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

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Abstract

This paper studies the labor market effects of recent U.S. policies that require employers to disclose salary information in job postings. Leveraging a difference-in-difference design, we show that employers increased the fraction of postings with salary information by 30 percentage points, although there remains substantial noncompliance. Across three datasets, we find consistent evidence of an increase in wages of around 1.3-3.6%. At the same time, we find no impacts on pay dispersion, 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 incumbent workers and firms that were always posting wages, neither of which were directly targeted by the policy.

JEL Codes: J30, J31, J38

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1 Introduction

The vast majority of job postings in the U.S. provide no wage information (Batra et al., 2023). This lack of transparency in the job search process has raised concerns among policymakers that it decreases wage competition and increases employers' marker power. In light of these concerns, there has been a wave of policies to increase pay transparency by making expected salary information a requirement in job postings. On January 1, 2021, Colorado enacted the first such pay transparency law in the U.S., requiring that all online job postings include information about the expected salary of the position. Ten states and multiple local municipalities have since passed similar rules requiring compensation be listed in job postings. 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.

This paper studies the effect of increased wage transparency in job postings by analyzing the roll-out of recent policy changes in the United States. 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 (Jäger et al., 2024), so by giving workers more information about salaries in other firms, they can potentially better aim their search to high-wage jobs. However, firms may also adjust in response to transparency laws, making the overall impact ambiguous. For instance, 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 the labor market, pay transparency within firms has been shown to lower wages, as firms can credibly reject renegotiations when wages become transparent (Cullen and Pakzad-Hurson, 2023). As a result of these competing mechanisms, the impact of pay transparency in job postings is theoretically ambiguous.

Leveraging a difference-in-difference design where we compare states that introduced a pay transparency law to states that did not, our analysis studies the impact of these policies on a wide range of outcomes across multiple datasets. First, we use data from Lightcast, which contains online job postings from over 45,000 websites, to measure compliance with the pay transparency law. 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. Second, we use data on self-reported earnings from Glassdoor to study the impact on realized wages, pay dispersion, and heterogeneity by new hires and incumbents. Given the sample period of the Lightcast and Glassdoor data, we only use them to study the impact of the first pay transparency law in the U.S., which went into effect in Colorado in January 2021. To extend our analysis to all recent policy changes, we implement an event study design using the Quarterly Census of Employment and Wages (QCEW), enabling us to study the impact on administrative wages and employment.

We document five main results. First, our analysis of the Lightcast data finds that the pay transparency law had a large and immediate impact on the fraction of job postings that contain expected salary 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%. We show evidence that firms often comply for certain types of job, but not others. Despite a large increase in pay transparency across the income distribution, the highest paying occupations still tend to be the ones with the lowest transparency, suggesting that employers prefer to bargain over the salaries of these positions.

Second, we find consistent evidence of a positive wage effect. Using the Lightcast data, we find that posted wages increased by 3.6% among jobs that were transparent both before and after the policy change, suggesting a market-wide increase in competition. However, these impacts on posted salaries may not materialize to actual wage gains if firms are posting higher ranges of salaries, but paying at the low end. Turning to Glassdoor data, we find that self-reported earnings increased by 1.3% more in Colorado relative to other states after the reform. This positive wage effect appears for both incumbent workers and new hires, indicating that the policy impacts workers not directly targeted by the law. Leveraging the QCEW, we extend our analysis to all state and municipal policy changes that went into effect before July 2024 and find a similar increase in wages of 1.3%. Thus, our estimates imply that pay transparency in job postings raise average wages and not only for job seekers hired at previously opaque firms, but also for incumbents and for always-transparent firms.

Third, we find no effect on pay dispersion. On one hand, pay transparency can lead to more pay dispersion if firms post wider salary ranges in their vacancies. On the other hand, it can compress wages if firms converge on similar salaries for the same occupation or if employers close pay gaps within the firm. Separately analyzing the impact on the minimum and maximum posted salary in the Lightcast data, we find that employers are not posting wider salary ranges in their vacancies. Thus, the policy does not decrease the informativeness of job postings. Next, using the Glassdoor data, our difference-in-difference estimates find no impact on the standard deviation of wages either within-occupation or within-firm. Overall, the policy appears to have little impact on wage inequality.

Fourth, we also find no negative impact on firm's labor demand. Given the increase in workers' salaries, employers can potentially respond by recruiting higher-skilled workers or by reducing the number of jobs. In the first case, 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 Colorado's pay transparency law on either education or experience requirements. As for the number of jobs, we show that the law in Colorado did not impact the number of job postings in the state. Extending to all policy changes in our study period, we similarly find no impact on aggregate employment measured in the QCEW. As a result, the pay transparency laws do not appear to have any negative impacts on employment.

Fifth, we explore the source of the increase in salaries after the passage of pay transparency laws. Our evidence is most consistent with increased transparency heightening competition between firms. This channel is conceptually distinct from prior work on internal pay transparency, which emphasizes how within-firm bargaining may lead to lower wages as firms can credibly reject renegotiations. To bolster support for the increased competition channel, we provide a number of results. First, the wage estimates in our Lightcast analysis are identified from jobs that had salary information both before and after the pay transparency law went into effect. Therefore, these firms are not revealing new information, and presumably have no change in the within-firm bargaining environment. Despite this, we find that posted salaries increased by 3.6%. Second, we link Glassdoor and Lightcast at the firm level to directly study heterogeneity in actual self-reported wages by baseline transparency. We find that the impact on self-reported wages does not depend on baseline transparency. Since always-transparent and newly-transparent firms experience similar wage increases, we interpret this as further evidence that the primary channel in our setting is a market-level shift in competition.

To summarize, we find that the recent wave of pay transparency laws substantially increased salary information 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 spillovers that affected both incumbent workers and firms that always posted wages, neither of which is directly targeted by the policy.

Our paper contributes to three distinct literature. First, it contributes to a growing literature on pay transparency. A recent review by Cullen (2024) documents three types of pay transparency: horizontal, vertical, and cross-firm. Studies of horizontal pay transparency find that revealing pay gaps between coworkers lowers morale (Card et al., 2012; Breza et al., 2018), leading firms to compress salaries and gender pay gaps (Mas, 2017; Bennedsen et al., 2022; Baker et al., 2023; Gulyas et al., 2023; Blundell et al., 2024). However, this pay compression is often driven by a reduction in average earnings, rather than a raise for workers at the bottom (Cullen and Pakzad-Hurson, 2023). On the other hand, vertical pay transparency where employees learn their boss' wage can increase worker productivity by causing individuals to work harder in an effort to be promoted (Cullen and Perez-Truglia, 2022). Relative to this literature, we focus on transparency in job postings, rather than transparency among incumbent workers. Similar to our work, contemporary studies also find a positive wage effect among new hires after Austria (Frimmel et al., 2022) and Slovakia (Skoda, 2022) started requiring firms to post salary information. Compared to these studies of national policies, we evaluate state policies that enable us to use unaffected states as a natural counterfactual comparison group. This variation allows us to identify significant spillovers even among firms already transparent prior to the new law.

Second, our study contributes to the literature on the role of wage information during job search. Belot et al. (2022) and Marinescu and Wolthoff (2020) show that higher wages tend to attract more applicants when posted wages are visible. However, there is growing evidence that workers have limited knowledge of wages at other firms. In a representative survey of U.S. workers, Hall and

Krueger (2012) documents that only 23% of new hires knew exactly how much they would be paid during their first interview. Consistent with this evidence, Jäger et al. (2024) find that workers tend to anchor their beliefs about their outside options at their current wages. As a result, new information can potentially improve workers' directed job search outcomes. For instance, Caldwell and Harmon (2019) finds that wage information through networks of former coworkers leads workers to negotiate higher wages or move to better jobs. We add to this literature by demonstrating that policies that explicitly aim to increase the amount of information during the application process can likewise increase wages for both incumbents and new hires.

Lastly, our paper adds to a broader 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., 2024), inquiring applicants' criminal record history (Agan and Starr, 2017; Cullen et al., 2022b), asking workers to post a preferred salary (Roussille, 2024), 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.

The remainder of the paper is organized as follows. Section 2 describes the recent laws requiring firms to include a salary range in job postings. Section 3 discusses various mechanisms through which pay transparency laws may impact labor-market outcomes. Section 4 discusses the data. Section 5 estimates the impact of the pay transparency law on the availability of salary information on postings. Sections 6, 7, and 8 estimate the impact of the law on average wages, pay dispersion, and labor demand, respectively. Section 9 discusses mechanisms and Section 10 concludes.

2 State and Local Pay Transparency Laws

2.1 The Equal Pay for Equal Work Act in Colorado

The focus of our analysis is on the Equal Pay for Equal Work Act (EPEWA), which was enacted on January 1, 2021 in Colorado. Through the EPEWA, employers are required to (1) include compensation in job postings, (2) notify employees of promotion opportunities, and (3) keep accurate records of job description and wage rate. Unlike pay transparency laws that were later introduced in other states, the Colorado act applied to all firms regardless of their size. Our focus will be on the first part of this act: including compensation in job postings.

In terms of the 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).[1] 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

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

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."

Given the novelty of the new rule, enforcement of the act has been primarily through education rather than fines. 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 would then investigate the claim and send a Compliance Assistance Letter to the firm, requesting that they remedy the violations. Legally, the Department can fine employers $500 to $10,000 per posting in violation of the law, but these are rare in practice. As of October 2024, the Department of Labor has received 1,747 complaints and collected a total of $238,000 in fines (Adamson, 2024).

After the act went into effect, 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. 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.

Besides the Equal Pay for Equal Work Act, Colorado also 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 that any wage effects 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 Colorado to only states that experienced a similar change in the minimum wage.

2.2 Other Recent Pay Transparency Laws

Following Colorado, ten states and five municipalities passed similar pay transparency laws as of March 2025. Appendix Table A.1 lists the dates that each of these laws went into effect. After Colorado, the next major law that received significant media attention was New York City's rule in November 2022, and then Washington's and California's laws in January 2023. While the timing of these laws are outside the years of our job postings data, we incorporate them in our analysis of quarterly Census records. This allows us to test the external validity of our results and to cleanly isolate the impact of the pay transparency rules outside of any potential Covid-related confounders.

3 Conceptual Framework

In this section, we discuss four mechanisms by which pay transparency in job postings may impact the labor market. First, when employers post a salary range in their postings, incumbent workers can learn about the wages of prospective new hires. A large literature on internal pay transparency has shown theoretically and empirically that releasing information about coworkers' pay tends to decrease wages (Cullen and Pakzad-Hurson, 2023; Cullen, 2024). The intuition is similar to that of a classic monopsony model - internal pay transparency makes it more costly for employers to increase the wages of any particular individual because other employees will demand a similar raise.

Second, pay transparency in job postings allows workers to learn not only about the wages at their own firm but also about the wages at other firms. Recent work by Jäger et al. (2024) shows that workers tend to underestimate their outside options, and this bias leads to lower market wages overall. Intuitively, if workers believe that their outside option is no better than their current salary, then their employers can just offer workers the reservation wage needed to keep them from searching for a new job. Thus, in contrast to the effects of within-firm pay transparency, across-firm transparency could raise wages. Moreover, this positive wage effect does not have to be constrained to the firms that were initially opaque as workers in already-transparent firms are also treated by the market-wide information shock.

Third, workers are not the only agents that gain new information. Pay transparency in job postings also enables employers to observe the wages offered at competing firms. To understand the effect of giving employers such information, Cullen et al. (2022a) show that providing employers with a salary benchmarking software reduces pay dispersion but has no effect on average wages. Similar to that setting, we may also expect pay transparency in job postings to compress the wage distribution. Alternatively, evidence from the product market suggests that firms may be able to collude based on the information, which in our setting would lead to lower wages for workers (Albæk et al., 1997).

Fourth, besides providing additional information to firms and workers, the requirement that employers post a wage may also change the cost of job search. To endogenize the decision to post or bargain over wages, Michelacci and Suarez (2006) develop a job search model that characterizes the trade-offs facing firms. On the one hand, posting wages enables firms to optimize between the cost of paying workers more and the benefit of filling vacancies quicker. In effect, posting wages is efficient in the sense that it satisfies the Hosios (1990) condition. On the other hand, if firms do not observe workers' productivity type, they can attract higher skilled workers by offering to bargain, but this results in a socially inefficient number of vacancies. Michelacci and Suarez (2006) show that because of this search inefficiency, a wage posting equilibrium Pareto improves workers' income relative to any equilibrium with wage bargaining. Thus, the pay transparency law can also raise wages by reducing search inefficiencies.

A complete model capturing all four of these mechanisms is beyond the scope of our study. However, we present a simple framework in Appendix B to illustrate the competing forces between the first two mechanisms. The main implication of the model is that the wage effect of pay transparency in job postings is theoretically ambiguous, just as we have discussed above. We next turn to our empirical evaluation of the pay transparency laws.

4 Data

4.1 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 a vacancy displays wage information. 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.

The time period of our Lightcast data extends from January 2013 to December 2021. We restrict the sample to job postings between 2020-2021 to 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) code, similar to prior work using the Lightcast data (e.g. 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.2 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). In Panel (b) of Table 1, we find that the distribution of occupations is quite similar between Colorado and 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 since the latter may use physical "help wanted" posters.

4.2 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 between January 2020 and December 2022. 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 that workers with only 1 year of experience are 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 Glassdoors 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."

4.3 Quarterly Census of Employment and Wages Indicators (QCEW)

Our final data source is the Quarterly Census of Employment and Wages (QCEW). The Census Bureau constructs the QCEW from administrative data that establishments report to State unemployment insurance (UI) programs. A strength of the QCEW data is that it is representative, as the UI programs cover about 97% of the workforce in the country. In addition to its representativeness, a key advantage of the QCEW is that we have records up to the second quarter of 2024. This longer panel allows us to study the labor market impacts of more recent pay transparency laws that were passed after Colorado's. However, the main limitation of the QCEW is that we only observe outcomes at the industry-by-county level, so we are unable to compare workers in the same occupation or firm.

Wages in the QCEW capture various 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.

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.

4.4 Relationship between Datasets

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. It also enables us to distinguish between new hires and incumbents by using respondents' reported years of work experience. On the other hand, the QCEW has more recent data that allows us to extend our analysis beyond Colorado and explore the impact of pay transparency laws that went into effect in other states after the Covid pandemic. Moreover, the QCEW 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.

5 Effect on Pay Transparency in Online Postings

5.1 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 becomes effective.[2] 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.[3] 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.

[2] A May 2024 notice by the Colorado Department of Labor and Employment finds that by late 2023, compliance with the law was "over 80%", but still below 100% (CDLE, 2024).

[3] Outside the U.S., Audoly et al. (2024) find that about 55% of job ads in Norway provide pay information.

5.2 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:

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

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 specification, this job fixed effect is a firm-SOC-FIPS interaction, where SOC (Standard Occupational Classification) is a 6-digit occupation code and FIPS is a county code. $\tau_{c(i),t}$ are month fixed effects that vary by some characteristics of the job $c(i)$. Our preferred specification controls for SOC-month fixed effects so that the coefficients of interest are only identified by within-occupation variation, comparing the same occupation across different locations. Without these fixed effects, if Colorado posts 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.

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 (1). 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 of 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.

5.3 Heterogeneous Compliance 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. Exploring which firms and jobs are not complying is important from a policy enforcement perspective.

We begin by examining 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, after the pay transparency law had already been passed. Rather than a subset of large firms becoming fully compliant, the evidence suggests that firms became moderately more transparent by selectively choosing the postings that include salary information.

To determine which types 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 became far more transparent relative to firms in the rest of the country. Taken together, Appendix 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.[4]

A potential explanation for the variation in compliance within-firm is that employers highly

[4]The claim that large firms experience a stronger first-stage effect is further supported by Appendix Figure A.3 where we plot the coefficients of equation 1 separately for small and large firms, controlling for firm and month fixed effects.

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 1 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.4 plots the estimates by occupation group, averaged over all months in 2021. We find sizable 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.

What explains the variation in compliance across occupations? In Figure A.5, 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, 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.[5] 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, consistent with the model of Michelacci and Suarez (2006). This suggests that at least part of the relationship between workers' salaries and the propensity to bargain over wages, as observed in the literature (Hall and 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.

6 Effect of Pay Transparency on Average Wages

In this section, we estimate the impact of pay transparency on workers' wages by estimating a dynamic difference-in-difference design of the following form:

$$Y_{it} = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot Treat_j + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \tag{2}$$

where Y_{it} is an outcome for unit i at time t. The variable $Treat_j$ equals one in jurisdictions that implement a pay transparency law. Given the coverage of the Lightcast and Glassdoor data, $Treat_j$ is equivalent to an indicator for Colorado when analyzing those datasets. However, we extend $Treat_j$ to include all policy changes when examining the QCEW, which has a broader coverage. In general, the outcome, unit of observation, and controls will differ depending on the dataset under consideration, which we explain in each analysis below.

[5] While there is some evidence that it is fairly costless for firms to post a vacancy given the prevalence of "ghost jobs" (Ng, 2024), to our knowledge, there are no estimates on the cost of including a wage in job postings.

6.1 Impact on Posted Salaries

We begin by estimating the impact of Colorado's pay transparency law on posted salaries using the Lightcast data. For postings with a range, we use the midpoint of the range as the outcome, and we will consider the minimum and maximum separately in Section 7. Our preferred specification controls for firm-SOC-FIPS and SOC-month fixed effects. The first set of fixed effects implies that our regression is identified using firms that post advertisements for the same occupation in the same location both before and after the policy. A firm may repost vacancies for the same job for multiple reasons including failure to fulfill the posting the first time, high turnover in the occupation, or simply to expand the number of employees in that position. We think it is particularly important to focus on variation within the same firm-SOC-FIPS over time, 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. We also include SOC-month fixed effects so the estimation is identified by within-occupation variation.

The key identification assumption of our analysis 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. In particular, the minimum wage in Colorado increased from \$12 to \$12.32 in 2021. We expect this policy to have minimal impact on our estimates since our control group includes 26 other states that also increased their minimum wages starting in January. However, to ensure that 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.

Besides the minimum wage, 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 2, the policy also requires firms to notify employees of promotion opportunities and maintain accurate wage records. It is unclear 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 that 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. Although we do not think these types 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.

Figure 3 plots our difference-in-difference over time. We find that salaries were trending similarly between Colorado and the rest of the U.S. prior to the policy change. 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. We observe increases in posted wages even for always-posting firms, suggesting the increase in transparency has led to competitive pressure that increases 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 (2) 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.[6] As a secondary test, column (3) restricts 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.

To evaluate the effect of the pay transparency law on the composition of job postings, column (4) reports the difference-in-differences estimate if we only control for firm and month fixed effects. In this case, we find that average posted salaries increased by 7.3% in Colorado compared to other states, twice as large as the estimate from our main regression. The observation that 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 to 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.

One concern with our empirical strategy is that firm-SOC-FIPS fixed effects are insufficient to control for changes in the composition of postings with observable wages. Appendix Table A.3 assesses the robustness of our results to alternative strategies to ensure that our regression is not admitting new jobs that were not previously posting salaries. First, to define a "job" more granularly, column (1) defines an occupation not by its 6-digit SOC code, but by the exact text of the job title used in the description of the vacancy. This restriction eliminates over half the sample relative to our main specification since multiple job titles may correspond with the same SOC code. Second, column (2) further restricts the sample by controlling for firm-title-time fixed effects.

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

This stricter specification compares jobs within the same multi-state firms over time. Third, as the strictest robustness check to account for all composition effects, columns (3) and (4) split the sample by whether the job was transparent across all postings in 2020 or only some. Intuitively, if a firm was fully transparent in 2020, then there would be no room to introduce newly transparent jobs in 2021 so it cannot be driven by composition changes. Across all specifications, we repeatedly find a positive effect on posted wages, suggesting that these impacts are not driven simply by changes in composition.

Appendix Table A.4 shows that our results are also robust to accounting for potential Covid-related labor market shocks. In columns (1) and (2), we control for county-level Covid case rates and death rates over time. Alternatively, columns (3) and (4) divides counties into ten groups depending on the severity of their Covid case and death rates in 2020, and controls for different time trends within each group. We find that these additional controls have very little impact on our estimates. In the next section, we fully account for any Covid effects by showing that our results in the QCEW data are similar when we assess policies that were passed after 2022.

6.2 Impact on Realized Salaries

Next, we estimate the impact of the pay transparency rule on realized wages using a similar difference-in-difference design to Equation 2. However, the exact specification depends on the source of the data. First, while the Lightcast data has over 14 million job postings from 2020-2021, the Glassdoor data contains less than 5 million reported salaries over the same time period. As a result, it is much less common to observe the same job (i.e. same occupation, firm, and location) in Glassdoor for 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 that 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.

Second, the analysis utilizing the QCEW is at the industry-county level, rather than at the worker-firm level. In particular, we measure average wages and employment for each 4-digit NAICS-by-county. Given that the data does not disaggregate by occupation, we control for market fixed effects, defined as a 4-digit NAICS-by-county cell, and time fixed effects. In addition to estimating the impact of the 2021 Colorado policy, the QCEW has the advantage of allowing us to extend our analysis to all pay transparency policies up to the second quarter of 2024. Leveraging the full set of policy variation, we estimate Equation 2 using the difference-in-difference method developed by Callaway and SantAnna (2021) to account for heterogeneous treatment effects over time. In this case, the control group for each policy change comprises of all counties that have yet to introduce a pay transparency law in job postings. Standard errors are clustered at the county level.

6.2.1 Heterogeneity by New Hires and Incumbents

Figure 4 plots our difference-in-difference estimates for two samples of the Glassdoor data. 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, we find a gradual increase in reported salaries that stabilizes after a year. Since the sample comprises mostly of stayers rather than job-switchers, the gradual increase in earnings may reflect the time it takes for workers to discover their outside options and then 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 4, 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 stable 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 6. 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' realized earnings as a result of increased pay transparency in job postings, and these effects appear to be stronger for new hires than incumbents.

6.2.2 Effect of Pay Transparency Laws Post-Covid

Turning to the QCEW, we estimate our event-study using two samples of the data. First, Panel (a) of Figure 5 replicates our wage effects for the 2021 Colorado policy. Similar to the Glassdoor results, we see a gradual increase in log average weekly wages salaries that stabilizes after a year. Second, Panel (b) extends our analysis to all state and municipal pay transparency laws that were passed from January 2021 to July 2024. While we observe some seasonal cyclicality in the years before the policy changes, these cyclical effects tend to center around 0. On the other hand, wages increase in the quarter of the policy change and remain elevated for the following two years.

Table 4 assesses the robustness of these results. Column (1) aggregates the treatment effects in Colorado and finds that there is a 1.4% increase in wages following the passage of the pay transparency law. Column (2) shows that the estimate is nearly identical when we leverage all 10 policy changes during our study period. Column (3) weighs each observation by the average employment level in the corresponding 4-digit NAICS-by-county cell to compute the effect on wages for the average worker. Again, we find a positive impact on average wages, albeit it is less precisely estimated. Lastly, column (4) restricts the sample to only policy changes that happened after 2021 (i.e. we drop Colorado). Since the first major policy in this last sample occurred in November 2022, the estimate is not confounded by the major labor market upheavals during the early stage of the Covid-19 pandemic. In all of these alternative specifications we continue to find qualitatively similar results that pay transparency in job postings increases workers' earnings between 0.9-1.4%.

7 Effect of Pay Transparency on Pay Dispersion

In addition to raising average wages, pay transparency in job postings can also impact the dispersion in wages. First, it can affect the posted salary range. While not legally permitted, employers may be responding to the reform by simply posting such a wide a range of salaries that they are effectively offering no real information. Second, it can affect the variance of wages within firms. If posted wages reveal internal pay disparities, then pay transparency can lead firms to reduce such pay gaps to maintain morale. Third, pay transparency in job postings can also affect pay dispersion across firms as employers compete for workers. This section evaluates the change in pay dispersion along each of these margins following the introduction of Colorado's pay transparency law.

7.1 Dispersion in Posted Salaries

Using the Lightcast data, we decompose the average income effect into the impact on the maximum and minimum posted salaries. Table 5 reports the treatment effect for four outcome variables: log maximum posted salary, log minimum posted salary, log of the ratio of the maximum and minimum posted salaries, and a dummy variable for whether the posting has a range or exact value. To focus on the same job over time, columns (1)-(3) control for firm-SOC-FIPS fixed effects. These regressions find that employers raised both the maximum and minimum posted salaries by approximately the same amount as a result of the Colorado pay transparency law. Thus, the positive effect on posted wages is not driven by an increase in the range of posted wages.

Although the policy had no impact on the salary range for always-transparent jobs, firms may post wide ranges for newly transparent jobs. After all, these are the jobs that employers had wanted to hide salary information before the policy change. To study this channel, column (4) of Table 5 estimates a specification that controls for only state and month fixed effects. In this case, if newly transparent postings 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 (5), where we show that newly transparent jobs are 3% more likely to post a range than an exact value. Overall, the policy had little impact on the informativeness of job postings.

7.2 Dispersion in Realized Salaries

Leveraging the Glassdoor data, we next estimate the impact of Colorado's pay transparency law on pay dispersion within-firm and within-occupation. To measure the variation of wages within-employer, we collapse the data and compute the standard deviation in wages at the employer-state level for 3-month intervals. To ensure that our measure of the standard deviation is not simply zero for employers with one Glassdoor review, we restrict the sample to firm-states with at least 100 observations in 2020. We then estimate Equation 2 controlling for employer-state and time fixed effects, so that we are following the same employer-state over time. We repeat a similar process by collapsing the data to the occupation-state level, restricting the sample to only occupation-states with at least 100 observations in 2020, and then estimate Equation 2 with occupation-state fixed effects.

Figure 6 plots our difference-in-difference estimates over time. In Panel (a), we find that the log standard deviation of wages within-firm was trending similarly between Colorado and other states prior to the 2021 reform. This trend then continues after the policy, with no obvious change in pay dispersion. Similarly, Panel (b) finds a flat pre-trend in the variation of wages within-occupation. There appears to be a small increase in variance after the policy change but it is not statistically significant.

To obtain more statistical power and to assess the robustness of our results, Table 6 reports an average post-treatment effect for two measures of pay dispersion. The first row reports the average impact on the log standard deviation of wages, analogous to the graphs in Figure 6. Columns (1) and (2) show that there is no detectable effect on wage dispersion within-employer or within-occupation. In column (3), we repeat our analysis at the city-occupation level because for many jobs, competition for workers occur at a more local geographic level than the state. Nevertheless, we continue to find no impact on the deviation in reported wages. The second row replicates our analysis using the 90/10 percentile ratio as the outcome variable. For this analysis, we compute the 90th and 10th percentile of wages within each unit of analysis, and estimate the impact on the ratio of these values. This alternative measure allows us to focus on the tails of the wage distribution. Similar to our analysis on the standard deviation of wages, we find no effect of pay transparency on the 90/10 percentile ratio. Overall, the policy does not appear to have much of an impact on income inequality within an occupation.[7]

An important caveat to keep in mind is that the clearest prediction on pay compression is between coworkers at the same job. As such, we would ideally like to estimate the impact on the dispersion of wages within employer-occupation. However, given the small sample size, we do not have enough observations to compute the standard deviation in wages for every quarter within granular employer-occupation cells. Thus, we may not be finding any effect on within-firm pay dispersion because we are comparing very different occupations at the same employer. An alternative approach would be to estimate wage dispersion using the larger sample in the Lightcast data, but that would be driven largely by composition changes. Since the policy is making more high-paying jobs reveal their expected compensation, the policy will naturally lead to more variance in posted wages. Consistent with that hypothesis, Appendix Table A.6 replicates our analysis using the Lightcast data and finds an increase in the standard deviation of wages within-employer and within-occupation.

8 Effect of Pay Transparency on Labor Demand

Given the increase in average wages, there is concern that employers may reduce their demand for workers. In this section, we explore the impact of pay transparency in job postings on three dimensions of labor demand: the quality of workers, the number of postings, and the number of employees.

8.1 Effect on Skill Requirements

First, we find no evidence that employers impose stricter job qualifications on new hires to offset the increase in wages. To test for such responses, Figure 7 estimates the impact of Colorado's pay transparency law on whether a job posting in the Lightcast data 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). The regressions control for firm-SOC-FIPS fixed effects to follow the same jobs over time. We find no impact of the pay transparency law on any skill requirement. Appendix Table A.7 summarizes the results presented in these figures and confirms that our result is also robust to allowing for changes in the composition of jobs.

8.2 Effect on Number of Postings

Second, the increase in wages also did not cause employers to decrease the number of job postings. To estimate the impact of the Equal Pay for Equal Work Act on number of postings, we estimate Equation (2) with the outcome as 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.

[7] As we will discuss, the impact does vary across occupations, with higher-wage occupations experiencing larger wage increases. Therefore, the policy does have an impact on aggregate inequality.

If there are no postings in a month for a given firm-SOC-FIPS cell, then we define the number of postings as zero. In other words, unlike the analysis on posted wages, the panel for our analysis on the number of postings is balanced by construction. The rest of the variables are defined in the same manner as Equation (1), and standard errors are again clustered at the employer level.

Panel (a) of Figure 8 plots our difference-in-difference estimates over time. 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 7 estimates a similar regression to Equation (2), 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 our 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 7. As we will show in Section 9, occupations that experienced a larger increase in the share of postings with salary info also experienced a larger increase in posted wages. Using this variation, we might expect to find a greater decrease in vacancies among jobs with above average transparency effects. To test for heterogeneity in labor demand responses 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. Similarly, we find no negative effects in columns (4) and (5) when we separate the sample by whether a firm has above or below median transparency at baseline. Overall, we can rule out even very small effects on the number of job postings.

8.3 Effect on Employment Levels

Third, we find that employers also did not decrease the number of jobs. Although there is no decrease in postings, employers can reduce employment either by increasing layoffs or by hiring fewer applicants per posting. For example, after Colorado passed its pay transparency law, there were reports that some employers sought to exclude Colorado workers from applying to jobs (Rubino, 2021). To directly measure the impact on employment, we return to the QCEW data and estimate Equation (2) with the outcome as log(*employment*) within a 4-digit NAICS-by-county cell. Panel (b) of Figure 8 plots the impact of pay transparency laws on log employment, averaged over all policy changes that have passed from January 2021 to July 2024. There is no clear change in employment after a pay transparency law goes into effect.

Table 8 tests the robustness of our results to alternative samples and specifications. To make our results comparable to our previous estimates on the change in job postings, column (1) reports the impact of only Colorado's pay transparency law on log-employment. We actually find a small positive impact on employment. However, one limitation of using log-employment as the outcome is that some industry-counties might fluctuate between positive and zero employment, leading to changes in sample composition. To keep all industry-counties in the data, even if they never have any employment, column (2) estimates the effect on employment-levels in Colorado. We find an average increase in employment of 2 workers on a baseline of 233 workers in 2020, implying an employment effect of 0.9%. Columns (3) and (4) repeat our analysis using all the policy changes in our time period and similarly find no negative impacts on employment. Even for our most negative estimate in column (3), we can rule out decreases in employment greater than 1%. Overall, laws that mandate pay transparency in job postings appear to have no negative impact on employers' labor demand.

9 Mechanisms

In contrast to the existing literature showing that pay transparency within-firms decreases average wages (Cullen, 2024), we find that pay transparency in job postings has the opposite effect. Theoretically, the key difference between within-firm and across-firm transparency is the potential for across-firm transparency to heighten competition as workers increasingly apply to high-wage jobs. However, in principle, multiple channels could be at play. For example, it could be that within an individual firm, incumbent workers use the newly revealed wages in postings as benchmarks in order to bargain for their own wages. To clarify the distinction between these two channels, imagine the experiment in which a single firm is required to post wages. In this experiment, there are no competitive changes due to additional information in the labor market. There may still be a direct impact on the wage at this firm due to information being revealed to incumbent workers.

We have established a few results that suggest the change in competition is the primary driver of our impacts. First, we found positive impacts on posted wages for always-transparent firms. These firms do not change posting behavior so within-firm transparency remains consistent over time. Therefore, they are exposed to the general change in market competition, but not the direct impact of now posting wages. The size of this impact on always-posting firms is similar to our overall market-level impacts estimated in Glassdoor and the QCEW. This suggests that the market-level shift in competition is sufficient to explain our results. However, given Glassdoor and Lightcast can be linked by firm name, we can also study this more directly by exploring whether the impact on self-reported wages depends on whether the firm was initially transparent or not. In Appendix C, we detail how the two datasets are merged.

In Appendix Table A.8, we estimate the impact of the pay transparency law on realized wages in Glassdoor, but allowing for an interaction with baseline transparency, which is a continuous measure that takes on values from 0 (no transparency) to 1 (full transparency). Our key focus in this table is on this interaction term. Overall, we find limited evidence of heterogeneous impacts by baseline transparency. In column (1), which controls for state-occupation fixed effects and month fixed effects, we find a positive interaction, suggesting firms that are more transparent initially experience larger wage impacts. This could be consistent with arguments found in the within-firm transparency literature. Newly-transparent firms weigh two considerations: there is a market-level shock that increases competition, which leads to higher wages, but they internalize that the presence of information at the hiring stage will increase the wages for incumbent workers. Therefore, the wages of newly-transparent firms rise less than wages of already-transparent firms. However, this interaction is not statistically significant, so overall, there is limited evidence that this channel is at play.

In Column (2), which controls for month-occupation fixed effects and state-occupation fixed effects, the interaction turns negative. Again, however, the estimate is not statistically significant. The final column which controls for state-occupation-firm fixed effects and month-occupation-firm fixed effects finds similar results. Overall, we interpret this as finding limited evidence of heterogeneity by baseline transparency, and therefore provides further support for a market-level shift in competition.

As additional evidence of the importance of market-level changes in competition, 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 where we observe the largest market-level shifts in competition.

To understand the heterogeneity in wage effects across occupations, Appendix Figure A.7 plots estimates of the impact on posted wage separately by 2-digit SOC codes using the Lightcast data. 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 9 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.4. The income estimates are from our Ligthcast data, implying they are estimated on always-transparent firms. These firms are the ones that are only exposed to the market-level shifts in competition and not the direct impact of now posting a wage. 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.

We present two robustness checks to assess the relationship between market-level changes in transparency and wage increases among always-transparent firms. First, Appendix Figure A.8 finds that the positive relationship still holds after dropping computer and mathematical occupations, which experienced an outlier increase in posted wages. Second, given that both the increase in wages and the increase in transparency are endogenous variables, the relationship between these two responses may reflect reverse causation or omitted variable bias. For instance, the most profitable firms could be the ones that are most likely to give raises and to find it easier to comply with the policy. To mitigate this selection bias, Appendix Figure A.9 plots the change in wages against the share of postings with salary information in 2020 for each two-digit occupation. Since baseline rates of pay transparency are not endogenous to the policy change, this alternative approach eliminates concerns that our results are driven by selection bias. Consistent with the view that pay transparency in job postings increased competition, we find that posted wages increased more among occupations that were more exposed to the policy in Colorado.

10 Conclusion

This paper studies the labor market effects of recent laws in the U.S. that require employers to include compensation information in all job postings. Using the near universe of online job postings data from Lightcast, we show that the 2021 policy in Colorado 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 likely to post salaries at baseline compared to small firms. Comparing the change in salaries of jobs 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. Overall, we find increases in posted wages and actual wages, with little changes in demand for labor or skill requirements of jobs. Our evidence suggests that pay transparency in postings is remarkably effective in increasing market-level wages even with limited enforcement.

Pay transparency in postings has become a popular policy in recent years. While we explored the impact in Colorado most completely, we find similar results in a group of more recent policy changes. Future work should consider additional outcomes of the policy. For example, a key focus of the policy is reducing pay gaps by gender and race. Data with more granular information on workers would be useful to understanding the impacts of these policies on these gaps and inequality more generally. Theoretically, there remains uncertainty as to why some firms prefer to post wages vs. bargain for wages. Pay transparency laws offer an exciting lens through which to understand wage determination more generally.

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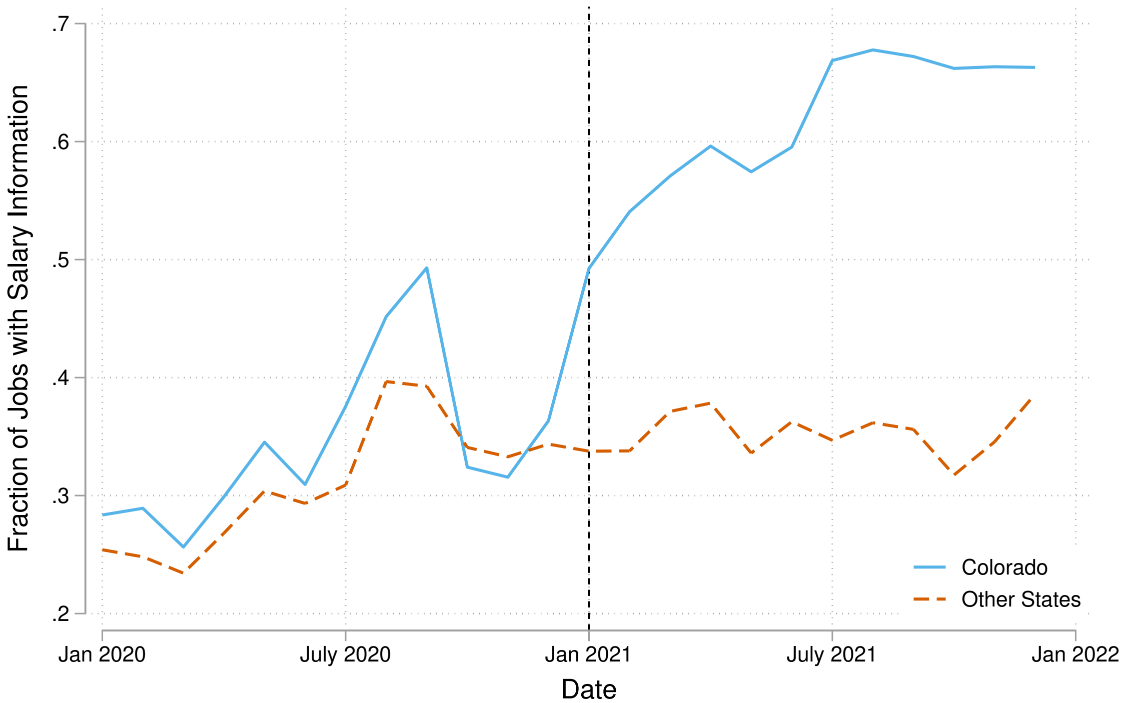
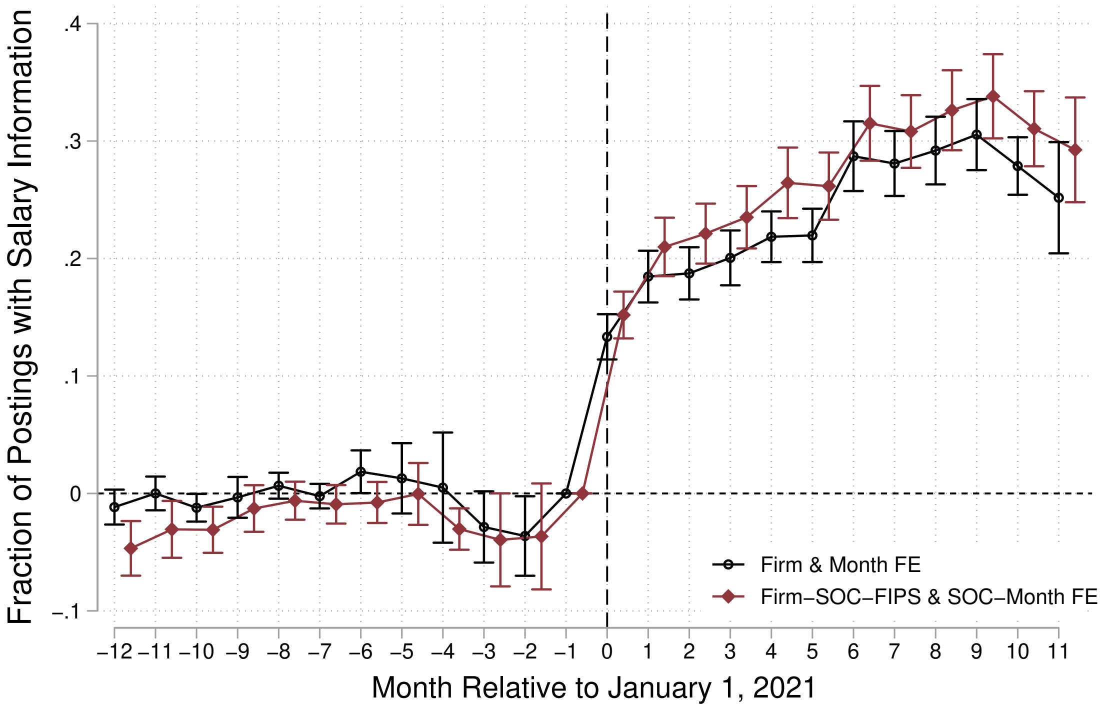
Figure 1: Fraction of Postings with Salary Information by State



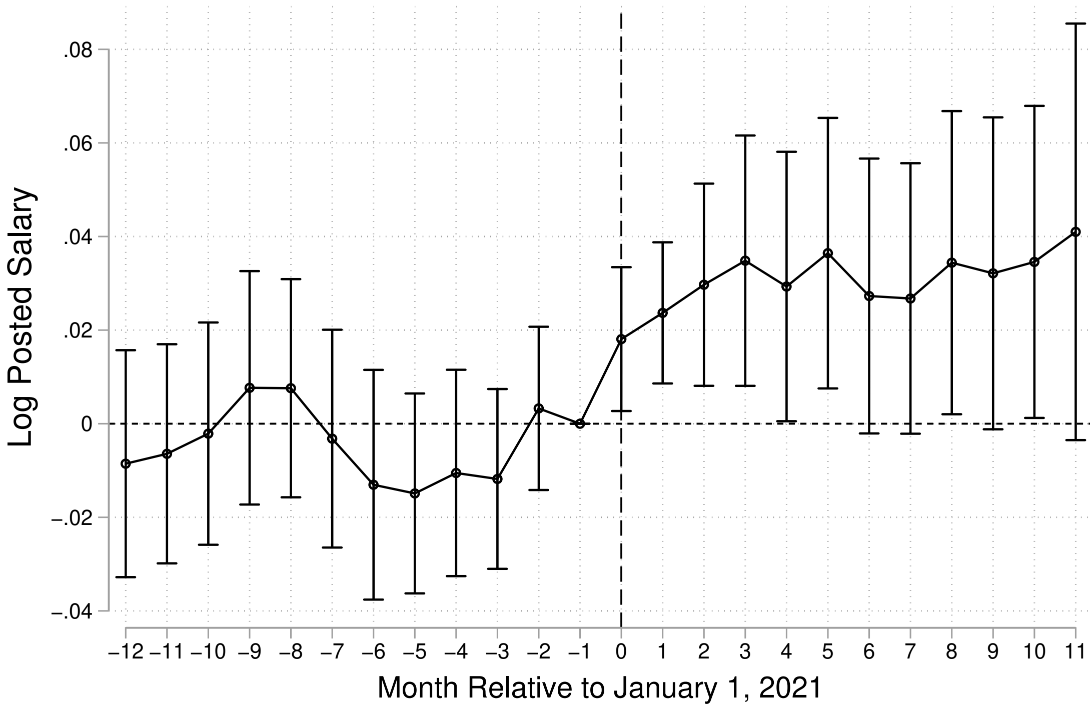
Note: This figure reports the fractions of job postings that contain salary information separately for Colorado and all other states.

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



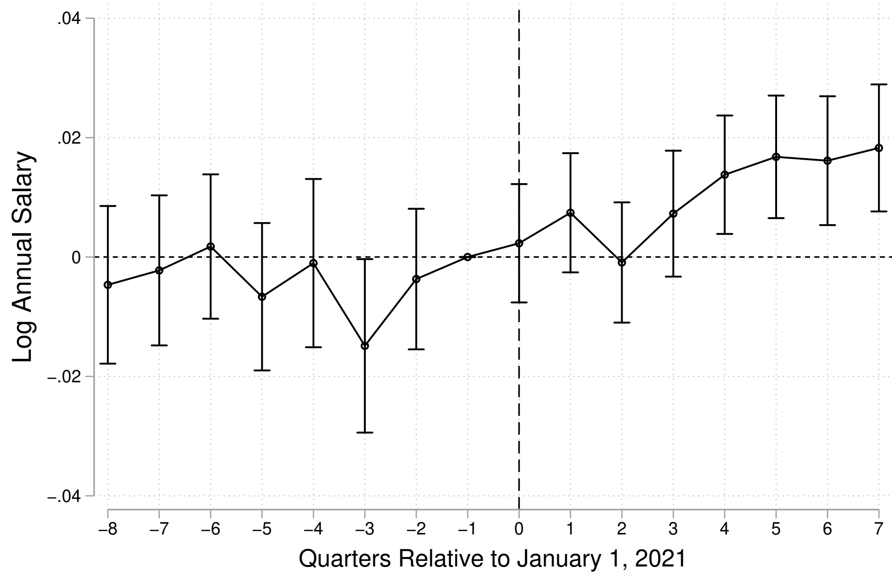
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information. The hollow circles include specifications that control for firm fixed effects and month fixed effects. The solid diamonds control for firm-SOC-FIPS and SOC-month 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

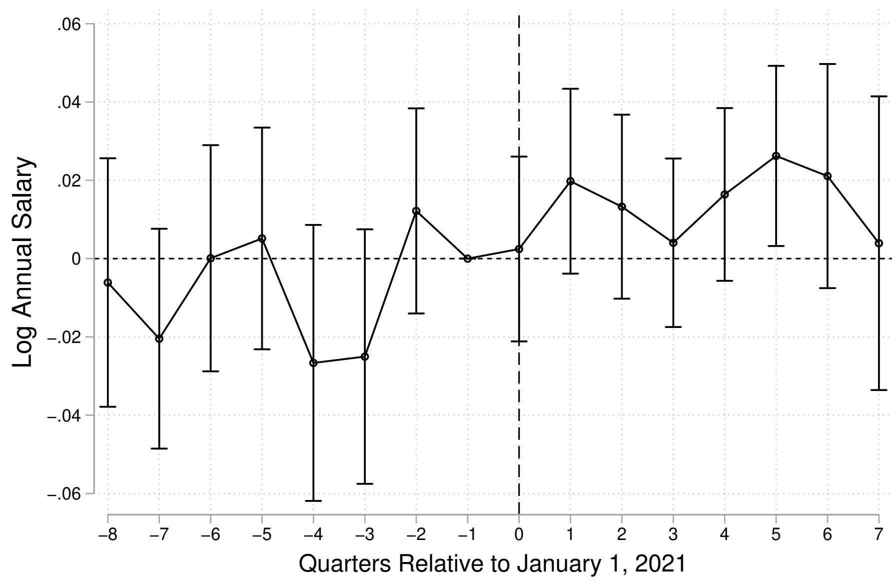


Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary following the specification in Equation (2). This specification controls for firm-SOC-FIPS fixed effects and SOC-month fixed effects. If a posting has a lower and upper bound for a salary, 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 Realized Salaries, Glassdoor



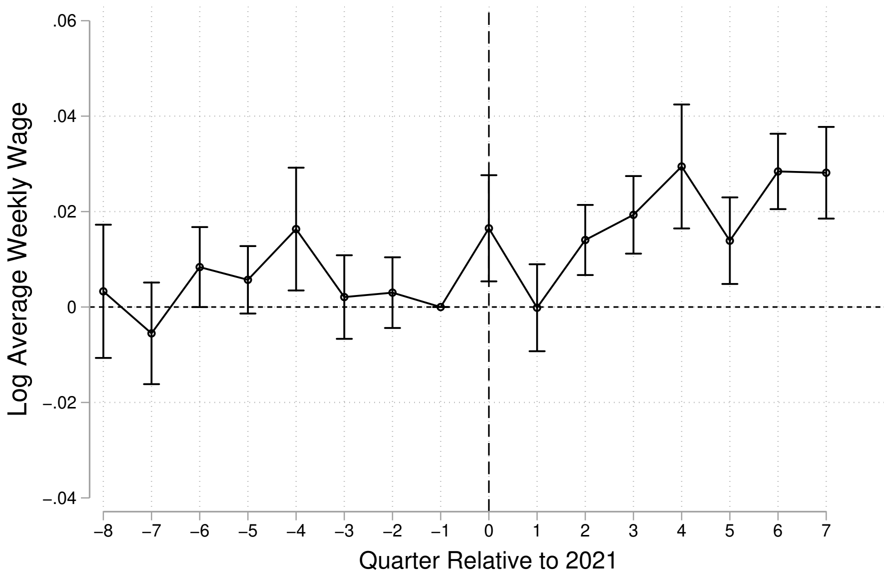
(a) Glassdoor: Full Sample



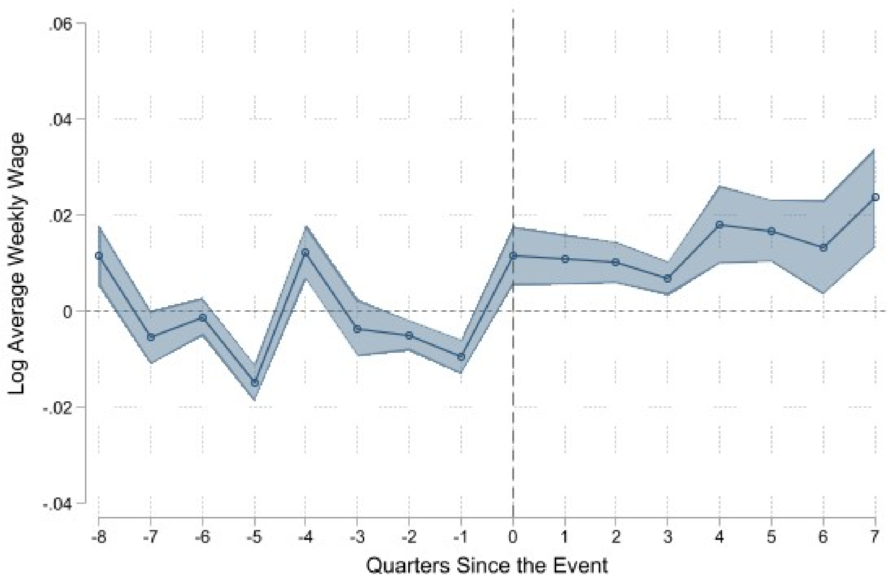
(b) Glassdoor: New Hires

Note: This figure plots the impact of the Colorado pay transparency law on the logarithm of reported salaries following the specification in Equation (2). 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. 95 percent confidence intervals clustered at the firm level are displayed.

Figure 5: Impact of Pay Transparency Law on Average Weekly Wages, QCEW



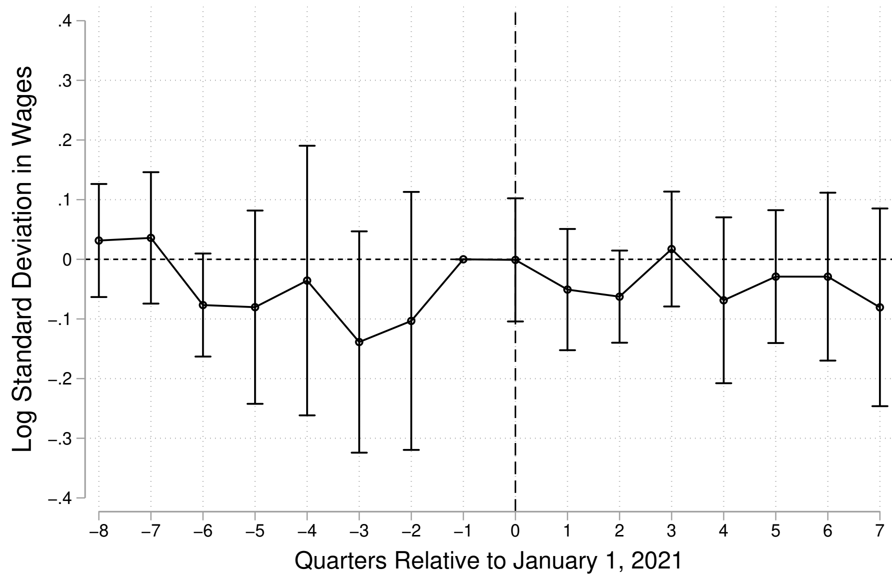
(a) Colorado



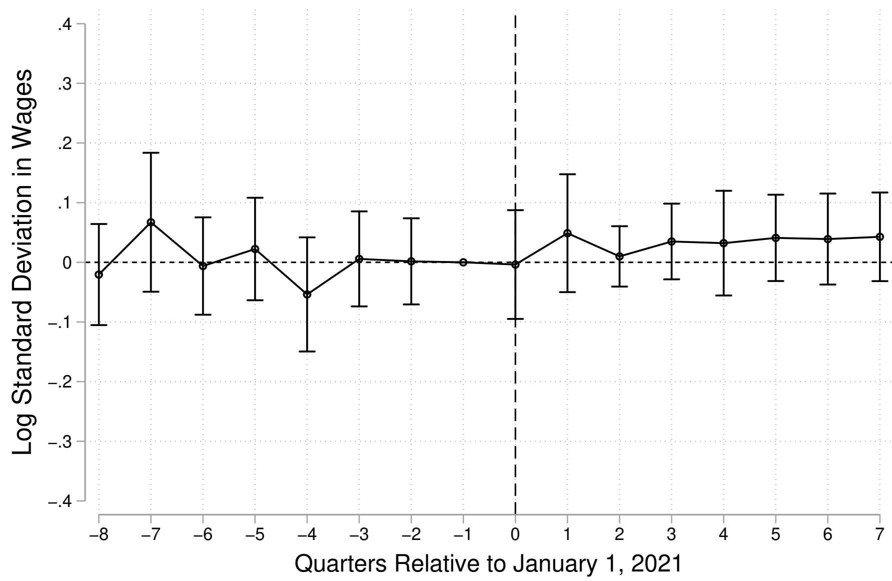
(b) All Events

Note: The figure plots the wage effects of pay transparency laws using the Quarterly Census of Employment and Wages (QCEW). Panel (a) plots difference-in-differences estimates of the impact from Colorado's 2021 pay transparency law on log average weekly wage measured at the 4-digit NAICS-by-county level. Panel (b) plots the average effect of all 10 state and municipal pay transparency laws that went into effect between January 2021 and July 2024 (see table A.1), estimated using the methodology developed in difference-in-difference method developed by Callaway and SantAnna (2021). 95 percent confidence intervals clustered at the county level are displayed.

Figure 6: Impact of Pay Transparency Law on Pay Dispersion

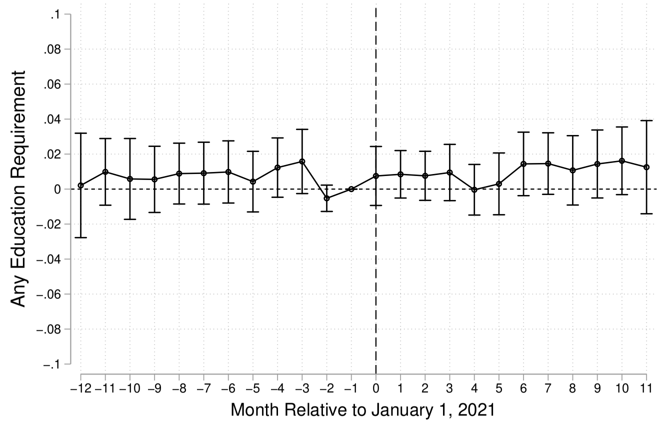


(a) Within Firm

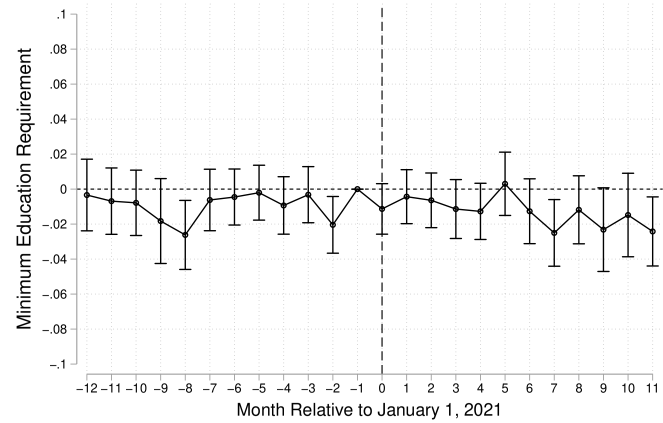


(b) Within Occupation

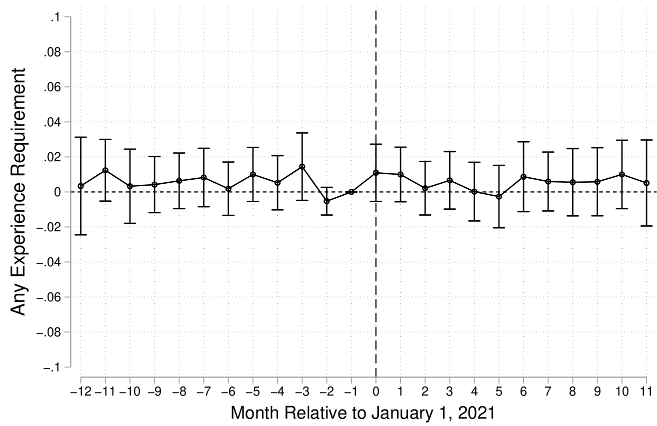
Note: The figure plots effect of Colorado's 2021 pay transparency law on the log standard deviation of wages within firm (Panel (a)) and within occupation (Panel (b)) using the Glassdoor data.



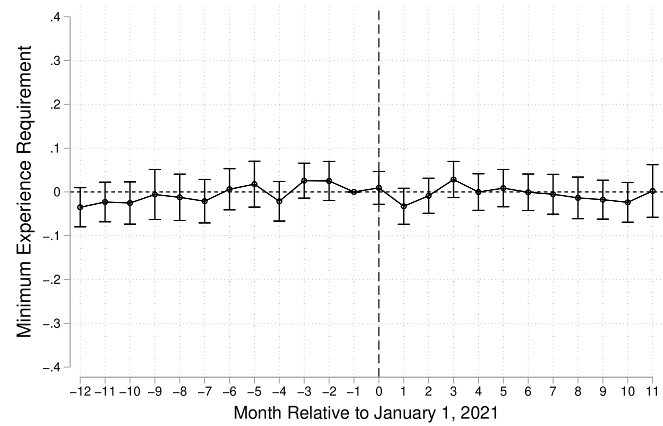
(a) Any Edu Requirement



(b) Min Edu Requirement



(c) Any Exp Requirement

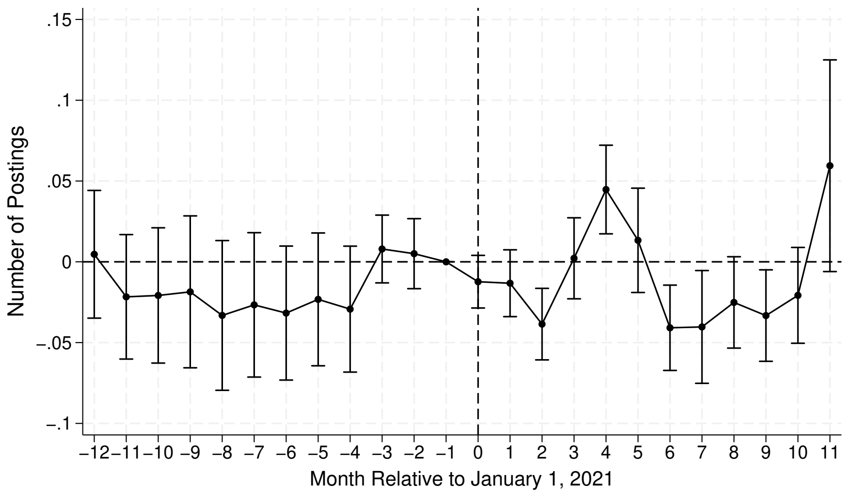


(d) Min Exp Requirement

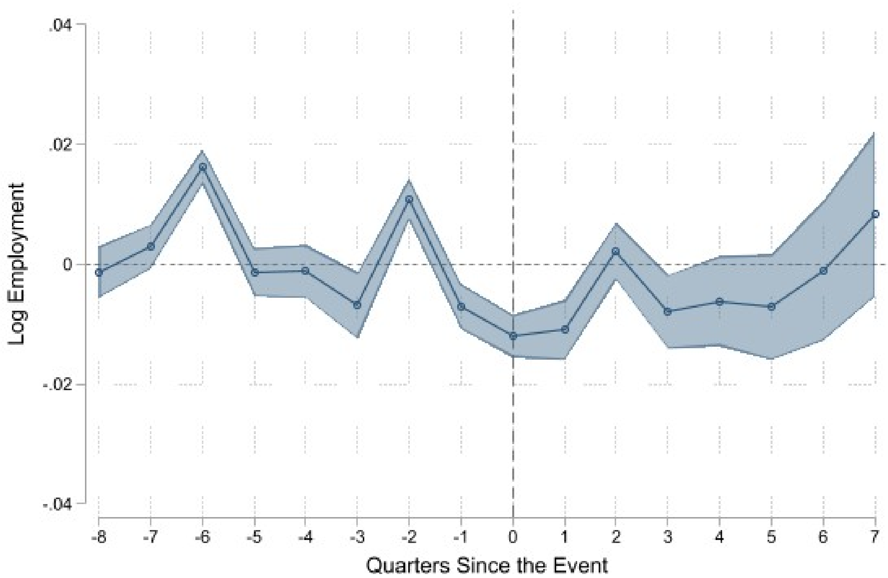
Figure 7: Effect of Pay Transparency Law on Education and Experience Requirements

Note: The figure plots estimates of Equation (2) 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. 95 percent confidence intervals clustered at the firm level are displayed.

Figure 8: Impact of Pay Transparency Law on Labor Demand



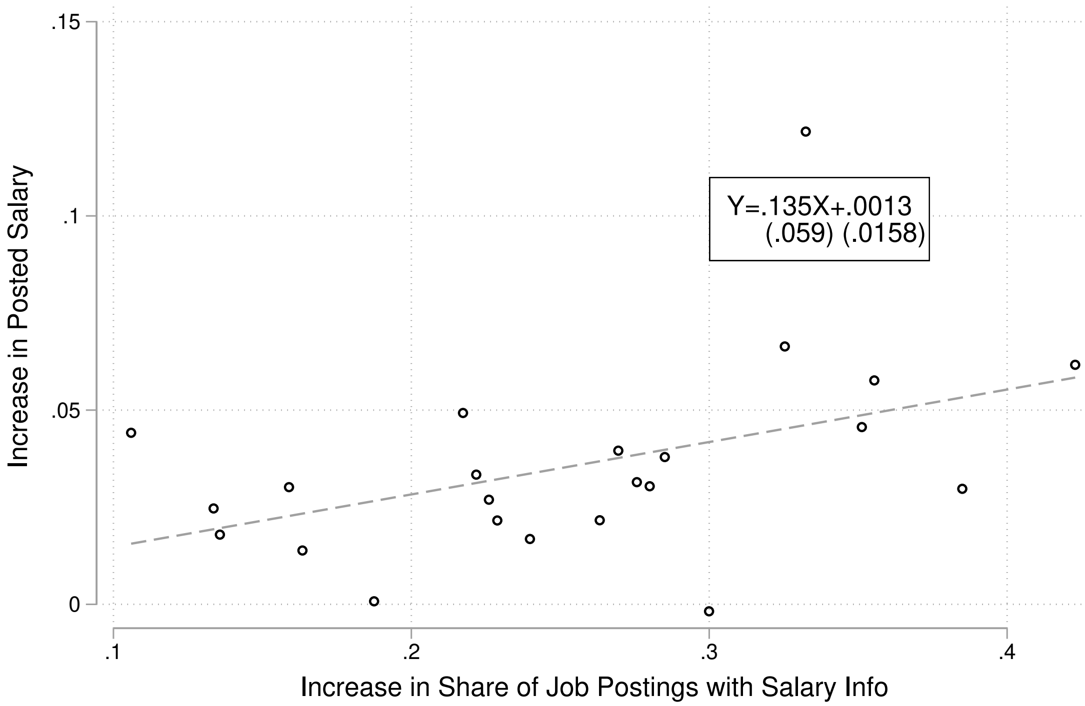
(a) Number of Job Postings



(b) Log Employment

Note: The figure plots the impact of pay transparency laws on the number of job postings and employment. Panel (a) compares the number of job postings in Colorado vs. all other states relative to the number of postings in December 2020. Postings are aggregated 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. Panel (b) plots event study estimates of the average impact of 10 pay transparency laws on log employment in 4-digit NAICS-by-county level cells, utilizing data from the QCEW. The reported estimates are estimated using the methodology developed in difference-in-difference method developed by Callaway and SantAnna (2021). 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 9: 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.

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

	Colorado	Other States
Panel A: Salary Information	(1)	(2)
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
Panel B: Occupational Characteristics		
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)
$Post \cdot Colorado$	.036	.044	.032	.073
	(.006)	(.007)	(.006)	(.009)
Sample	All	Above MW	Similar MW	All
Firm FE				X
Time FE				X
Firm-SOC-FIPS FE	X	X	X	
SOC-Time FE	X	X	X	
N	14,465,056	8,611,123	3,965,636	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) controls only for employer and time fixed effects. Standard errors are clustered at the firm level.

Table 3: Effect of Transparency Law on Self-Reported Wages in Glassdoor

	(1)	(2)	(3)	(4)	(5)	(6)
Post · Colorado	.013	.01	.009	.018	.013	.03
	(.002)	(.003)	(.005)	(.005)	(.007)	(.009)
Sample	All	All	All	New Hires	New Hires	New Hires
State-Occupation FE	X			X		
Month-Occupation FE	X	X		X	X	
State-Occ-Firm FE		X	X		X	X
Month-Occ-Firm FE			X			X
N	10,068,696	6,009,886	3,126,783	1,156,113	539,249	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 Pay Transparency Law on Log Average Weekly Wages in QCEW

	(1)	(2)	(3)	(4)
$Post \cdot Treat$	.014	.013	.009	.011
	(.002)	(.002)	(.009)	(.003)
Sample	Colorado	All	All	All Excl. CO
4-digit-NAICS-FIPS FE	X	X	X	X
Weighted	No	No	Employment	No
N	2,960,685	4,761,632	4,761,632	4,672,498

Note: This table displays difference-in-difference estimates that estimate the impact of pay transparency laws on log average weekly salaries at the 4-digit NAICS-by-county level. Column (1) reports the unweighted (i.e. each 4-digit NAICS-by-county cell receives a weight of one) impact of Colorado's 2021 law. Column (2) reports the effect aggregated across all laws passed by July 2024. Column (3) weights the regression in column (2) by the average level of employment in the 4-digit NAICS-by-county cell. Column (4) excludes Colorado from the event study. Standard errors are clustered at the county level.

Table 5: Effect of Transparency Law on the Range of Posted Wages

	(1)	(2)	(3)	(4)	(5)
$Post \cdot Colorado$	.035	.037	-.002	.003	.032
	(.008)	(.005)	(.006)	(.006)	(.011)
Outcome	Log(Max)	Log(Min)	Log(Max/Min)	Log(Max/Min)	Posted Range
State FE				X	X
Time FE				X	X
Firm-Soc-Fips FE	X	X	X		
Soc-Time FE	X	X	X		
N	14,465,056	14,465,056	14,465,056	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 and minimum salary in each job posting, respectively. Columns (3)-(4) report the effect on the ratio of the maximum and minimum salaries. Column (5) reports the effect on whether the job posting is a range or point. Standard errors are clustered at the firm level.

Table 6: Effect of Pay Transparency Law on Wage Dispersion in Glassdoor

	(1)	(2)	(3)
Log Standard Deviation	.009	.028	-.029
	(.029)	(.02)	(.02)
90/10 Ratio	-.064	.008	-.062
	(.092)	(.033)	(.061)
Unit	Employer	Occupation	City-Occupation
Baseline SD	32380	25208	30205
Baseline 90/10	3.189	2.752	2.857
Employer-State FE	X		
Occupation-State FE		X	
City-Occupation FE			X
Month FE	X	X	X
N	13,067	35,187	13,135

Note: This table displays difference-in-difference estimates of the impact of Colorado's pay transparency law on the log of the standard deviation and 90-10 percentile ratio of wages within employer (column 1), occupation (column 2), and city-occupation (column 3). Standard errors are clustered at level of the unit of analysis.

Table 7: Effect of Transparency Law on Number of Job Postings

	(1)	(2)	(3)	(4)	(5)
Post · Colorado	.006	.059	-.004	-.001	.012
	(.016)	(.063)	(.01)	(.031)	(.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	.387	.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. Standard errors are clustered at the firm level.

Table 8: Effect of Pay Transparency Law on Employment in QCEW

	(1)	(2)	(3)	(4)
$Post \cdot Treat$	.011	2.049	-.005	4.163
	(.004)	(4.066)	(.003)	(4.217)
Sample	Colorado	Colorado	All	All
Outcome	Log	Level	Log	Level
Baseline	4.796	233.668	5.324	643.680
4-digit-NAICS-FIPS FE	X	X	X	X
Quarter FE	X	X	X	X
Data	QCEW	QCEW	QCEW	QCEW
N	2,960,685	7,353,344	4,761,632	11,929,279

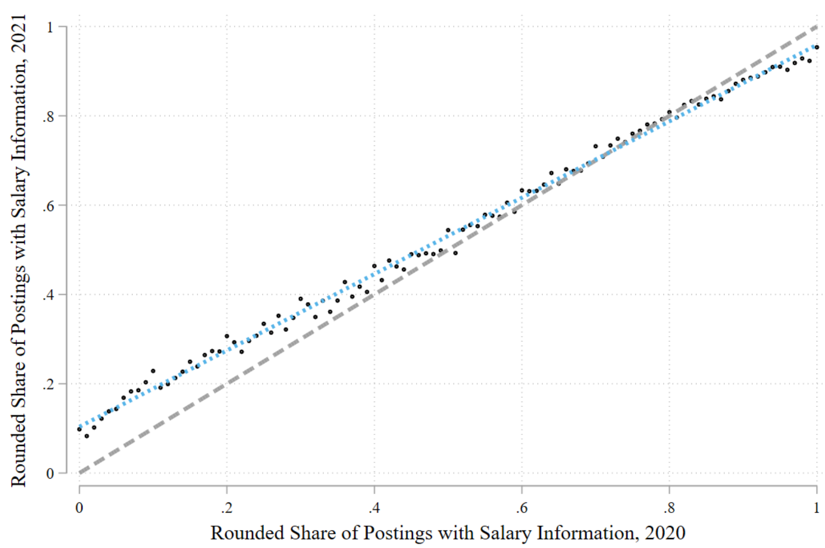
Note: This table displays difference-in-difference estimates that estimate the impact of pay transparency laws on employment at the 4-digit NAICS-by-county level. Columns (1) and (2) report the impact of Colorado's 2021 law on log employment and average employment, respectively. Columns (3) and (4) reports analogous estimates for the impact of all laws passed from Januyary 2021 to July 2024. Standard errors are clustered at the county level.

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

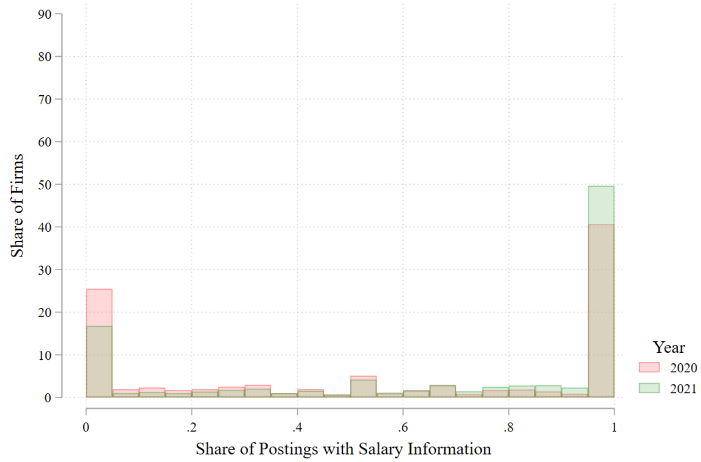
(a) Colorado



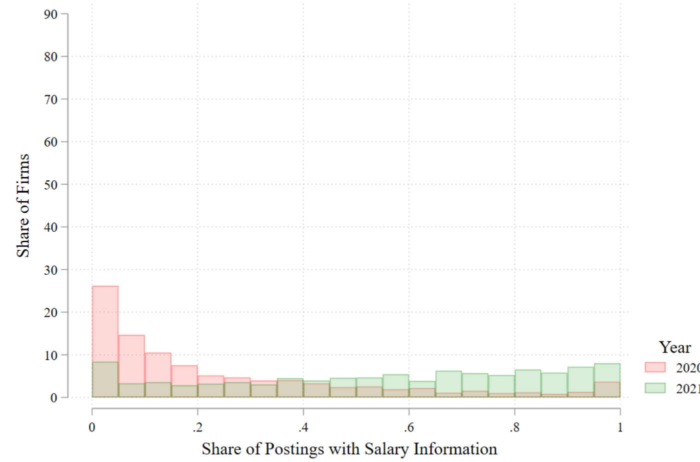
(b) Other States

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.

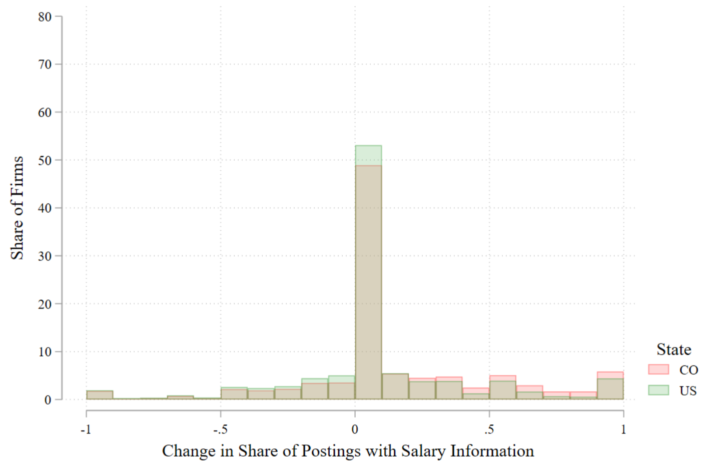
Appendix Figure A.2: Distribution Postings with Salary Information, 2020 vs 2021



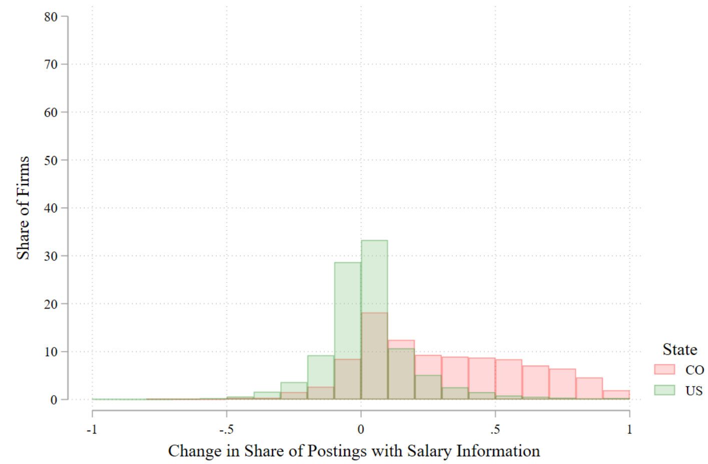
(a) Colorado, 10-100 Postings



(b) Colorado, >100 Postings



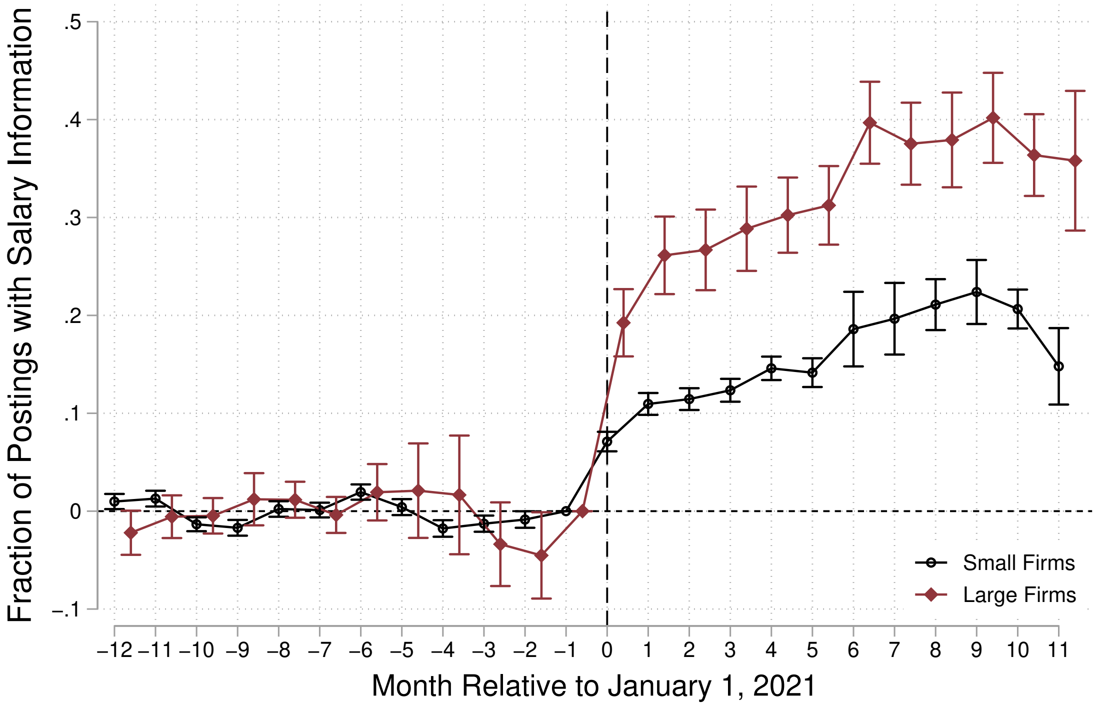
(c) Colorado vs. U.S., 10-100 Postings



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

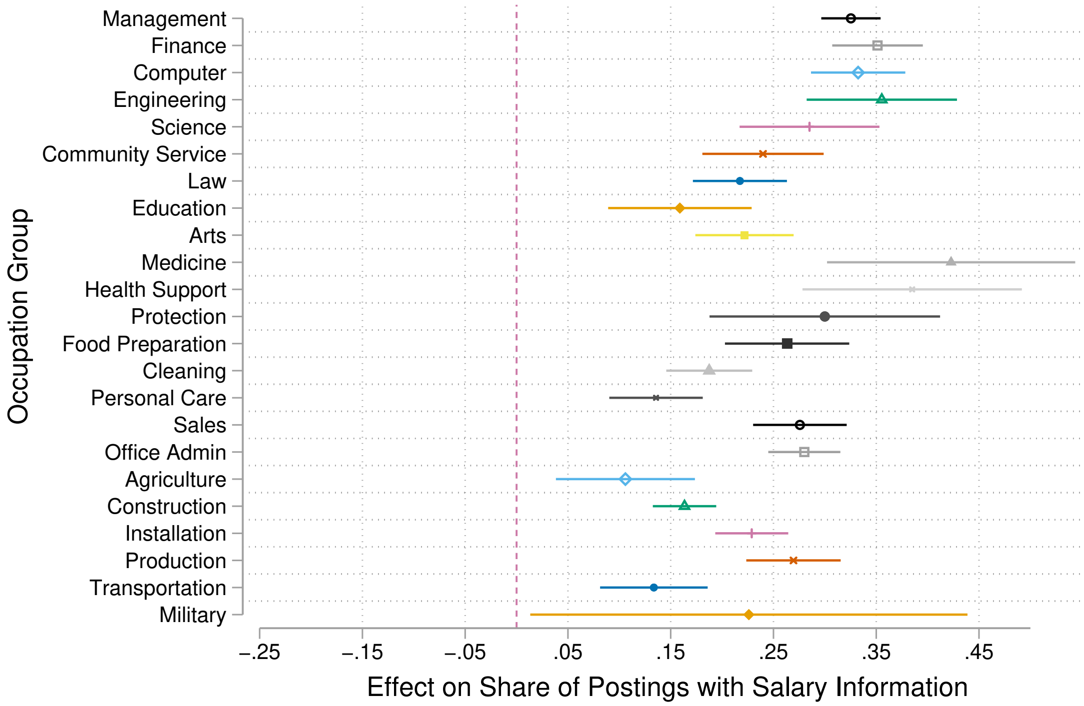
Note: Panel (a) plots the distribution of employers in Colorado by the share of their postings with salary information, separately for 2020 and 2021. The sample is restricted to employers that post jobs in Colorado in both years, with between 10-100 postings in 2020. Panel (b) plots an analogous figure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



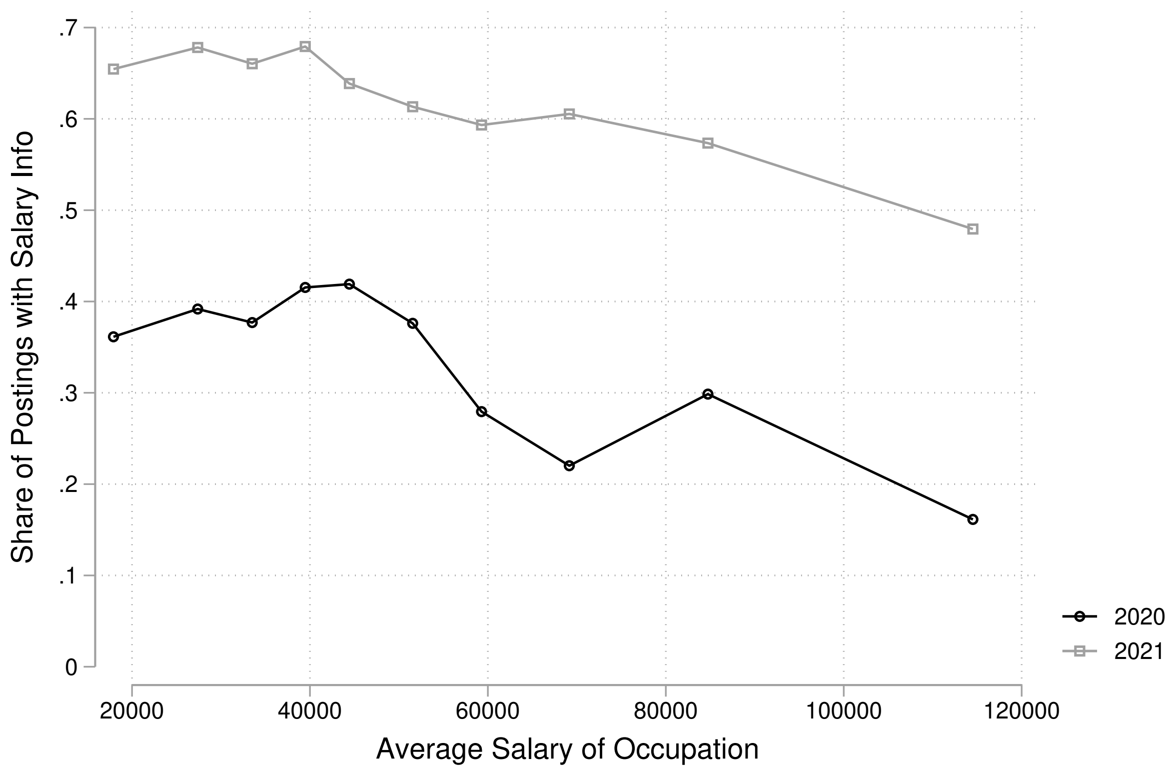
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for firm-states with fewer than 100 posting in 2020 and firm-states with more than 100 postings in 2020. 95 percent confidence intervals clustered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

Appendix Figure A.4: Impact of Pay Transparency Law on Fractions of Postings with Salary Information, by Occupation



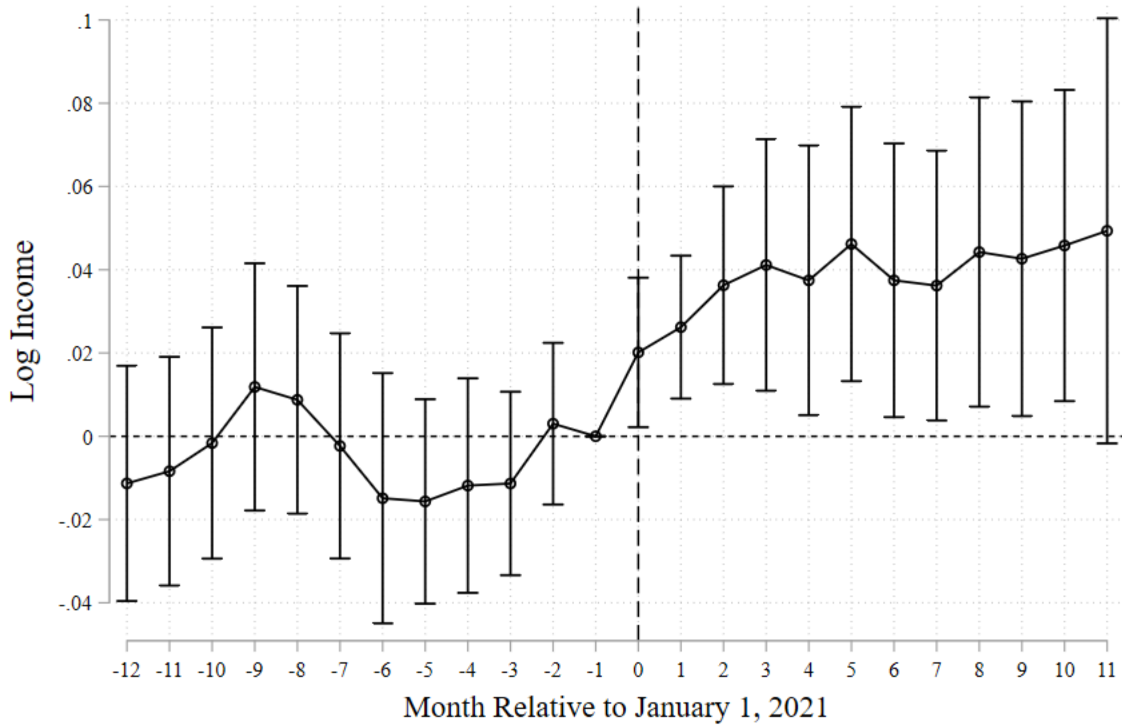
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for each 2-digit SOC occupation code, following the specification in Equation 1. 95 percent confidence intervals clustered at the firm level are displayed.

Appendix Figure A.5: Transparency by Wage of Occupation, within Colorado



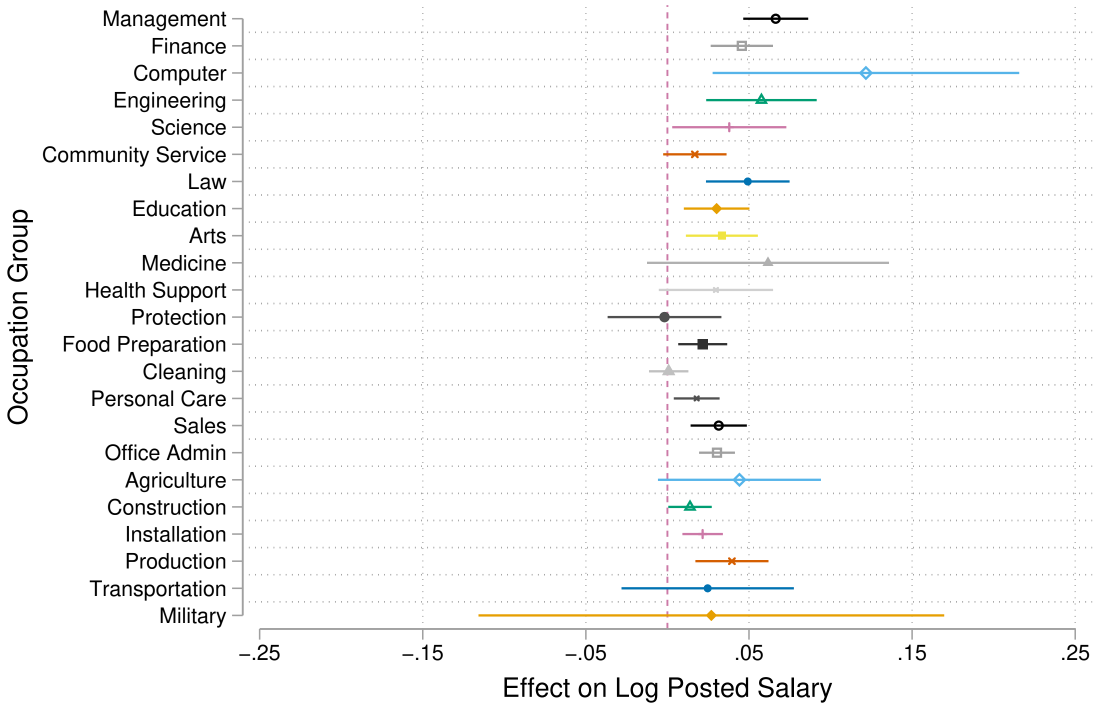
Note: The figure plots the average probability that a job posting has salary info, as a function of the average salary of the posting's 5-digit SOC code computed from the 2015-2020 ACS. The postings are aggregated over deciles of average salary across occupations.

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



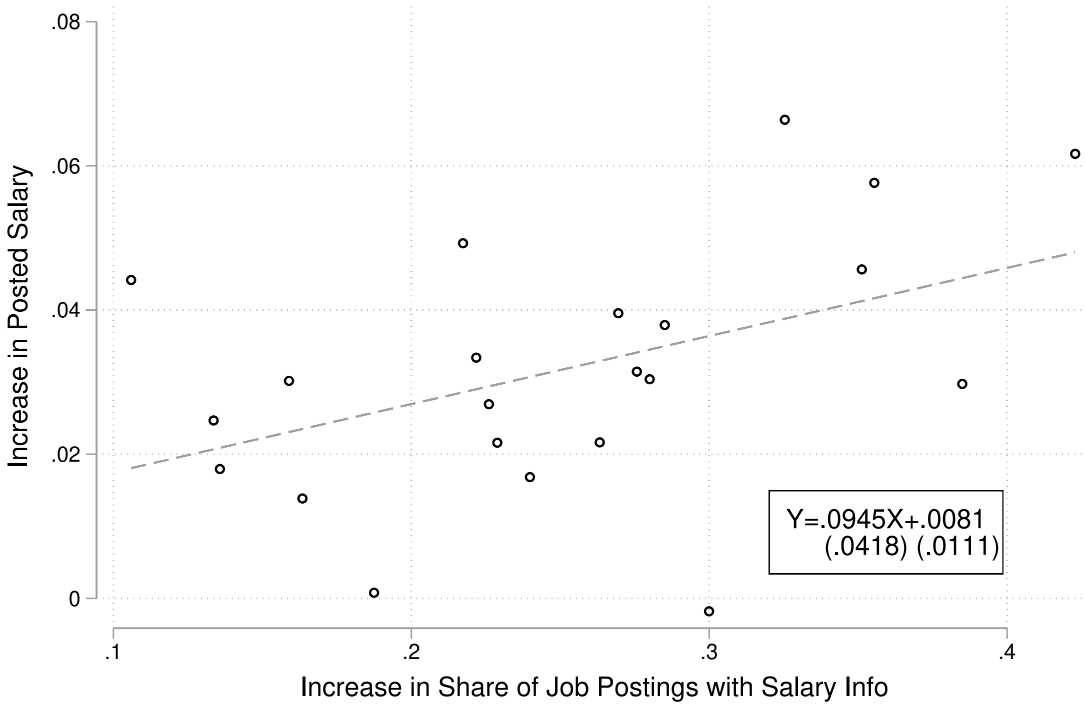
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary following the specification in Equation (2). The sample is restricted to firm-SOC-FIPS with an average wage of at least \$14/hr in 2020. This specification controls for firm-SOC-FIPS 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.

Appendix Figure A.7: Impact of Pay Transparency Law on Log Posted Salary, by Occupation



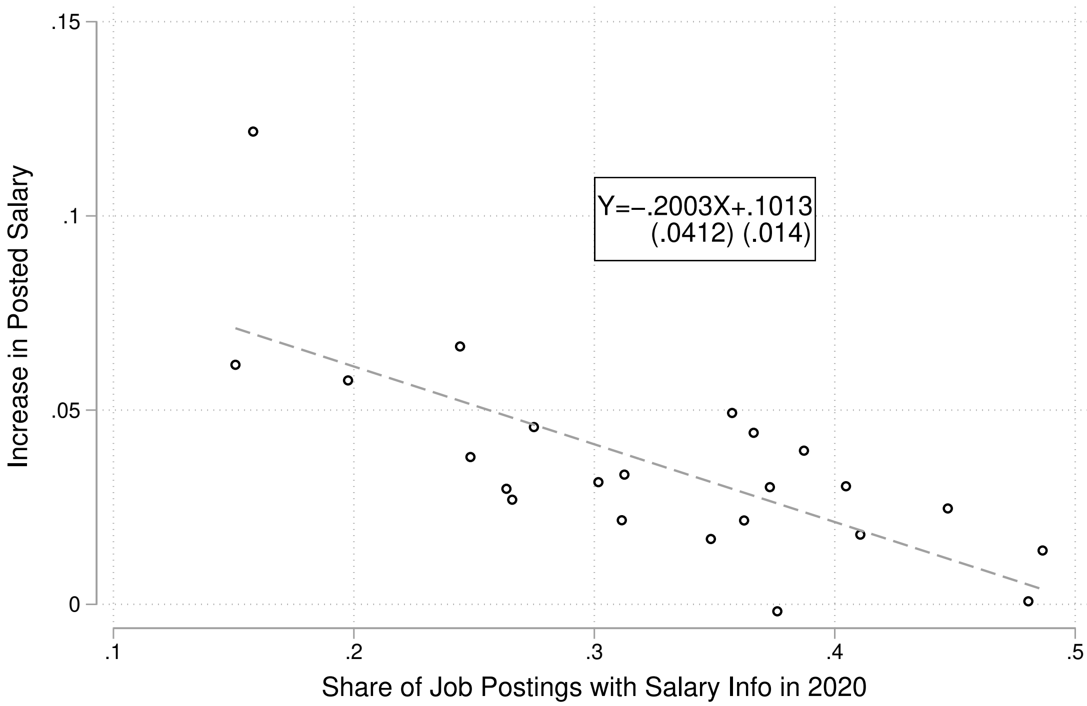
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary, separately for each 2-digit SOC occupation code. 95 percent confidence intervals clustered at the firm level are displayed.

Appendix Figure A.8: Impact of Pay Transparency Law on Log Posted Salary vs. Share of Postings with Salary Info, by Occupation



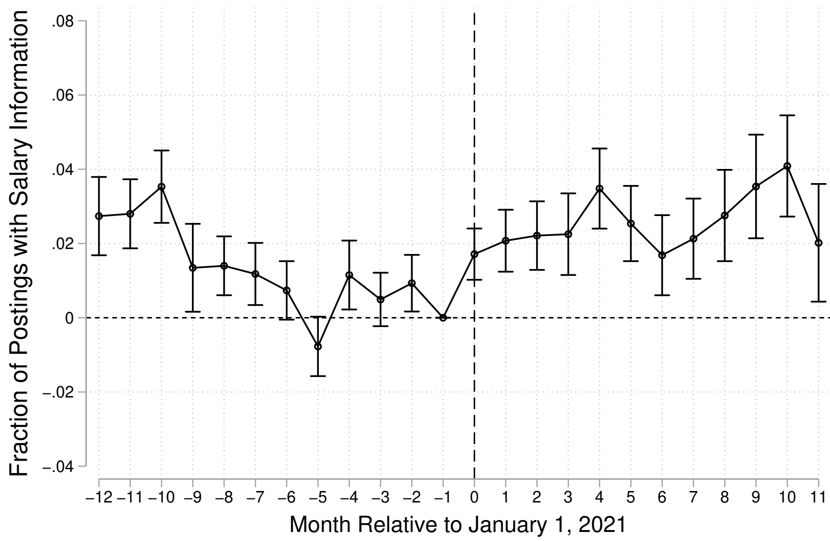
Note: This figure plots the point estimates of the wage effect of the Colorado pay transparency law against the effect on the share of postings with salary info, where each point represents a 2-digit occupation group. Excluded in the figure are computer and mathematical occupations. The equation in the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

Appendix Figure A.9: Impact of Pay Transparency Law on Log Posted Salary vs. Share of Postings with Salary Info in 2020, by Occupation

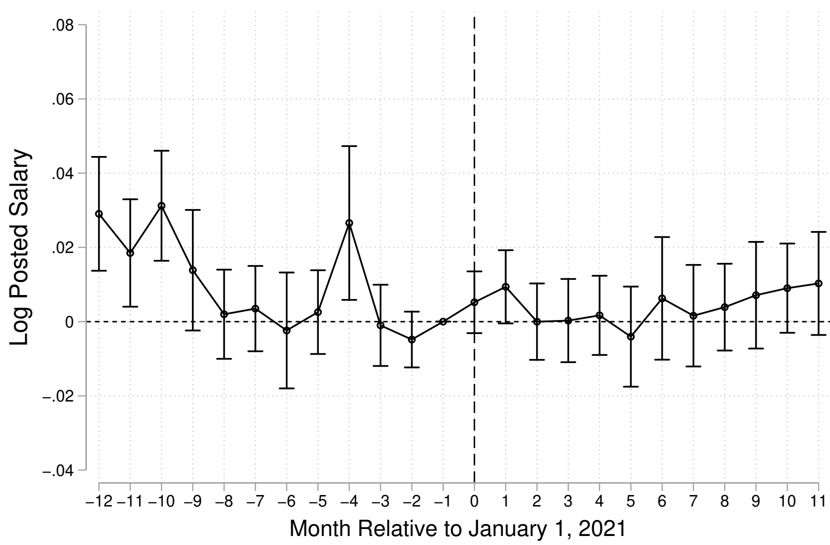


Note: This figure plots the point estimates of the wage effect of the Colorado pay transparency law against the share of postings with salary information in 2020, where each point represents a 2-digit occupation group. The equation in the box reports the estimates of the OLS prediction line, along with standard errors in parentheses.

Appendix Figure A.10: Spillover Effects on Multi-State firms



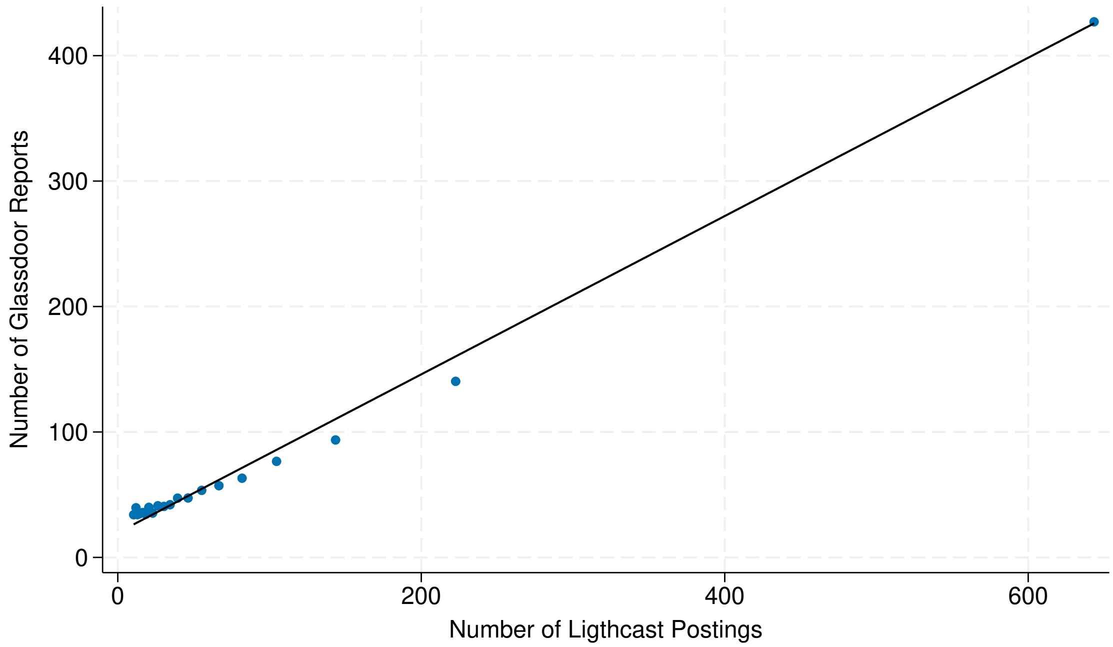
(a) Pay Transparency



(b) Posted Salaries

Note: The figure plots difference-in-difference estimates comparing multi-state firms with and without job postings in Colorado in 2020. Panel (a) plots the impact on the share of postings with salary information and Panel (b) plots the effect on posted salaries. The sample is restricted to only postings outside of Colorado. All regressions control for firm-SOC-FIPS and SOC-time fixed effects.

Appendix Figure A.11: Binned Scatterplot Comparing Number of Postings in Lightcast vs. Number of Salary Reports in Glassdoor



Note: This figure presents a binned scatterplot showing the relationship between the number of job postings in Lightcast vs. the number of salary reports in Glassdoor.

Appendix Table A.1: Date of Pay Transparency Laws

Level	**Location**	**Effective Date**
State	Colorado	Jan 2021
Municipality	Jersey City, NJ	April 2022
Municipality	Ithaca, NY	September 2022
Municipality	New York City, NY	November 2022
Municipality	Westchester County, NY	November 2022
State	Washington	January 2023
State	California	January 2023
State	New York	September 2023
State	Hawaii	January 2024
Municipality	Washington, D.C.	July 2024
State	Maryland	October 2024
State	Illinois	January 2025
State	Minnesota	January 2025
State	New Jersey	June 2025
State	Massachusetts	July 2025
State	Vermont	July 2025

Notes: This table reports in chronological order, the dates that pay transparency laws in the U.S. went into effect.

Appendix Table A.2: 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.3: Effect of Transparency Law on Posted Wages, Robustness to Composition Changes

	(1)	(2)	(3)	(4)
$Post \cdot Colorado$	.022	.023	.009	.061
	(.004)	(.007)	(.003)	(.01)
Sample	All	All	100% Trans.	<100% Trans
Firm-SOC-FIPS FE			X	X
SOC-Time FE			X	X
Firm-Title-FIPS FE	X	X		
Title-Time FE	X			
Firm-Title-Time FE		X		
N	6,455,098	3,934,497	5,514,404	6,097,834

Note: This table displays difference-in-difference estimates that compare the log posted salaries in Colorado to other US states, before and after 2021, with alternative samples and job-title definition. Column (1) defines an occupation using the specific job title listed in the posting and Column (2) further adds firm-occupation-month fixed effects. Column (3) restricts the sample to only employers with 100% pay transparency in 2020, and Column (4) keeps only employers with less than 100% transparency. Standard errors are clustered at the firm level.

Appendix Table A.4: Effect of Transparency Law on Posted Wages, by Exposure to Covid-19

	(1)	(2)	(3)	(4)
$Post \cdot Colorado$	.035	.036	.036	.036
	(.007)	(.007)	(.006)	(.006)
Firm-SOC-FIPS FE	X	X	X	X
SOC-Time FE	X	X	X	X
Covid Case Rate	X	X		
Covid Death Rate		X		
2020 Case Decile-Time FE			X	
2020 Deaths Decile-Time FE				X
N	13,566,388	13,243,632	14,461,501	14,461,501

Note: This table displays difference-in-difference estimates that compare the log posted salaries in Colorado to other US states, before and after 2021, with various controls for the impact of Covid 2020 by county. Column (1) controls for the rate of Covid cases each month, column (2) further controls for monthly Covid death rate, columns (3) and (4) control for county-specific time trends given their exposure to Covid in 2020. Standard errors are clustered at the firm level.

Appendix Table A.5: Effect of Transparency Law on Self-Reported Wages in Glassdoor

	(1)	(2)	(3)	(4)
$Post \cdot Colorado$	.017	.016	.034	.021
	(.002)	(.003)	(.007)	(.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
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 ezperience. Standard errors are clustered at the state level.

Appendix Table A.6: Effect of Transparency Law on Salary Compression

	(1)	(2)	(3)
Log Standard Deviation	.269	.069	.078
	(.022)	(.017)	(.019)
90/10 ratio	.259	-.019	-.13
	(.037)	(.038)	(.051)
Unit	Employer	Occupation	County-Occupation
Baseline SD	17385	24404	23740
Baseline 90/10	2.087	2.579	2.632
Unit FE	X	X	X
Month FE	X	X	X
N	227,070	131,491	341,412

Note: This table displays difference-in-difference estimates of the impact of Colorado's pay transparency law on the log of the standard deviation and 90-10 percentile ratios of posted wages within employer (column 1), occupation (column 2), and county-occupation (column 3). Standard errors are clustered at level of the unit of analysis.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post · Colorado	-.003	.003	-.026	-.005	-.01	0.0001	-.11	.001
	(.006)	(.004)	(.034)	(.005)	(.006)	(.005)	(.041)	(.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.8: Effect of Transparency Law on Realized Wages: Heterogeneity by Baseline Transparency

	(1)	(2)	(3)
$Post \cdot Colorado$	.009	.014	.018
	(.008)	(.006)	(.01)
$Post \cdot Colorado \cdot BaselineTransparency$	.012	-.008	-.022
	(.02)	(.016)	(.034)
Sample	All	All	All
State-Occupation FE	X		
Month-Occupation FE	X	X	
State-Occ-Firm FE		X	X
Month-Occ-Firm FE			X
Data	Glassdoor	Glassdoor	Glassdoor
N	1,241,810	1,102,212	648,246

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 specifications. The variable Baseline Transparency computes the fraction of jobs within an employer-occupation cell that contain salary information in the period before the transparency law. Standard errors are clustered at the firm level.

Appendix B. Model of Interval vs. External Pay Transparency

The main goal of this section is to illustrate two mechanisms through which pay transparency in postings may impact overall wages. First, pay transparency in job postings introduces within-firm bargaining, whereby incumbent workers may use the posted wage to bargain over their own wages. Second, posting a salary updates workers' beliefs about the wages offered, allowing employers to draw more applicants for any given wage. As a result of these competing channels, the overall impact of a pay transparency law on wages is ambiguous.

To capture these mechanisms, we consider a two-stage monopsony model in which firms want to maximize their profits

$$\pi = (\theta - w) \cdot N(\alpha w) + I \cdot (\theta - w_I(w)) \tag{3}$$

where θ is the productivity of each worker, N is the number of new hires, I is the number of incumbent workers, and $w_I(w)$ are the wages of incumbents as a function of the wages of new hires. The parameter α represents workers' beliefs about the wages in new jobs. For example, if $\alpha < 1$, then workers underestimate the wages that they would actually receive.

In the first stage, a representative firm decides whether or not to post a wage. If they post a wage, we assume that workers have perfectly accurate beliefs about the salary offered (i.e. $\alpha = 1$), but incumbents use the wages of new hires to bargain for higher wages (i.e. $\frac{\partial w_I}{\partial w} > 0$). On the other hand, if they do not offer a wage, then workers underestimate their outside options (i.e. $\alpha < 1$), but incumbent workers do not know the wages of new hires (i.e. $\frac{\partial w_I}{\partial w} = 0$).

In the second stage, the firm chooses a wage w to maximize its profits. We solve the model via backward induction from the second stage. Given α and $w_I(\cdot)$, the optimal offered wage satisfies the following first order condition:

$$\underbrace{\frac{\theta - w^*}{w^*}}_{\text{Wage Markdown}} = \frac{1 + R_I \cdot \frac{\partial w^I}{\partial w}}{\alpha \eta} \tag{4}$$

where R_I is the ratio of incumbents to entrants and η is the elasticity of new hires with respect to entry wages. The expression in Equation (4) is similar to a standard monopsony markdown rule, with two exceptions.[8]

First, following Cullen and Pakzad-Hurson (2023), the wages of incumbents may respond to the wages of new hires via $\frac{\partial w^I}{\partial w}$, which reflects how incumbent workers' wages change with respect to entrants' wages. The entire term in the numerator $R_I \cdot \frac{\partial w^I}{\partial w}$, can be interpreted as an inframarginal cost of posting a higher wage driven by the impact on incumbent wages. This spillover effect acts as a force to decrease wages relative to the standard monopsony model. Second, following Jäger et al. (2024), we assume that workers underestimate their outside options when wages are not posted. If $\alpha < 1$, then the information friction effectively decreases the labor supply elasticity facing the firm, allowing them to set lower wages.

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

Suppose w_p^* is the optimal wage if the firm is transparent and w_{np}^* is the optimal wage if not. In the first stage, the firm would choose to post a wage if

$$\underbrace{\pi(w_p^*|\alpha = 1, \frac{\partial w^I}{\partial w} > 0)}_{\text{Profits if post}} > \underbrace{\pi(w_{np}^*|\alpha < 1, \frac{\partial w^I}{\partial w} = 0)}_{\text{Profits if not post}} \tag{5}$$

Note that because the choice to be transparent affects not only the optimal wage, but also the entire profit function, the firm does not necessarily always choose the option with the lowest wage. Formally, the difference in profits between posting and not posting is equal to

$$\pi_p^* - \pi_{np}^* = \underbrace{\theta \cdot [N(w_p^*) - N(\alpha w_{np}^*)]}_{\text{Effect on labor supply}} - \underbrace{[w_p^* N(w_p^*) - w_{np}^* N(\alpha w_{np}^*)]}_{\text{Effect on wage bill of new hires}} - \underbrace{I \cdot [w_I(w_p^*) - \bar{w}_I)]}_{\text{Effect on wage bill of incumbents}} \tag{6}$$

Even in the very niche case where $w_p^* = w_{np}^*$, the firm may prefer to post or not post wages depending on its impact on the labor supply of entrants and the wage bill of incumbents. Ultimately, the optimal wage depends on the marginal returns to increasing the wage, whereas the decision to post depends on the aggregate difference.

Without any policy requirement, heterogeneity in $w_I(\cdot)$, α, and the labor supply elasticity across firms will lead to differences in the decision of whether or not to post a salary. 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.

Not-posting firms: The impact on wages is ambiguous for firms that were not posting wages before the law change. As discussed, employers are not necessarily choosing the option with the lowest wage. Given their initial decision to not post a wage, if the internal bargaining channel dominates, then forcing firms to be transparent will actually lower entry wages. However, if the effect of overcoming the information friction is sufficiently strong, then transparency will increase wages for new entrants.

Always-posting firms: In the model, the labor supply elasticity η is taken as exogenous. However, this parameter can 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.

One could further enrich this model in a number of directions. For example, in the current framework, we do not model how firms use information they gather from competitors (Cullen et al., 2022a). Additionally, worker productivity is assumed to be known by the firm, while in reality firms will form beliefs about workers' productivity. Uncertainty over productivity could be an important reason why firms post wage ranges or do not post wages at all (Michelacci and Suarez, 2006). 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.

Appendix C. Linking Glassdoor and Ligthcast

Neither Glassdoor or Lightcast contain unique firm identifiers, such as employer identification numbers (EINs) that can be used to link the two. Instead, each dataset contains the name of the firm that made the posting as well as the location. To match the two datasets, we perform a fuzzy name matching. However, we require three additional criterion for a match to be formed. First, we drop small firms from the data. In particular, we drop firms that have less than 10 postings in Ligthcast or 10 reviews in Glassdoor. The goal of the merge is to study impacts by baseline transparency. If we have few postings, the measure of baseline transparency will be inherently noisy. Therefore, we drop small firms from the analysis.

Second, we perform the match state-by-state. For Lightcast, a firm exists in every state that it has a posting. In Glassdoor, a firm exists in every state that an individual reports working from. Therefore, the unit-of-observation that we match at is firm-by-state (i.e. for national firms, like Walmart, we match Walmart in California in Ligthcast to Walmart in California in Glassdoor). The reason we do this is both computational, but also because some names are relatively common, so having a notion of location limits the potential matches to choose between. Lastly, we require that the first letter of the firm name match in both datasets. These restrictions greatly reduce the total number of firms that a given firm may match to. This will both increase the quality of the matches, as well as reduce the computational burden of the matching process.

Within these criterion, we then use a fuzzy name procedure. In particular, we use a bigram similarity score which is computed from the ratio of the number of common two consecutive letters of the two strings and their average length minus one. An exact match between two firm names has a bigram score of 1, while two words with no common two consecutive letters has a bigram score of 0. We choose the match with the highest bigram score, but dropping all matches with a score of less than 0.9.

In total, after dropping small firms, there are 115,981 firm-state observations in Glassdoor and 148,604 firm-state observations in Ligthcast. In total, we successfully match 60,403 firms between the two datasets. To probe the effectiveness of the matching strategy, Figure A.11 plots a binned scatterplot with the number of Ligthcast postings on the horizontal axis and the number of Glassdoor observations on the vertical axis. As can be seen in the figure, there is a strong correlation between size in the two datasets, providing evidence that the matching strategy finds comparable firms in terms of size.