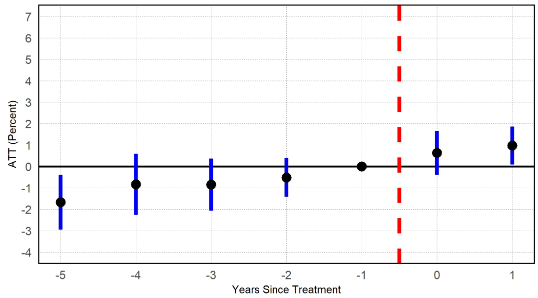
Figure 9: Event Studies for Second-Stage Outcomes of Prime Age Workers

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 $t \geq 1$. Prime age individuals are those between the age of 25 and 54 years. 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.

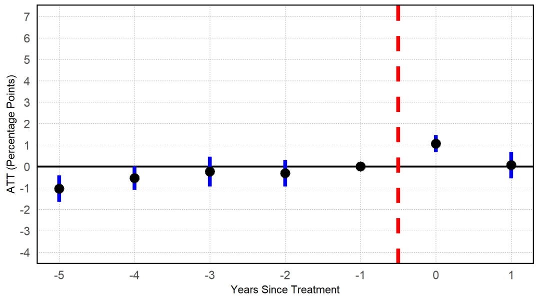
D.3 Private Sector + With Controls



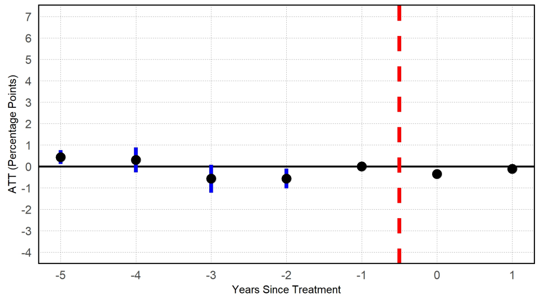
(a) Wages: Colorado



(b) Wages: California-Washington



(c) Employment: Colorado

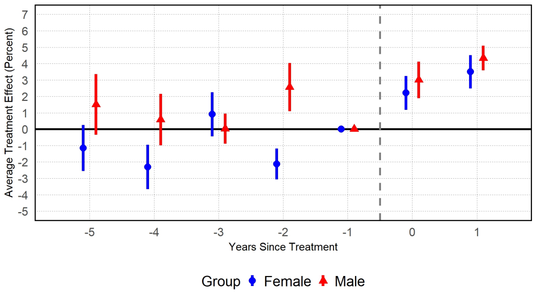


(d) Employment: California-Washington

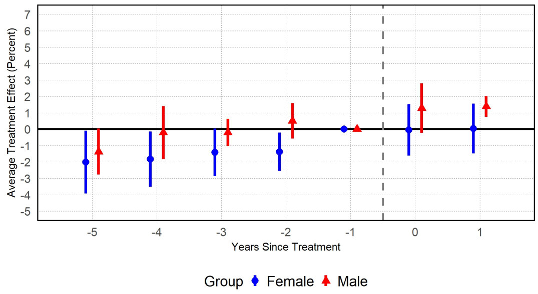
Figure 10: Event Studies for Second-Stage Outcomes Of Private Sector Workers

Notes: We use ACS data at the individual level. All ATT estimates are obtained via the Sun and Abraham (2021) estimator using demographic controls. In the event studies, $t = 1$ represents the binned time period $t \geq 1$. The private sector estimates are obtained using data on individuals employed in the private sector. For private sector employment, we redefine the outcome variable as 1 if employed in the private sector, and 0 otherwise. The blue lines represent 95% confidence intervals. All standard errors are clustered at the state level.

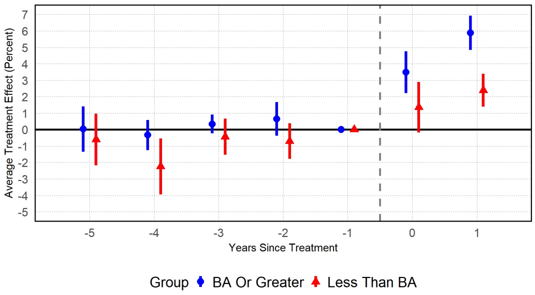
D.4 Heterogeneity Across Demographic Groups



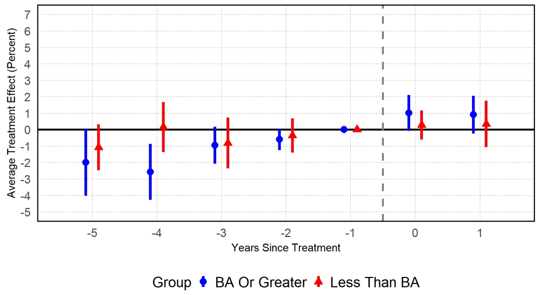
(a) Colorado: By Gender



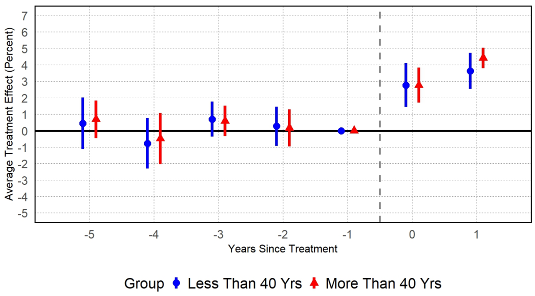
(b) California-Washington: By Gender



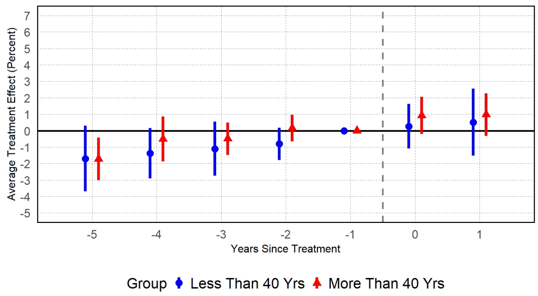
(c) Colorado: By Education



(d) California-Washington: By Education



(e) Colorado: By Age



(f) California-Washington: By Age

Figure 11: Event Studies For The Effect On Wages For Various Demographic Groups

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 gray 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.

E Results Using Two-Way Fixed Effects

In this section, we show that our results regarding the effect on wages due to pay transparency laws remain unchanged if we use the Two-Way Fixed Effects estimator (given by Equation 4) instead of the Sun and Abraham (2021) estimator (given by Equation 3). Table 7 provides the relevant ATT estimates for wages for both estimators using specifications with and without demographic controls.

E.1 Robustness With Sun And Abraham

$$y_{ist} = \lambda_s + \theta_t + \mathbf{\Gamma X}_{ist} + \sum_{i \notin C} \sum_{\tau \neq -1} \delta_{E,\tau} \cdot \mathbb{I}(i \in E_s) \cdot \mathbb{I}(t - t_s = \tau) + \nu_{ist}, \tag{3}$$

$$y_{ist} = \lambda_s + \theta_t + \mathbf{\Gamma X}_{ist} + \delta \cdot \mathbb{I}(Treated_s \times Post_t) + \nu_{ist} \tag{4}$$

Sample	All Employed (SA)	All Employed (SA)	All Employed (TWFE)	All Employed (TWFE)
Overall	1.469***	1.200***	1.484**	1.246**
	(0.256)	(0.203)	(0.544)	(0.477)
Colorado	4.373***	3.532***	4.520***	3.679***
	(0.396)	(0.381)	(0.381)	(0.359)
California +	0.680**	0.566***	0.866***	0.678**
Washington	(0.313)	(0.260)	(0.281)	(0.263)
Controls	No	Yes	No	Yes

Notes: N = 3.52 million for the overall regression estimates. We use ACS data at the individual level. All values are in percentage terms. SA = Sun and Abraham (2021) estimator, TWFE = Two-Way Fixed Effects. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. All standard errors (in parentheses) are clustered at the state level.

Table 7: Sun And Abraham v.s. Two-Way Fixed Effects

E.2 Bootstrapped Standard Errors

In our main analysis, we cluster our standard errors at the state level since that is also the level of our treatment. However, since we have a small number of clusters, there is still a chance that we find significant results when it is not true (i.e., when we tend to over-reject our null hypothesis of no significant effects). To resolve this issue, we obtain bootstrapped standard errors using the Wild Cluster Bootstrap method, which provides more reliable standard errors when there is a small number of clusters (MacKinnon et al., 2023).

In Table 8, we present the p-values from the baseline TWFE exercise and the p-values obtained using the boottest method in Stata developed by Roodman et al. (2019). We conclude that our results remain significant with bootstrapped standard errors.

	All Employed	All Employed	Prime Age Employed	Private Sector Employed
Coefficient	1.484**	1.246**	1.741***	1.231***
P-Value	0.016	0.021	< 0.001	< 0.001
P-value (Bootstrap)	0.003	< 0.001	0.003	0.001
Controls	No	Yes	Yes	Yes

Notes: We use ACS data at the individual level. All coefficient values in Table 8 are in percentages. All ATT estimates are obtained via the Two-Way Fixed Effects estimator using the refined control group. Prime age individuals are those between the age of 25 and 54 years. The private sector estimates are obtained after limiting the focus only to individuals employed in the private sector. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. The 'P-Value' row provides P-values obtained from standard errors after clustering at the state level. The 'P-value (Bootstrap)' row provides P-values calculated using the Wild Cluster Bootstrap method (MacKinnon et al., 2023) and implemented in Stata using the boottest package (Roodman et al., 2019).

Table 8: P-Values From Bootstrapped Standard Errors For The Wages ATT Estimates

E.3 Triple Difference-In-Differences And The Gender Wage Gap

In our heterogeneity analysis in the main text, we find suggestive evidence that the ATT on the wages of male workers was larger in magnitude than the ATT on female workers' wages. To test whether the difference in the wage effects for men and women is statistically significant, we present the results from a triple difference-in-differences specification in this section. Using this specification, we can formally examine whether cross-firm pay transparency helped close or widen the gender pay gap.

The regression specification for our triple difference-in-differences analysis is given in Equation 5 which is similar to the static Two-Way Fixed Effects specification in Equation 4:

$$y_{ist} = \lambda_s + \theta_t + \mathbf{\Gamma X}_{ist} + \delta_1 \cdot \mathbb{I}(Treated_s \times Post_t) + \delta_2 \cdot \mathbb{I}(Treated_s \times Post_t \times Female_i) + \nu_{ist} \tag{5}$$

Note that $Treated_s$ and $Post_t$ are absorbed by state and time fixed effects respectively. The state fixed effects λ_s and time fixed effects θ_t each include interactions with $Female_i$. We also include interactions of $Female_i$ with SOC-3 occupation fixed effects and NAICS-3 industry fixed effects in this specification.

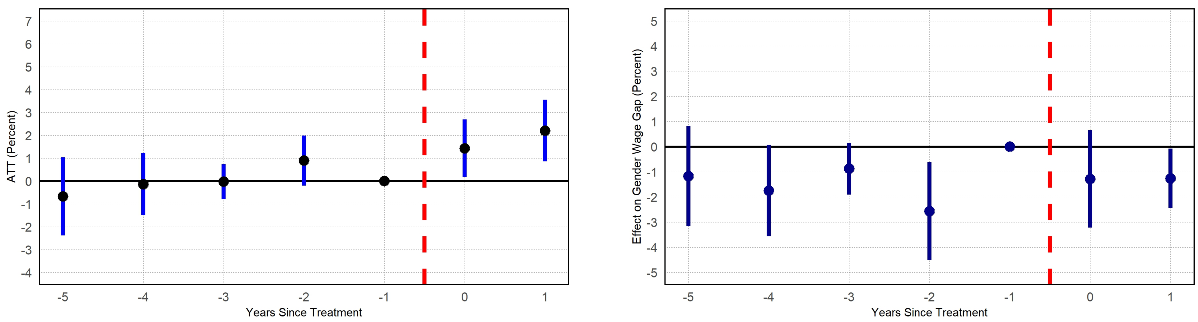
Table 9 shows the results for the triple difference-in-differences estimator. We also keep the ATT estimates from separate regressions on male and female workers for reference. Column 5 (labelled 'Wage Gap ATT') indicates that the difference in wage increases between men and women was statistically significant, which is consistent with inter-firm pay transparency widening the gender pay gap.

However, we do not interpret this as a causal effect. Figure 12 shows that the pre-trends for our event study on the gender wage gap do not satisfy the parallel pre-tends assumption, thereby preventing us from making causal statements about cross-firm pay transparency's effect on the gender pay gap.

	Separate Regressions For Men And Women (SA)			Triple Difference-in-Differences		
Sample	Male ATT	Female ATT	Implied Wage Gap ATT	Male ATT	Wage Gap ATT	Implied Female ATT
Overall	1.829***	0.518	-1.311	1.862***	-1.267**	0.595
	(0.325)	(0.377)		(0.399)	(0.543)	
Colorado	3.847***	3.061***	-0.786	4.017***	-0.873**	3.144
	(0.359)	(0.490)		(0.345)	(0.399)	
California &	1.279***	-0.169	-1.448	1.351***	-1.342*	0.009
Washington	(0.369)	(0.490)		(0.404)	(0.744)	

Notes: We use ACS data at the individual level. All values are in percentage terms. SA = Sun and Abraham (2021) estimator. Demographic controls include race, education, marital status, and age (quadratic) variables. Gender is excluded as a control from the SA regressions to prevent multi-collinearity. Implied Wage Gap ATT = Female ATT - Male ATT. Implied Female ATT = Male ATT + Wage Gap ATT. All standard errors (in parentheses) are clustered at the state level.

Table 9: ATT Estimates For The Effect On The Gender Wage Gap



(a) Main Effect

(b) Effect On The Gender Wage Gap

Figure 12: Event Studies For The Triple Difference-In-Differences Specification

Notes: We use ACS data at the individual level. All ATT estimates are obtained using a triple difference-in-differences specification with demographic controls. The main effect corresponds to the effect on wages of men. The gender wage gap corresponds to the difference between the wages of women and men. 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.

F Why Do We Observe Strong Wage Effects In Colorado?

In this section, we discuss possible reasons why the wage effects of pay transparency laws were stronger in Colorado compared to California and Washington. First, we provide evidence and arguments against certain prospective mechanisms. We conclude by discussing why we believe that this heterogeneity stems from prevailing labor market conditions.

F.1 Differences In First Stage Effects

A reason why Colorado may experience stronger wage effects is if the first stage effects were stronger in Colorado relative to California-Washington. However, as we see in Table 4 and Table 5, the first stage effects were quantitatively similar in these two treatment groups across all relevant outcomes, so it is an unlikely driver of the heterogeneous effects in wages.

F.2 Differences In Composition Of Occupations And Industries

Another reason for the differences in wage effects could be that Colorado has a different composition of occupations and/or industries. We check whether this is true using the two-sample Kolmogorov-Smirnov (KS) test. This method allows us to test whether two samples come from the same distribution or not. A large test statistic — and correspondingly small p-value (for example, $p < 0.1$) — provides evidence against the null hypothesis that the two distributions are identical.

We first consider three groups: (i) Colorado, (ii) California-Washington, (iii) States in our control group. For groups (i) and (ii), we calculate the share of job postings in each occupation and industry in the two-year time period pre-treatment and post-treatment using Lightcast data. We do the same for group (iii) for each time period considered for groups (i) and (ii). This gives us a distribution of job postings across occupations and industries for each group, pre-treatment and post-treatment. We repeat the exercise using ACS data to obtain distributions of employment shares across occupations and industries for each group.

Table 10 shows the results from our exercise. For example, using the KS test, we obtain a p-value of 0.785 when we test whether the pre-treatment employment distribution across occupations is different in Colorado compared to the control group. For each case we consider in Table 10, we get a p-value > 0.1. This implies that we cannot reject the null hypothesis that any two groups we consider have similar compositions of occupations and industries. Specifically, we conclude that Colorado is similar to California and Washington, so the heterogeneity in the wage effects is not driven by a difference in the composition of occupations and industries in these two groups.

F.3 Salary History Bans And Minimum Wage Laws

Salary history ban laws prevent employers from inquiring about a job applicant's previous salary during wage negotiations. These laws were implemented with the hope that a worker's wages are decided based on their potential match quality with their employer, rather than their previous salary history. This should allow low-wage workers to increase their wages if they are able to find a job that is a good match for them.

	Colorado vs Control Group		CA+WA vs Control Group		Colorado vs CA+WA	
	SOC3	NAICS3	SOC3	NAICS3	SOC3	NAICS3
I. Job Postings						
Lightcast (Pre)	0.999	0.999	0.992	0.962	0.999	0.962
Lightcast (Post)	0.987	0.999	0.999	0.962	0.965	0.962
II. Employment Share						
ACS (Pre)	0.785	0.906	0.999	0.994	0.958	0.906
ACS (Post)	0.888	0.967	0.999	0.994	0.992	0.999

Notes: We use Lightcast and ACS data. This table provides P-values obtained from a two sample Kolmogorov-Smirnov test, to test whether the composition of occupations and industries is different between two groups of states. CA = California, WA = Washington. For Colorado, the "Pre" period consists of 2019-2020, and the "Post" period consists of 2021-2022. For California-Washington, the "Pre" period consists of 2021-2022, and the "Post" period consists of 2023-2024.

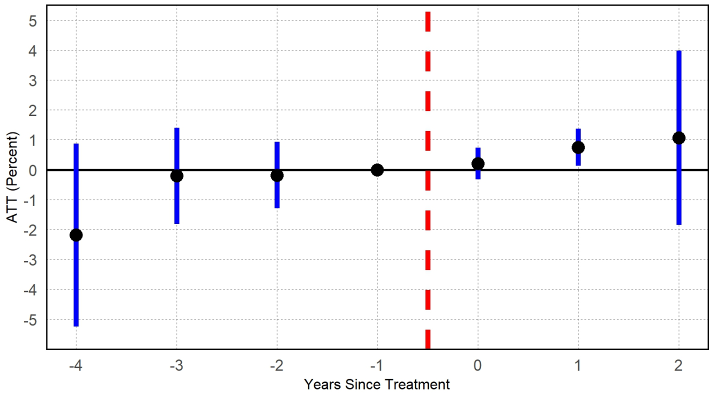
Table 10: P-Values From KS Tests For Industry And Occupation Distributions

Colorado also implemented a salary history ban in January 2021 as a part of its Equal Pay For Equal Work Act. It may be the case that a part of the increase in wages in Colorado is driven by positive wage effects due to the salary history ban, and a part of it is driven by the effect of the pay transparency laws.

To analyze the effect of salary history bans on wages, we again use the Sun and Abraham (2021) estimator. Our treatment group consists of states that implemented salary history ban laws between 2014 and 2018 (California, Delaware, Massachusetts, New Jersey, New York, Oregon, and Vermont). Our control group consists of states that implemented salary history ban laws after 2021 (Colorado, Nevada, and Rhode Island). We exclude states that implemented these laws between 2019 and 2020 (Alabama, Connecticut, Illinois, Maine, Maryland, Washington). We do this because the post-treatment period for these states will overlap with the post-treatment period for our control states, giving us biased estimates of ATT.[8]

We show in Figure 13 that salary history bans seem to have a minimal effect on wages (ATT = 0.306, SE = 1.361). If we believe that the salary history ban in Colorado will have similar wage effects as in the other states with the same law, then we can assert that the strong wage effects in Colorado are not driven by salary history bans.

Doing a causal analysis of minimum wage laws is outside the scope of this paper. However, we observe that college-educated workers in Colorado experience strong positive effects on their wages due to pay transparency laws. Table 2 in the main text indicates that the ATT for this group is 5.24%. Assuming that college-educated workers usually work in high-wage jobs, they would benefit little from minimum wage laws. Therefore, any increase in their wages post-treatment can be solely attributed to pay transparency laws.



Notes: For this analysis, we use ACS data and the Sun and Abraham (2021) estimator. 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.

Figure 13: Event Study For The Effect Of Salary History Bans On Wages

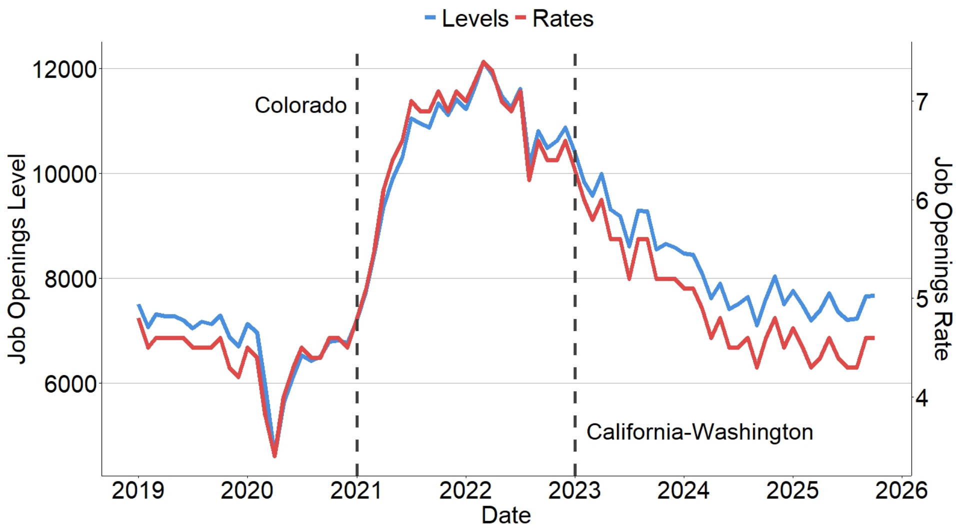


Figure 14: Time Series For Job Opening Levels And Job Opening Rates (JOLTS)

F.4 Labor Market Conditions

We believe that labor market conditions matter in driving the effects of pay transparency laws. Referring to Figure 14, we see that job openings increased post-Covid as employers increased their labor demand. Colorado implemented its pay transparency law during this period, on January 1, 2021 (given by the dashed line on the left in Figure 14). On the other hand, California and Washington implemented their pay transparency laws on January 1, 2023 (given by the dashed line on the right), when labor demand had started slowing down.

All workers across the US benefited from the increase in labor demand as real wages also increased during this time period. However, we propose that workers in Colorado were able to utilize the additional wage information in job postings to secure higher wage offers. On the other hand, California and Washington implemented their laws in a period of declining labor market activity. As hiring slows, there is less opportunity for workers to receive better wage offers even if this information is available in job postings.

Based on this logic, we form the hypothesis that the effect of pay transparency laws depends on the prevailing labor market conditions. When labor demand is low, workers are unable to enter wage negotiations and get better wage offers. On the other hand, when labor demand is high, workers are more likely to receive job offers where they can secure better wage offers by taking advantage of the wage information in job postings.

[8]We identify the states with salary history ban laws from Table 1 in Cullen (2024).

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