

The Impact of Pay Transparency in Job Postings on the Labor Market*

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Abstract

A number of states in the US have recently passed policies that require employers to provide salary information in job postings. Although the goals of these policies are to increase wages and decrease gender wage gaps, there are ambiguous theoretical predictions and limited empirical research on their impacts. This paper studies the labor market effects of a January 2021 law in Colorado that required job postings to contain expected salary information. Using data from Lightcast, we compare job vacancies in Colorado to other states and find that this law increased the fraction of postings with salary information by 30 percentage points, although there remains substantial noncompliance. Leveraging a combination of data from Lightcast, Glassdoor, and the Quarterly Census of Employment and Wages, we find consistent evidence of an increase in wages of around 1.3-3.6%. At the same time, we find no impacts on employment, the number of job postings, or skill and education requirements. Overall, we find evidence that pay transparency in postings increased competition in the labor market, leading to positive impacts on wages even for firms that were always posting wages as well as incumbent workers, neither of which were directly targeted by the policy. *JEL Codes:* J30, J31, J38

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1 Introduction

Pay transparency laws are increasingly being discussed as a means of improving labor market outcomes for workers around the world. To date, many of these laws have focused on the transparency of current employees, particularly in the public sector. For example, in 2007, salaries for all public sector employees in California were deemed public information by the State Supreme Court (Mas, 2017).¹ Recently, there has been a wave of interest in increasing transparency at the hiring stage by making expected salary information a requirement in job postings. For example, on January 1, 2021, Colorado enacted a pay transparency law that requires online job postings to include information about the expected salary of the position. Nine states have since passed similar rules requiring compensation be listed in job postings.² Despite the growing popularity of these laws, there is limited empirical research on the impact of pay transparency in job postings on the labor market.

The public narrative on pay transparency has focused on its potential positive impacts on workers' salaries. Recent survey evidence finds that workers tend to underestimate their outside options (Jager et al., 2021), so by giving workers more information about salaries in other firms, they can better aim their search to high-wage jobs. However, firms may also adjust in response to transparency laws, making the overall impact ambiguous. For example, in certain cases, price transparency in product markets has led to increases in prices rather than decreases due to more collusion between firms (Albæk et al., 1997). In labor markets, pay transparency within firms has been shown to lead to lower wages, as firms can credibly reject renegotiations when wages become transparent (Cullen and Pakzad-Hurson, 2021).

This paper studies the impact of increased wage transparency in job postings by analyzing the impact of the Colorado law which mandated that employers disclose expected compensation in online job postings. Our analysis proceeds in three parts. First, we use data from Lightcast, which contains online job postings from over 45,000 websites, to estimate the impact of the law on pay transparency in postings. Next, we use the job posting data to study the impacts on various aspects of the job vacancy, including expected salary, job requirements, and the volume of postings. Lastly, we use data on actual earnings from Glassdoor and the Quarterly Census of Employment and Wages (QCEW) to understand the impact of the policy on realized wages. Throughout the analysis, we find a key role for general equilibrium responses. While the policy directly targets firms that do not post wages in job postings, the impacts go far beyond this group. In particular, we find impacts on firms that always posted salary information, as well as on incumbent workers that are not directly impacted by the increased transparency in the hiring stage.

In the first part of the paper, our difference-in-difference estimates find that the pay transparency law had a large and immediate impact on the fraction of job postings that contain expected salary

¹Similarly, since April 2017 in the U.K., firms with more than 250 employees must publish information about their gender pay gaps.

²These states and the effective dates of their laws include Washington (Jan 2023), California (Jan 2023), New York (Sept 2023), Maryland (Oct 2024), Illinois (Jan 2025), Minnesota (Jan 2025), New Jersey (June 2025), Massachusetts (July 2025), Vermont (July 2025).

information. In Colorado, the fraction of postings with salary information before the law was about 35%. This jumps to around 50% immediately after the policy is effective, and then further increases to nearly 70% in the following months. In comparison, pay transparency in job postings remained fairly stable in other states. Therefore, while the policy did have a large and persistent impact on pay transparency in posting, compliance is still far from 100%.

In the next part of the paper, we study the impact of pay transparency on various aspects of the job posting. First, we find that posted wages in Colorado increase by about 7.3% relative to other states after the pay transparency reform. However, a substantial portion of this is due to compositional impacts since the law changed the types of jobs with wage information. Therefore, we next focus on within-job changes in salaries by estimating a dynamic difference-in-differences model that includes employer-by-occupation-by-county fixed effects. This implies that we identify the impact on jobs for which salary information is available both before and after the pay transparency law went into effect. These jobs are not directly impacted by the law, but may be indirectly impacted by changes in the overall level of competition in the labor market.

We find that posted salaries within-job increase by about 3.6% following the passage of the pay transparency law, a response that occurs quickly and remains persistent in the year following the law. This increase in posted salaries is not simply due to an increase in salaries ranges or an increase in skill requirements. In the data, jobs often include both a minimum and maximum salary as part of the expected compensation range. In our analysis, we calculate the expected salary as the average of these two values. However, one potential response to the law could be for firms to post wider salary ranges, which presumably give workers less information about the expected compensation. To understand whether firms are responding in this way, we estimate the income effect separately for the minimum and maximum salary posted in each vacancy. We find that employers did not respond to the pay transparency requirement by posting wider salary ranges. Instead, the evidence suggests that employers raised both the lower and upper salary bounds by approximately the same amount.

To understand why posted salaries increased even among firms that were already fully transparent before the policy reform, we show evidence that the positive wage effect likely reflects a general equilibrium response. If employers raised wages in response to new information about their competitor's wages, then we would expect the income effect to be strongest in markets that saw the greatest increase in pay transparency. To test this hypothesis, we estimate the income effect and the first-stage transparency effect separately by occupations. Consistent with the general equilibrium prediction, we find a positive correlation between the magnitude of the income and transparency effects across occupations.

Although posted salaries increased, we find no other margins of responses that may negatively impact workers' welfare. First, prior work has found that firms respond to minimum wage hikes by increasing education requirements (Clemens et al., 2021). However, in our setting, we find no impact of the policy on either education or experience requirements. Second, employers may reduce the total number of openings in response to the policy. Instead, we find no evidence that

the number of postings within Colorado changed. Lastly, firms could be posting higher wages, but paying at the lower ends of the range. In that case, an increase in posted wages may not necessarily translate into higher realized wages. We explore this channel next.

In the final part of the paper, we study realized wages by utilizing self-reported earnings data from Glassdoor as well as data from the Quarterly Census of Employment and Wages (QCEW). The Glassdoor data allows us to control flexibly for occupation and firm, but is selected due to self-reporting of wages to the website. The QCEW has the strength that it is built from administrative records of earnings. However, earnings and employment are only observable at the county-by-industry level. In the Glassdoor data, we find that realized earnings increase by about 1.3%. This confirms that the increase in posted salaries actually translates to real changes in earnings. We also replicate this result in the QCEW, finding a similar 1.4% increase in average earnings for workers in Colorado. By using self-reported work experience in Glassdoor, we are also able to separate the effect for incumbents and new hires. We find that both groups of workers experience similar increases in salaries, but the effect was more immediate for new hires. Therefore, again we find a key role for a general equilibrium response being an important factor in explaining the empirical results. While incumbent workers were not directly targeted by the pay transparency law, their wages nevertheless increased as a result of the reform.

To summarize our results, we find that the 2021 Colorado pay transparency policy substantially increased wage transparency in job postings, which in turn increased posted and realized salaries but did not decrease the number of vacancies or increase the requirements of jobs. Our results suggest that pay transparency during the hiring process enhances competition in the labor market. In particular, we find that the policy had significant general equilibrium impacts. The reform affected both incumbent workers, as well as firms that always posted wages, neither of which are directly targeted by the policy.

Our paper contributes to three distinct literature. First, it contributes to a growing literature on pay transparency. Many prior papers have studied the impact of disclosing salaries between existing workers at the same firm. In general, these papers find that workers dislike being paid less than their peers (Card et al., 2012; Breza et al., 2018; Cullen and Perez-Truglia, 2022). As a result, internal pay transparency leads firms to compress salaries and gender pay gaps (Mas, 2017; Baker et al., 2019). However, the pay compression is often driven by a reduction in average earnings, rather than a raise for workers at the bottom. To explain these results, Cullen and Pakzad-Hurson (2021) develop a general equilibrium model to show that pay transparency leads employers to bargain more aggressively, as a raise to one worker has spillovers that results in a raise for all workers.

Relative to the prior literature, our focus is on transparency in postings, rather than transparency for incumbent workers. While both transparency in postings and transparency for incumbents are often discussed as ways to address similar policy issues - such as low worker bargaining power and gender wage gaps - they may have very different impacts in practice. In contrast to the literature on pay transparency among coworkers, we find that cross-firm transparency actually raises wages. While relatively less is known about the effects of transparency in postings, there are

two notable exceptions. First, Frimmel et al. (2022) study a 2011 policy change in Austria that mandated the inclusion of minimum wage offers in job postings. Using data from a public employment agency, they find a reduction in the gender wage gap specifically for vacancies that need to be urgently filled. Second, Skoda (2022) combines data from a major job board with administrative social security records to study a similar 2018 law in Slovakia. Skoda (2022) finds that the wages of new hires increased by 3% in firms that did not provide salary information in their postings prior to the reform, relative to always-transparent firms. Skoda (2022) further shows that jobs that post pay information receive more clicks and applications, supporting the job search channel as a primary mechanism through which pay transparency increases wages. Compared to these studies of national policies, we evaluate a state policy that enables us to use unaffected states as a natural counterfactual comparison group. In other words, we are able to study the general equilibrium impacts of a pay transparency policy, even among firms that were already transparent prior to the new law. We find an important role for these general equilibrium impacts.

Second, our study also contributes to the empirical literature on the prevalence of wage posting and wage bargaining in the labor market. Models of wage posting and wage bargaining have featured prominently in discussions of monopsony power and rent sharing (Manning, 2011). Despite their theoretical importance, economists have only recently started empirically evaluating the extent of the two wage setting mechanisms in the economy. In a representative survey, Hall and Krueger (2012) find that about a third of workers bargained their wage before accepting their current job. More recently, studies have tested for the presence of wage bargaining by examining the impact of improved outside options on workers' wages. Using variation in outside options from the network of past coworkers' wage (Caldwell and Harmon, 2019) and the wages of dual jobholders (Lachowska et al., 2022), these studies find that wage bargaining appears to be more frequent among high-income workers. However, it is unclear why wage bargaining occurs more often for high income individuals. Our paper contributes to this literature by examining the wage bargaining and wage posting decision from the perspective of the firm. Consistent with previous studies, we find that jobs that become transparent after the policy change tend to have higher wages than previously transparent postings. However, the imperfect compliance that we observe even within-firms suggests that employers benefit from the option to bargain wages for certain jobs. Our analysis finds that these non-compliant postings are more likely to be in high-paying occupations. Together, the results suggest that part of the relationship between workers' incomes and the propensity to wage bargain is driven by employer's preferences. These findings also relate to nascent theoretical work to endogenize the wage setting protocol used by firms (Flinn and Mullins, 2021).

Lastly, our paper adds to a broad literature on the effects of regulating the types of information available during the hiring process. In particular, studies have evaluated the impact of rules that forbid employers from posting gender preferences in job vacancies (Kuhn and Shen, 2013; Card et al., 2021), inquiring applicants' criminal record history (Agan and Starr, 2017; Cullen et al., 2022), asking workers to post a preferred salary (Roussille, 2020), and observing applicants' compensation history (Barach and Horton, 2021). Similar to these studies, we find evidence that the type of

information available in vacancies affects broader labor market outcomes, namely, the posted and realized wage. Interestingly, we find a positive wage response even among jobs that were already posting salaries prior to the rule change, suggesting that the public disclosure of additional salary information leads to general equilibrium responses whereby employers raise salaries to match their competitors.

The remainder of the paper is organized as follows. Section 2 introduces a conceptual framework through which to interpret the impacts of pay transparency laws. Section 3 discusses the institutional details and introduces the data. Section 4 estimates the impact of the pay transparency law on the availability of salary information on postings. Sections 5, 6, and 7 estimate the impact of the law on posted salaries, realized wages, and the number of jobs, respectively. Section 8 concludes.

2 Conceptual Framework

The main goal of the conceptual framework is to illustrate various mechanisms through which pay transparency in postings may impact overall wages. To begin, we initially assume that firms can decide whether or not to post a wage. Posting a salary has the potential to draw a larger pool of applicants, but we assume the firm must commit to the posted wage. Second, the firm internalizes a within-firm bargaining channel, in which incumbent workers may use the posted wage to bargain over their own wages. This second mechanism will incentivize the firm to post lower wages, all else equal. Given these competing channels, the overall impact of a pay transparency law on wages is ambiguous.

We first consider the optimal wage conditional of firms that choose not to post a wage on the job vacancy. If the firm does not post a wage, then they receive $N(w_0)$ job applications that the firm takes as exogenous. They then choose the optimal wage conditional on receiving $N(w_0)$ applications by choosing w_{np} to maximize profits:

$$\pi = \operatorname{argmax}_{w_{np}} (\theta - w_{np}) \cdot N(w_0) \cdot P(A|w_{np}), \quad (1)$$

where θ is the productivity of the worker, which we will assume to be a constant for illustrative purposes. $P(A|w_{np})$ is the probability an individual worker will accept the job offer given an offered wage of w_{np} . Given this setup, the optimal offered wage satisfies the following first order condition:

$$\underbrace{\frac{\theta - w_{np}^*}{w_{np}^*}}_{\text{Wage Markdown}} = \frac{1}{\eta_H} \quad (2)$$

where η_H is the elasticity of firm size with respect to offered wages.³ The expression in Equation (2) is similar to a standard monopsony markdown rule.⁴ The only difference here is the intermediate step of receiving applications and then deciding whether to accept or reject a given offer. In a

³In terms of the model, $\eta_H = \frac{\partial P(A|w_{np})}{\partial w_{np}} \cdot \frac{w_{np}}{P(A|w_{np})}$.

⁴See Ashenfelter et al. (2010) for a reference to a standard static monopsony model.

perfectly competitive model, workers would be infinitely responsive to deviations below the market wage, implying $\eta_H = \infty$. In this case, the wage of the workers will be equal to the marginal revenue product of the worker, which is equal to θ in this model.

Next, we will consider the optimal wage if the firm does decide to post a wage. If a firm decides to post a wage, it will have two additional considerations relative to the situation in which the firm does not post a wage. First, the firm will internalize the fact that a higher wage will yield a larger pool of applicants. Second, the firm will internalize the fact that posting a higher wage may impact the wage paid to incumbent workers.

This second channel is similar to the channel discussed in Cullen and Pakzad-Hurson (2021), which studies within-firm pay transparency. In their model, pay transparency leads to lower wages for workers because firms can now credibly refuse to renegotiate wages. Similarly, we are positing that increases in the posted wage might spill over to other workers in the same firm. Therefore, if firms decide to post, they will chose the wage that maximizes profits, which is given by:

$$\pi = \operatorname{argmax}_{w_p} (\theta - w_p) \cdot N(w_p) \cdot P(A|w_p) + I \cdot (\theta^I - w_I(w_p)), \quad (3)$$

where I is the number of incumbent workers, θ^I is the productivity of incumbents and $w_I(w_p)$ is the wage that the firm will pay to incumbent workers conditional on posting a wage w_p to entrants. In the previous case where the firm decides not to post a wage, we implicitly assumed that the incumbent workers will not learn of new hires' wages, and therefore the wage of the incumbents is independent of the wage paid to entrants. Again, a key assumption of this model is that firms are obligated to pay the posted wage w_p to entrants. It is not possible in this model to offer a high wage to attract applicants, and then offer a low wage in practice.

The first order condition for the optimal posted wage now satisfies:

$$\frac{\theta - w_p^*}{w_p^*} = \frac{1 + R_I \cdot \frac{\partial w^I}{\partial w_p}}{\eta_H + \eta_A} \quad (4)$$

where R_I is the ratio of incumbents to entrants. $\frac{\partial w^I}{\partial w_p}$ captures how incumbent workers' wages change with respect to entrants' posted wages. The entire term in the numerator $R_I \cdot \frac{\partial w^I}{\partial w_p}$, can be interpreted as an inframarginal cost of posting a higher wage driven by the impact on incumbent wages. The higher the ratio of incumbents to entrants, the more this inframarginal cost plays into firm decision-making. This term acts as a force to decrease wages. The term $\eta_A > 0$ is the elasticity of applications with respect to the posted wage. Firms internalize the fact that increasing wages will result in a larger number of applicants, which will yield a larger number of entrants into the firm and raise overall profits. This force will incentivize firms to increase wages.

Without any policy requirement, heterogeneity in $R_I \cdot \frac{\partial w^I}{\partial w_p}$ and the labor supply elasticities across firms will lead to differences in the decision of whether to post a salary or not. Next we consider the impact of a pay transparency law on overall wages in the labor market. To do so, we separately consider the impacts on firms that originally do not post wages vs. firms that always

post wages.

One could further enrich this model in a number of directions. For example, in the current model, it is implicit that workers form beliefs about the expected wage if a firm does not post a wage, but we do not model this aspect of worker behavior. Additionally, worker productivity is known by the firm, while in reality firms will form beliefs about a workers' productivity. Uncertainty over productivity could be an important reason why firms post wage ranges or do not post wages at all. They may be unsure of the quality of applicants, which would impact the eventual wage they offer to the candidate. However, the main goal of this model is to illustrate a few simple channels through which transparency may impact wages and to show that these channels lead to ambiguous impacts on wages.

Not-posting firms: For firms that were not posting wages before the law change, the overall impact on wages is ambiguous. If the bargaining channel dominates, then firms will offer lower wages to entrants. If the incentive to draw in new applicants is sufficiently strong, then the firm will post higher wages than the previously offered wage, increasing wages for entrants overall.

Always-posting firms: In the model, the parameters η_H and η_A are taken as exogenous. These parameters, which capture how likely a worker is to accept a job offer and apply to a job posting, respectively, will also change after a pay transparency law goes into effect. The direction of the change depends on the response of the firms that are directly impacted by the policy. For example, imagine firms that previously did not post wages start posting and draw workers away from the always-posting firms. This increased competition will impact the decisions of workers when deciding where to apply and what offers to accept. If the labor market becomes more competitive, making labor supply more elastic, then always-posting firms will increase wages. Theoretically, however, the opposite could occur. As discussed above, directly impacted firms could lower wages in response to the pay transparency act. This might lower competition in the labor market, leading to falling wages at always-posting firms.

3 Institutions and Data

3.1 The Equal Pay for Equal Work Act

On January 1, 2021, the Equal Pay for Equal Work Act (EPEWA) became effective in Colorado. This act requires employers to (1) include compensation in job postings, (2) notify employees of promotion opportunities, and (3) keep accurate records of job description and wage rate. Our focus will be on the first part of this act: including compensation in job postings.

In terms of information that must be posted, the act requires firms to disclose in each job posting “the hourly or salary compensation, or a range of hourly or the salary compensation, and a general description of all of the benefits and other compensation to be offered to the hired applicant”

(CDLE, 2021).⁵ The salary range may extend from the lowest to the highest amount that the employer actually believes it might pay for a particular job. Ultimately, there is no requirement that an employer actually pay within the posted range. However, a firm that posts very large ranges for all jobs, independent of the occupation, would not be complying with the law. For example, the Colorado Department of Labor states that “an employer cannot post the same \$30,000-\$100,000 range for janitor and accountant jobs alike, if it does not genuinely anticipate offering an accountant the low end, or a janitor the high end.”

After the act became effective, enforcement came primarily through education rather than fines. For example, individuals can submit a non-compliance letter to the Colorado Department of Labor if they find an employer is not complying with the law. The Colorado Department of Labor will then send a Compliance Assistance Letter to the firm, who has an opportunity to comply. Legally, the fines can vary from \$500 to \$10,000 per violation, but are rare in practice, with only six monetary fines issued as of September 2024. The largest fine was issued against Lockheed Martin for \$151,000.

After the act became effective, news articles reported that some firms excluded Colorado workers from remote jobs in order to avoid having to comply with the legislation (Rubino, 2021). In response, the Colorado Department of Labor clarified which employers and job postings must comply with the law. In particular, if an employer has a single employee in Colorado, then the employer must post salary ranges for remote jobs, even if the posting specifies that the employee cannot perform the work from Colorado.⁶ Jobs tied to a specific location outside of Colorado or remote jobs from companies that have no employees from Colorado are outside the jurisdiction of the law. In the empirical analysis, we will directly estimate whether the policy decreased the number of postings for jobs within Colorado, which could occur if firms transfer some jobs to locations not impacted by the legislation.

The last relevant institutional detail is that Colorado increased the minimum wage from \$12.00 an hour to \$12.32 an hour on January 1, 2021, the same day the pay transparency law went into effect. Colorado is not the only state to have a minimum wage change on this date, with 26 states also increasing the minimum wage. Still, to ensure any wage impacts are not driven by the minimum wage change, we consider robustness checks that estimate the impact only for jobs that were paying greater than \$14.00 an hour before the policy as well as comparing only to states that experienced a similar change in the minimum wage.

3.2 Lightcast Data

Our data on job postings come from Lightcast. Lightcast scrapes data from over 45,000 internet sources, including job boards and company websites. Importantly for our purposes, Lightcast job postings data contains information on whether the job posting has associated wage information.

⁵The general description of benefits must include health care benefits, retirement benefits, and any benefits permitting paid days off.

⁶Unlike pay transparency laws in other states like California, the Colorado law applies to all firms regardless of their size.

Hourly wages are reported in some postings, while in others, annual salaries are reported. In order to make these two types of reporting comparable, hourly wages are converted to annual salaries by multiplying by 2080 (52 weeks times 40 hours a week). Postings with salary information often include a lower and upper bound. For simplicity, unless otherwise specified, we refer to a job’s “posted salary” as the average between the minimum and maximum values posted.

We focus on job postings between 2020-2021 given our focus on the Colorado transparency law that was implemented on January 1st, 2021. To construct our main sample, we drop any observations for which we do not observe an employer name, occupation, or county. We define an occupation by a six-digit Standard Occupational Classification (SOC) system, which has been used in prior work using Lightcast data to define occupations, for example, Azar et al. (2020). We also drop records where the date of the posting does not match the date recorded in Lightcast’s files. Appendix Table A.1 shows the number of observations remaining after each sample restriction. In total, we lose about 23% of all observations. The bulk of this is due to dropping postings with missing employers (17% of the data is dropped due to this restriction).

Table 1 provides summary statistics of job postings in Colorado compared to all other states in 2020, the year prior to the law being enacted. Column (1) shows that in 2020, about 34% of all job postings contained expected salary information in Colorado. This is slightly higher than the fraction of job postings with salary information in the rest of the country, at 31%. Among jobs with salary information posted, the average posted salary is slightly higher in Colorado than in other states (\$53,300 vs. \$51,000).

Turning to panel (b) of Table 1, the distribution of occupations is quite similar in Colorado vs. all other states. However, some occupations are over-represented in Lightcast compared to representative data. For example, jobs in computer and mathematical occupations are over-represented relative to jobs in the food service sector. In Colorado, the Equal Pay for Equal Work Act specifically targeted online job postings. Therefore, the jobs that are over-represented in the data are also the jobs that are most exposed to this policy. While it would be interesting to explore spillovers on jobs not posted online, doing so is beyond the scope of this project.

3.3 Glassdoor Data

While the Lightcast data allows us to measure posted wages over time, it is uncertain whether changes in posted wages actually translate to realized wages. To study the impact of pay transparency on realized earnings, we complement our analysis with data from Glassdoor. Glassdoor worker-level data contains self-reported salaries, along with the name and location of the establishment at which the worker is employed. This allows us to compare earnings for jobs located in Colorado to earnings in jobs in other states both before and after the pay-transparency reform. Following previous users of the data, we also distinguish between new hires and incumbents using respondents’ years of experience at the time they reported their salary (Dahl and Knepper, 2022). In particular, we assume workers with only 1 year of experience to be new hires.

A potential limitation of the Glassdoor data is that its salary information is self-reported by

users to their website, and thus may not be representative and could contain measurement error. To understand the validity of the data, we refer to a paper by Karabarbounis and Pinto (2018) that compares Glassdoor’s data with administrative data from the Quarterly Census for Employment and Wages (QCEW) and survey data from the Panel Study of Income Dynamics (PSID). In terms of representativeness, they find that Glassdoor tends to over-represent tech and finance jobs, while undercounting the share of employment in healthcare. In terms of validity, they find that salaries in Glassdoor closely match the mean salaries across industries and regions computed using the QCEW, and the within-industry dispersion in the PSID. For example, the authors find a cross-industry correlation of 0.87 between the QCEW and Glassdoor. Thus, while there is selection in who chooses to report salaries, the authors conclude that “...the wage distribution (conditional on industry or region) in Glassdoor represents the respective distributions in other datasets, such as QCEW and PSID fairly well.” Given these features of the data, we plan to control for state and occupation fixed effects in our analysis.

3.4 Quarterly Census of Employment and Wages Indicators (QCEW)

We also replicate our analysis using publicly available information from the QCEW. The Census Bureau constructs the QCEW from administrative data that establishments report to State UI programs. The strength of the QCEW data is that it is representative, as the UI programs cover about 97% of the workforce in the country. Wages in the QCEW capture a variety of forms of compensation, including regular wages, bonuses, stock options, severance pay, the cash value of meals and lodging, tips and other gratuities, and, in some states’ employer contributions to certain deferred compensation plans, such as 401(k) plans.

The main limitation of the QCEW is that we only observe outcomes at the industry-by-county level. Our main outcomes are log average weekly wages and annual employment by 4-digit NAICS-county. To construct this measure, the QCEW adds up the total amount of wages in the year and divides by the total employment to construct an average annual wage. The weekly version divides this annual wage by 52. The annual employment is constructed by summing up 12 months of employment numbers and then dividing by 12.

Each of the datasets we use have their strengths and weaknesses. The Lightcast data allows us to identify a first-stage impact on pay transparency, as well as impacts on job posting characteristics. The Glassdoor and QCEW data both allow us to measure actual earnings outcomes. An advantage of the Glassdoor data over the QCEW is that it contains information on the occupation and employer of respondents, thereby allowing us to more flexibly compare the same jobs over time. While we cannot control for firm and occupation in the QCEW, it is more representative than the other datasets, as it is built from administrative records that capture the vast majority of employment in the U.S.

4 The Impact of EPEWA on Pay Transparency in Online Postings

4.1 State-wide Trends

Figure 1 plots the fraction of job postings with salary information in Colorado vs. other states, both before and after the law mandating transparency in online postings became effective. As can be seen in the figure, there is a sharp increase in the fraction of jobs with salary information in Colorado. In 2020, there are somewhat large fluctuations in the fraction of postings with salary information month-to-month, however, on average, roughly 35% of job postings contain salary information. This fraction jumps to around 50%, before increasing further to almost 70% a year after the law has been effective. In comparison, for all other states, the fraction of jobs with salary information fluctuates from 30% to 40%, with a relatively flat trend over time. Overall, it is clear from Figure 1 that the law had a large and immediate impact on transparency in online postings in Colorado. We next proceed to a dynamic difference-in-differences analysis that allows us to directly assess pre-trends and estimate the magnitude of the change in transparency.

4.2 Dynamic Difference-in-differences Design

To estimate the impact of the Equal Pay for Equal Work Act on salary information, we implement a dynamic difference-in-difference design of the following form:

$$Y_{it} = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot \text{Colorado}_i + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \quad (5)$$

where Y_{it} is a dummy variable for whether posting i at time t includes salary information. $\psi_{j(i)}$ is a job fixed effect that controls for at least the employer of the posting. In our preferred specifications, this job fixed effect is a firm-SOC-FIPS interaction, where SOC (Standard Occupational Classification) is a 6-digit occupation code and the FIPS code is a county code. $\tau_{c(i),t}$ are month fixed effects that vary by some characteristics of the job $c(i)$. For example, in our preferred specifications, we include SOC-month fixed effects so that the coefficients of interest are only identified by within-occupation variation. Without these fixed effects, if Colorado post jobs in different occupations, and these occupations are on different trends than occupations in other states, then our estimates of the impact of the pay transparency law would be biased. For example, if Colorado posts more technology-related jobs and these jobs are becoming more transparent even absent the policy, then we would falsely identify that the policy was effective in making jobs more transparent. By including occupation-by-month interactions, the effect of the act is only identified by comparing the same occupation across different locations.

The key coefficient of interest is δ_k , which is the coefficient on the interaction between month t and whether the job is located in Colorado. $k = 0$ corresponds to January 2021, the date the pay transparency in Colorado became effective. The month before the policy, $k = -1$, is omitted from the estimation in order for the model to be identified. Each δ_k represents the difference between

treated and control jobs relative to the difference that occurred in the month prior to January 2021. To summarize the results, we sometimes report the average effect of the policy by replacing the dynamic treatment indicators with a binary post-event indicator.

Our identifying assumption is that the presence of salary information in postings would have trended similarly in Colorado vs. other states absent the mandate to post salary information on online postings in Colorado. Given relatively parallel pre-trends and the sharp increase in salary information, we think it unlikely that coinciding shocks or confounding variables explain the results. We therefore defer a more detailed discussion of the identifying assumptions of our framework to the analysis of wage effects.

Figure 2 plots $\hat{\delta}_k$ from estimating Equation (5). We overlay the coefficients from a simple regression with only employer and time fixed effects, along with a specification that includes firm-SOC-FIPS and SOC-month fixed effects. In both cases, the fraction of postings with salary info increases by about 14 percentage points in January 2021. This impact gradually grows throughout the course of the year, reaching a peak around 30 percentage points. The lack of any pre-trends before the policy becomes effective and the sharp break in January 2021 makes it clear that the policy had an immediate and lasting impact on the fraction of online job postings with wage information.

4.3 Heterogeneity by Firm and Occupation

While we document a large impact on the fraction of jobs with salary information on average, there is potentially considerable heterogeneity in how different firms and occupations respond. This heterogeneity is relevant for two reasons. First, while we document an impact on the fraction of jobs with salary information, we have also found compliance is still far from complete. Exploring which firms and jobs are not complying is important from a policy enforcement perspective. Second, in a future section we will explore general equilibrium responses in wages, arguing larger changes in transparency lead to larger changes in wages. In this section we explore which occupations experienced the largest changes in transparency.

We begin by exploring firm-level heterogeneity in compliance. Appendix Figure A.1 plots the share of an employer’s postings that contain salary information in 2021 as a function of the share of postings with salary information in 2020. Two features of the figure are worth highlighting. First, the decision of whether or not to include salary information appears to be a persistent firm-specific trait. On average, there is a positive, nearly linear relationship between the posting behavior of firms in 2021 and their behavior in 2020. Second, the first-stage effect of the EPEWA is strongest for firms that seldom include salary information in their job postings. Among firms in Colorado that had nearly zero transparency in 2020, we observe a 40 percentage point increase in the share of postings with salary information. On the other hand, there is no change in transparency among firms that already posted salaries for at least 80% of jobs in 2020. As evidence that the steep increase in pay transparency does not simply reflect reversion to the mean, we find only a minor deviation from the 45-degree line among employers outside Colorado.

To understand whether variation in compliance is a within or across firms phenomenon, Appendix Figure A.2 plots the distribution of firms by their share of postings with salary information, separately for 2020 and 2021. Panel (a) suggests that relatively small firms (defined as having between 10-100 postings) appear to engage in an all-or-nothing form of compliance, with nearly 70% of employers either having full transparency or no transparency. On the other hand, panel (b) shows that firms with at least 100 postings appear to be more selective in which jobs they choose to reveal salary information. Unlike small firms, less than 20% of large firms had either full or no compliance in 2021. Rather than a subset of large firms becoming fully compliant, the evidence suggests that it was many firms becoming moderately more transparent by selectively choosing the postings that include salary information.

To determine which type of firms responded more strongly to the new law, panels (c) and (d) plot the distribution of the change in the share of postings with salary information between 2020 and 2021. In both Colorado and other states, we find that small firms do not significantly change their pay transparency over time. In contrast, large firms in Colorado become far more transparent relative to firms in the rest of the country. Taken together, Figure A.2 suggests that Colorado’s pay transparency law had a stronger effect on large firms relative to small firms, as many small firms were already fully transparent prior to the policy change.⁷

A potential explanation for the variation in compliance within-firm is that employers highly value the option to bargain over salaries in certain occupations. To test whether the effect of the transparency law varied across occupations, we estimate Equation 5 separately for each 2-digit SOC code while controlling for firm-SOC-FIPS and SOC-time fixed effects at the 6-digit SOC level. Figure A.3 plots the estimates by occupation group, averaged over all months in 2021. We find sizeable differences in the first stage response to the Colorado reform across occupations. For example, the share of postings with salary information only increased by about 13 percentage points among transportation jobs, but approximately 34 percentage points among health care support jobs. We will return to this variation when we study the wage impacts of the policy.

What explains the variation in compliance across occupations? In Figure A.4, we show that a significant predictor of compliance with the pay transparency law is the salary of the posting’s occupation. To construct the average salary of an occupation, we use information on actual earnings, rather than posted wages, which is sourced from the American Community Survey 2015-2020. In the bottom income-decile of occupations, about 65% of postings had salary information in 2021. In contrast, less than 50% of postings in the top income-decile had salary information. If the cost of posting a wage is zero, then we would expect firms to fully comply to avoid the potential penalty of breaking the law. However, the observation that noncompliance is largest among high paying occupations indicates that employers face a greater cost of publicly revealing the salary of high paying jobs than low paying ones. This suggests that at least part of the relationship between the propensity to bargain over wages and workers’ salaries, as observed in the literature (Hall and

⁷The claim that large firms experience a stronger first-stage effect is further supported by Appendix Figure A.5 where we plot the coefficients of equation 5 separately for small and large firms, controlling for firm and month fixed effects.

Krueger, 2012; Caldwell and Harmon, 2019; Lachowska et al., 2022), is driven by a firm preference for not revealing the wages of high paying jobs.

To understand the relative role of employers and occupation in determining compliance with the 2021 pay transparency policy, Appendix Table A.2 reports R-squared estimates from regressing a wage posting dummy on a series of fixed effects. To start, we find that 43% of variation in compliance can be predicted by the employer that posted the job. In comparison, 6-digit occupation codes are a poor predictor of compliance, explaining only 4.5% of the variation. Column (3) shows that including the occupation fixed effects does not improve the predictions of compliance relative to column (1) where we simply control for employer fixed effects. However, controlling for the firm-SOC interaction in column (4) improves the adjusted R-squared by 15% (or 6.5 percentage points), suggesting that different firms select different occupations to post salaries. Lastly, columns (5) and (6) find that even narrowing the employer by county can only increase the R-squared to 0.62. This implies that whether a posting includes salary information may vary even within the same job at the same employer-location. Overall, it appears the employer is the best predictor of compliance.

5 The Impact of Pay Transparency in Postings on Posted Salaries

5.1 Dynamic difference-in-differences

While there is a large impact in the transparency of job postings, it is unclear how this increase in information will impact posted wages. In this section, we estimate the impact of pay transparency on posted wages by estimating a dynamic difference-in-difference design of the following form:

$$\log(\text{salary}_{it}) = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot \text{Colorado}_j + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \quad (6)$$

Where $\log(\text{salary}_{it})$ is the log annual salary of the job. Postings with hourly rates are annualized by multiplying by 2080. The rest of the variables are defined in the same manner as Equation (5) and standard errors are again clustered at the employer level.

Relative to Equation (5), this specification can only be estimated for job postings with salary information available both before and after the policy change. This eliminates about 40% of postings in Colorado and around 65% in other states. Additionally, given the inclusion of firm-SOC-FIPS fixed effects ($\psi_{j(i)}$) this estimation requires that the firm posts advertisements for the same occupation in the same location both before and after the policy. We think it is particularly important to include these set of fixed effects in this estimation, as the composition of jobs with salary information has changed dramatically in Colorado following the law. Not controlling for the specific job (i.e. a given occupation offered by a given firm in a given location) would imply that any wage effect could be driven by these compositional changes. Additionally, we also include SOC-month fixed effects ($\tau_{c(i),t}$), implying the estimation is identified by within-occupation variation.

The interpretation of any impacts in this section, therefore, are only driven by firms that are

indirectly impacted by the policy. If the firm is posting wages both before and after, then the reason we may see an increase is due to general equilibrium effects. When we turn to our alternative data sources, we will be able to capture both direct and indirect effects of the policy. Additionally, it is important to note that this section only identifies the impact on posted salaries, which may or may not translate into actual wage impacts. To study actual impacts on earnings, section 6 analyzes Glassdoor data and QCEW data, both of which allow us to study actual earnings.

5.2 Assumptions and Interpretation of Salary Effects

The key identifying assumption is that outcomes for Colorado jobs would follow similar trajectories to jobs in other states in the absence of pay transparency in online postings. As before, we assess this assumption by analyzing pre-trends in posted salaries between Colorado and other states. However, even if pre-trends appear parallel, shocks that occur contemporaneously with the policy change may bias the interpretation of the results.

There are several potential coinciding shocks that may be concerning for this design. First, the Equal Pay for Equal Work act made several policy changes, one of which included mandating expected salary information in postings. As discussed in Section 3, the policy also made it mandatory to notify employees of promotion opportunities as well as maintain wage records. It is not immediately clear how these other policy details would impact posted salaries, but there are potential mechanisms for this. For example, if firms must post promotion opportunities to current employees, then it is possible they will reduce external hiring after the policy. This could impact the composition or number of jobs that firms advertise. However, given the inclusion of firm-SOC-FIPS fixed effects, this type of impact will not be captured in the empirical design. If the composition of jobs changes in ways not captured by location and occupation, then this could in principle be part of the effect of the policy. While we do not think these type of effects are particularly likely to bias the results, a conservative way to interpret our estimates is the aggregate impact of the Equal Pay for Equal Work Act in Colorado on posted salaries, without specifying the transparency in online postings as the only channel.

A key coinciding shock in this setting is the increase in the minimum wage in Colorado from \$12 to \$12.32. As discussed in Section 3, 26 other states also had increased minimum wages starting in January. However, to ensure this is not driving the results we provide additional robustness checks by (1) restricting to jobs with average salaries above \$14 before the policy change and (2) restricting control states to those that experienced a similarly-sized minimum wage change.

5.3 Impact on Posted Salaries

Figure 3 plots $\hat{\delta}_k$ from estimating Equation (6). After January 1, 2021, posted salaries increase by roughly 3.6% in Colorado, an effect that remains relatively stable over time. Since the specification follows the same firm-occupation-county over time, the increase in wages in Colorado is not driven solely by a compositional effect. Even for always-posting firms, we observe increases in posted

wages, suggesting the increase in transparency has led to competitive pressure that increase wages at all firms.

Table 2 tests the robustness of this income effect to alternative specifications. In order to summarize the effect, we replace the dynamic treatment indicators in Equation (6) with a single dummy for Colorado post-reform. Column (1) reports the estimate corresponding to Figure 3. The 95% confidence bound implies that the pay transparency law increased posted salaries by 2.4% to 4.8%. In column (2), we show that the income effect is not driven solely by a contemporaneous increase in the minimum wage by restricting the sample to firm-SOC-FIPS that had an average wage of at least \$14/hr in 2020, well above the minimum wage of \$12.32.⁸ As a secondary test, we restrict the control group to the 15 states that had a minimum wage increase of less than 8% in 2021. Among that group, the minimum wage change in Colorado ranked on the lower end so we would expect the estimate of the income effect to be biased downwards. Nevertheless, we still find a significant positive income effect in Colorado compared to other states that increased their minimum wages, providing evidence that minimum wage changes are not driving the results.

Next, columns (4) and (5) examines heterogeneity by baseline transparency. One concern is that even within firm-SOC-FIPS, there is variation in transparency that leads to composition bias. Columns (4) and (5) split the sample by whether a firm-SOC-FIPS had transparency in all postings in 2020 or only some. The analysis finds that conditional on posting salary info for at least one job posting in 2020, the majority of firm-SOC-FIPS already include salary info in all postings. These fully transparent jobs actually have a larger positive wage effect than the partially transparent ones.

In column (6) of Table 2, we show that the preferred estimate of the income effect is robust to controlling for firm-SOC-time fixed effects. In this stricter specification, we identify the income effect by considering whether employers that post jobs for the same occupation in multiple states change posted wages in Colorado relative to other states. While this further reduces the influence of confounding variables by focusing on within-firm variation, it identifies the impact on a specific population: firms that post jobs in multiple states. The restricted variation estimates a statistically significant income effect that is about half the size of the estimate from the main specification. The smaller point estimate could be for multiple reasons. First, prior studies have shown that firms sometimes set national wage policies Hazell et al. (2021), implying that any impacts on Colorado wages could spill over to establishments in other states within the same firm. Second, the set of firms that post the same job in multiple states may simply be different than the average firm, therefore, treatment effect heterogeneity could also explain the small variation in effect sizes.

To evaluate the effect of the pay transparency law on the composition of job postings, column (7) reports the difference-in-differences estimate if we only control for firm and month fixed effects. In this case, we find average posted salaries increased by 7.3% in Colorado compared to other states, about 2 times larger than the estimate from our main regression. The observation that

⁸Appendix Figure A.6 plots the analogous event-study estimates for jobs that paid well above minimum wage in 2020.

posted salaries increased significantly more at the firm level than within establishment-occupations implies that, before the reform, firms were less likely to release salary information for high paying occupations relative low paying occupations. This result is consistent with previous empirical findings that highly-educated, high-income workers tend to bargain over their wages rather than be provided a posted salary (Hall and Krueger, 2012; Lachowska et al., 2022). Given that high paying jobs are less transparent at baseline, our main wage estimates that control for firm-SOC-FIPS fixed effects identify the impact of the pay transparency rule on jobs at the lower end of the pay distribution.

Next, we decompose the average income effect into the impact on the maximum and minimum posted salaries. While not legally permitted, employers may be responding to the reform by simply posting a range of salaries so wide that they are effectively offering no real information and leaving room for bargaining during the interview stage of the hiring process. Appendix Table A.3 averages the post treatment estimates from Equation (6) for three different outcome variables: log maximum posted salary, log minimum posted salary, and log of the ratio of the maximum and minimum posted salaries. We highlight three results from columns (1) to (6). First, employers raised both the maximum and minimum posted salaries as a result of the Colorado pay transparency law. Second, the increase in the minimum posted salary is approximately the same as the increase in the maximum posted salary. Third, these results are robust to focusing on jobs that are likely to be unaffected by changes to the minimum wage.

One important point to reemphasize in these results is that these specifications control for firm-SOC-FIPS fixed effects. Therefore, the impact on the minimum and maximum salary is identified from jobs that had wage information both before and after the policy change. If firms are posting very large ranges only in newly transparent jobs, the response would not be picked up in the prior analysis. To study this channel, column (7) estimates a specification that controls for only firm fixed effects, rather than firm-SOC-FIPS. In this case, if the newly transparent jobs within a firm have very large bounds, then we would expect the ratio between maximum posted salary and the minimum posted salary to increase after the policy. However, we find essentially zero impact along this margin and the 95% confidence interval rules out any increases in the salary range above 1.5%. Therefore, the results suggest firms are not posting exceptionally large salary ranges even for jobs that become transparent as a direct result of the reform. The only evidence we find that job postings became less informative is in column (8), where we show that newly transparent jobs are 3% more likely to post a range than an exact value.

To accompany the higher posted wages, employers could potentially be imposing stricter job qualifications on new hires. To test for any such response, Appendix Figure A.7 estimates the impact on whether a job posting has any education requirements (panel (a)), the minimum number of years of education needed (panel (b)), any experience requirements (panel (c)), and the number of years of experience needed (panel (d)). We find no impact of the pay transparency law on any of these outcomes. Appendix Table A.4 summarizes the results presented in these figures and confirms that this result is robust to the type of fixed effects included in the specification.

To summarize, the analysis finds that employers increased posted salaries in response to Colorado’s pay transparency law. The increase in posted salaries is not due to contemporaneous changes to the minimum wage, and is robust to various controls for composition changes. Employers also do not dilute the informativeness of posted salaries by increasing the range of eligible wages, nor do they become more selective in terms of education and experience requirements.

5.4 The Importance of General Equilibrium Channels

In the last section, we found evidence that always-posting firms increased wages, providing the first evidence on the importance of general equilibrium impacts of the pay transparency policy. To explore this potential explanation further, we next study heterogeneity by occupation. In particular, we test whether occupations that experienced a larger increase in transparency are also the occupations that experienced a larger increase in wages. The logic is that markets that experienced larger shifts in transparency would also be the markets we observe the largest general equilibrium impacts. Similar to our previous analysis, all effects will be estimated on always-posting firms, therefore isolating general equilibrium impacts.

Appendix Figure A.8 plots estimates of the posted wage separately by 2-digit SOC codes. While there is no occupation in which the policy has a statistically significant negative income effect, we are more confident of a positive wage response in select sectors. With the exception of production jobs, the positive income effects appear to be concentrated in primarily white collar occupations such as management, finance, engineering, and law. To test our hypothesis that competitive pressures due to increased transparency led to the increase in wages, Figure 4 plots the point estimates of the income effects, separately by 2-digit SOC codes, against the estimates of the increase in pay transparency from Appendix Figure A.3. Broadly, there is a statistically significant positive relationship between the magnitude of the income effect and the increase in the share of jobs with salary information across occupations. A linear regression predicts zero income effects among hypothetical occupations that experience no increase in pay transparency, and each 10 p.p increase in occupation-level pay transparency translates to a 1.35% increase in posted salaries. The results are therefore consistent with the view that always-transparent firms are responding to broader increases in transparency at the market level.

6 Impact of Pay Transparency on Realized Wages

The evidence so far suggest that pay transparency in job postings increased market wages. However, as previously discussed, changes in posted wages may not translate to equivalent raises in actual earnings on the job. In this section, we analyze the impact on realized wages using the Glassdoor data and the Quarterly Census of Employment and Wages.

6.1 Dynamic Difference-in-Difference Design

Similar to prior analysis, we estimate the impact of the pay transparency rule on realized wages by estimating difference-in-difference regressions. However, the exact specification depends on the source of the data. First, for the Glassdoor data, we estimate regression specifications of the following form:

$$\log(\text{salary}_{it}) = \sum_{k=-8}^7 \delta_k \cdot \mathbf{1}_{q(t)=k} \cdot \text{Colorado}_j + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \quad (7)$$

where an observation i is a review on Glassdoor. Unlike our analysis with the Lightcast data, which used over 14 million observations on job postings from 2020-2021, the Glassdoor data contains less than 5 million observations over the same time period. In particular, in Glassdoor, it is much less common to observe the same job (i.e. same occupation, firm, and location) in both 2020 and 2021. Therefore, our main specification for the Glassdoor analysis will control for occupation-state and occupation-month fixed effects. These controls imply we compare the evolution of salaries within the same occupation over time between Colorado and other states, but not necessarily within the same employer. All standard errors are clustered at the firm level.

The analysis utilizing the QCEW is not at the worker-firm level, but rather at the industry-county level. In particular, we measure labor-market outcomes at the 4-digit NAICS-by-county level. To estimate the impact of the pay transparency law on labor-market outcomes in the QCEW, we estimate:

$$\log(\text{earnings}_{mt}) = \sum_{k=-8}^7 \delta_k \cdot \mathbf{1}_{q(t)=k} \cdot \text{Colorado}_m + \psi_m + \tau_{c(m),t} + u_{it} \quad (8)$$

where earnings_{mt} is the average weekly earnings for workers in industry m at time t . The key strength of the QCEW is that the data is constructed from information from UI records, which contains coverage of the majority of employment in the U.S. This allows us to have accurate measures of both wages and employment at the labor-market level. The key weakness relative to both Lightcast and Glassdoor is that we cannot control flexibly for occupation or firm. Instead, we include unit fixed effects (ψ_m) for the market (defined as a 4-digit NAICS-by-county cell), as well as industry-by-time fixed effects ($\tau_{c(m),t}$).

6.2 Impact on Realized Wages

Figure 5 plots the δ_k estimates of equation 7 for two samples. First, panel (a) uses the full sample of Glassdoor respondents. To validate our identification assumption, we observe no differences in pre-trends between Colorado and other states prior to the pay transparency law. After the reform though, we see a gradual increase in reported salaries that stabilizes after a year. The gradual increase in earnings may reflect the time it takes for workers to discover their outside options and then either switch jobs or bargain for higher wages with their employer. If that were the case,

we would expect a quicker response in wages among new hires since they are directly exposed to the newly transparent salary information. To test this hypothesis, panel (b) restricts the sample to only individuals who report having worked less than 1 year at their job on Glassdoor. While the estimates are noisier, we find estimates centered around zero in the pre-period followed by an immediate increase in reported earnings in the quarter after the pay transparency rule went into effect.

Table 3 assesses the robustness of our Glassdoor results. Columns (1) and (4) correspond to the estimates in Figure 5, except we replaced the dynamic treatment effects with a simple post-treatment indicator. Our preferred specification for the Glassdoor analysis compares reported salaries in the same occupation over time. As a robustness check of our Glassdoor analysis, columns (2) and (4) estimate the difference-in-difference with state-firm and month-firm fixed effects, which compares salaries within-employer over time. Lastly, for a fully saturated model, columns (3) and (6) include the full interaction of state-occupation-firm and month-occupation-firm fixed effects. This strict specification compares the evolution of salaries over time between Colorado and other states, specifically for jobs within the same occupation-firm. As we include these stricter fixed effects, the number of effective observations drop as they become absorbed in the controls.

Overall, the estimates remain fairly similar across all specifications. In all cases, there is a statistically significant positive estimate for the impact of the pay transparency rule on workers' earnings. The point estimates for the full sample range from 0.9% to 1.4%, whereas those for solely new hires range from 1.8% to 3.0%. These estimates are comparable to the impact on posted salaries that we estimated in section 5. Similar to the analysis in the previous section, we show in Appendix Table A.5 that our results for realized wages is not driven by the contemporaneous increase in the minimum wage. Whether we restrict the sample to jobs that paid above \$14 on average in 2020 or only states that raised the minimum wage, we continue to find a similar impact on reported salaries. Taken together, our analysis consistently finds a positive impact on workers' earnings as a result of increased pay transparency in job postings.

Turning to the QCEW, we find similar overall results. Panel (a) of Figure 6 plots the estimate wage impacts in the QCEW data. Similar to the Glassdoor results, we see a gradual increase in log average weekly wages salaries that stabilizes after a year. Overall, after two years, the increase in wages is about 3%, slightly larger though qualitatively similar to the Glassdoor results. Table 4 summarizes the results. Our preferred specification in Column (1) finds that there is a 1.4% increase in wages following the passage of the pay transparency law. The remaining columns of Table 4 assess the robustness of these results. Column (2) weights each observation by the average employment level in the corresponding 4-digit NAICS-by-county cell in the year prior to the policy change. Columns (3) and (4) again test whether the change may be attributed to a contemporaneous increase in the minimum wage by restricting to markets that pay above the minimum wage and states that had a similar increase in the minimum wage. In all of these alternative specifications we continue to find qualitatively similar results.

7 Impact on Number of Postings and Employment

So far, we have found that the Equal Pay for Equal Work Act increased the presence of salary information in postings, and in turn, increased the level of posted and realized wages, even within the same job (i.e. same firm, occupation, and location). However, it is possible that the act decreased the number of jobs in Colorado. After the passing of the act, there were reports that some employers sought to exclude Colorado workers from applying to jobs (Rubino, 2021). In the next section we estimate the impact of the pay transparency law on the number of postings in the Lightcast data and the employment level in the QCEW.

7.1 Impact on Number of Postings and Employment

In the first part of this section we use Lightcast data to understand aggregate trends in postings. In panel (a) of Figure 7, we plot the number of postings in Colorado vs. all other states, relative to the number of postings in December 2020. As can be seen in the figure, there are some large shifts in the number of postings throughout the year. For example, in January 2021, there are about 1.2 times as many postings as in December 2020. However, this increase in postings is nearly identical in Colorado vs. other states. In general, the trends are quite similar, and there is no evidence of an aggregate decline in the number of postings in Colorado, which could theoretically occur if firms decide to shift work to states that do not have pay transparency laws.

To formally estimate the impact of the Equal Pay for Equal Work Act on number of postings, we implement a dynamic difference-in-difference design similar to the prior specifications:

$$Postings_{it} = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot Colorado_j + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \quad (9)$$

where $Postings_{it}$ is the number of postings in a firm-SOC-FIPS cell i in month t . Therefore, the unit of analysis for this specification is a firm-SOC-FIPS cell rather than a posting. If there are no postings for a given firm-SOC-FIPS cell, then the number of postings is equal to zero. In other words, unlike much of the prior analysis, the panel is balanced for this specification by construction. The rest of the variables are defined in the same manner as Equation (5), and standard errors are again clustered at the employer level.

Panel (b) of Figure 7 plots $\hat{\delta}_k$ from estimating Equation (9). As can be seen in the figure, there is no clear evidence that the number of postings decreased in Colorado. To summarize the effect on the number of vacancies, Table 5 estimates a similar regression to Equation (9), but replaces the month-specific estimates with a simple post-2021 dummy interacted with a dummy for Colorado state. As expected, column (1) finds no statistically significant effect on the number of job postings in the full sample. The 95% confidence bounds can rule out any decrease in postings larger than 0.025 per month, which is economically small relative to the baseline of 0.4 postings per month in 2020 for the average firm-SOC-FIP.

We show the robustness of the null employment effect to alternative specifications in columns

(2)-(5) of Table 5. Since occupations that experienced a larger increase in the share of postings with salary info also experienced a larger increase in posted wages (see Figure 4), we might expect to find a greater decrease in vacancies among jobs with above average transparency effects. To test for heterogeneity in labor demand response by exposure to the policy, we repeat the analysis using two different partitions of the data. First, we separate firm-SOC-FIPS by whether their 2-digit occupation code had an above or below median first stage impact on pay transparency. Columns (2) and (3) reports the difference-in-difference estimates for these two groups and find no heterogeneous treatment effects. To take a more non-parametric approach, Appendix Figure A.9 plots the the effect on number of job postings within each 2-digit occupation code, ordered by the magnitude of their first-stage transparency response. Unlike Figure 4 where we observed a positive correlation between the posted wage effect and the pay transparency effect, there is no correlation between the change in employment and the first-stage.

One important caveat to the analysis on the number of job postings is that avoidance behavior in remote jobs may not be captured in this design. For example, a remote job for a company located in California may specify that the work can be performed anywhere, except Colorado. We will not capture this as a reduction in the number of jobs posted in Colorado, as this will be coded as a California job in the data. There is evidence that some firms did exclude Colorado workers in the aftermath of the passage of the EPEWA. For example, an Atlantic article (Desai, 2021) found that some well-known companies such as Nike and Oracle posted job advertisements that excluded Colorado workers.⁹ The Lightcast data allows us to understand if companies shift work to other states, but not necessarily whether certain remote jobs exclude Colorado workers.

To directly measure the impact on employment in Colorado, we return to the QCEW data and estimate:

$$\log(employment_{mt}) = \sum_{k=-8}^7 \delta_k \cdot \mathbf{1}_{q(t)=k} \cdot Colorado_m + \psi_m + \tau_{c(m),t} + u_{it} \quad (10)$$

Panel (b) of Figure 6 plots the impact of the pay transparency law on employment. As can be seen in the figure, if anything, employment in Colorado increased relative to other states. However, this interpretation should be made with caution. There is a slight pretrend in employment, and any impact on employment is only temporary. However, these results do not find any evidence that the pay transparency laws led to a decrease in employment in Colorado.

8 Conclusion

This paper studies the labor market effects of a 2021 law in Colorado that requires employers to include compensation information in all job postings. Using the near universe of online job postings data from Lightcast, we show that the policy led to a sharp increase in the share of job postings containing salary information. The transparency effect is strongest among large firms that were less

⁹The Colorado Department of Labor has since clarified that any employer with workers in Colorado need to include salary ranges on remote jobs.

likely to post salaries at baseline compared to small firms. Comparing the change in salaries of job in Colorado to that of other states, we find evidence that the policy caused employers to post salaries for high paying jobs that they would have otherwise preferred to bargain over wages. Controlling for composition changes by comparing salaries for the same job (i.e. same firm, occupation, and location) both before and after the policy, we find that the increase in transparency is accompanied by a 3.6% increase in posted salaries. It is perhaps surprising that the policy increased the salaries of jobs that were already posting salaries. However, we show evidence that the increase in posted salaries reflects a general equilibrium response. In particular, the positive wage response was larger among occupations that experienced the largest increase in pay transparency.

Consistent with the increase in posted wages, we analyze labor market data from Glassdoor and the QCEW, finding an increase in realized salaries of similar magnitude. The positive income effect exists among both incumbents and new hires, although there is suggestive evidence that the adjustment was quicker for job switchers. The policy could have had an effect on incumbents for two reasons. First, if fairness concerns incline incumbents to demand salaries at least equal to new hires at the same firm, then increases in the salaries of entrants will affect the salaries of incumbents too (Quach, 2024). Second, information on the availability of outside options may cause incumbents to bargain for higher salaries (Caldwell and Harmon, 2019).

While a growing literature has explored the effects of within-firm pay transparency on wages, far less is known about the effects of pay transparency across firms. The results thus far suggests that these policies accomplish their intended goal of raising workers' salaries, but there are remaining areas for future research. It would be important to examine the impact of such laws on inequality, gender pay gaps, and racial pay disparities. It would also be interesting to link the empirical findings to theories of how employers choose between wage posting and wage bargaining protocols.

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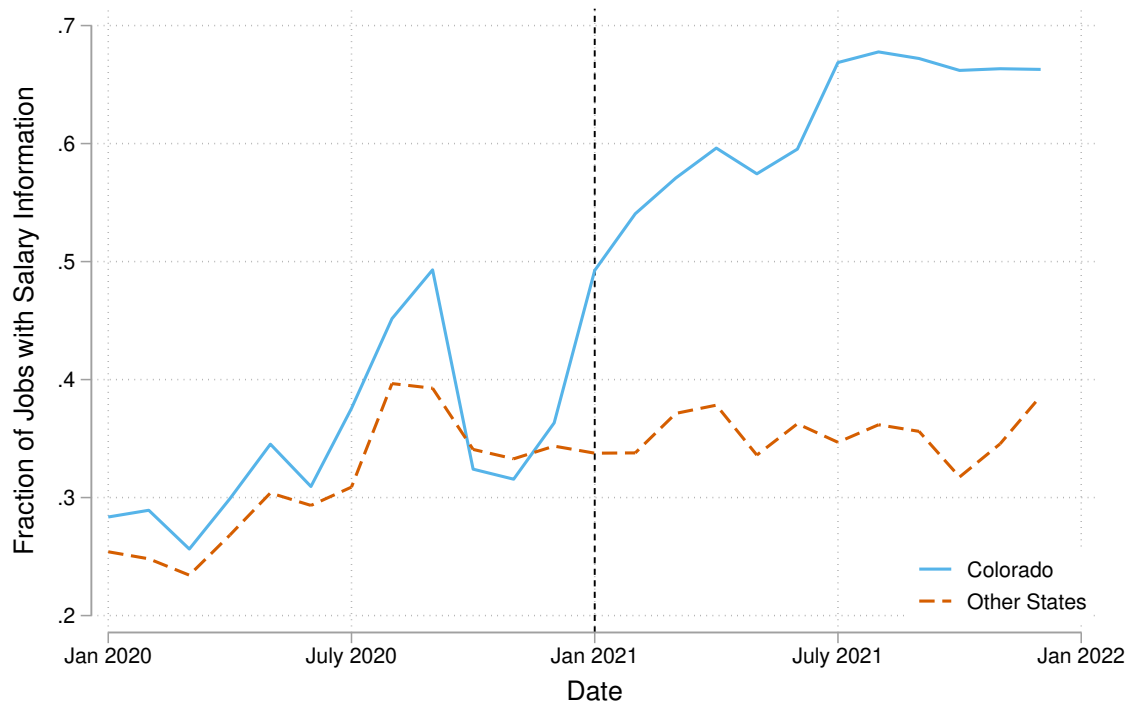
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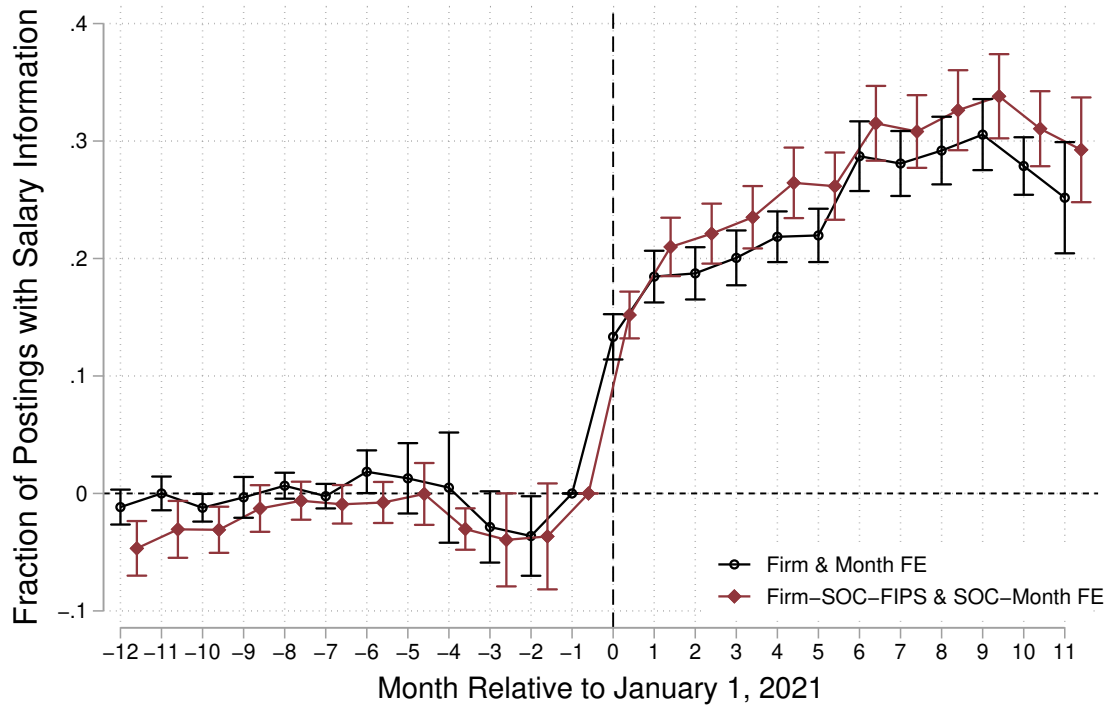
Skoda, Samuel, “Directing Job Search in Practice: Mandating Pay Information in Job Ads,” *JMP*, 2022.

Figure 1: Fraction of Postings with Salary Information by State



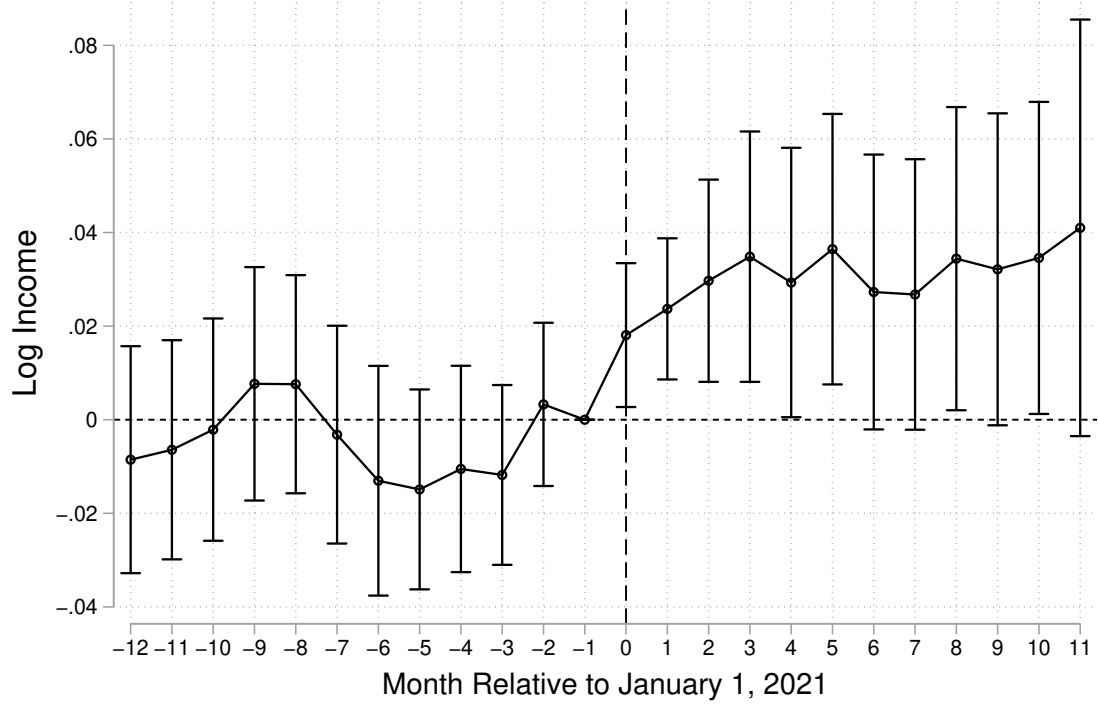
Note: This figure reports the fractions of job postings that contain salary information separately for Colorado and all other states.

Figure 2: Impact of Pay Transparency Law on Fractions of Postings with Salary Information



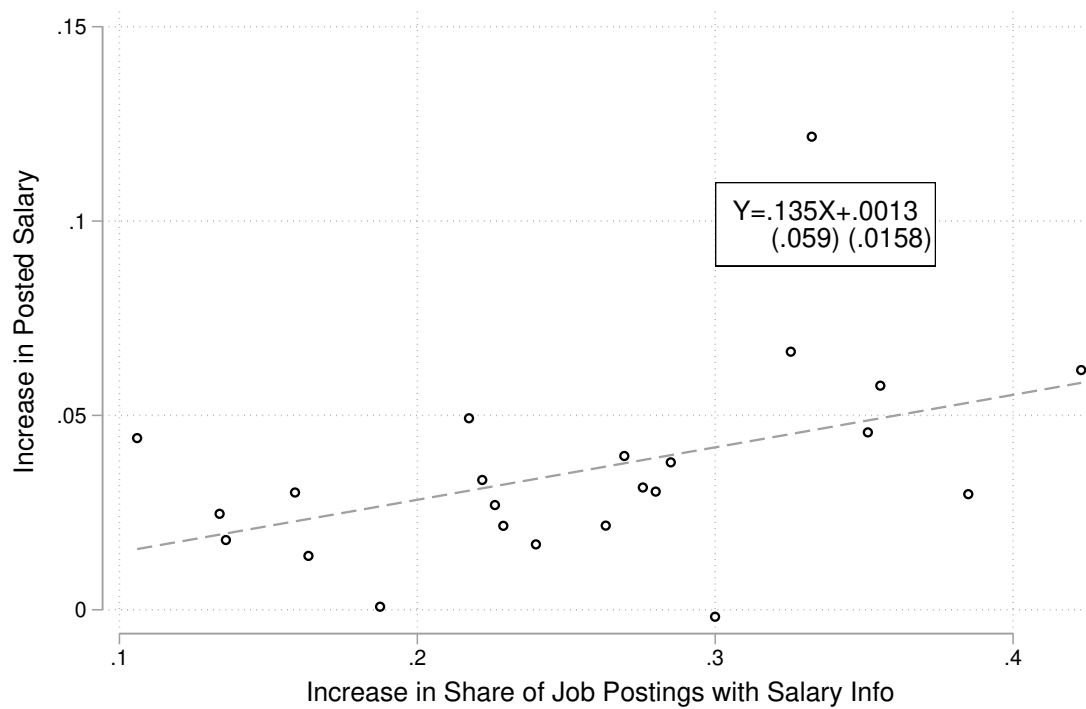
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information. The hollow circles include specifications that control for firm fixed effects and month fixed effects. The solid diamonds control for firm-SOC-FIPS and SOC-month fixed effects, where the SOC is the 6-digit industry code and FIPS is the county code. 95 percent confidence intervals clustered at the firm level are displayed.

Figure 3: Impact of Pay Transparency Law on Log Posted Salary



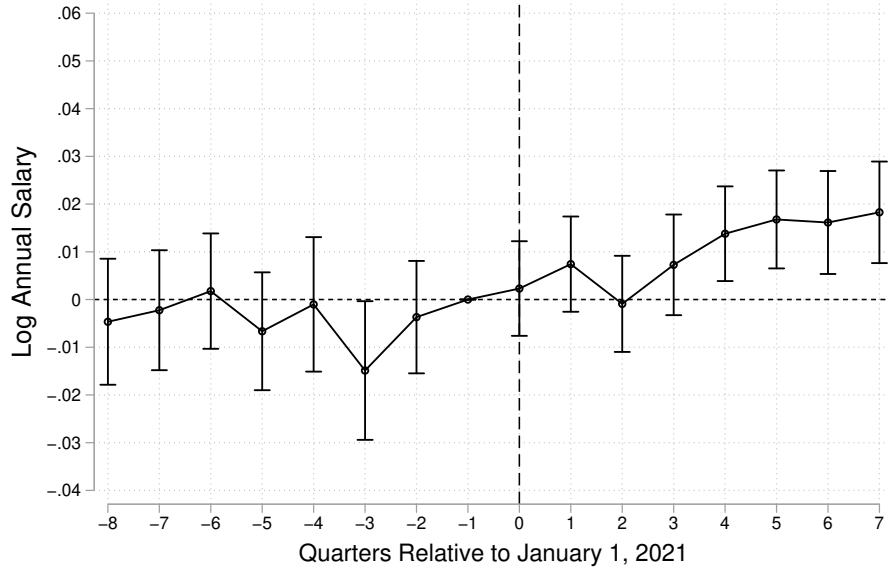
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary following the specification in Equation (6). This specification controls for firm-SOC-FIPS fixed effects and SOC-month fixed effects. If a posting has a lower and upper bound for a salary, the expected salary is equal to the average between the two. 95 percent confidence intervals clustered at the firm level are displayed.

Figure 4: Impact of Pay Transparency Law on Log Posted Salary vs. Share of Postings with Salary Info, by Occupation

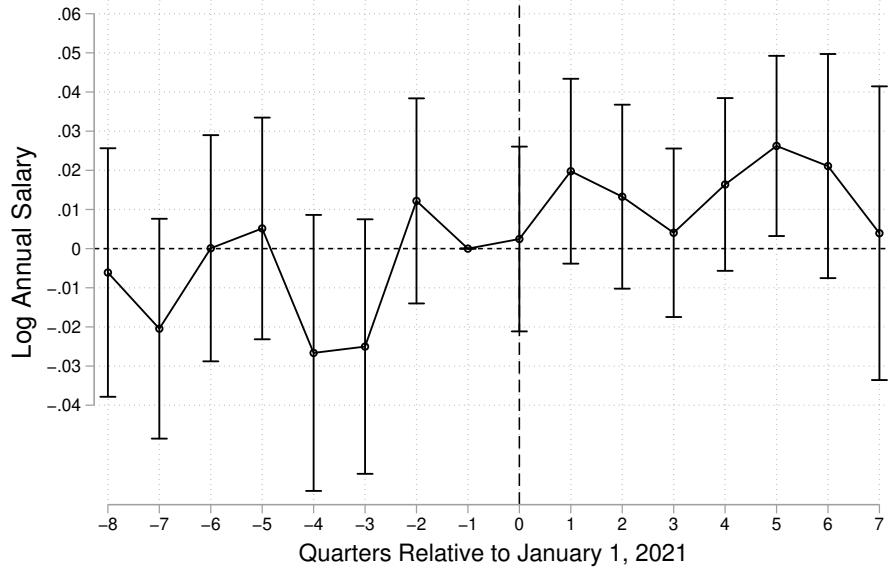


Note: This figure plots the point estimates of the wage effect of the Colorado pay transparency law against the effect on the share of postings with salary info, where each point represents a 2-digit occupation group. The equation in the box reports the estimates of the OLS prediction line, along with standard errors in parentheses.

Figure 5: Impact of Pay Transparency Law on Realized Salaries, Glassdoor



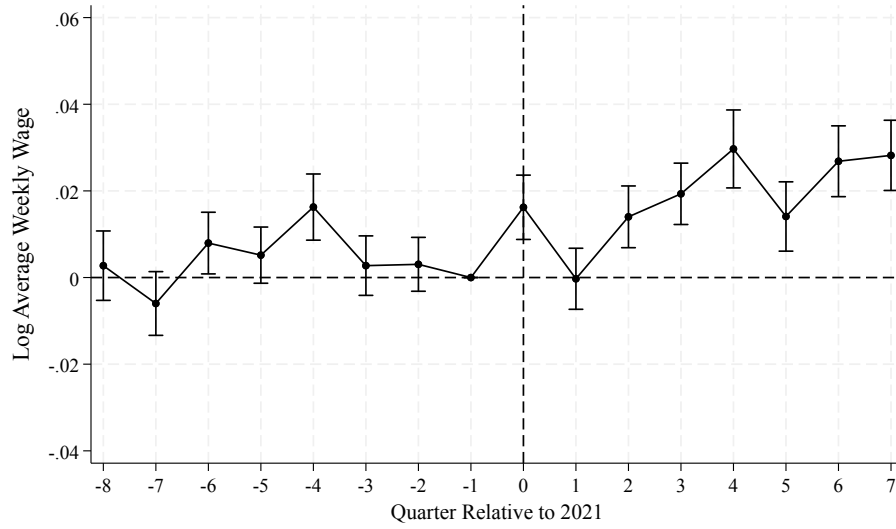
(a) Glassdoor: Full Sample



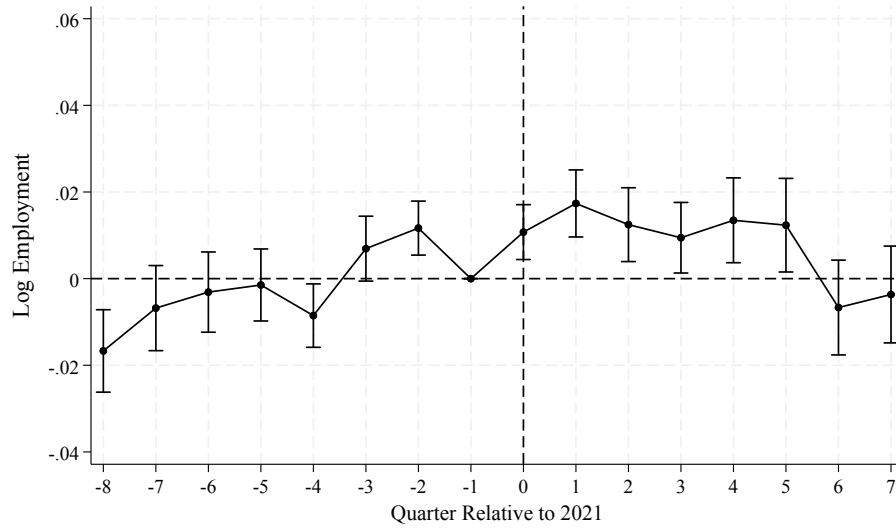
(b) Glassdoor: New Hires

Note: This figure plots the impact of the Colorado pay transparency law on the logarithm of reported salaries following the specification in Equation (6). Panel (a) uses the full sample in the Glassdoor data. Panel (b) restricts the sample to only individuals who reported less than 1 year of work experience. Each specification controls for SOC-state fixed effects and SOC-month fixed effects. Standard errors are clustered at the firm level.

Figure 6: Impact of Pay Transparency Law on Average Weekly Wages and Employment, QCEW



(a) Log Average Weekly Wage



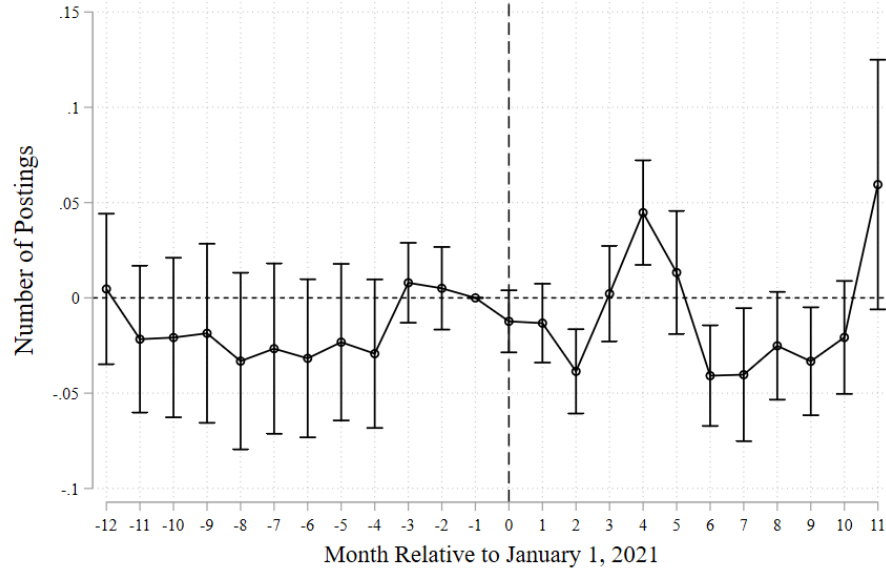
(b) Log Employment

Note: The figure plots the impact of the pay transparency law in Colorado on labor-market outcomes measured in the Quarterly Census of Employment and Wages (QCEW). Panel (a) plots difference-in-differences estimates of the impact of the pay transparency law on log average weekly wage measured at the 4-digit NAICS-by-county level. Panel (b) plots difference-in-differences estimates of the impact of the pay transparency law on log employment in the 4-digit NAICS-by-county level cell. Each specification controls for 4 digit NAICS-FIPS fixed effects and 4 digit NAICS-quarter fixed effects. 95 percent confidence intervals clustered at the 4-digit NAICS-by-county level are displayed.

Figure 7: Impact of Pay Transparency Law on Number of Job Postings



(a) State Trends



(b) Regression Specification Controlling for Firm-SOC-FIPS

Note: The figure plots the impact of the pay transparency law in Colorado on the number of job postings. Panel (a) plots the number of job postings in Colorado vs. all other states relative to the number of postings in December 2020. Therefore, the value for both Colorado and other states is mechanically equal to one in December 2020. Panel (b) aggregates postings at the firm-SOC-FIPS level, with the outcome being the number of postings in the firm-SOC-FIPS cell. The regression controls for firm-SOC-FIPS fixed effects and SOC-month fixed effects. 95 percent confidence intervals clustered at the firm level are displayed.

Table 1: Characteristics of Jobs in Colorado vs. Other States Before Passage of Pay Transparency in Online Postings Law

	Colorado	Other States
	(1)	(2)
<i>Panel A: Salary Information</i>		
Contains Salary Information	0.34	0.31
Minimum Posted Salary	47,178.16	44,520.12
Maximum Posted Salary	59,354.46	56,958.15
Average Posted Salary	53,266.31	50,739.13
Posted a Range	0.22	0.21
<i>Panel B: Occupational Characteristics</i>		
Management	0.10	0.11
Business and Financial Operations	0.06	0.06
Computer and Math	0.10	0.09
Architecture and Engineering	0.03	0.02
Life, Physical, and Social Science	0.01	0.01
Community and Social Service	0.01	0.01
Legal	0.01	0.01
Education	0.03	0.03
Arts and Entertainment	0.02	0.02
Healthcare Practitioner	0.10	0.11
Healthcare Support	0.03	0.03
Protective Services	0.01	0.02
Food Services	0.05	0.05
Building and Grounds Maintenance	0.02	0.02
Personal Care and Service	0.03	0.02
Sales	0.11	0.12
Office and Administrative Support	0.11	0.11
Construction and Extraction	0.02	0.01
Installation, Maintenance, and Repair	0.04	0.04
Production	0.02	0.03
Transportation	0.09	0.08
Unique Employers	63,729	1,322,088
Unique Employer-Occupations	211,008	4,824,788
Unique Employer-Occupations-County-Months	533,428	18,245,394
Total Job Postings	818,461	27,258,007

Note: This table displays the average characteristics for the analysis sample in 2020, the year before the Equal Pay for Equal Work Act became effective. The sample is composed of all jobs in the Lightcast dataset with non-missing location, employer, and occupation information.

Table 2: Effect of Transparency Law on Posted Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Post · Colorado</i>	.036 (.006)	.044 (.007)	.032 (.006)	.013 (.003)	.05 (.008)	.018 (.005)	.073 (.009)
Sample	All	Above MW	Similar MW	Low Trans.	Full Trans.	All	All
Firm FE							X
Time FE							X
Firm-SOC-FIPS FE	X	X	X	X	X	X	
SOC-Time FE	X	X	X	X	X		
Firm-SOC-Time FE						X	
N	14,465,056	8,611,123	3,965,636	1,091,920	7,516,337	9,470,249	19,901,376

Note: This table displays difference-in-difference estimates that compare the log posted salaries in Colorado to other US states, before and after 2021, for various samples of the data. Column (1) keeps the full data sample. Column (2) keeps only firm-SOC-FIPS with an average wage above \$14/hr in 2020. Column (3) restricts the control group to the 15 states with minimum wage changes of less than 8%. Columns (4) and (5) imposes the same restriction as column (2), but separates the sample into firm-SOC-FIPS with less than 100% and equal to 100% transparency in 2020. Column (6) estimates the difference-in-differences using the full sample while including firm-SOC-month fixed effects. Column (7) controls only for employer and time fixed effects. Standard errors are clustered at the firm level.

Table 3: Effect of Transparency Law on Log Reported Salaries in Glassdoor

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Post · Colorado</i>	.013 (.002)	.014 (.003)	.009 (.002)	.018 (.003)	.029 (.006)	.03 (.005)
Sample	All	All	All	New Hires	New Hires	New Hires
Month-Occupation FE	X			X		
State-Occupation FE	X			X		
Month-Firm FE		X			X	
State-Firm FE		X			X	
Month-Occ-Firm FE			X			X
State-Occ-Firm FE			X			X
Data	Glassdoor	Glassdoor	Glassdoor	Glassdoor	Glassdoor	Glassdoor
N	10,068,696	7,443,201	3,126,783	1,156,113	655,110	261,740

Note: This table displays difference-in-difference estimates that compare the log salaries in Colorado to other US states, before and after 2021, for various samples. Columns (1)-(3) use the full Glassdoor sample. Columns (4)-(6) use new hires in the Glassdoor data. Standard errors are clustered at the firm level.

Table 4: Effect of Transparency Law on Log Average Weekly Wage in QCEW

	(1)	(2)	(3)	(4)
<i>Post · Colorado</i>	.014 (.002)	.011 (.003)	.017 (.003)	.013 (.002)
Sample	All	All	Above MW	Similar MW
4-digit-NAICS-FIPS FE	X	X	X	X
Weighted	No	Employment	No	No
N	2,910,054	2,848,822	1,969,874	670,553
Data	QCEW	QCEW	QCEW	QCEW

Note: This table displays difference-in-difference estimates that estimate the impact of Colorado's pay transparency law on the log average weekly salary at the 4-digit NAICS-by-county level. Column (1) is unweighted (i.e. each 4-digit NAICS-by-county cell receives a weight of one). Column (2) weights the regression by the pre-event (before 2020) average level of employment in the 4-digit NAICS-by-county cell. Column (3) keeps only 4-digit NAICS-by-county level cells that have an average wage of \$14 per hour before the pay transparency policy. Column (4) keeps only states that had similarly-sized changes in the minimum wage as Colorado in 2021.

Table 5: Effect of Transparency Law on Number of Job Postings

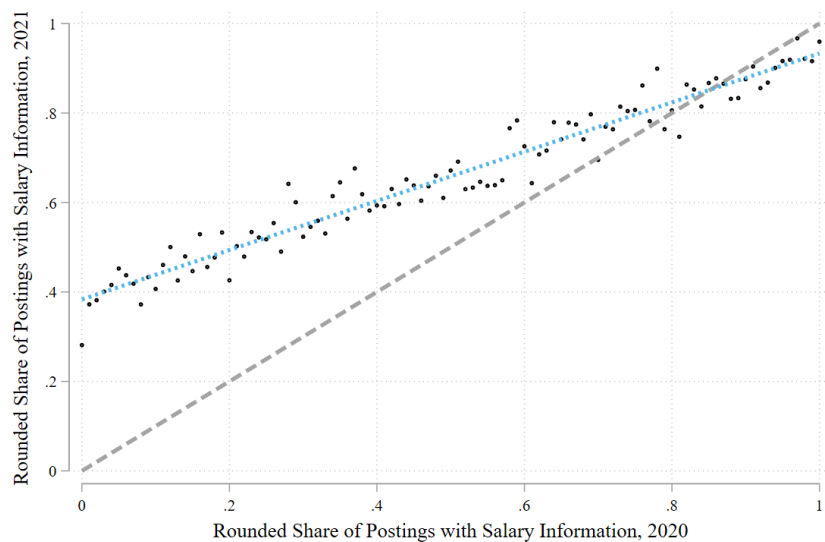
	(1)	(2)	(3)	(4)	(5)
<i>Post · Colorado</i>	.006 (.016)	.059 (.063)	-.004 (.01)	-.001 (.031)	.012 (.005)
Sample	All	Above Median	Below Median	High Trans.	Low Trans.
Firm-SOC-FIPS FE	X	X	X	X	X
SOC-Time FE	X	X	X	X	X
Avg. postings per month in 2020	.398	.457	.422	.522	.277
N	69,295,584	10,760,448	58,535,136	34,275,024	35,020,176

Note: This table displays difference-in-difference estimates that compare the number of job postings in Colorado to other US states, before and after 2021, for various samples of the data. Column (1) keeps the full data sample. Column (2) keeps only 2-digit occupations with above median transparency effect. Column (3) keeps only 2-digit occupations with below or equal to median transparency effect. Column (4) keeps only firms with more than half of postings in 2020 with salary information. Column (5) keeps only firms with less than or equal to half of postings in 2020 with salary information.

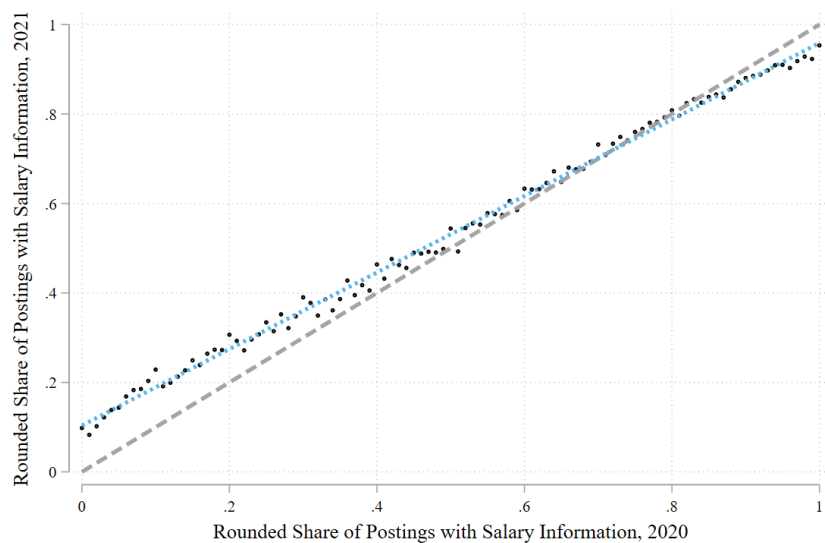
Appendix: For Online Publication

Appendix A. Additional figures and tables

Appendix figure A.1: Share of Postings with Salary Information 2021 vs 2020, by Employer



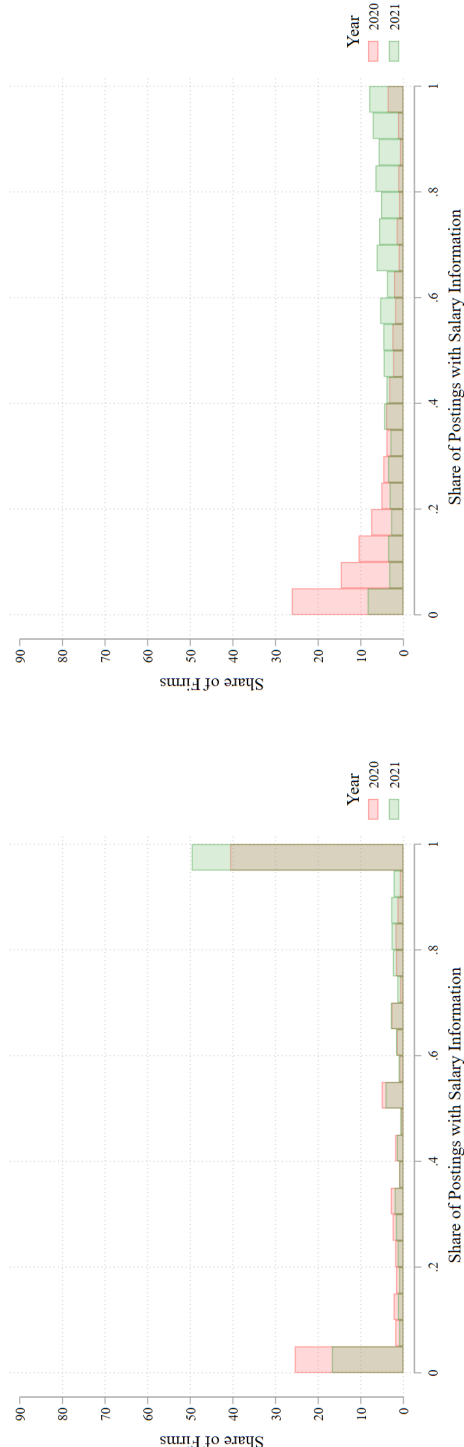
(a) Colorado



(b) Other States

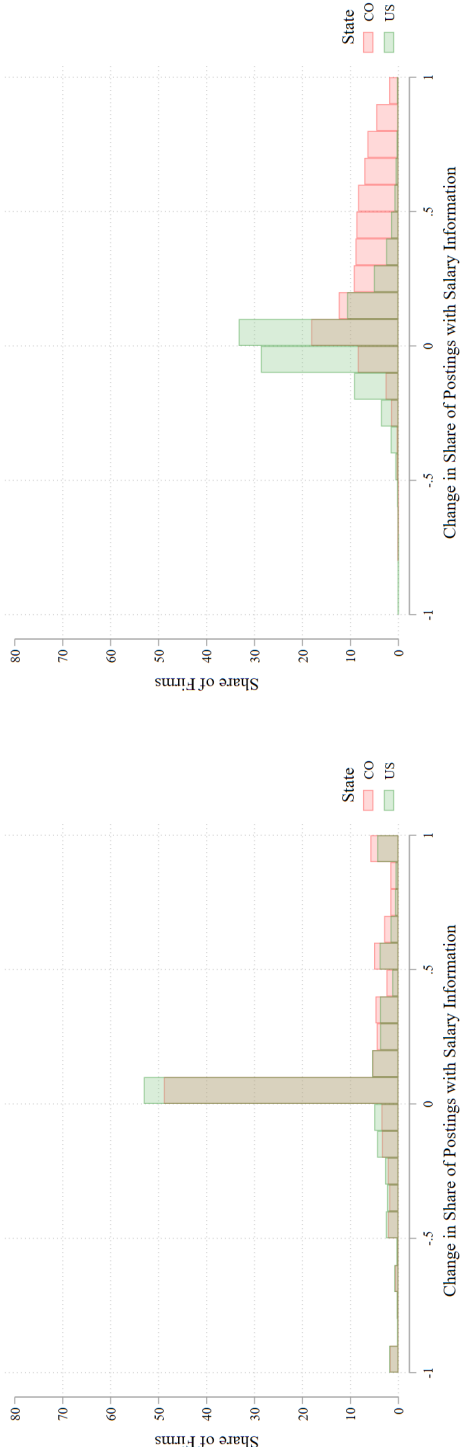
Note: The figure plots the share of postings in 2021 with salary information as a function of the share of postings by the same employer with salary information in 2020. Employers are averaged along the horizontal axis in 0.01 bins. The dotted blue line denotes the predicted values of an OLS regression, and the dashed 45-degree line represents the share of postings with salary information if employers never change their behavior.

Appendix figure A.2: Distribution Postings with Salary Information, 2020 vs 2021

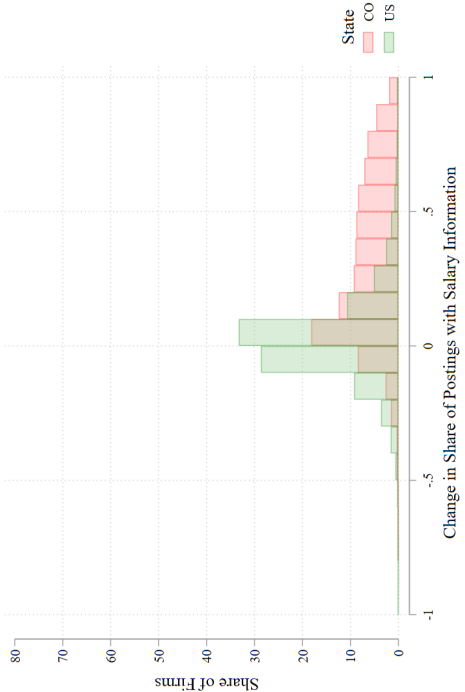


(a) Colorado, 10-100 Postings

(b) Colorado, >100 Postings



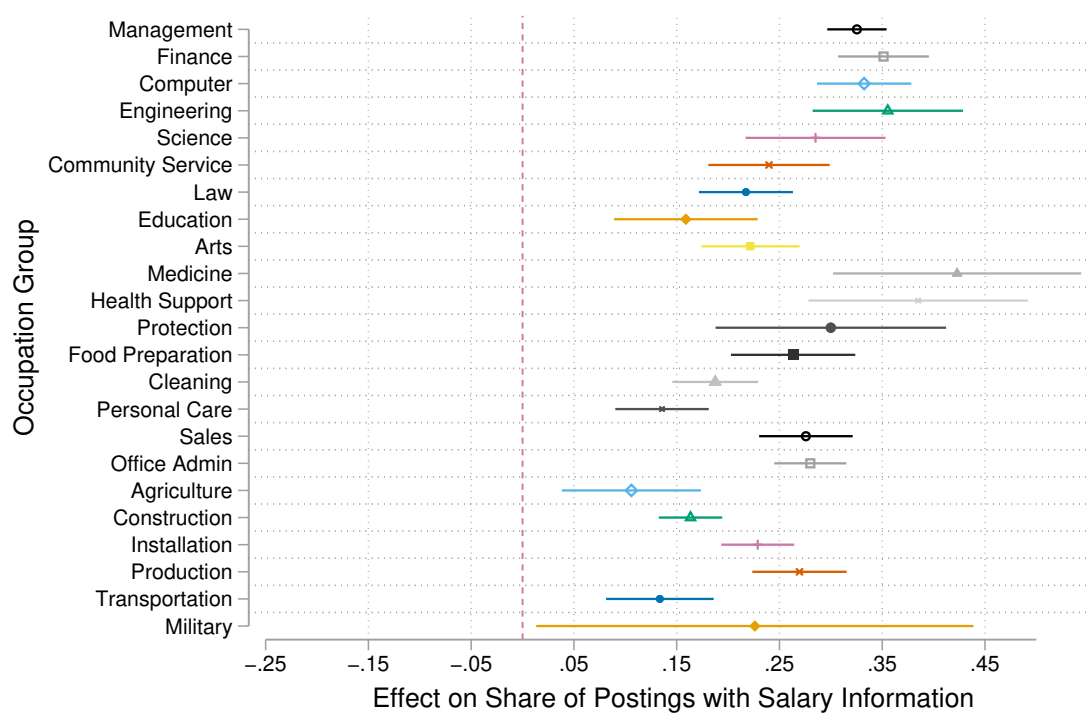
(c) Colorado vs. U.S., 10-100 Postings



(d) Colorado vs. U.S., >100 Postings

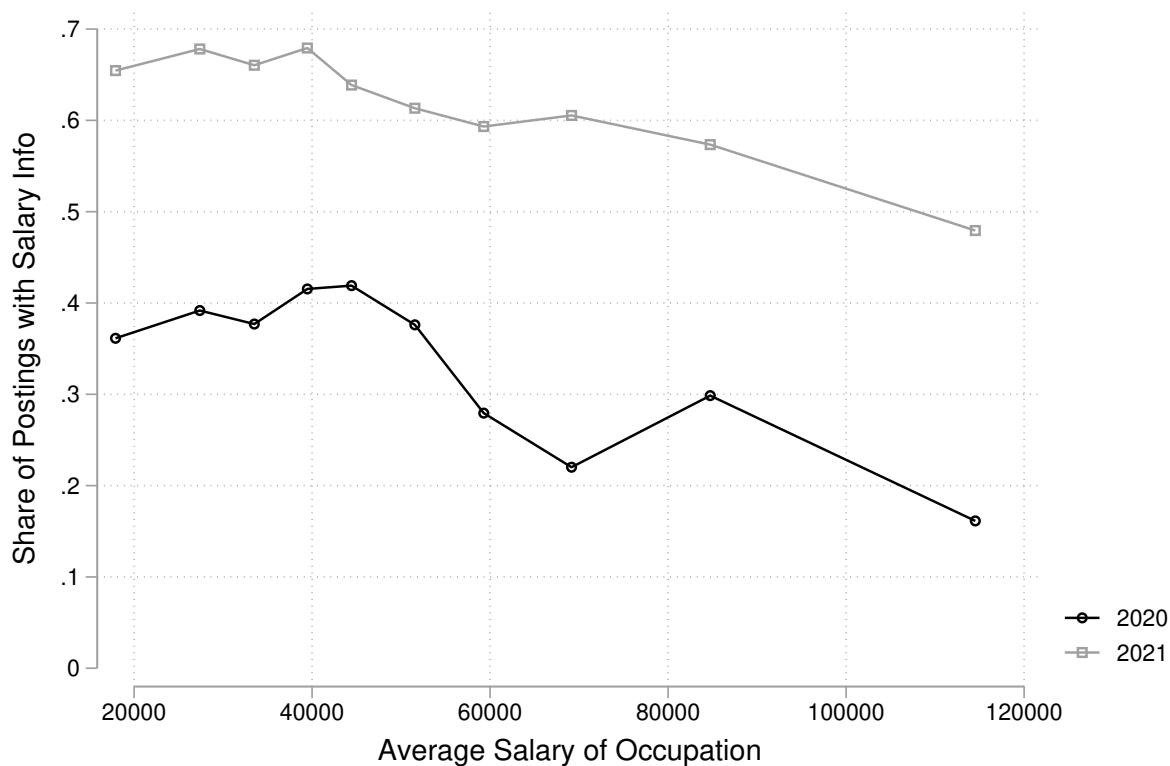
Note: Panel (a) plots the distribution of employers in Colorado by the share of their postings with salary information, separately for 2020 and 2021. The sample is restricted to employers that post jobs in Colorado in both years, with between 10-100 postings in 2020. Panel (b) plots an analogous figure for firms in Colorado with at least 100 postings. Panel (c) plots the distribution of firms by the change in their share of postings with salary information between 2020 and 2021, restricting the sample to small firms. Panel (d) plots a similar distribution for large firms.

Appendix figure A.3: Impact of Pay Transparency Law on Fractions of Postings with Salary Information, by Occupation



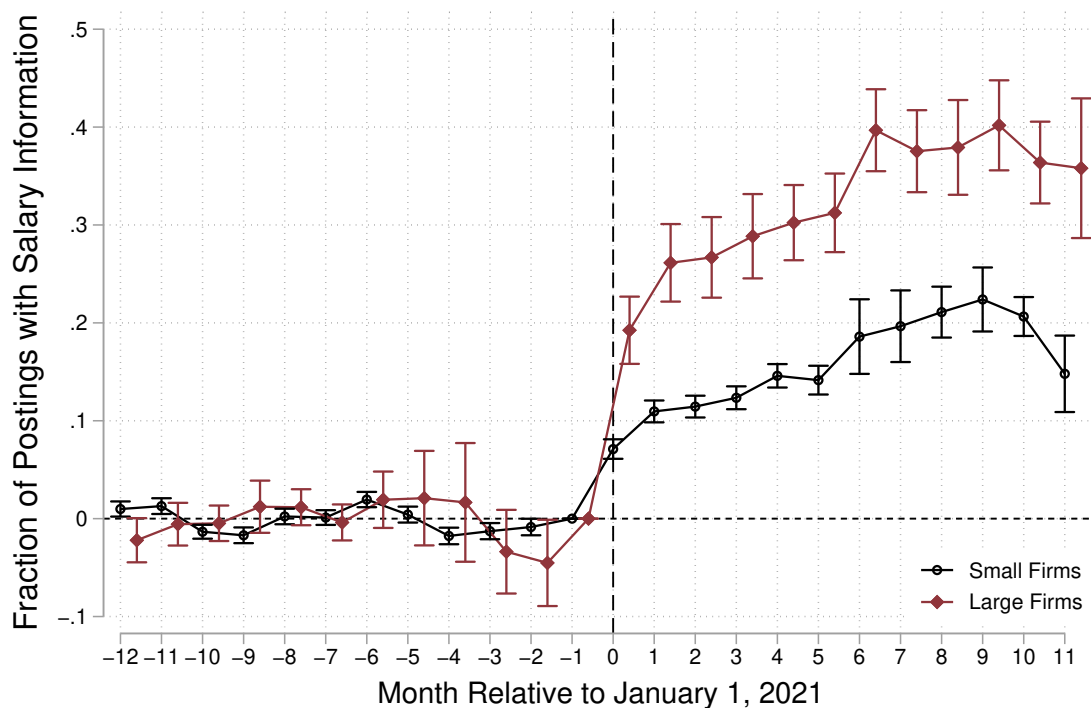
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for each 2-digit SOC occupation code, following the specification in Equation 5. 95 percent confidence intervals clustered at the firm level are displayed.

Appendix figure A.4: Transparency by Wage of Occupation, within Colorado



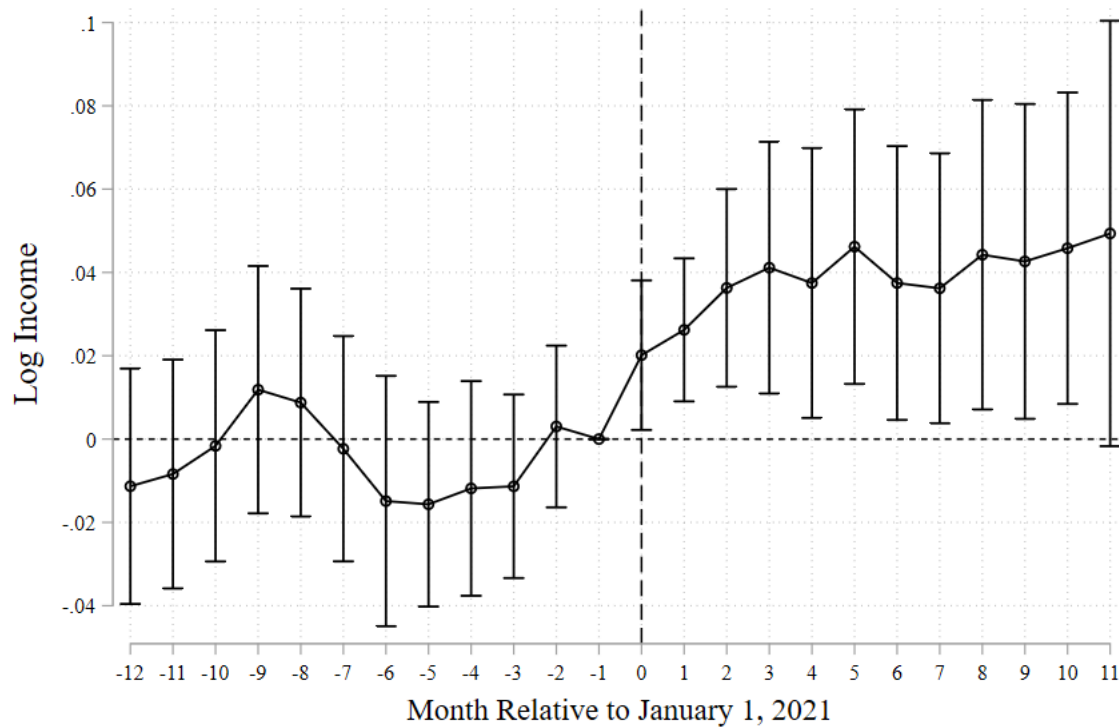
Note: The figure plots the average probability that a job posting has salary info, as a function of the average salary of the posting's 5-digit SOC code computed from the 2015-2020 ACS. The postings are aggregated over deciles of average salary across occupations.

Appendix figure A.5: Impact of Pay Transparency Law on Fractions of Postings with Salary Information, by Size

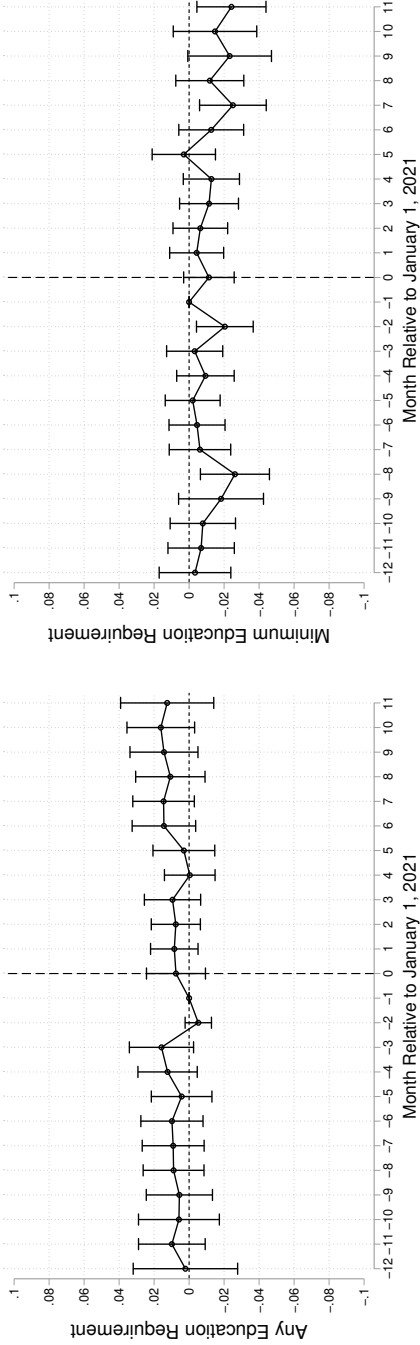


Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for firm-states with fewer than 100 posting in 2020 and firm-states with more than 100 postings in 2020. 95 percent confidence intervals clustered at the firm level are displayed.

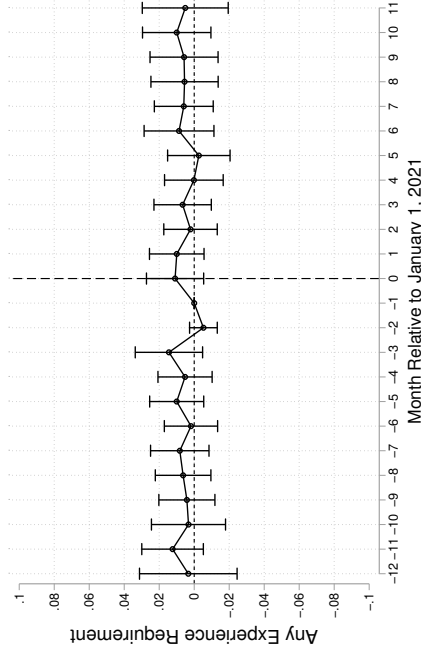
Appendix figure A.6: Impact of Pay Transparency Law on Log Posted Salary for Jobs with Wage $\geq \$14/\text{hr}$ in 2020



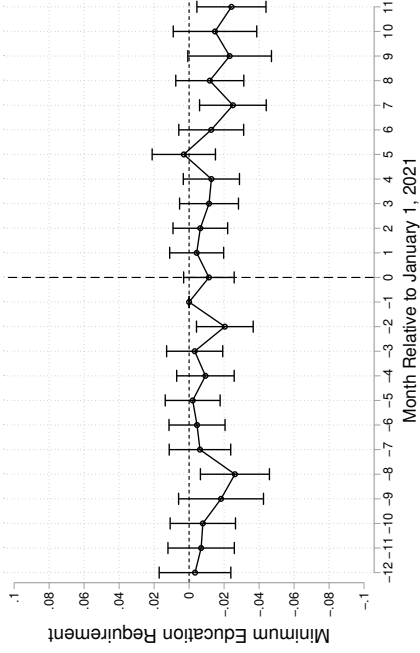
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary following the specification in Equation (6). The sample is restricted to firm-SOC-FIPS with an average wage of at least \$14/hr in 2020. This specification controls for firm-SOC-FIPS fixed effects and SOC-month fixed effects. If a posting has a lower and upper bound for a salary, the expected salary is equal to the average between the two. 95 percent confidence intervals clustered at the firm level are displayed.



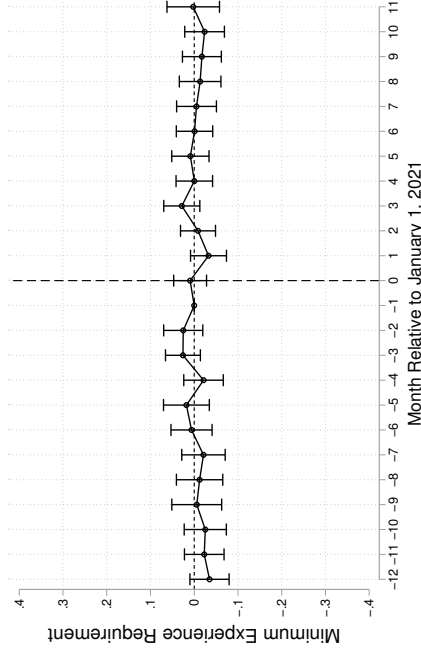
(a) Any Edu Requirement



(c) Any Exp Requirement



(b) Min Edu Requirement

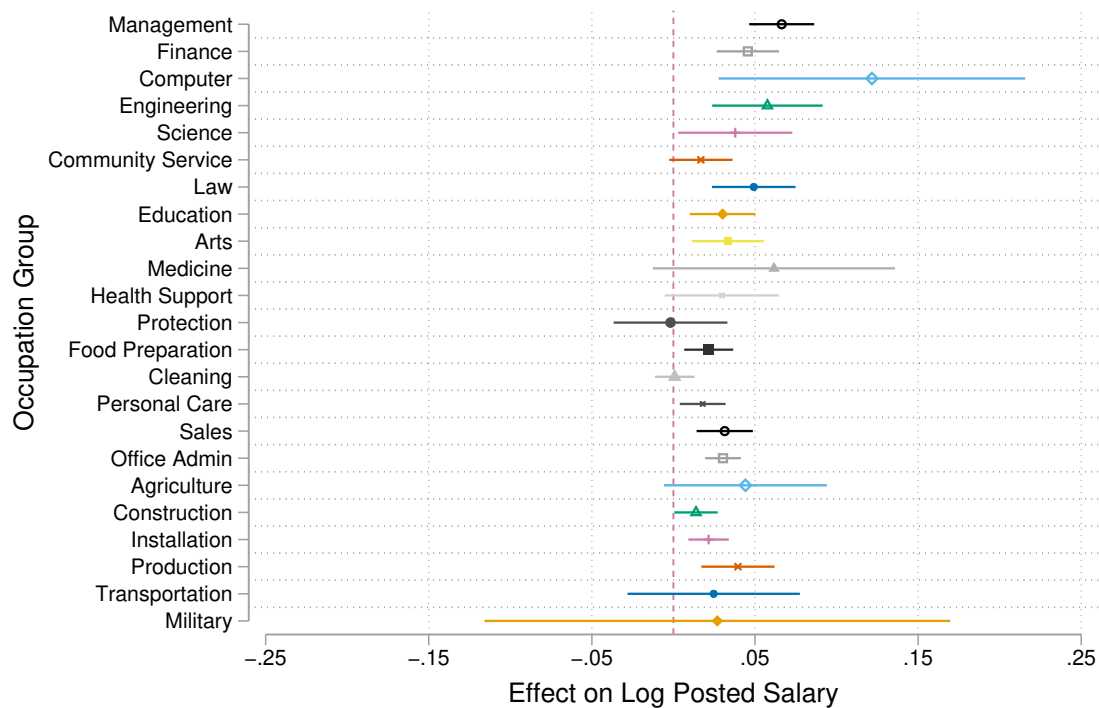


(d) Min Exp Requirement

Appendix figure A.7: Effect of Pay Transparency Law on Education and Experience Requirements

Note: The figure plots estimates of Equation (6) for four outcome variables: 1) a dummy for whether a posting includes any education requirement, 2) the minimum number of years of education, 3) a dummy for any experience requirement, and 4) the minimum number of years of experience. All specifications control for Firm-SOC-FIPS and SOC-time fixed effects.

Appendix figure A.8: Impact of Pay Transparency Law on Log Posted Salary, by Occupation



Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary, separately for each 2-digit SOC occupation code. 95 percent confidence intervals clustered at the firm level are displayed.

Appendix figure A.9: Impact of Pay Transparency Law on Number of Job Postings vs. Impact on Share of Postings with Salary Info, by Occupation



Note: This figure plots the point estimates of the employment effect of the Colorado pay transparency law against the effect on the share of postings with salary information, where each point represents a 2-digit occupation group.

Appendix Table A.1: Number of Observations

	2020	2021
Total number of postings	36,470,652	45,517,309
After dropping NA employers	29,830,697	37,824,804
After dropping NA FIPS	29,556,563	37,220,114
After dropping NA SOC	28,391,282	35,648,744
After dropping unmatched year	28,391,282	35,617,484
After dropping unmatched month	28,076,468	35,147,684

Note: This table displays number of postings observed in 2020 and 2021 respectively after dropping missing employers, states, occupations, and locations.

Appendix Table A.2: The Role of Employer and Occupation in Predicting Compliance

	(1)	(2)	(3)	(4)	(5)	(6)
R^2	.458	.045	.464	.569	.598	.615
Adj. R^2	.433	.045	.439	.504	.52	.615
Firm FE	X		X			
Soc FE		X	X			
Firm-SOC FE				X	X	
Firm-FIPS FE					X	
Firm-SOC-FIPS FE						X
N	1,045,807	1,045,807	1,045,807	1,045,807	1,045,807	1,045,807

Note: This table displays R^2 and adjusted R^2 of regressing share of postings that have salary information respectively on (1) employer only, (2) occupation only, (3) employer and occupation, (4) interaction between employer and occupation, (5) employer-occupation and employer-county, and (6) interaction between employer, occupation, and county. The sample is restricted to postings in Colorado in 2021.

Appendix Table A.3: Effect of Transparency Law on the Range of Posted Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Post · Colorado</i>	.035	.044	.037	.044	-.002	-.001	.003	.032
	(.008)	(.009)	(.005)	(.005)	(.006)	(.006)	(.006)	(.011)
Outcome	Log(Max)	Log(Max)	Log(Min)	Log(Min)	Log(Max/Min)	Log(Max/Min)	Log(Max/Min)	Posted Range
Sample	All	Above MW	All	Above MW	All	Above MW	All	All
State FE							X	X
Time FE							X	X
Firm-Soc-Fips FE	X	X	X	X	X	X		
Soc-Time FE	X	X	X	X	X	X		
N	14,465,056	8,611,123	14,465,056	8,611,123	14,465,056	8,611,123	20,624,244	20,624,244

Note: This table displays difference-in-difference estimates that compare postings in Colorado to other US states, before and after 2021. Columns (1) and (2) report the effect on the maximum salary in each job posting, for the full sample and for firm-SOC-FIPS with an average wage above \$14/hr in 2020, respectively. Columns (3)-(4) reports the effects for the minimum posted salary, and columns (5)-(7) report the effect on the ratio of the maximum and minimum salaries. Column (8) reports the effect on whether the job posting is a range or point. Standard errors are clustered at the firm level.

Appendix Table A.4: Effect of Transparency Law on Education and Experience Requirements

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Post · Colorado</i>	-.003 (.006)	.003 (.004)	-.026 (.034)	-.005 (.005)	-.01 (.006)	0.0001 (.005)	-.11 (.041)	.001 (.012)
Outcome	Any Edu.	Any Edu.	Min Edu.	Min Edu.	Any Exp.	Any Exp.	Min Exp.	Min Exp.
Avg. in 2020	.56	.56	14.11	14.11	.47	.47	3.26	3.26
State FE	X		X		X		X	
Time FE	X		X		X		X	
Firm-Soc-Fips FE		X		X		X		X
Soc-Time FE		X		X		X		X
N	62,224,026	51,263,890	34,750,911	27,941,482	62,224,026	51,263,890	29,043,812	23,137,064

Note: This table displays difference-in-difference estimates that compare education and experience requirements in Colorado to other US states, before and after 2021. Column (1) and (2) estimate effects on whether there is an education requirement. Column (3) and (4) estimate effects on minimum education requirement. Column (5) and (6) estimate effects on whether there is an experience requirement. Column (7) and (8) estimate effects on minimum experience requirement. Standard errors are clustered at the firm level.

Appendix Table A.5: Effect of Transparency Law on Realized Wages in Glassdoor

	(1)	(2)	(3)	(4)
<i>Post · Colorado</i>	.017 (.002)	.016 (.003)	.034 (.007)	.021 (.004)
Sample	All	All	New Hires	New Hires
Restriction	Above MW	Similar MW	Above MW	Similar MW
Month-Occupation FE	X	X	X	X
State-Occupation FE	X	X	X	X
Data	Glassdoor	Glassdoor	Glassdoor	Glassdoor
N	1,849,060	6,312,252	134,025	707,868

Note: This table displays difference-in-difference estimates that compare the log salaries in Colorado to other US states, before and after 2021, for various samples. Columns (1)-(2) restricts the full Glassdoor sample to state-occupations with average wages in 2020 above 14 dollars per hour and states with similar minimum wage increases as Colorado, respectively. Columns (3)-(4) repeats the same analysis with the added restriction that individuals have less than 1 year of work experience. Standard errors are clustered at the state level.