Effects of Pay Transparency Laws on the Labor Market*

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PRELIMINARY AND INCOMPLETE

Abstract

Pay transparency laws aim to improve the labor market conditions of workers who might have incorrect beliefs about their available options (Jäger et al., 2024). However, recent research shows that their impact is mixed and depends on the form of pay transparency laws implemented in different countries. Using online vacancy postings data from Lightcast, we analyze pay transparency reforms in the US that make it mandatory for firms to provide wage information in job postings. In states implementing these laws, we see a 25 percentage points increase in job postings specifying wage offers. Using the difference-in-differences estimator developed by Sun and Abraham (2021), we find that wages increase in Colorado by 3-4%, and by 0.5% in California and Washington. Our results indicate that workers can benefit from pay transparency laws that make wage information publicly available, in contrast to intra-firm pay transparency laws that lead to a negative effect on wages (Cullen and Pakzad-Hurson, 2023).

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

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1 Introduction

Pay transparency laws are considered an important tool in addressing wage inequality. It is believed that providing workers - especially those coming from disadvantaged backgrounds - with information about the salary levels of their peers can allow them to negotiate better wages for themselves. This is why, in the last few years, multiple countries have implemented some form of pay transparency laws, and economists have shown an interest in studying their effects and unintended consequences.

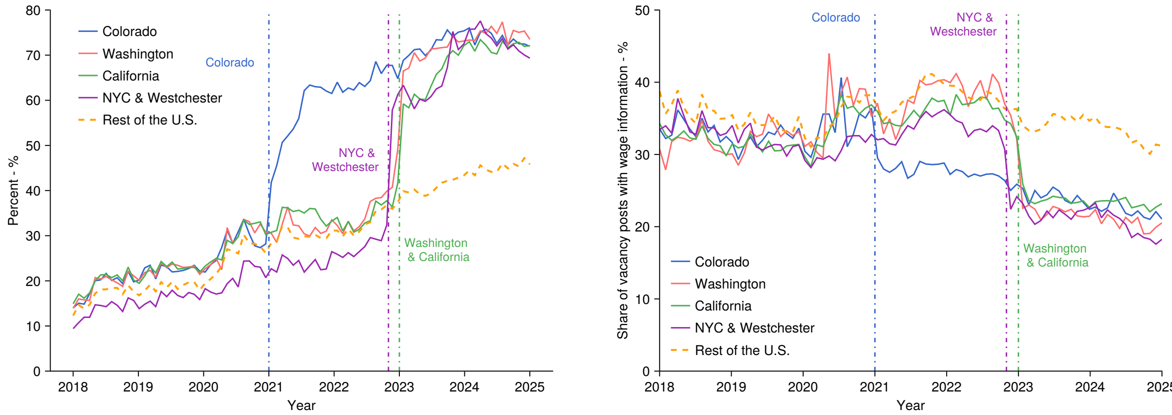
In this paper, we will study a specific type of pay transparency law that has been passed in multiple jurisdictions in the US. These laws make it mandatory for employers to provide wage information in job postings. Given the public nature of job postings, these laws can potentially be more effective in making pay information transparent, compared to other forms of pay transparency laws that only provide wage information of employees within a firm.

It might come as a surprise that in the US, only 30-40% job postings have wage information, and only 30% of the wage offers are in terms of a point wage rather than a wage range (Figure 1). Therefore, there is a lot of scope for improving the amount of information that employees have about the wage levels in their occupations. Providing more wage information in job postings can reduce the search costs for job seekers. Moreover, it can allow firms to observe what competing firms pay for the same job, leading to reduced wage dispersion (Cullen et al., 2022).

Using a dataset containing the near universe of job postings in the US since 2018, we find that after the implementation of these pay transparency laws in the states of Colorado, Washington, California, New Jersey, and New York, the proportion of job postings containing wage information increased by 25 percentage points. However, this alone might not imply that the wage information for job seekers necessarily improves. We find that now more job postings contain wage offers in the form of wage ranges instead of point wages, and there is no reduction in the median range width of these wage ranges.

Next, we consider the impact that these pay transparency laws have on various 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 wages in Colorado increase significantly by 3-4%, while wages in California and Washington increase by 0.5%, which is insignificant at the 95% confidence level. We also find only small, insignificant effects on the usual number of hours worked per week, and the probability of being employed in the treated states. Additionally, we find that there is a 5-8% decline in the number of vacancies in these states after pay transparency laws are implemented.

Jäger et al. (2024) show that German workers have inaccurate beliefs about their outside options, and hypothesize that pay transparency laws can benefit workers by providing them with more wage information. Cullen and Pakzad-Hurson (2023) analyzed intra-firm



(a) Share Of Job Postings With Wage Information (b) Share Of Wage Offers In Terms Of Point Wages

Figure 1: Trends In Wage Information Present In Job Postings

pay transparency laws in the US and found that they have a negative effect on wages. They theorize that as pay transparency can increase the probability of wage renegotiations, employers are increasingly likely to offer lower wages (*demand effect*), and workers are more likely to accept them as well (*supply effect*). However, we propose that the reason why we find positive effects from our pay transparency laws is because wage information in job postings is publicly available, so employers who post low wage offers will lose out as workers will direct their search to higher paying jobs. A Bertrand-like competition between employers for workers would ensue, leading to a positive effect on wages overall.

Our analysis also contributes to the search and matching models in the macro labor literature. In baseline wage posting models, it is assumed that workers have correct beliefs about the wage distribution, which allows them to optimally accept or reject wage offers, or direct their search accordingly. However, this might not be true if firms don't post wages (as we show in Figure 1). In such a scenario, our results indicate that workers benefit from receiving more wage information. Additionally, the impact of receiving more wage information may depend on labor market conditions. We hypothesize that the strong effects on wages in Colorado might be due to the tight labor market conditions in 2021 when the state implemented its pay transparency law. On the other hand, we find insignificant effects in California and Washington due to the weak labor market in 2023, when these states implemented their pay transparency laws.

Section 2 contains a brief review of the relevant literature on pay transparency laws. Section 3 provides details about the laws studied in our paper. Section 4 describes our dataset and data cleaning procedure. Section 5 documents our empirical strategy, and Section 6 shows our results. Section 7 concludes.

2 Literature Review

Our paper contributes to the recent literature on pay transparency laws. Studies have been conducted on pay transparency reforms in multiple countries such as Denmark (Bennedsen et al., 2022), Canada (Baker et al., 2023), UK (Duchini et al., 2020), Austria (Böheim and Gust, 2021; Gulyas et al., 2023), Germany (Seitz and Sinha, 2023; Brütt and Yuan, 2022; Ahrens and Scheele, 2022), and the US (Mas, 2017; Burn and Kettler, 2019; Obloj and Zenger, 2022; Sinha, 2022).

These papers show that the effect of pay transparency laws is mixed, primarily depending on the information employers reveal. Bennedsen et al. (2022) analyze the effects of a 2006 law in Denmark, which requires all private sector firms with more than 35 employees to report salary statistics broken down by gender to their employees. They find a 1.9 percentage point drop in the gender wage gap (13% fall relative to the pre-treatment mean). Interestingly, a slowdown in the wage growth of men - rather than an improvement in women's wages - drives this change.

Baker et al. (2023) observe a similar pattern in Canada. They study pay transparency laws that require the public disclosure of the salaries of individual faculty members of universities if they surpass a certain threshold. These laws lead to a 20-30% decrease in the gender wage gap in the short run, which increases to 25-40% in the long run.[1] Like Bennedsen et al. (2022), they observe that a slowdown in male wage growth is the major contributor to this fall in some empirical specifications. Duchini et al. (2020) find a related pattern in the UK, where from 2017 onwards, firms with more than 250 employees had to publish gender equality indicators online (for example, the percentage gaps in mean and median hourly pay). This law led to a 19% decline in the gender wage gap, driven by a 2.9% fall in the wage growth for men.

In other countries, pay transparency laws have not had a significant impact. Böheim and Gust (2021) and Gulyas et al. (2023) in the context of Austria, and Seitz and Sinha (2023), Brütt and Yuan (2022), and Ahrens and Scheele (2022) in the context of Germany find no changes in the gender wage gap after the implementation of pay transparency laws. These papers argue that the effectiveness of these laws depends on the change in workers' beliefs about their position in the wage distribution in their occupation. If employees do not ask their employers for wage information (as in Germany), or if this information does not change their prior beliefs about the wage distribution (as in Austria), then their job search behavior will not change, and we will not see any effects in the labor market.

Cullen and Pakzad-Hurson (2023) makes an important contribution to this literature by analyzing the impact of pay transparency laws in a general equilibrium setting. Accounting for the reactions not only of workers, but also of firms, they are able to give a theoretical explanation for why average wages drop after the introduction of pay transparency laws. In

[1] Obloj and Zenger (2022) find a similar effect when the salary data for SU academics is made available online.

environments where workers have more bargaining power, higher pay transparency reduces firms' willingness to pay for labor to avoid costly renegotiations. Using an event study framework, they show that US states that implemented pay transparency laws in the past experienced a negative effect on wages.

Why do our results differ from those of Cullen and Pakzad-Hurson (2023)? The main point of departure seems to be the type of pay transparency laws that are studied. Cullen and Pakzad-Hurson (2023) analyzes pay transparency within firms. However, requiring pay transparency in job postings increases the accessibility of a firm's wage offers not only to current employees, but also to potential job applicants and employers outside the firm. If employers compete with each other to attract workers, this could lead to a form of Bertrand competition, driving up wages without any unintended consequences for workers.

The papers discussed above till now focused primarily on laws that affected pay transparency within firms. A few countries have also implemented laws that require employers to provide wage information in job postings. Skoda (2022) finds that mandating pay information in job postings led to a 3% increase in the wages of newly hired workers in Slovakia. Frimmel et al. (2023) analyze a similar law in Austria and find that the gender wage gap reduced due to an increase in women's wages, while the earnings of men remained unaffected.

We complement Skoda (2022) and Frimmel et al. (2023) by analyzing the effects of mandating wage information in job postings in the US. Our contribution to this research question is methodological - Since pay transparency laws in the US are implemented at the state or county level, this gives us a clear treatment group and a comparable control group, so that we can make causal statements and estimate the average treatment of pay transparency laws accurately. Irrespective of the empirical methodology, our conclusion remains the same: pay transparency in job postings improves wages, especially for women.

The closest papers to ours are Arnold et al. (2022) and Feng (2024). They focus on the effect of the Equal Pay For Equal Work Act (EPEWA) passed in January 2021 in Colorado. They find that this law increased the proportion of job postings containing explicit wage offers in Colorado by 30-40 percentage points. This led to an increase in average wages by 1.3-3.6% and a reduction in the gender wage gap by 4-11%. We expand their analysis by including other jurisdictions that have passed similar laws (California, New Jersey, New York, and Washington) in our sample. Our results are similar for Colorado, however, we also find that pay transparency laws seem to be ineffective in California and Washington. This finding highlights the importance of testing for heterogeneous effects across different treated units, and is our main contribution to the work done by Arnold et al. (2022) and Feng (2024).

3 Laws Regarding Pay Transparency In Job Postings

Pay transparency laws are widely perceived as useful tools in tackling wage discrimination Cullen (2024). The aim is to "*...ensure that employees with similar job duties are paid the same* *wage rate regardless of sex, or sex plus another protected status."*[2]

In the past, multiple countries and US states have passed labor laws to tackle wage discrimination.[3] These laws can take different forms: (i) preventing employers from penalizing or discriminating against employees and job applicants who enquire or discuss the salary range in their jobs (US, Germany, etc.), (ii) requiring mandatory reports of salary statistics based on race, ethnicity, and gender at regular intervals (Denmark, Austria, UK, etc.), (iii) banning employers from making employment decisions based on a candidate's wage history (US, Canada).

Requiring employers to include wage information in job postings is not new. Countries like Latvia, Slovakia, Austria, and Lithuania already have such a law. Colorado was the first state/ jurisdiction in the US to introduce similar legislation. This law was signed in May 2019 and implemented on January 1, 2021. The provision of interest is the following:

> *"An employer shall disclose in each posting for each job opening the hourly or salary compensation, or a range of the hourly or salary compensation, and a general description of all of the benefits and other compensation to be offered to the hired applicant."*

Many other states and counties soon followed suit. Table 1 provides essential details about the pay transparency laws in the states we will consider in our study. All these jurisdictions require employers to explicitly state a point wage or wage range that they would offer to a job applicant in good faith, but they can differ in the details.[4]

The first significant difference is in the coverage of the laws. Colorado requires all employers to provide wage information in job postings, but California and Washington only require firms with more than 15 employees to do so. Some jurisdictions - Washington D.C. and Westchester County - make an exception for government jobs. To prevent confusion, some jurisdictions (like Colorado and California) explicitly specify when an employer outside the jurisdiction needs to provide wage information (for example, in the case of remote jobs). Additionally, a few jurisdictions require employers to provide descriptions of all non-monetary benefits and compensation (Colorado, Washington, Illinois).

Other forms of pay transparency laws limit the disclosure of salary statistics to current employees or job applicants. However, requiring pay transparency in job postings provides wage information to a broader audience. Job postings make wage information accessible not only to job applicants and current employees of an employer but also to the general public and other employers. For this reason, policymakers believe that pay transparency in job postings can also reduce the search costs of job seekers and employers.

[2] Colorado Senate Bill 19-085, Equal Pay For Equal Work Act .

[3] These include Canada, U.K., Germany, Denmark, and Austria among others. Refer to Cullen (2024) for more details.

[4] The US Congress has also introduced a similar bill (H.R. 1599 Salary Transparency Act) that will apply to all employers all across the US. This law has not yet been passed in Congress as of the date we are writing this paper.

Table 1: Pay Transparency Laws Pertaining To Job Postings In Different Regions

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.
Illinois	Jan 1, 2025	Employers with at least 15 employees.

Over time, they can also lead to improved matches, as job seekers have a better knowledge of the salary and compensation that different employers provide for the same kind of work, and they can direct their search accordingly. In the short run, we might see higher job separations as workers switch jobs for better wages and amenities. However, in the medium run, improved matches should lead to longer tenures and fewer separations.

We want to highlight an important provision of the pay transparency laws that we study. Even though the law requires employers to provide wage information, they are not required to mention point wage offers alone. Employers can provide a range of wages they expect to pay for a job. This provision can lead to an unintended consequence where employers report wage ranges that are so large that they do not provide any relevant information to job seekers. We will test whether this hypothesis is true in Section 6.

4 Data

We primarily use two datasets for our empirical analysis. To analyze the change in wage-related information in job postings, we use Lightcast's (formerly Burning Glass Technologies) US job postings dataset. For our analysis on the consequent impact of this change in wage information on real labor market outcomes, we use microdata from the American Community Survey (ACS).

4.1 Lightcast Data

Lightcast collects data on the near universe of job postings in the US and extracts meaningful information from them (location, wage offers, skills required, employer name, etc.). This dataset is quite useful for our analysis since the pay transparency laws target the wage information in job postings, and through this data we can study how wage information changes after the implementation of these laws. Based on the magnitude of changes in wage information in different places, we can formulate hypotheses about the effectiveness of pay transparency laws in affecting labor market outcomes, and any heterogeneity in these changes across locations.

For our analysis, we use microdata on over 207 million online vacancy postings from 2018 until 2024 from Lightcast. While we have Lightcast data from the beginning of 2010, we only use data from 2018 onward for our analysis. This is because in 2018, there was a major structural change in Lightcast's scraping algorithm, which resulted in significant changes to the underlying pool of online job boards from which data on individual vacancy postings are extracted.[5]

We also make several significant modifications to the Lightcast data to address measurement issues. We first apply several layers of cleaning that remove observations as laid out in Table 2. While most steps are standard in the literature, we make two additional changes. Notably, we remove vacancies that are posted *exclusively* to the website Craigslist.org, which is a classified advertisements website where casual and/or one-off jobs, which are fundamentally different from standard jobs, are often posted. We only remove the observation if it is posted only on Craigslist to mitigate the risk of incorrectly removing a 'standard' job.[6]

[5] See A.1 for a detailed discussion.

[6] 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 Vacancies	% 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: 1. 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. 2. 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 2: Number of Job Postings in Lightcast Data (2018 - 2024)

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. (2023) and reclassifying jobs that contain specific strings, such as "estimated wage", as not having any wage information.

Secondly, we modify vacancies 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 publicly available, pre-specified pay schedule, which outlines how pay varies across dimensions such as the successful applicant's experience and education, that all applicants can access.

Employers cannot specify any salary information on the vacancy 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.[7]

Lastly, we find that Lightcast's algorithm incorrectly classifies a significant share of vacancy postings as not having any wage information. There are two possible reasons for this. First, Lightcast reports the annual advertised salary of a vacancy, yet there are some niche occupations where reporting an annual salary is difficult since their pay is variable across time.[8] 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. The second possible reason behind

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

[8]For example, truck drivers are paid in terms of cents per mile, and salesmen/women are paid in terms of commission.

the presence of false negatives is that the way pay information is presented is not standardized across job boards, introducing significant difficulties for Lightcast's wage extraction algorithm.[9]

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 vacancy 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 vacancies out of approximately 114 million that Lightcast classified as not having any wage information, implying that the false negative error rate was approximately 7.49%.[10]

4.2 ACS Data

The American Community Survey (ACS) is a survey of a representative sample of respondents from the US that collects data on an individual's demographic information and labor market outcomes, among other things. An advantage of using ACS data over other datasets (like the Current Population Survey) is its large sample size (approximately 3.5 million individuals each year), giving us more precise estimates of the average treatment effects that we are interested in.

From ACS, we collect information on demographic variables (gender, marital status, education, race, and age) and labor market outcomes (wages, employment status, and hours worked). For location data, we use an individual's reported state and county of work. We also use information on an individual's industry and occupation using NAICS 3-digit and SOC 3-digit codes, respectively.[11]

We limit our analysis to individuals between 18 and 64 years of age, and we remove those who are self-employed or report working in military and unclassified occupations. Our sample period starts from 2016 and ends in 2023.[12]

[9] For example, based on manually inspecting the body of a vacancy, we find that use abbreviations like '50K-60K' instead of '$50,000-$60,000' tend to confound Lightcast's algorithm.

[10] Our code to address this issue can be found here. Further details on how the algorithm works can be found here. 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.

[11] The NAICS and SOC systems are revised once every few years. Therefore, we use the following crosswalks provided by IPUMS for SOC codes and NAICS codes to keep them as consistent as we can over time. (https://usa.ipums.org/usa/volii/occsoc18.shtml and https://usa.ipums.org/usa/volii/indnaics.shtml)

[12] At present, 2023 is the latest year for which ACS data is available. We plan to include data from the 2024 ACS sample when it becomes available in late 2025 as well.

5 Empirical Strategy

Our estimation strategy compares the change in labor market outcomes in states implementing pay transparency laws to those states where such laws were not implemented. Therefore, we use a difference-in-difference design using Two-Way Fixed Effects (TWFE) in our baseline specification to estimate the effect of pay transparency laws on various outcome variables:[13]

Equation 1 provides us with estimates of the average treatment effect on various outcomes. Here, y_{igt} is our outcome of interest, λ_g denotes a set of geographical fixed effects, and θ_t denotes time fixed effects. We also allow for covariates, denoted by $\mathbf{X}_{igt}$, and $\mathbb{I}(t - t_g = \tau)$ is an indicator variable for the time since treatment date t_g.[14] Our object of interest is δ, which gives us the average difference in the outcome variable between the treated states and the untreated states in the post-treatment period, i.e., when pay transparency laws were implemented ($\tau \geq 0$).

$$y_{igt} = \lambda_g + \theta_t + \Gamma \mathbf{X}_{igt} + \delta \sum_{\tau \geq 0} \mathbb{I}(t - t_g = \tau) + \nu_{igt} \tag{1}$$

Equation 1 provides us with ATT estimates, but we need to do more to establish the validity of these estimates. A key property to verify in difference-in-difference studies is the similarity of the outcome trends in our treatment and control groups. If we can establish that the outcome values for the treatment and control groups have similar trends pre-treatment, we can credibly use the post-treatment outcome trend for the control group to create a counterfactual post-treatment trend for the treatment group. We can do so by modifying Equation 1 as follows:

$$y_{igt} = \lambda_g + \theta_t + \Gamma \mathbf{X}_{igt} + \sum_{\tau=-k}^{-2} \delta_\tau \mathbb{I}(t - t_g = \tau) + \sum_{\tau=0}^{l} \phi_\tau \mathbb{I}(t - t_g = \tau) + \nu_{igt} \tag{2}$$

Equation 2 splits the ATT estimate δ from Equation 1 into an ATT estimate for each period. Here, ϕ_τ give us the average post-treatment effect of pay transparency laws in each period after the law was passed. On the other hand, the δ_τ values give us a sense of the similarity of trends in the outcome for the treatment and control groups, relative to a reference period ($t = -1$ in our case). If δ_τ values are consistently significant, it implies that outcome trends move differently in the treatment and control groups, so the control group cannot act as a good counterfactual for the treatment group, and this can bias our ATT estimates. However, if the δ_τ values are insignificant, it means that the outcome variable grows in a similar way in both the treatment and control group. This is called the parallel

[13] Arnold et al. (2022) uses a difference-in-difference specification with firm fixed effects and other interactions on individual vacancy posts. However, this relies on firm names being clean. See Appendix A for a detailed discussion.

[14] For example, if $t_g = 2021$ and $\tau = 2$, then $\mathbb{I}(t - t_g = \tau) = 1$ for observations in 2023 that were treated in 2021. Since the control group is never treated, $\mathbb{I}(t - t_g = \tau) = 0$ for observations in the control group, for all values of τ.

pretrends condition, which is necessary for difference-in-differences to give unbiased ATT estimates.

In our analysis, we first aim to understand the change in the amount of wage information available in job postings after pay transparency laws were implemented. In this first stage analysis, we use the Lightcast job postings dataset and aggregate it to the state-occupation-industry-month level. Then, we analyze several outcomes, which include (i) the proportion of job postings containing wage offers, (ii) the proportion of point wage offers among all wage offers, (iii) the median range width among wage offers that provide a range, (iv) a wage information index that combines outcomes (i)-(iii) in a single measure, and (v) posted wages.

We use Equation 1 to get the first-stage ATT estimates and Equation 2 to test the parallel pretrends condition. In both cases, λ_g denotes state-level fixed effects, and θ_t accounts for month fixed effects. We also control for the occupation and industry of the job postings, using the 3-digit SOC and NAICS classification systems.

We use a similar approach for our second stage analysis, where we try to understand the changes in real labor market outcomes due to the change in wage information in job postings. For this, we will use ACS data at the individual level and focus on the following outcomes: (i) real wages in the 12 months before the interview, (ii) usual number of hours worked per week, and (iii) employment status (whether the respondent was employed or not at the time of the interview). For the second state analysis, λ_g denotes state or county-level fixed effects, and θ_t are year fixed effects. We also include 3-digit SOC and NAICS code variables, and we report estimates for specifications with and without demographic control variables (gender, marital status, age (quadratic), education, and race). For both the first-stage and second-stage estimates, we use clustered standard errors at the state level.

A recent line of research acknowledges that standard TWFE estimators might not correctly estimate the average treatment effect on the treated (ATT) when (i) we have staggered treatment (ii) more than 2 time periods, and (iii) parallel trends conditional on covariates (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Goodman-Bacon (2021) discusses how TWFE estimators in staggered settings make *"forbidden comparisons"*, where early-treated groups are used as control groups for later-treated groups. Sun and Abraham (2021) also show that pre-trend TWFE estimates can be written as a linear combination of pre-treatment and post-treatment effects. Therefore, the test of parallel pre-trends is contaminated by post-treatment effects and, it cannot be used reliably. We address these concerns about TWFE estimators by also showing that our results are consistent after using a new event study estimator proposed by Sun and Abraham (2021) that can estimate the unbiased ATT in a setting with heterogeneous and dynamic treatment effects.

To use the Sun and Abraham (2021) estimator, we first define E_g to denote groups of all the observations that started receiving the treatment at the same time, and C as the group of all observations that never receive the treatment, i.e., the control group. We make the following changes to the TWFE specification in Equation 2:

$$y_{igt} = \lambda_g + \theta_t + \boldsymbol{\Gamma}\mathbf{X}_{igt} + \sum_{i \notin C} \sum_{\tau \neq -1} \delta_{E,\tau} \mathbb{I}(i \in E_g) \cdot \mathbb{I}(t - t_g = \tau) + \nu_{igt} \tag{3}$$

For example, in our second stage analysis, all individuals working in Colorado will be in one group (let's call it $G2021$), since they all started getting affected by Colorado's pay transparency law 2021 onwards. Similarly, we construct $E_g = G2022$ (which contains Jersey City and the New York counties treated in 2022), and $E_g = G2023$ (which contains California, Washington, and the New York counties treated in 2023). We will report estimates for $\delta_{E,\tau}$ from Equation 3 for both the first-stage and second-stage analysis.[15]

6 Results

We divide our analysis into two parts. In the first stage, we analyse whether wage information in job postings actually increases in jurisdictions where pay transparency laws are implemented. We find that more job postings contain wage information, but there is also a rise in job postings that contain wage ranges instead of point wages as wage offers. We see that the median range width remains unaffected, however, posted wages (measured by the midpoint of a wage range) increase by 4-5%.

In the second stage, we use ACS data to analyse the effect of pay transparency laws on various labor market outcomes. We primarily focus on the effect on wages change since reducing wage dispersion (specifically, the gender wage gap) is the primary objective of these pay transparency laws. Across different specifications, we consistently find that average wages in Colorado increase by approximately 3-4%, and this is driven by an increase in the wages of women and non-white individuals.

6.1 First Stage: Wage Information In Job Postings

In the first stage, we analyze the change in wage information present in job postings after pay transparency laws were implemented in the states of Colorado, California, Washington, New Jersey, and New York. We start by looking at the amount of wage information present in job postings before and after these laws are passed. Letting y_{gt} be the proportion of job postings having wage information, we use Equation 1 to get our Two Way Fixed Effects estimates and Equation 2 to test for the parallel pretrends condition.

Figure A.8a shows due to the pay transparency laws, 25 p.p. more job postings contain wage information in the treated states relative to the untreated states. If we consider the pre-treatment mean of approximately 30% job postings containing wage information, then this means that the pay transparency laws led to an 83% increase in the number of job postings containing wage information.

[15] To make estimates from Equations 2 and 3 comparable, we will use the same specification for both, the only difference being the definition of the ATT estimates (δ_τ and ϕ_τ in Equation 2), and $\delta_{E,\tau}$ in Equation 3.

Even though the wage information in job postings has increased, we still don't know how precise this information is. The pay transparency laws only require that firms should provide wage information, but this information is not restricted to be a point wage offer. To incentivize firms to comply with the laws, they are allowed to post wage ranges, and no limit is put on the size of these ranges. Therefore, in theory, firms could post unrealistically large wage ranges to comply with the laws without providing any wage information to job seekers.

	Wage Info Share		Point Offers Share		Median Range Size		Wage Info Index		Log(Posted Wages)		Log(Vacancies)	
	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA
Overall	24.950***	25.380***	-7.654***	-7.248***	0.003	-0.020	21.788***	22.120***	5.184***	4.749***	-8.083***	-5.868**
	(1.844)	(0.527)	(0.771)	(0.429)	(0.382)	(0.286)	(1.676)	(0.532)	(0.814)	(0.216)	(2.556)	(2.280)
Without NY, NJ	24.253***	25.800***	-7.164***	-8.073***	0.352	0.239	21.048***	22.359***	5.820***	5.179***	-10.380***	-6.839**
	(1.143)	(0.623)	(0.880)	(0.568)	(0.417)	(0.363)	(1.023)	(0.653)	(1.043)	(0.258)	(3.108)	(3.368)
Colorado	25.599***	27.361***	-6.708***	-8.207***	-0.304*	-0.469**	22.664***	24.069***	8.575***	7.733***	-6.420***	-5.456***
	(0.421)	(0.586)	(0.395)	(0.450)	(0.177)	(0.189)	(0.353)	(0.520)	(0.400)	(0.371)	(0.723)	(0.592)
CA & WA	23.583***	24.573***	-7.326***	-7.864***	0.583	0.806	20.233***	21.004***	4.747***	3.073***	-12.638***	-7.797
	(1.341)	(0.892)	(1.173)	(0.907)	(0.519)	(0.807)	(1.007)	(1.006)	(0.414)	(0.377)	(3.520)	(6.209)

Notes: The above tables use Lightcast job postings data, which is aggregated up to the state-SOC3-NAICS3-month level. All values are in percentage point terms, except for the estimates for posted wages and vacancies, which are in percentage terms. TWFE = Two-Way Fixed Effects, SA = Sun & Abraham. Row 1 contains estimates when we consider all treated groups together. Row 2 provides estimates after removing New York and New Jersey, as we show that these states add noise to our second-stage estimates. Please refer to the appendix for more details. Row 3 gives estimates for the treatment effects in Colorado, and Row 4 gives estimates for California and Washington, which were treated together in 2023. $* \Rightarrow p < 0.1, ** \Rightarrow p < 0.05, *** \Rightarrow p < 0.01$. All standard errors (in parentheses) are clustered at the state level.

Table 3: Average Treatment Effects of Pay Transparency Laws On Job Postings

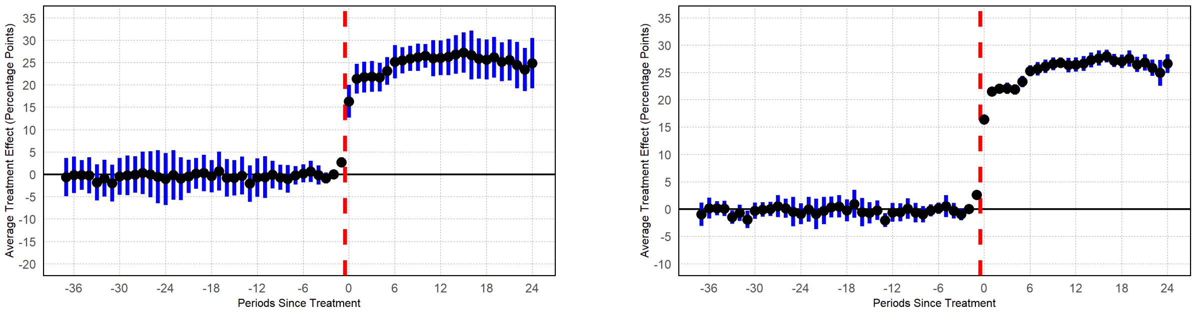
Figures A.3a and A.4a show the impact on the precision of wage information found in job postings.[16] Figure A.3a shows that after the laws are passed, the proportion of wage offers in terms of point wages reduces by 7.6 percentage points (approximately a 20% decrease relative to the pre-treatment mean). This implies that even though the wage information in job postings increases, a higher proportion of this wage information is in the form of wage ranges rather than point wages. However, if we consider the size of these ranges in Figure A.4a, we do not see any significant effect on the median range width.

We also construct a wage information index to combine the effects seen in Figures A.8a-A.4a in a single figure. This index is defined as follows:

$$\text{Index} = \begin{cases} 0, & \text{if no wage information present} \\ 1, & \text{if point wage offer} \\ 1 - \frac{\text{upper bound - midpoint}}{\text{midpoint}} \in (0,1), & \text{if wage range offer} \end{cases}$$

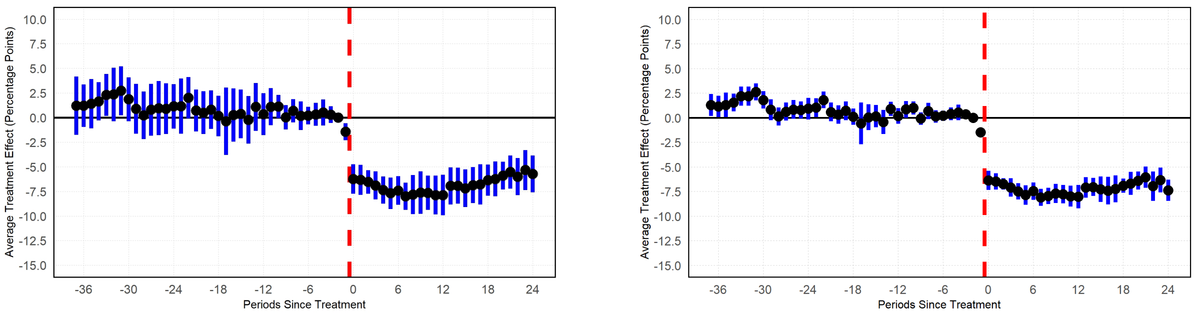
Looking at the effect of pay transparency laws on our constructed index in Figure A.3b, we can say that wage information increased by 21.8 p.p after the implementation of these

[16]For these figures, we restrict our analysis to only those job postings that contain some form of wage information.



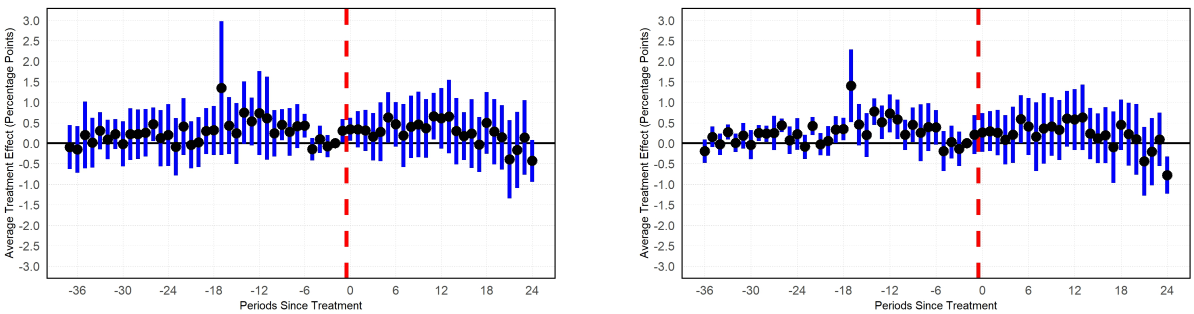
(a) Two Way Fixed Effects (b) Sun & Abraham

Figure 2: Wage Information In Job Postings Increases by 25 p.p.



(a) Two Way Fixed Effects (b) Sun & Abraham

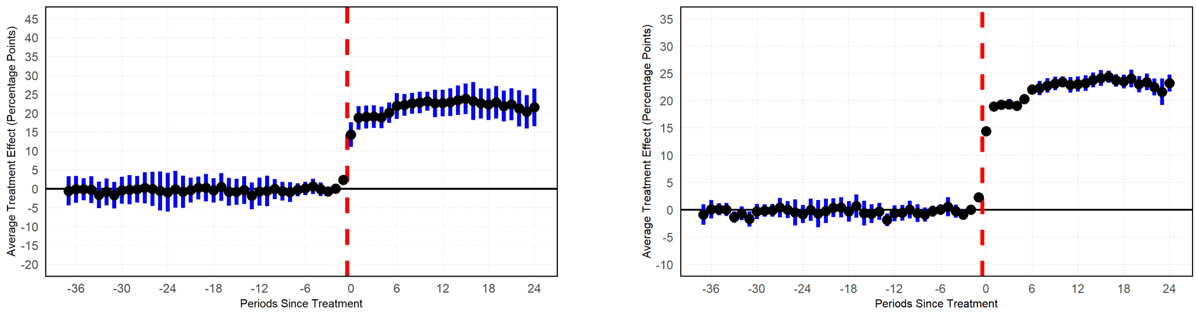
Figure 3: The Proportion Of Wage Offers In Terms Of Point Wage Offers Falls By 7 p.p.



(a) Two Way Fixed Effects (b) Sun & Abraham

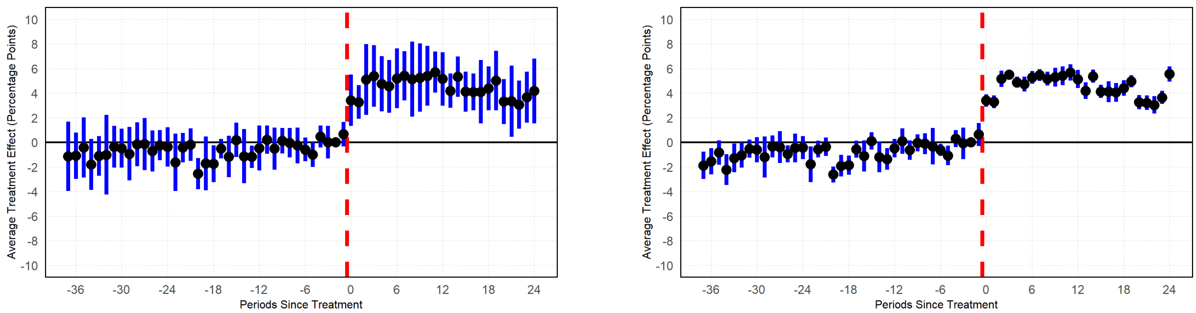
Figure 4: The Median Wage Range Width Remains Unaffected By Pay Transparency Laws.

Notes: The above figures use Lightcast job postings data, which is aggregated up to the state-SOC3-NAICS3-month level. Wage information in job postings is defined as the proportion of job postings in a group containing some wage information in the body text. Point wage offers are defined as wage offers that only report a single number as the offered wage in the job posting. Their proportion is calculated with respect to the number of all wage offers in a group, not all job postings in a group. A wage range offer is defined as a wage offer containing a minimum and maximum value, and the range width is calculated as the difference between the two. Period $t = 0$ is the month when a pay transparency law is implemented in a state. We use $t = -2$ as the reference period, as we observe some anticipation effects in $t = -1$. All standard errors are clustered at the state level.



(a) Two Way Fixed Effects (b) Sun & Abraham

Figure 5: Our Wage Information Index Increases by 22 p.p. Due To Pay Transparency Laws.



(a) Two Way Fixed Effects (b) Sun & Abraham

Figure 6: Posted Wages (Measured By The Midpoint Of Wage Ranges) Increase by 4-5%.

Notes: The above figures use Lightcast job postings data, which is aggregated up to the state-SOC3-NAICS3-month level. Our wage index is defined as given in Section 6.1. We define posted wages as either the value of the point wage offer, or the midpoint of a wage range offer. Period $t = 0$ is the month when a pay transparency law is implemented in a state. We use $t = -2$ as the reference period, as we observe some anticipation effects in $t = -1$. All standard errors are clustered at the state level.

laws.[17]

Along with increasing the presence of wage information in job postings, there also seems to be an increase in the level of wages that are posted. We measure the level of posted wages by either the value of the point wage offer, or the midpoint of a wage range offer. As Figure A.6a shows, posted wages increase by 5% on average.

As discussed in the previous section, the Two Way Fixed Effects estimator could give us biased estimates if the treatment effects are dynamic and heterogeneous across treated groups. To address this concern, Figures A.5b-A.6b show us that the Sun & Abraham estimator gives quite similar results to the TWFE Estimator. Moreover, the Sun & Abraham estimator consistently gives us more precise estimates compared to the TWFE estimator.

Table 3 summarizes all the important ATT estimates from our first stage analysis. As we can observe here, the first stage effects seem to be relatively homogeneous across different treatment groups. The results are also invariant to the choice of our estimator. Figures A.8a-A.6b also give us confidence that are estimates are valid, since we get parallel pretrends in all the figures we show here.[18]

For each of our outcome variables above, we consider multiple robustness checks by modifying our preferred empirical specification. We experiment with different definitions of our group and time fixed effects. We switch between the state and county level geography, monthly and yearly frequency, occupations defined at the SOC 2-digit up to SOC 6-digit levels, and industries defined at the NAICS 2-digit up to NAICS 5-digit levels. We find that our results hold across different possible combinations of fixed effects.

6.2 Second Stage: Labor Market Outcomes

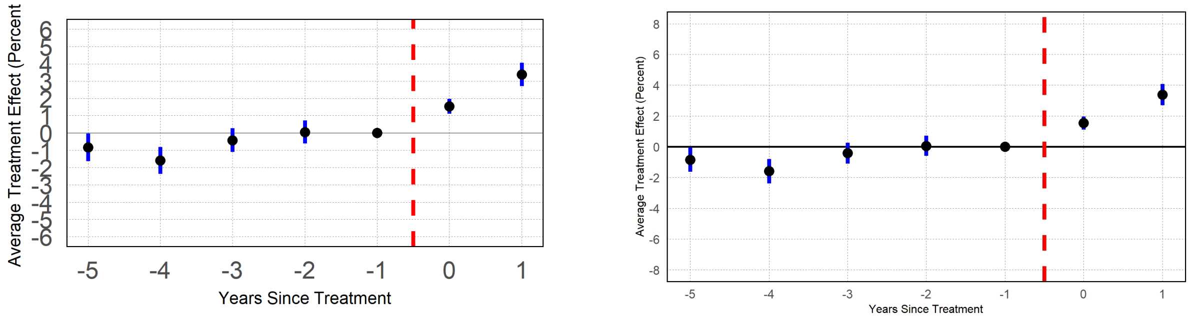
Next, we analyze how the change in wage information in job postings impacts workers' actual labor market outcomes. For this purpose, we use ACS data and study the impact on pay transparency laws on wages, hours worked, and employment status.

Wages In The Last 12 Months

Our outcome variable is wages earned by a respondent in the last 12 months. Table 4 provides our main result: Our TWFE estimates show that there is a 2-4% increase in wages on average in the treated states relative to the untreated states. The ATT estimates for Colorado are similar if we use the Sun & Abraham estimator instead of the TWFE estimator. However, the effect on wages turns out to be insignificant when we look at the Sun & Abraham estimates for Washington and California. As the Sun & Abraham estimator is

[17] However, this increase in wage information does not mean that it will be useful for job searchers. Our data cannot answer whether this new information is precise enough to improve the accuracy of the beliefs of individuals about the wage distribution in their occupation. To answer this question, we would need to collect data on workers' beliefs about the wage distribution and compare that with the wage offers we see in job postings.

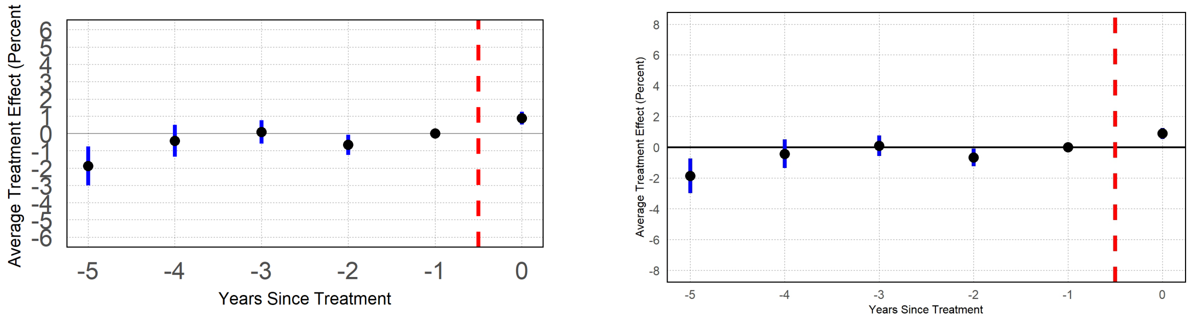
[18] For event study plots for the treated states separately, please refer to the Appendix.



(a) Colorado: TWFE

(b) Colorado: SA

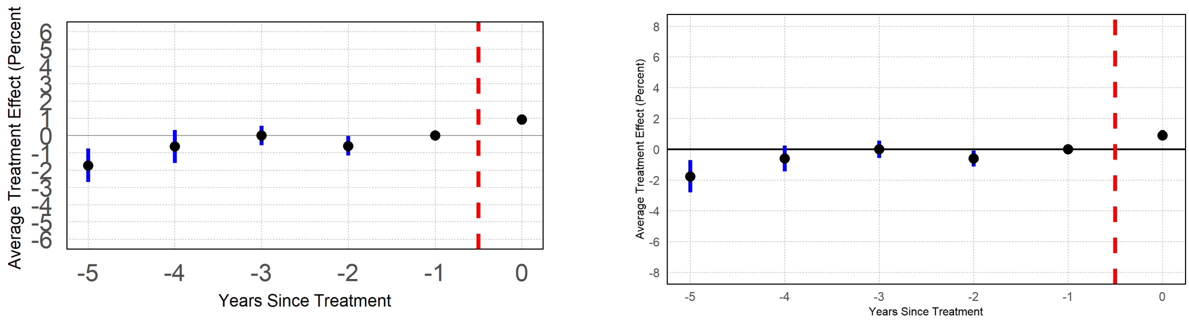
Figure 7: Wages Increase In Colorado By 3-4%



(a) California & Washington: TWFE

(b) California & Washington: SA

Figure 8: Wages Increase In California And Washington by 0.5-3%



(a) Combined: TWFE

(b) Combined: SA

Figure 9: Wages Increase Overall By 1.5-3%

Notes: The event studies above are constructed using data from the ACS. The outcome variable is wages earned by a respondent in the 12 months before the survey date. The year before pay transparency laws are implemented is taken as the reference period ($t = -1$). The blue lines are confidence intervals at the 95% significance level. We exclude New York and New Jersey from the analysis since their event studies don't satisfy the parallel pretrends assumption, and they add noise to our estimates. Please refer to the appendix for more details. Please refer to the appendix for more details. TWFE = Two Way Fixed Effects, SA = Sun & Abraham estimator. Standard errors are clustered at the state level.

	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA
Overall	0.861	1.720**	1.100	1.965**	1.476	1.733**	1.746*	1.935**
	(1.101)	(0.243)	(1.110)	(0.263)	(0.931)	(0.238)	(0.972)	(0.258)
Without NY, NJ	2.497***	1.544**	2.783***	1.544**	2.901***	1.452**	3.244***	1.632**
	(0.432)	(0.172)	(0.365)	(0.172)	(0.451)	(0.164)	(0.408)	(0.172)
Colorado	3.344***	2.816**	3.327***	3.302***	3.771***	3.248***	3.868***	3.857***
	(0.310)	(0.290)	(0.281)	(0.311)	(0.336)	(0.309)	(0.327)	(0.331)
CA & WA	2.356***	0.561	2.757***	0.571	2.789***	0.502	3.230***	0.502
	(0.403)	(0.359)	(0.377)	(0.383)	(0.426)	(0.336)	(0.417)	(0.336)
State FE	✓	✓	✓	✓				
County FE					✓	✓	✓	✓
Demographic Controls	✓	✓			✓	✓		
SOC3 + NAICS3 FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The above tables use ACS data at the individual level. All values are in percentage terms. TWFE = Two-Way Fixed Effects, SA = Sun & Abraham. Row 1 contains estimates when we consider all treated groups together. Row 2 provides estimates after removing New York and New Jersey, as we show that these states add noise to our second-stage estimates. Please refer to the appendix for more details. Row 3 gives estimates for the treatment effects in Colorado, and Row 4 gives estimates for California and Washington, which were treated together in 2023. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. $* \Rightarrow p < 0.1, ** \Rightarrow p < 0.05, *** \Rightarrow p < 0.01$. All standard errors (in parentheses) are clustered at the state level.

Table 4: Average Treatment Effects of Pay Transparency Laws On Realized Wages

supposed to correct for the bias in TWFE estimates, we prefer these estimates over the TWFE estimates.

The validity of our estimates can be seen in Figures 7a-9b. First, we see that the coefficients before treatment ($t \leq -1$) are wither insignificant or quite close to 0. This provides evidence of parallel pretrends for Colorado, the treatment group containing California and Washington, and the treatment group containing all treated states. Second, similar to the ATT estimates in Table 4, we see a strong rise in coefficients for wages in Colorado, with a smaller, but still positive effect on wages in California and Washington.

One observation of interest is the comparison between the ATT for posted wages and the ATT for realized wages in Tables 3 and 4. For each treatment group, the ATT for realized wages is less than the ATT for posted wages. This indicates that even after receiving more information on wage offers, there are search frictions or the potential for bargaining present, so realized wages do not increase one-for-one as posted wages.

Our results also indicate the value of more wage information for workers. If workers had enough information on wage offers to form correct beliefs about the wage distribution, then we should have seen no effects of pay transparency laws on wages. However, the increase in wages after pay transparency laws implies that workers find the additional wage information in job postings useful, and use it in their job search or wage bargaining process to improve

	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA
Overall	-0.504**	0.243**	-0.463*	0.252**	-0.381**	0.214**	-0.333*	0.226**
	(0.252)	(0.072)	(0.263)	(0.067)	(0.183)	(0.071)	(0.192)	(0.065)
Without NY, NJ	-0.317***	-0.131	-0.244***	-0.099	-0.282***	-0.140	-0.201**	-0.102
	(0.080)	(0.099)	(0.077)	(0.090)	(0.082)	(0.098)	(0.083)	(0.089)
Colorado	-0.325***	0.055	-0.246***	0.184	-0.333***	0.046	-0.235***	0.199
	(0.047)	(0.123)	(0.045)	(0.124)	(0.049)	(0.126)	(0.052)	(0.128)
CA & WA	-0.329***	-0.163	-0.252***	-0.172	-0.277***	-0.171*	-0.195**	-0.182*
	(0.098)	(0.113)	(0.096)	(0.105)	(0.096)	(0.112)	(0.098)	(0.105)
State FE	✓	✓	✓	✓				
County FE					✓	✓	✓	✓
Demographic Controls	✓	✓			✓	✓		
SOC3 + NAICS3 FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The above tables use ACS data at the individual level. All values are in percentage terms. TWFE = Two-Way Fixed Effects, SA = Sun & Abraham. Row 1 contains estimates when we consider all treated groups together. Row 2 provides estimates after removing New York and New Jersey, as we show that these states add noise to our second-stage estimates. Please refer to the appendix for more details. Row 3 gives estimates for the treatment effects in Colorado, and Row 4 gives estimates for California and Washington, which were treated together in 2023. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. $* \Rightarrow p < 0.1, ** \Rightarrow p < 0.05, *** \Rightarrow p < 0.01$. All standard errors (in parentheses) are clustered at the state level.

Table 5: Average Treatment Effects of Pay Transparency Laws On Usual Hours Worked

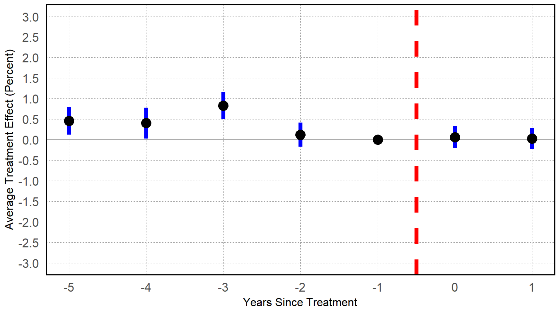
their labor market outcomes.

Usual Hours Worked

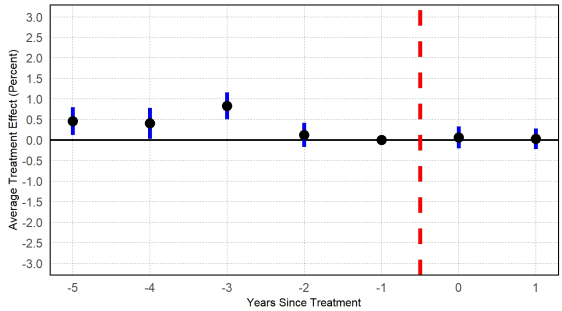
The goal of pay transparency laws was to improve the wages of disadvantaged individuals who could benefit from more wage information to use in their wage bargaining process. However, other labor market outcomes can change too. ACS also collects information on the usual number of hours a person works per week, and we analyze whether this is affected by pay transparency laws.

As table 5 shows, our TWFE estimates are consistently negative, between -0.2% to -0.5%. However, assuming on average a worker works 35 hours, this translates to a 4-10 minutes reduction in the amount of time worked per week. Moreover, our estimates from the Sun and Abraham estimator are even smaller in magnitude and insignificant in most cases. Therefore, we conclude that hours worked are unaffected due to pay transparency laws.

We test the validity of our ATT estimates in Table 5 using Figures 10a-12b. We observe that the pre-treatment coefficients are either insignificant, or close to 0. This implies that we can be confident that our ATT estimates satisfy the parallel pretrends condition.

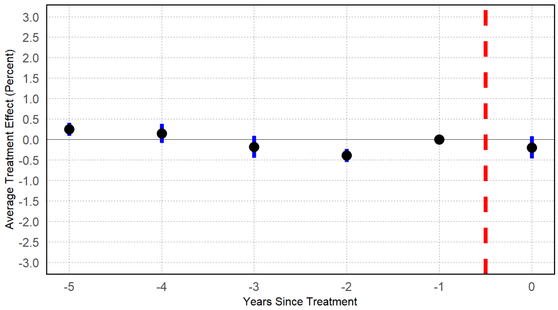


(a) Colorado: TWFE

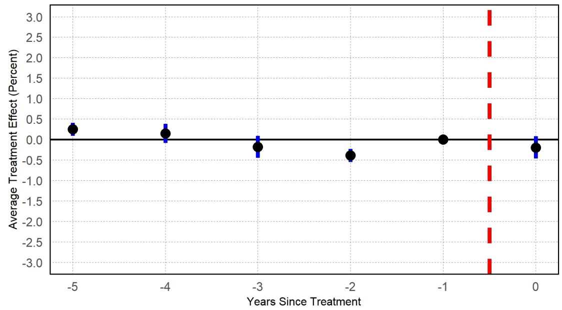


(b) Colorado: SA

Figure 10: The Effect On Hours Worked In Colorado

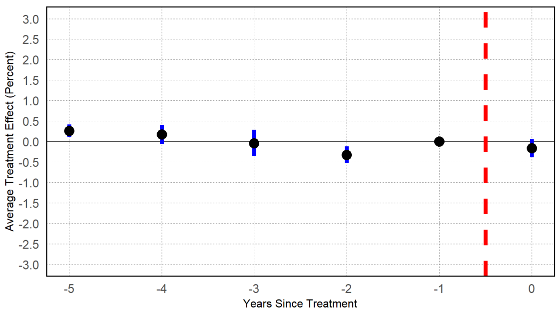


(a) California & Washington: TWFE

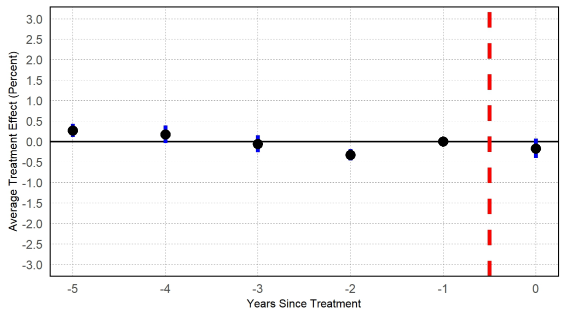


(b) California & Washington: SA

Figure 11: The Effect On Hours Worked In California And Washington



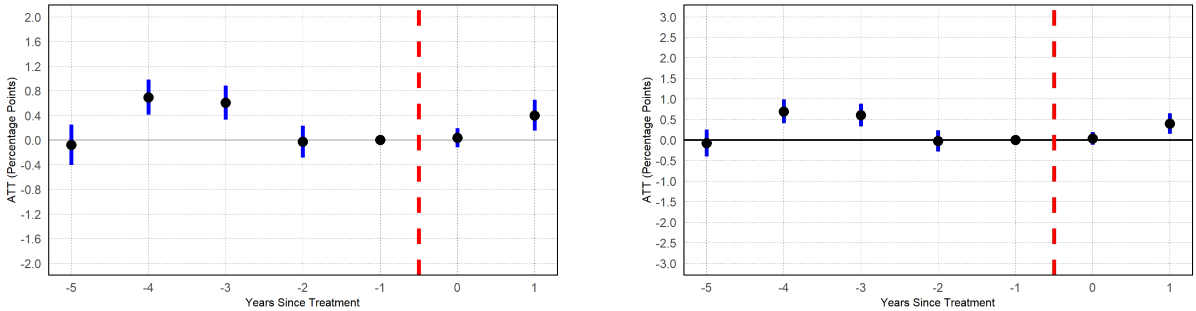
(a) Combined: TWFE



(b) Combined: SA

Figure 12: The Effect On Hours Worked In All Treated States

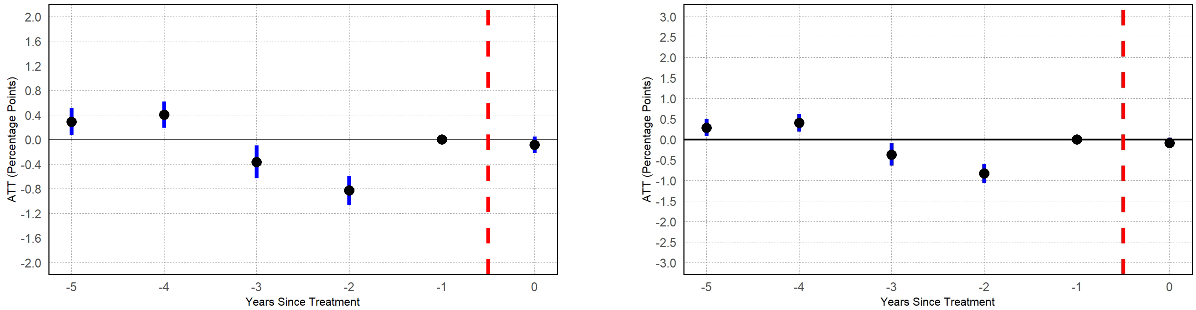
Notes: The event studies above are constructed using data from the ACS. The outcome variable is the number of hours usually worked by a respondent in a week. The year before pay transparency laws are implemented is taken as the reference period ($t = -1$). The blue lines are confidence intervals at the 95% significance level. We exclude New York and New Jersey from the analysis since their event studies don't satisfy the parallel pretrends assumption, and they add noise to our estimates. Please refer to the appendix for more details. Please refer to the appendix for more details. TWFE = Two Way Fixed Effects, SA = Sun & Abraham estimator. Standard errors are clustered at the state level.



(a) Colorado: TWFE

(b) Colorado: SA

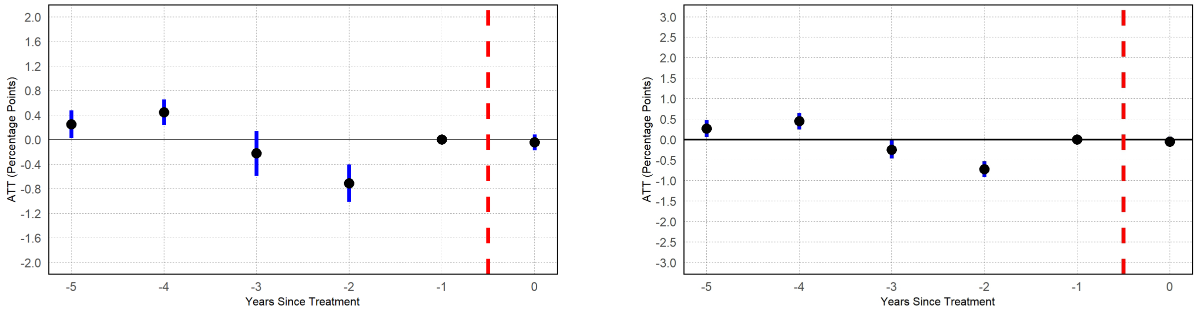
Figure 13: The Effect On Employment In Colorado



(a) California & Washington: TWFE

(b) California & Washington: SA

Figure 14: The Effect On Employment In California And Washington



(a) Combined: TWFE

(b) Combined: SA

Figure 15: The Effect On Employment In All Treated States

Notes: The event studies above are constructed using data from the ACS. The outcome variable is the employment status of the respondent on the survey date (1 = employed, 0 = not employed). The year before pay transparency laws are implemented is taken as the reference period ($t = -1$). The blue lines are confidence intervals at the 95% significance level. We exclude New York and New Jersey from the analysis since their event studies don't satisfy the parallel pretrends assumption, and they add noise to our estimates. Please refer to the appendix for more details. Please refer to the appendix for more details. TWFE = Two Way Fixed Effects, SA = Sun & Abraham estimator. Standard errors are clustered at the state level.

	TWFE	SA	TWFE	SA	TWFE	SA	TWFE	SA
Overall	-0.564	-0.432**	-0.692	0.036	-0.626	0.271**	-0.682	-0.071*
	(0.391)	(0.063)	(0.463)	(0.057)	(0.847)	(0.093)	(0.900)	(0.066)
Without NY, NJ	0.009	-0.443**	-0.008	0.049	-0.409	0.539**	-0.457	-0.130*
	(0.079)	(0.059)	(0.128)	(0.057)	(0.871)	(0.084)	(0.936)	(0.069)
Colorado	0.041	-0.029	0.231***	0.140	0.538	1.275*	0.678	-0.219
	(0.076)	(0.311)	(0.088)	(0.191)	(0.683)	(0.643)	(0.736)	(0.084)
CA & WA	-0.243	-0.028	-1.080***	0.151	-0.715	1.499**	-0.809	-0.263**
	(0.257)	(0.331)	(0.095)	(0.202)	(0.892)	(0.683)	(0.948)	(0.105)
State FE	✓	✓	✓	✓				
County FE					✓	✓	✓	✓
Demographic Controls	✓	✓			✓	✓		
SOC3 + NAICS3 FE								

Notes: The above tables use ACS data at the individual level. All values are in percentage terms. TWFE = Two-Way Fixed Effects, SA = Sun & Abraham. Row 1 contains estimates when we consider all treated groups together. Row 2 provides estimates after removing New York and New Jersey, as we show that these states add noise to our second-stage estimates. Please refer to the appendix for more details. Row 3 gives estimates for the treatment effects in Colorado, and Row 4 gives estimates for California and Washington, which were treated together in 2023. Demographic controls include gender, race, education, marital status, and age (quadratic) variables. $* \Rightarrow p < 0.1, ** \Rightarrow p < 0.05, *** \Rightarrow p < 0.01$. All standard errors (in parentheses) are clustered at the state level.

Table 6: Average Treatment Effects of Pay Transparency Laws On Employment Status

Employment Status

We also test whether more wage information in job postings leads to a higher chance of being employed. Theoretically, unemployed individuals would be able to search for jobs more efficiently if more wage information is available to them. This could reduce their unemployment duration and make it more likely that they are employed. Similarly, individuals who are not in the labor force could be encouraged to search for work due to more wage information and higher wage offers being available to them.

The ATT estimates in Table 6 indicate that we cannot reject the null hypothesis of there being no change in employment for various specifications. Figures 13a-15b indicate the same. However, our pretrends are not perfect. Some pretrends are significantly different from 0, but their magnitude lies within 1 p.p. in absolute terms. This implies that even if the pretrends for the treated and control group deviate, they don't do so by a large amount.

Robustness Checks

Our results are robust to various changes in the regression specifications. As Tables 4-6 show, our results remain consistent irrespective of whether we use TWFE or the Sun and Abraham estimator. Our results also do not depend on the choice of state or county fixed

	Gender		Education		Race	
	Male	Female	Less Than BA	BA Or Higher	White	Non-White
Overall	2.329***	1.077**	1.384***	2.245***	1.473***	1.471***
	(0.220)	(0.437)	(0.278)	(0.317)	(0.282)	(0.298)
Without NY, NJ	1.694***	1.019***	0.860***	2.164**	1.646***	0.824***
	(0.263)	(0.319)	(0.205)	(0.237)	(0.172)	(0.282)
Colorado	2.921***	2.560***	1.194***	4.865***	3.795***	-0.293
	(0.437)	(0.216)	(0.385)	(0.251)	(0.269)	(0.401)
CA & WA	1.150***	-0.063	0.605**	0.545	-0.136	0.935***
	(0.278)	(0.730)	(0.278)	(0.514)	(0.393)	(0.338)

Notes: The above tables use ACS data at the individual level. All values are in percentage terms. We report the ATT estimates from the Sun and Abraham estimator, for a regression specification that contains state, SOC3, NAICS3, and year fixed effects. The regression specification also includes demographic controls (excluding the ones based on which the subsamples are defined). Row 1 contains estimates when we consider all treated groups together. Row 2 provides estimates after removing New York and New Jersey, as we show that these states add noise to our second-stage estimates. Please refer to the appendix for more details. Row 3 gives estimates for the treatment effects in Colorado, and Row 4 gives estimates for California and Washington, which were treated together in 2023. $* \Rightarrow p < 0.1, ** \Rightarrow p < 0.05, *** \Rightarrow p < 0.01$. All standard errors (in parentheses) are clustered at the state level.

Table 7: ATT of Pay Transparency Laws On The Wages For Different Subsamples

effects, or whether we include or exclude demographic covariates.[19]

Heterogeneity In Effects Across Different Groups

Here, we also show results to test the heterogeneous effects across various demographic groups in the ACS data. Table 7 summarizes our results. We find that the effect on wages are quite similar for male and female workers. However, in Colorado, white workers benefit significantly more from pay transparency laws than non-white workers.

We also find that more highly educated employees benefit more from the additional wage information. This could be true because occupations requiring higher skills are also the jobs that did not provide wage information pre-treatment. Therefore, higher-educated workers are more likely to benefit from the additional wage information available now due to pay transparency laws.

7 Conclusion

In this project, we have analyzed how wage information in job postings changes after firms are required to comply with pay transparency laws in the US. We find that after these laws

[19] Please refer to Appendix A.4 for the event studies for different regression specifications.

are passed, more job postings contain wage information. However, this wage information is more likely to be in terms of wage ranges rather than point wage offers. Moreover, we find that posted wages increase by 4-5% on average.

Our next step is to look at the effect of pay transparency laws on labor market outcomes. We find that wages increase by 3-4% on average in Colorado, while our Sun and Abraham estimates suggest that wages in California and Washington are unaffected. Other labor market outcomes like hours worked and employment status do not change by much due to pay transparency laws. Our results for Colorado are similar to those found by Arnold et al. (2022) and Feng (2024), improving our confidence that people in Colorado definitely benefit from pay transparency in job postings.

Our ATT estimates are different to the ones derived by Cullen and Pakzad-Hurson (2023). They find that intra-firm pay transparency laws lead to a decline in wages, as firms reduce their wage offers and workers accept the reduced wage offers if there is a scope to renegotiate wages in the future. One reason why we find positive or insignificant effects - and not negative effects - could be because pay transparency in job postings provides information about a firm's willingness to pay to a larger audience, including other employers and workers outside the firm. If a firm tries to reduce its wage offers in reaction to the pay transparency laws, job applicants could easily apply to other firms that offer higher wages. This could lead to a Bertrand-like competition for workers among employers, leading to higher wages instead of lower wages.

Our Sun and Abraham estimates suggest that wages increase in Colorado by 3-4%, while there is no significant effect on wages in California and Washington. One reason why this might happen is due to labor market conditions at the time of implementation. Colorado implemented its pay transparency law in January 2021, around the time when the labor market was tight, so workers had more bargaining power and they could take advantage of the additional wage information to get better wage offers. However, California and Washington implemented their pay transparency laws in January 2023, when they labor market was beginning to slow down. As firms reduced their hiring around this time, workers couldn't benefit from the additional wage information in job postings to renegotiate their wages.

Our setting also related to job search models used to study macro labor questions. In the baseline DMP or directed search models, job searchers are assumed to have full information about the wage distribution, so they can rationally determine whether to accept/reject a wage offer or decide where to direct their job search. However, our results, along with Jäger et al. (2024), suggest that workers have incorrect beliefs about the wage distribution and they can benefit from receiving more wage information. We will try to show this by developing a search model with incomplete information, where an exogenous increase in the amount of wage information received by job searchers leads to more efficient search and better labor market outcomes.

At this time, multiple states and even the US federal government are working on implementing laws to require some form of pay transparency in job postings. We believe that our analysis can help policymakers understand the impact of pay transparency laws on the U.S. labor market so that they can improve the effectiveness of these laws in achieving their intended goals.

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A Appendix

A.1 Addressing Concerns About Wage Information In Lightcast Data

We want to address a few issues in the Lightcast data highlighted by recent papers. Batra et al. (2023) use Lightcast data from 2012-2017 to show that only 13.5% job postings have wage information (either as point offers or ranges). They also find a 520% spike in the number of annual job postings in 2018 relative to 2017, along with a doubling of the fraction of postings with wage information.

Batra et al. (2023) attribute this increase to the emergence of job boards that include their own estimates of wage offers in job postings. Callaci et al. (2023) and Lafontaine et al. (2023) 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 retrospective changes when necessary. These retrospective 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 (like job boards or company websites). Figure A.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 A.1 also shows that these sources contain more wage information.[20] 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 do.

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. (2023) and Lafontaine et al. (2023) 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 A.1 shows the proportion of wage postings for each major job source that contains wage information after we take care of the imputed wages issue.

These findings suggest that the increase in the number of job postings and the increase

[20] 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 considered job postings coming from a single job source.

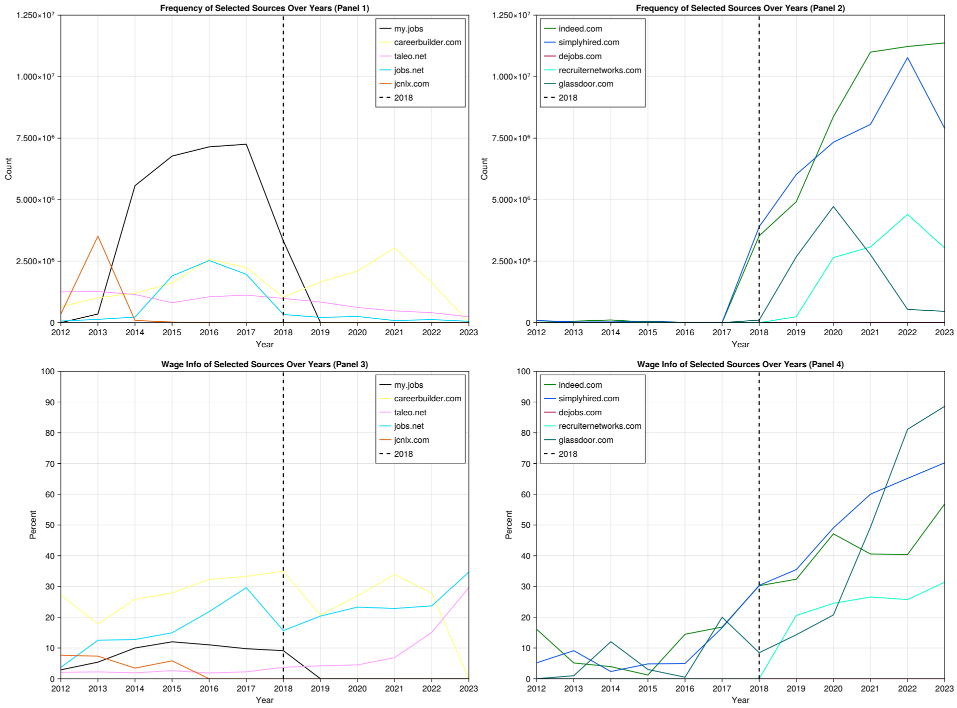


Figure A.1: Change in Composition Of Job Sources And Wage Information Post-2018

in wage information post-2018 is driven by the inclusion of data from websites like Indeed and Simply Hired. We agree with the conclusion of Batra et al. (2023) 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. (2023) and Lafontaine et al. (2023), we remove postings with imputed wages, but we find that the spike in the wage information in job postings after 2018 remains.

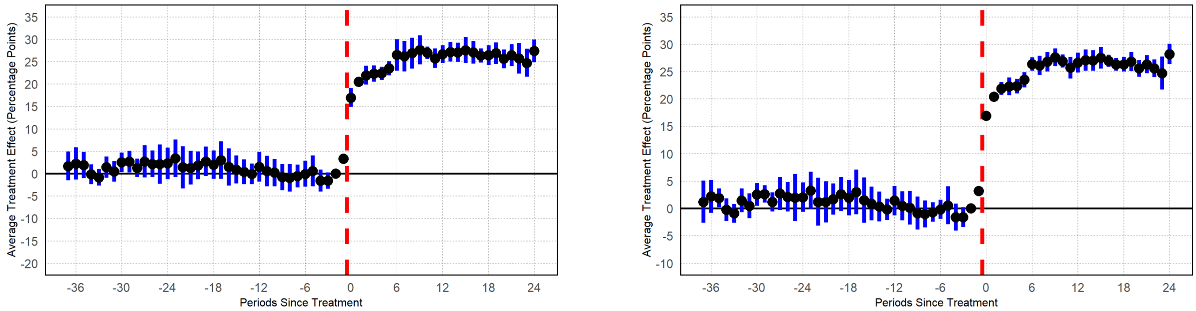
A.2 Summary Statistics (ACS)

Variable	2017	2018	2019	2020	2021	2022	2023
Female	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Married	0.49	0.49	0.48	0.49	0.49	0.49	0.49
Age							
18-24	0.15	0.15	0.15	0.15	0.15	0.15	0.15
25-34	0.22	0.22	0.23	0.23	0.22	0.22	0.22
35-44	0.21	0.21	0.21	0.21	0.22	0.22	0.22
45-54	0.21	0.21	0.20	0.20	0.20	0.20	0.20
55-64	0.21	0.21	0.21	0.21	0.21	0.21	0.21
Race							
White	0.72	0.72	0.71	0.62	0.60	0.60	0.59
Black	0.13	0.13	0.13	0.13	0.13	0.13	0.13
Other	0.15	0.15	0.16	0.25	0.27	0.27	0.28
Education							
High School	0.61	0.60	0.60	0.59	0.59	0.59	0.58
Bachelors	0.30	0.31	0.31	0.33	0.33	0.34	0.34
Labor Market							
Employed	0.72	0.73	0.74	0.71	0.72	0.75	0.75
Private Sector	0.66	0.67	0.65	0.65	0.65	0.66	0.66
40+ Weeks	0.67	0.67	0.69	0.66	0.65	0.69	0.70
35+ Hours	0.62	0.62	0.63	0.63	0.62	0.64	0.64
Total	1,920,398	1,924,001	1,921,823	1,570,074	1,909,987	1,972,948	1,977,297

Notes: The above table provides summary statistics for our ACS sample for the period 2017-2023. 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, while 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.

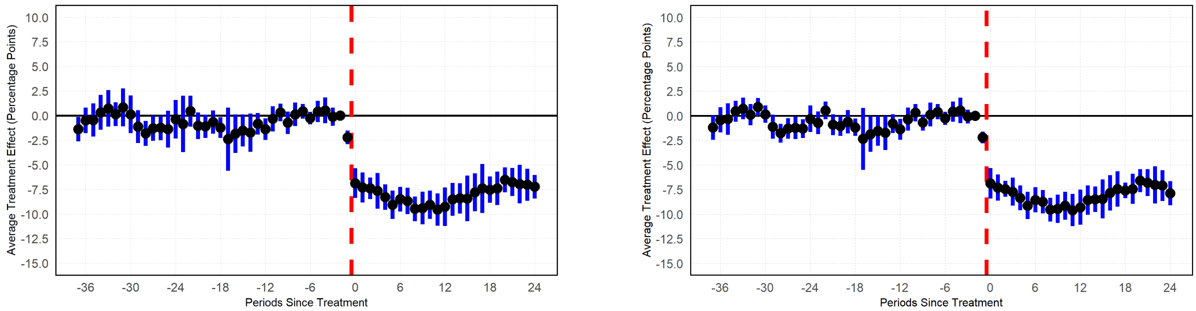
Table A.1: Summary Statistics For American Community Survey Sample (2017-2023)

A.3 First Stage Event Studies (Excluding New York and New Jersey)



(a) Two Way Fixed Effects (b) Sun & Abraham

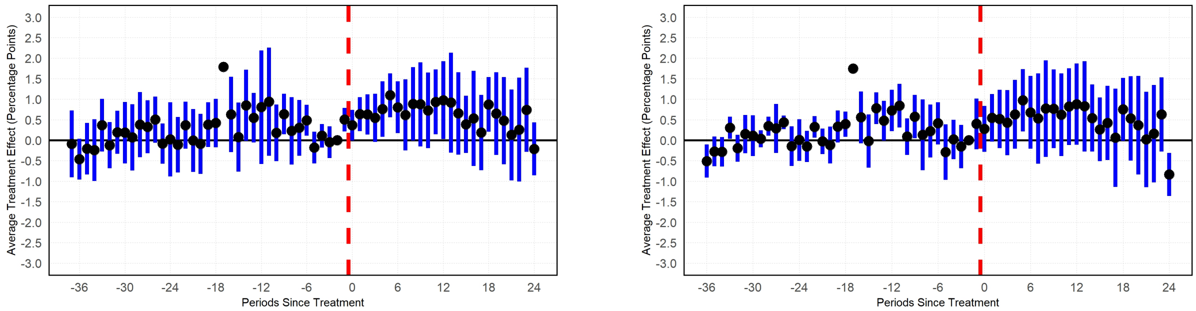
Figure A.2: Wage Information In Job Postings Increases.



(a) Two Way Fixed Effects (b) Sun & Abraham

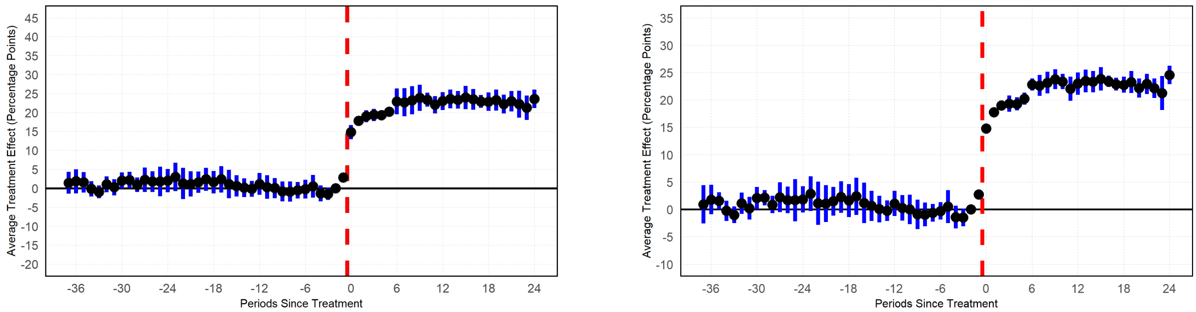
Figure A.3: The Proportion Of Wage Offers In Terms Of Point Wage Offers Falls.

Notes: The above figures use Lightcast job postings data, which is aggregated up to the state-SOC3-NAICS3-month level. Wage information in job postings is defined as the proportion of job postings in a group containing some wage information in the body text. Point wage offers are defined as wage offers that only report a single number as the offered wage in the job posting. Their proportion is calculated with respect to the number of all wage offers in a group, not all job postings in a group. Period $t = 0$ is the month when a pay transparency law is implemented in a state. We use $t = -2$ as the reference period, as we observe some anticipation effects in $t = -1$. All standard errors are clustered at the state level.



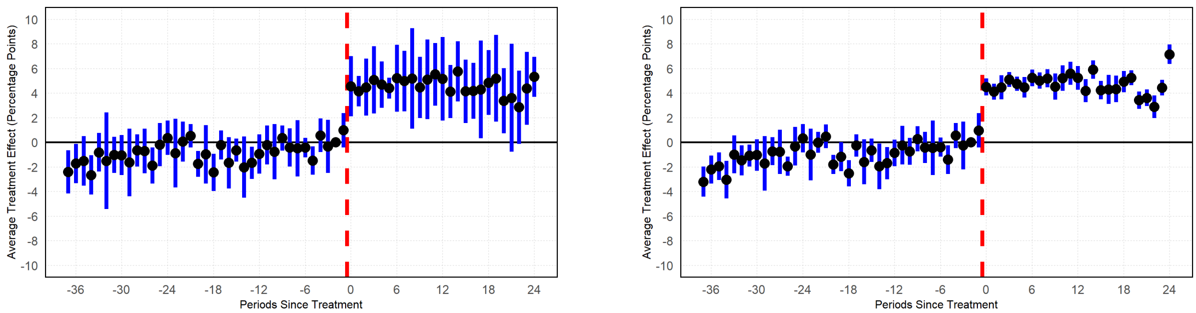
(a) Two Way Fixed Effects (b) Sun & Abraham

Figure A.4: The Median Wage Range Width Remains Unaffected By Pay Transparency Laws.



(a) Two Way Fixed Effects (b) Sun & Abraham

Figure A.5: Our Wage Index Increases Due To Pay Transparency Laws.



(a) Two Way Fixed Effects (b) Sun & Abraham

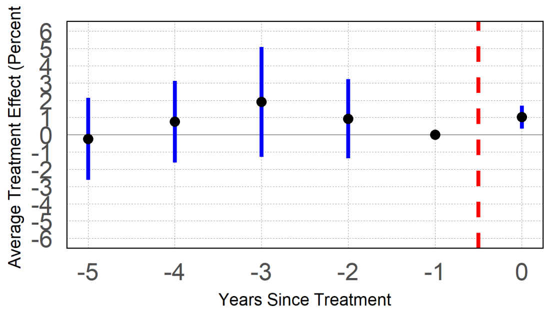
Figure A.6: Posted Wages (Measured By The Midpoint Of Wage Ranges) Increase.

Notes: The above figures use Lightcast job postings data, which is aggregated up to the state-SOC3-NAICS3-month level. A wage range offer is defined as a wage offer containing a minimum and maximum value, and the range width is calculated as the difference between the two. Our wage index is defined as given in Section 6.1. We define posted wages as either the value of the point wage offer, or the midpoint of a wage range offer. Period $t = 0$ is the month when a pay transparency law is implemented in a state. We use $t = -2$ as the reference period, as we observe some anticipation effects in $t = -1$. All standard errors are clustered at the state level.

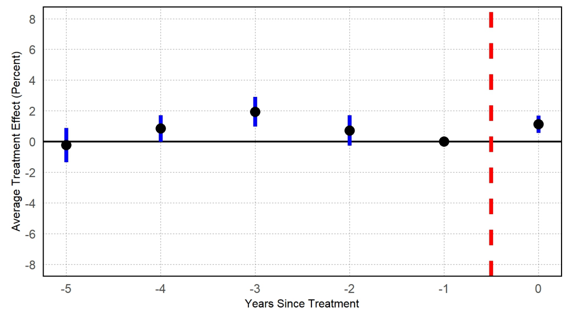
A.4 Additional Second Stage Results

Why We Exclude New York And New Jersey

Some cities and counties in New York implemented pay transparency laws in 2022 (Ithaca, Westchester, and New York City), while the rest of New York implemented its pay transparency law in 2023. Jersey City in New Jersey also implemented its pay transparency law in 2022. As we observed in Table 4 and as we show below, including New York and New Jersey in our analysis can add noise to our estimates.

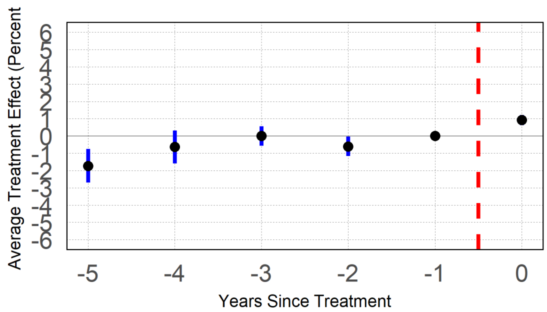


(a) TWFE

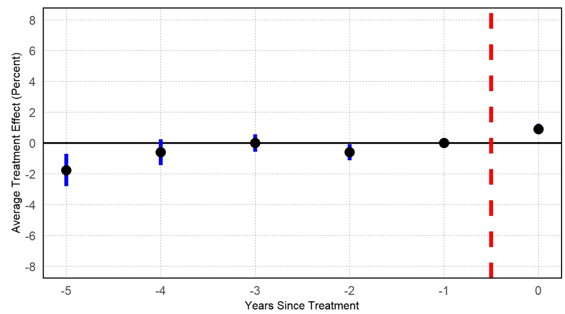


(b) SA

Figure A.7: Effect On Wages When We Include New York And New Jersey



(a) TWFE



(b) SA

Figure A.8: Effect On Wages When We Exclude New York And New Jersey

If we look at the raw wage trends for our treatment and control groups in Figure A.9, we observe that wages in New York and New Jersey move differently compared to the other states before they are treated. Here, we take 2018 as the reference period and look at movement in wage trends. Group 0 (red line) contains the untreated states, group 5 (green line) denotes Colorado, group 6 (blue line) denotes New York and New Jersey, and group 7 (purple line) denotes California and Washington.

As the figure shows, group 5 and group 7 have similar trends as group 0 does, implying that the parallel pretrends condition could hold for these groups. However, wages in New York and New Jersey seem to have a strong, negative effect, which leads to a violation of the parallel pretrends condition for group 6.

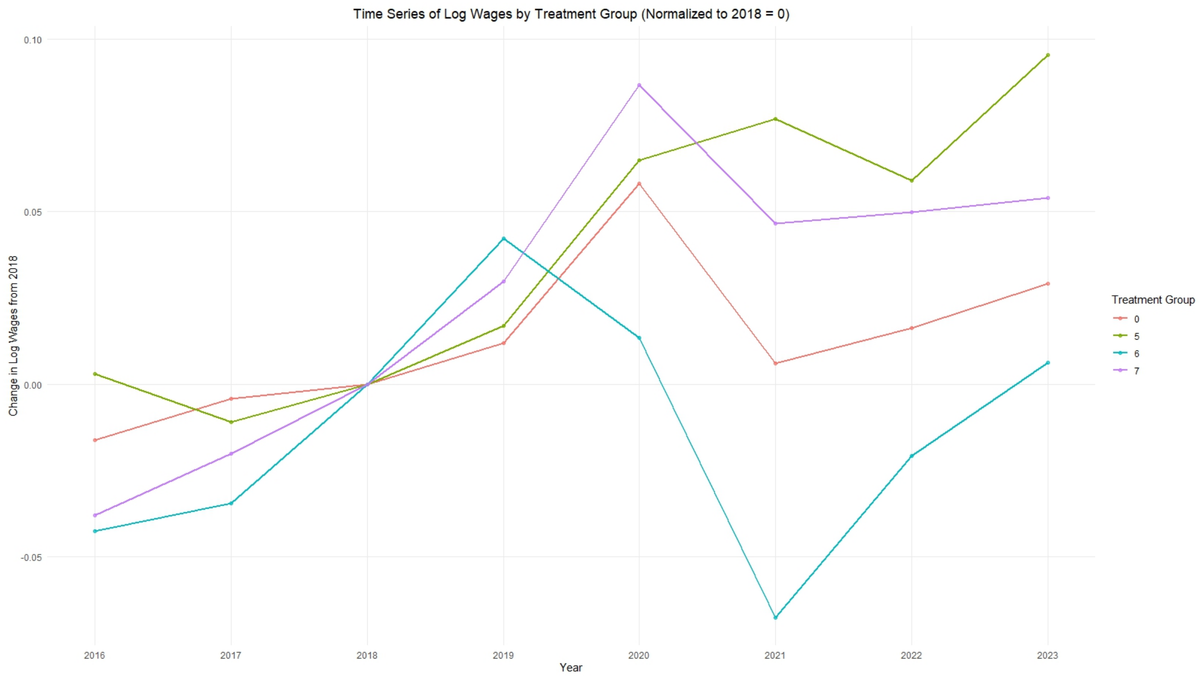


Figure A.9: Raw Wage Trends For States Grouped By Treatment Status