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

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Abstract

In recent years, pay transparency in job postings has been proposed as one way to combat wage stagnation and reduce gender and racial wage gaps. However, there is limited empirical research on the impact of pay transparency in job postings on the labor market. While it may increase information to workers, potentially strengthening bargaining power, it also increases information to firms, potentially leading to tacit collusion in wage setting. The paper studies the impact of a January 2021 law in Colorado that required job postings to contain expected salary information. Using data from Burning Glass Technologies, we find that this law increased the fraction of postings with salary information by 30 percentage points, although there remains substantial non-compliance. For employers that posted salaries both before and after the policy, we find that posted salaries increased by about 3.6 percent, on average.

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1 Introduction

Pay transparency laws are increasingly being discussed as a means to improve labor-market outcomes for workers around the world. To date, many laws have often focused on transparency of current employees, particularly for the public sector. For example, in 2007, salaries for all public employees in California were deemed public information by the State Supreme Court (Mas, 2017). Similarly, since April 2017 in the U.K., firms with more than 250 employees must publish information about the gender pay gap in their firm. Recently, however, there has been a wave of interest in increasing transparency at the hiring stage by making expected salary information a requirement in job postings. For example, on January 1, 2021, Colorado enacted a pay transparency law that requires online job postings to include information about the expected salary of the position. New York, California, and Washington have passed similar laws that went into effect by January 2023. Despite the growing popularity of these laws, there is limited empirical research on the impact of pay transparency in job postings on the labor market.

The public narrative has focused on the potential positive impacts on workers' salaries. Recent survey evidence finds that workers tend to underestimate their outside options (Jager et al., 2021), so by giving workers more information about salaries in other firms, they can better aim their search to high-wage jobs. However, firms may also adjust in response to transparency laws, making the overall impact ambiguous. For example, in certain cases, price transparency in product markets has led to increases in price rather than decreases (Albæk et al., 1997) due to increased collusion between firms.

In this paper, we study the impact of increased wage transparency in job postings by analyzing the impact of the Colorado law which mandated that employers must disclose expected compensation in online job postings. To study the impact of this law, we use data from Burning Glass Technologies, which contains a large sample of online job postings. Importantly, the data contains information on whether a given posting contains expected salary information, allowing us to study both the impact of the law on the presence of salary information as well as the impact on posted salary levels.

We find 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 percent. This jumps to around 50 percent immediately after the policy is effective, and then further increases to nearly 70 percent in the following months. Therefore, while the policy did have a large and persistent impact on pay transparency in posting, compliance is still far from 100 percent.

Given the non-negligible rate of non-compliance, we next study heterogeneity in compliance. We find that the reform had a larger impact on the pay transparency of large firms since small employers already tended to post salaries even without the law. Within firms, we find that employers are selective about which job postings include salary information. In other words, non-compliance with the new rule is not driven simply by some rogue firms, but rather, by the imperfect compliance

of many firms. Part of the non-compliance within-firm can be explained by the location and occupation of the job. For example, we find that postings for high paying occupations are less likely to be compliant with the pay transparency law. However, even a model with comprehensive firm-occupation-county fixed effects can only explain 62% of the variation in compliance.

In the next part of the paper, we study the impact of pay transparency on posted salaries. Given the large increase in the fraction of jobs with expected salary information, any impact could in principle be driven by compositional impacts. For example, if low-wage firms were more likely to post salaries before the policy, then we may find an increase in market-level posted salaries due to high-wage firms now posting the wage in job advertisements. Therefore, to focus on within-job changes in salaries, we estimate a dynamic difference-in-differences model that includes employer-by-occupation-by-county fixed effects. This implies that we identify the impact on jobs for which salary information is available both before and after the pay transparency law went into effect.

We find that posted salaries increase by about 3.6 percent following the passage of the pay transparency law, a response that occurs quickly and remains persistent in the year following the law. Interestingly, if we do not control for compositional changes, we find the effect is twice as large. This suggests that high-salary positions are the most opaque prior to the policy, so that when the policy becomes effective, firms start to post expected salary information for relatively higher paying jobs. One potential concern with these results is that contemporaneous changes in policy may be driving the wage impact. In particular, Colorado also increased the minimum wage from $12.00 to $12.32 on January 1, 2021. However, we find similar effects after restricting to jobs that pay above $14.00 an hour before the policy. Additionally, we likewise find similar results after restricting our control group to states that also experienced a similar-sized change in the minimum wage on January 1, 2021.

The increase in posted salaries is not simply due to an increase in the range of salaries, nor an increase in skill requirements. In the data, it is common for jobs to have a range associated with the expected salary. In the prior analysis, if a job contains a minimum and maximum, we take the average of the two as the expected salary. One potential response to the law is to begin posting wide ranges of salary, which presumably give workers less information about the expected salary. To understand whether firms are responding in this way, we estimate the income effect separately for the minimum and maximum salary posted in each vacancy, as well as for the ratio of the maximum and minimum. We find that employers did not respond to the pay transparency requirement by posting wider salary ranges. Instead, the evidence suggests that employers raised both the lower and upper salary bounds by approximately the same percent. The null effect on the range of salaries holds true whether we keep newly transparent jobs in the sample, or focus on always-transparent jobs. Using the same empirical design, we also show that firms were neither more likely to introduce nor increase education and experience requirements in their postings.

Using variation in the size of the first-stage transparency effect across occupations, we find that the income effect is largest among occupations that experienced the largest increase in the share of postings with salary information. The positive correlation between the magnitude of the

income and transparency effects is consistent with the view that the increase in income arises from a general equilibrium response whereby employers adjust their posted salaries in response to new information about their competitors' wages. That would also explain why posted salaries increased even among firms that were already fully transparent even before the policy reform.

Although posted salaries increased in response to the pay transparency rule, we find no evidence of a decrease in the number of job postings. Comparing the change in the number of vacancies in Colorado establishments to those of other states, we can precisely rule out even small reductions in vacancies. The same result holds even when we restrict the sample to occupations and firms that experienced the largest increase in pay transparency as a result of the new law.

To summarize our results, we find that the 2021 Colorado pay transparency policy led to a large increase in transparency in postings, which in turn, increased posted salaries but did not decrease the number of vacancies. There are a few important caveats to keep in mind when interpreting our results. First, we identify the impact on posted salaries, not actual salaries. It is possible that firms increase the posted salary without increasing the actual salary paid to employees.[1] Second, given our data on online vacancies, we are unable to identify the total impact of the policy on the labor market. For example, if pay transparency increases posted salaries, then incumbent workers may use this increase in hiring salaries to negotiate for raises. These types of impacts are not be detectable in our empirical design.[2]

Our paper contributes to a few distinct literature. First, it contributes to a growing literature on pay transparency. Many prior studies have studied impacts of disclosing salaries for existing workers at firms. For example, Mas (2017) studies the impact of a mandate that required online disclosure of municipal salaries and finds that it resulted in the compression of salaries in California. Card et al. (2012) studies the impact of revealing peer salaries on job satisfaction and search behavior. Cullen and Pakzad-Hurson (2021) uses a general equilibrium model to show that pay transparency can lead to declines in wages in equilibrium, and find evidence of decreased bargaining power following the roll-out of laws that protect the right of workers to inquire about the salaries of their coworkers. Several papers also study the impact of transparency laws on gender wage gaps, which is a common motivation for pay transparency laws. For example, Baker et al. (2019) finds that public sector salary disclosure laws in Canada reduced the gender pay gap by around 20-40 percent for university faculty.

Relative to the prior literature, our focus is on transparency in postings, rather than transparency for incumbent workers. While both transparency in postings and transparency for incumbents are often discussed as ways to address similar policy issues (e.g. low worker bargaining power and gender wage gaps), they may have very different impacts in practice. While relatively less is known about the effects of transparency in postings, there are two notable exceptions. First,

[1] Derenoncourt et al. (2021) finds that increases in posted salaries in Burning Glass data do translate to increases in actual salaries in their setting.

[2] In ongoing work, we are linking postings data to firm-level data that has information on income for all workers both before and after the policy, to identify the impacts on firms that previously did not post salaries. This will also allow us to study whether posted salary effects translate into real salary effects, and determine how the impact differs between incumbents and new hires.

Frimmel et al. (2022) uses a 2011 law change in Austria that mandated a minimum wage offer on job postings to identify the impact of pay transparency on the gender wage gap. Using data from a public employment agency, they find a reduction in the gender wage gap for vacancies that need to be urgently filled in Austria. Second, Skoda (2022) combines data from a major job board with administrative social security records to study a similar 2018 law in Slovakia. That paper finds that the wages of new hires increased by 3% in firms that did not provide salary information in their postings prior to the reform, relative to always-transparent firms. Compared to these studies of national policies that rely primarily on time-variation to identify causal effects, we evaluate a State policy that enables us to use unaffected states as a natural counterfactual comparison group. As a result, we are the first study to provide evidence of general equilibrium wages effects from pay transparency in job postings, even among firms that were already transparent prior to the new law.[3]

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

Lastly, our paper adds to a broad literature on the effects of regulating the types of information available during the hiring process. In particular, studies have evaluated the impact of rules that forbid employers from posting gender preferences in job vacancies (Kuhn and Shen, 2013; Card et al., 2021), inquiring applicants' criminal record history (Agan and Starr, 2017; Cullen et al., 2022b),

[3]Cullen and Pakzad-Hurson (2021) find general equilibrium effects of U.S. state laws that protect the right of private-sector workers to communicate salary information with their coworkers.

asking workers to post an asking salary (Roussille, 2020), and observing applicants' compensation history (Barach and Horton, 2021). Similar to these studies, we find evidence that the type of information available in vacancies affects broader labor market outcomes, namely, the posted wage. Interestingly, we find a positive wage response even among jobs that were already posting salaries prior to the rule change, suggesting that the public disclosure of additional salary information leads to general equilibrium responses whereby employers raise salaries to match their competitors.

The remainder of the paper is organized as follows. Section 2 discusses the institutional details and introduces the data. Section 3 estimates the impact of the pay transparency law on the availability of salary information on postings, while section 4 estimates the impact of the law on posted salaries. Section 6 concludes with a discussion of upcoming extensions to our current analysis.

2 Institutions and Data

2.1 The Equal Pay for Equal Work Act

On January 1, 2021, the Equal Pay for Equal Work Act (EPEWA) became effective in Colorado. This act requires employers to (1) include compensation in job postings, (2) notify employees of promotion opportunities, and (3) keep job description and wage rate records. Our focus will be on the first part of this act: including compensation in job postings. While the other components may have impacts on the labor market, given our data is only on job postings, we cannot study whether notifying employees of promotional opportunities impacts firm turnover or incumbent wages.

In terms of information that must be posted, the act requires firms to disclose in each job posting "the hourly or salary compensation, or a range of hourly or the salary compensation, and a general description of all of the benefits and other compensation to be offered to the hired applicant" (CDLE, 2021).[4] The salary range may extend from the lowest to the highest pay the employer actually believes it might pay for a particular job. Ultimately, there is no requirement that an employer actually pay within the posted range. However, a firm that posts very large ranges for all jobs, independent of the occupation, would not be complying with the law. For example, the Colorado Department of Labor states that "an employer cannot post the same $30,000-$100,000 range for janitor and accountant jobs alike, if it does not genuinely anticipate offering an accountant the low end, or a janitor the high end."

After the act became effective, enforcement came primarily through education rather than fines. For example, individuals can submit a non-compliance letter to the Colorado Department of Labor if they find an employer is not complying with the law. The Colorado Department of Labor will then send a Compliance Assistance Letter to the firm, who has an opportunity to comply. Six months from the initial passage, the Colorado Department of Labor found that 100 percent of employers fixed non-compliance issues following an assistance letter, and therefore waived fines

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

associated with noncompliance. Legally, the fines can vary from $500 to $10,000 per violation.

After the act became effective, news articles reported that some firms excluded Colorado workers from remote jobs in order to avoid having to comply with the legislation (Rubino, 2021). In response, the Colorado Department of Labor clarified which employers and job postings must comply with the law. In particular, if an employer has a single employee in Colorado, then the employee must post salary ranges for remote jobs, even if the posting specifies that the employee cannot perform the work from Colorado.[5] Jobs tied to a specific location outside of Colorado or remote jobs from companies that have no employees from Colorado are outside the jurisdiction of the law. In the empirical analysis, we will directly estimate whether the policy decreased the number of postings for jobs within Colorado, which could occur if firms transfer some jobs to locations not impacted by the legislation.

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

2.2 Data

Our data on job postings come from Burning Glass Technologies (BGT). BGT scrapes data from over 45,000 internet sources, including job boards and company websites. Importantly for our purposes, BGT job postings data contains information on whether the job posting has associated wage information. In some postings hourly wages are reported, while in others, annual salaries are reported. In order to make these two types of reporting comparable, hourly wages are converted to annual salaries by multiplying by 2080 (52 weeks times 40 hours a week). Postings with salary information often include a lower and upper bound. For simplicity, unless otherwise specified, we refer to a job's "posted salary" as the average between the minimum and maximum values posted.

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

[5] Unlike pay transparency laws in oter states like California, the Colorado law applies to all firms regardless of their size.

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. As can be seen in column (1), in 2020, about 34 percent 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 percent. Among jobs with salary information posted, the average posted salary is slightly higher in Colorado than in other states ($53,300 vs. $51,000).

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

3 The Impact of EPEWA on Pay Transparency in Online Postings

3.1 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 percent of job postings contain salary information. This fraction jumps to around 50 percent, before increasing further to almost 70 percent a year after the law has been effective. In comparison, for all other states, the fraction of jobs with salary information fluctuates from 30 to 40 percent, with a relatively flat trend over time. Overall, it is clear from Figure 1 that the law had a large and immediate impact on transparency in online postings in Colorado. We next proceed to a dynamic difference-in-differences analysis that allows us to directly assess pre-trends and estimate the magnitude of the change in transparency.

3.2 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 specifications,

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

The key coefficient of interest is δ_k, which is the coefficient on the interaction between month t and whether the job is located in Colorado. $k = 0$ corresponds to January 2021, the date the transparency in online postings law 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 the 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 as the average of all the post-event coefficients $\bar{\delta} = \sum_{k=1}^{11} w_k \cdot \hat{\delta}_k$, where the weights are proportional to the number of observations per month. Lastly, standard errors are clustered at the employer level.

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 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.

3.3 Heterogeneity by Firm and Occupation

While we document a large impact on the fraction of jobs with salary information on average, there is potentially considerable heterogeneity in how different firms and occupations respond. This heterogeneity is relevant for two reasons. First, while we document an impact on fraction posted, we have also found compliance is still far from complete. Exploring which firms and jobs are not

complying is important from a policy enforcement perspective. Second, the next section will study the impact of the policy on posted salaries. Heterogeneity in the posted wage effect will depend on how the magnitude of the policy's first stage impact on the likelihood of posting salaries vary across firms and occupations.

We begin by exploring firm-level heterogeneity in compliance. Figure 3 plots the share of an employer's postings that contain salary information in 2021 as a function 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 the nature of imperfect compliance in Colorado, Figure 4 plots the distribution of firms by their share of postings with salary information, separately for 2020 and 2021. Panel (a) suggests that relatively small firms (defined as having between 10-100 postings) appear to engage in an all-or-nothing form of compliance, with nearly 70% of employers either having full transparency or no transparency. On the other hand, panel (b) shows that firms with at least 100 postings appear to be more selective in which jobs they choose to reveal salary information. Unlike small firms, less than 20% of large firms had either full or no compliance in 2021. Rather than a subset of large firms becoming fully compliant, the evidence suggests that it was many firms becoming moderately more transparent by selectively choosing the postings that include salary information.

To determine which type of firms responded more strongly to the new law, panels (c) and (d) plot the distribution of the change in the share of postings with salary information between 2020 and 2021. In both Colorado and other states, we find that small firms do not significantly change their pay transparency over time. In contrast, large firms in Colorado become far more transparent relative to firms in the rest of the country. Taken together, Figure 4 suggests that Colorado's pay transparency law had a larger effect on large firms relative to small firms, as many small firms were already fully transparent prior to the policy change.[6]

A potential explanation for the variation in compliance within-firm is that employers highly value the option to bargain over salaries in certain occupations. To test whether the effect of the transparency law varied across occupations, we estimate Equation 1 separately for each 2-digit Standard Occupation Classification code while controlling for firm-SOC-FIPS and SOC-time fixed effects at the 6-digit SOC level. Figure 5 plots the estimates by occupation group, averaged over

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

all months in 2021. We find sizeable differences in the first stage response to the Colorado reform across occupations. For example, the share of postings with salary information only increased by about 13 percentage points among transportation jobs, but approximately 34 percentage points among health care support jobs.

In figure 6, we show that a significant predictor of compliance with the pay transparency law is the salary of the posting's occupation, as measured from the American Community Survey. In the bottom income-decile of occupations, about 65% of postings had salary information in 2021. In contrast, less than 50% of postings in the top income-decile had salary information. If the cost of posting a wage is zero, then we would expect firms to fully comply to avoid the potential penalty of breaking the law. However, the observation that noncompliance is largest among high paying occupations suggests that employers face a greater cost of publicly revealing the salary of high paying jobs than low paying ones. This suggests that at least part of the relationship between the propensity to bargain over wages and workers' salaries, as observed in the literature (Hall and Krueger, 2012; Caldwell and Harmon, 2019; Lachowska et al., 2022), is driven by a firm preference for not revealing the wages of high paying jobs.

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

4 The Impact of Pay Transparency in Postings on Posted Salaries

4.1 Dynamic difference-in-differences

While there is a large impact in the transparency of job postings, it is unclear how this increase in information will impact posted wages. In this section, we estimate the impact of pay transparency on posted wages. To estimate the impact of the Equal Pay for Equal Work Act on posted salaries, we implement a dynamic difference-in-difference design of the following form:

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

Where $\log(salary_{it})$ is the log annual salary of the job. Again, jobs with hourly rates posted are annualized by multiplying by 2080. The rest of the variables are defined in the same manner as Equation (1) and standard errors are again clustered at the employer level. Relative to Equation (1), this specification can only be estimated for job postings with salary information available both before and after the policy change. This eliminates about 40 percent of postings in Colorado and around 65 percent in other states. Additionally, given the inclusion of firm-SOC-FIPs fixed effects ($\psi_{j(i)}$) this estimation requires that the firm posts an advertisements for the same occupation in the same location both before and after the policy. We think it is particularly important to include these set of fixed effects in this estimation, as the composition of jobs with salary information has changed dramatically in Colorado following the law. Not controlling for the specific job (i.e. a given occupation offered by a given firm in a given location) would imply that any wage effect could be driven by these compositional changes. Additionally, we also include SOC-month fixed effects ($\tau_{c(i),t}$), implying the estimation is identified by within-occupation variation. Lastly, given a large hiring expansion by Amazon in late-2020 that has an outlier effect on our estimates, we drop Amazon from the sample.

4.2 Assumptions and Interpretation of Salary Effects

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

There are several potential coinciding shocks that may be concerning for this design. First, the Equal Pay for Equal Work act made several policy changes, one of which included mandating expected salary information in postings. As discussed in Section 2, the policy also made it mandatory to notify employees of promotion opportunities as well as maintain wage records. It is not immediately clear how these other policy details would impact posted salaries, but there are potential mechanisms for this. For example, if firms must post promotion opportunities to current employees, then it is possible they will reduce external hiring after the policy. This could impact the composition or number of jobs that firms advertise. However, given the inclusion of firm-SOC-county fixed effects, this type of impact will not be captured in the empirical design. If the composition of jobs changes in ways not captured by location and occupation, then this could in principle be part of the effect of the policy. While we do not think these type of effects are particularly likely to bias the results, a conservative way to interpret any effects 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 main channel.

A key coinciding shock in this setting is the increase in the minimum wage in Colorado from $12 to $12.32. As discussed in Section 2, 26 other states also had increased minimum wages starting in January. However, to ensure this is not driving the results we provide additional robustness

checks by (1) restricting to jobs with average salaries above $14 before the policy change and (2) restricting control states to those that experienced a similarly-sized minimum wage change.

Additionally, it is important to note that there are limitations to what we can identify given information only on posted salaries. First, as discussed before, we include firm-SOC-FIPS fixed effects ($\psi_{j(i)}$) into the estimation strategy, given our primary goal is to identify changes in the posted salaries for the same job. Therefore, the wage effect is only identified for jobs that have salary information both before and after the policy. It would be interesting to study the impact of the law on jobs that previously did not have salary information available, but unfortunately, this is not possible in the data. Lastly, since we do not have information on actual salaries, it could be that firms are promising higher salaries in the advertisement, but actually paying lower salaries in practice. Derenoncourt et al. (2021) use the BGT data combined with data from Glassdoor and the Current Population Survey to show that increases in posted salaries do translate to increases in actual salaries in their setting. This suggests that the impacts we identify will translate into real salary changes, unless the act itself impacted how changes in posted wages translate to changes in real wages.

4.3 Impact on Posted Salaries

Figure 7 plots $\hat{\delta}_k$ from estimating Equation (2). As can be seen in the figure, there are relatively similar trends in posted salaries between jobs in Colorado and jobs in other states prior to January 1, 2021. After January 1, 2021, posted salaries increase by about 3.6 percent in Colorado, an effect that remains relatively stable over time.

Table 3 tests the robustness of the income effect to alternative specifications. In order to summarize the effect, we average the estimates of the treatment effect over all months in 2021. Column (1) reports the estimate corresponding to Figure 7. 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 at $12.32.[7] As a secondary test, we restrict the control group to the 15 states that had a minimum wage increase of less than 8% in 2021. Among that group, the minimum wage change in Colorado ranked on the lower end so we would expect the estimate of the income effect to be biased downwards. Nevertheless, we still find a significant positive income effect in Colorado compared to other states that increased their minimum wages, providing evidence that any minimum wage changes are not driving the results.

Next, columns (4) and (5) attempt to make use of the heterogeneity in baseline transparency. Given that a firm-SOC-FIPS may comprise of multiple job postings over a year, there is potentially variation in the share of postings that contain salary information. Columns (4) and (5) split the sample by whether a firm-SOC-FIPS had transparency in all postings in 2020 or only some. The

[7] Appendix Figure 2 plots the analogous event-study estimates for jobs that paid well above minimum wage in 2020.

analysis finds that conditional on posting salary info for at least one job posting in 2020, the majority of firm-SOC-FIPS already include salary info in all postings. These fully transparent jobs actually have a larger positive wage effect than the partially transparent ones.

In column (6) of Table 3, we show that the preferred estimate of the income effect is robust to controlling for firm-SOC-time fixed effects. In this stricter specification, we identify the income effect by considering whether employers that post jobs for the same occupation in multiple states change posted wages in Colorado relative to other states. While this further reduces the influence of confounding variables by focusing on within-firm variation, it identifies the impact on a specific population: firms that post jobs in multiple states. The restricted variation estimates a statistically significant income effect that is about half the size of the estimate from the main specification. The smaller point estimate could be for multiple reasons. First, prior studies have shown firms sometimes set national wage policies Hazell et al. (2021), implying any impacts on Colorado wages could spill over to establishments in other states if firms have national wage-setting policies. Second, the set of firms that post the same job in multiple states may simply be different than the average firm, therefore, treatment effect heterogeneity could also explain the small variation in effect sizes.

To evaluate the effect of the pay transparency law on the composition of job postings, column (7) reports the difference-in-differences estimate if we only control for firm and month fixed effects. In this case, we find average posted salaries increased by 7.3% in Colorado compared to other states, about 2 times larger than the estimate from our main regression. The observation that 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 jobs relative low paying occupations. This result is consistent with previous empirical findings that highly-educated, high-income workers tend to bargain over their wages rather than be provided a posted salary (Hall and Krueger, 2012; Lachowska et al., 2022). Given that high paying jobs are less transparent at baseline, our main wage estimates that control for firm-SOC-FIPs fixed effects identify the impact of the pay transparency rule on jobs at the lower end of the pay distribution.

Next, we decompose the average income effect into the impacts on the maximum and minimum posted salaries. While not legally permitted, employers may be responding to the reform by simply posting a range of salaries so wide that they are effectively offering no real information and leaving room for bargaining during the interview stage of the hiring process. Table 4 averages the post treatment estimates from Equation (2) for three different outcome variables: log maximum posted salary, log minimum posted salary, and log of the ratio of the maximum and minimum posted salaries. We highlight three results. First, employers raised both the maximum and minimum posted salaries as a result of the Colorado pay transparency law. Second, the increase in the minimum posted salary is approximately the same as the increase in the maximum posted salary. Third, these results are robust to focusing on jobs that are likely to be unaffected by changes to the minimum wage.

One important note in these results so far is that these specifications control for firm-SOC-FIPs fixed effects. Therefore, the impact on the minimum and maximum salary is identified from jobs

that had wage information both before and after the policy change. If firms are posting very large ranges only in newly transparent jobs, the response would not be picked up in the prior analysis. To study this channel, column (7) estimates a specification that controls for only firm fixed effects, rather than firm-SOC-FIPs. In this case, if the newly transparent jobs within a firm have very large bounds, then we would expect the ratio between maximum posted salary and the minimum posted salary to increase after the policy. However, we find that this ratio actually decreases within the firm, by about 1 percent. Therefore, the results suggest firms are not posting exceptionally large salary ranges for jobs that become transparent as a direct result of the reform.

Table 5 reports the effect of the pay transparency rule on the education and experience requirements listed in job postings.[8] Columns (1) presents the estimate from a difference-in-difference regression with a binary outcome variable that indicates whether a job posting had any education requirement. We find precise zero effect on the probability that postings in Colorado would include an education requirement, and this estimate is robust to imposing stricter firm-Soc-FIPs fixed effects in column (2). Following the same empirical strategy, columns (3) and (4) likewise show that among postings with an education requirement, there is no change in the minimum years of required schooling. We repeat the analysis for experience requirements in columns (5) to (8), and find no systematic change in either the probability of including an experience requirement or the minimum years of experience.

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

4.4 Evidence of General Equilibrium Response

The fact that we find positive income effects even among jobs that already posted salary information suggests the presence of general equilibrium responses. For example, publicly revealing salary information may increase competition between firms, causing a market-level increase in posted salaries. To understand how the positive wage response varies across different labor markets, Figure 8 plots estimates of Equation (2) separately by 2-digit SOC codes. While there is no occupation in which the policy has a statistically significant negative income effect, we are more confident of a positive wage response in select sectors. With the exception of production jobs, the positive income effects appear to be concentrated in primarily white collar occupations such as management, finance, engineering, and law.

If the rise in income is due to general equilibrium responses, then we would expect the positive income effect to be concentrated among occupations that experienced a large increase in transparency across all firms. To test this hypothesis, Figure 9 plots the point estimate of the income effects, separately by 2-digit SOC codes, against the estimate of the increase in pay transparency

[8] Equivalent figures that show the parallel trends assumption holds is available in appendix 3.

from Figure 5. 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 increase in transparency at the market level.

Aside from increasing average wages, the ability for employers to observe each others' wages may lead to pay compression across firms in the same labor market. For example, Cullen et al. (2022a) finds that the availability of a salary benchmarking tool causes employers to bunch new hires' salaries at the median salary set by other firms for the same occupation-industry-region. To test whether a similar phenomenon occurs in our setting, we collapse our data by FIPs interacted with 6-digit SOC codes, and examine whether the spread of salaries within county-occupations shrink in Colorado after 2021 relative to other states. We measure the spread of salaries in two ways: 1) the standard deviation of salaries, and 2) the ratio of the 90th and 10th percentile across postings in the same county-occupation. We then estimate the following regression

$$\text{spread}_{fst} = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot Colorado_f + \alpha_{fs} + \alpha_{st} + u_{it} \tag{3}$$

where spread_{fst} is a measure of wage dispersion for county f, 6-digit SOC s, at month t. We control for SOC-FIP and SOC-time fixed effects, so that we are comparing the evolution of the wage compression within the same occupation over time between states. In our preferred specification, we restrict the sample to postings from firms that were fully transparent in 2020. That way, we avoid changes in wage dispersion due to changes in the composition of firms with observable wages. Moreover, to calculate wage dispersion over a reasonable number of postings per SOC-FIPs, we aggregate postings by quarter and require each SOC-FIP cell to have at least 10 postings per quarter. Standard errors are clustered at the 6-digit SOC level.

Appendix Figure 4 plots the estimates of equation (3) over time. For both the 90/10-ratio and standard deviation measures of wage dispersion across firms within the same SOC-FIP, we find no visable change in salary compression after the enactment of the Colorado pay transparency law. We test the robustness of our result in appendix table 2, where we summarize the estimates from equation (3) by replacing the dynamic summation with a post-2021 dummy for vacancies in Colorado. To start, column (1) estimates the regression using all SOC-FIPs. In this simple case, we find a statistically significant increase in wage dispersion for both measures. However, this may simply reflect a change in composition whereby newly transparent postings come from occupations for which there is more wage variation across firms. Indeed, when we restrict the sample to firm-occupations that were already transparent in 2020, we show in column (2) that the increase in the standard deviation is no longer statistically distinguishable from 0. In column (3), we further restrict the sample to only SOC-FIPs with at least 10 postings per quarter, analogous to our

preferred specification in appendix figure 4. In this case, we find that the effect on the 90/10-ratio actually reverses sign, and is also no longer statistically significant. Lastly, in columns (4) and (5), we partition the sample by whether the SOC-FIPs had below or above 50% pay transparency in 2020. If the pay transparency law did lead to pay compression, it likely would have done so within occupations that were not very transparent before the policy. While we do find a larger decrease in the 90/10 ratio among low-transparency county-occupations, the difference is not significant. Overall, there is no evidence of a decline in wage dispersion across SOC-FIPs as a result of the Colorado pay transparency law.

5 Impact on Number of Postings

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

In this section, we test whether there was an aggregate decline in the number of postings in Colorado relative to other states. First, to understand aggregate trends, in Panel (a) of Figure 10, we plot the number of postings in Colorado vs. all other states, relative to the number of postings in December 2020. As can be seen in the figure, there are some large shifts in the number of postings throughout the year. For example, in January 2021, there are about 1.2 times as many postings as in December 2020. However, this increase in postings is nearly identical in Colorado vs. other states. In general, the trends are quite similar, and there is no evidence of an aggregate decline in the number of postings in Colorado, which could theoretically occur if firms decide to shift work to states that do not have pay transparency laws.

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

$$\log(Postings_{it}) = \sum_{k=-12}^{11} \delta_k \cdot \mathbf{1}_{t=k} \cdot Colorado_j + \psi_{j(i)} + \tau_{c(i),t} + u_{it} \tag{4}$$

Where $Postings_{it}$ is the number of postings in a firm-SOC-FIPS cell i in month t.[9] Therefore, unlike the prior analysis, the unit of analysis for this specification is the firm-SOC-FIPS cell, rather than the posting level. 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 (b) of Figure 10 plots $\hat{\delta}_k$ from estimating Equation (4). 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 number of vacancies, table 6 estimates a similar regression to Equation (4), but replaces the

[9] If there are no postings for a given firm-SOC-FIPS cell, then the number of postings is equal to zero. In other words, unlike much of the prior analysis, the panel is balanced for this specification by construction.

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 0.07 per month, which is economically small relative to the baseline of 1.69 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 6. Since occupations that experienced a larger increase in the share of postings with salary info also experienced a larger increase in posted wages (see figure 9), we might expect to find a greater decrease in vacancies among jobs with above average transparency effects. To test for heterogeneity in labor demand response by exposure to the policy, we repeat the analysis using two different partitions of the data. First, appendix figure 5 plots the the effect on number of job postings within each 2-digit occupation code, ordered by the magnitude of their first-stage transparency response. Columns (2) and (3) summarizes the occupation-specific estimates by averaging them across 2-digit occupation with above and below median increases in pay transparency, respectively. Second, columns (4) and (5) repeat the analysis by partitioning the sample between firms that were already relatively transparent in 2020 and those that had fewer than half their postings transparent. Since we found a positive effect on posted wages among already-transparent firms, we might expect to find a negative effect on the number of postings among this sample. However, in all cases, we find insignificant effects on the number of job postings, and no correlation between the vacancy response and the increase pay transparency.

One important caveat to this analysis is that avoidance behavior in remote jobs may not be captured in this design. For example, a remote job for a company located in California may specify that the work can be performed anywhere, except Colorado. We will not capture this as a reduction in the number of jobs posted in Colorado, as this will be coded as a California job in the data. There is evidence that some firms did exclude Colorado workers in the aftermath of the passage of the EPEWA. For example, an Atlantic article (Desai, 2021) found that some well-known companies such as Nike and Oracle posted job advertisements that excluded Colorado workers.[10] The data in our paper allows us to understand if companies shift work to other states, but not necessarily study whether certain remote jobs exclude Colorado workers.

6 Conclusion

This paper studies the labor market effects of a 2021 law in Colorado that requires employers to include compensation information in all job postings. Using the near universe of online job postings data from Burning Glass Technologies, we show that the policy lead 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 job in Colorado to that of other states, we find evidence that the policy

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

caused employers to post salaries for high paying jobs that they would have otherwise preferred to bargain salaries. Controlling for composition changes by comparing salaries for the same job (i.e. same firm, occupation, and location) both before and after the policy, we find that the increase in transparency is accompanied by a 3% increase in posted salaries. It is perhaps surprising that the policy increased the salaries of jobs that were already posting salaries. One explanation could be a market equilibrium response whereby employers adjust their salaries to match their competitors.

In future research, we plan to merge the job postings data with administrative employer-employee matched UI data to explore the impact of the pay transparency policy on additional outcomes and groups. In particular, we intend to leverage the variation in exposure across firms to evaluate the impact of the policy on employment. The data will also permit us to test whether the realized salaries of new hires increased. By observing the salaries of all workers, we will also be able to study the response of employers that were not initially posting salary information. Lastly, we plan to examine the effect of the policy on incumbent workers. Given that we already see an increase in posted salaries, we hypothesize that the salaries of incumbents will also rise. The spillover effect onto incumbents may occur for two reasons. First, if fairness concerns incline incumbents to demand salaries at least equal to new hires at the same firm, then increases in the salaries of entrants will affect the salaries of incumbents too. Second, information on the availability of outside options may cause incumbents to bargain for higher salaries (Caldwell and Harmon, 2019).

While a growing literature has explored the effects of within-firm pay transparency on wages, far less is known about the effects of pay transparency across firms. The results thus far support the intended policy effect of raising workers' salaries, but there are remaining areas for future research. Aside from our plan to explore in more detail the overall employment and income responses, policymakers are also interested in the effect of pay transparency on inequality, gender pay gaps, and racial pay disparities. It would also be interesting to link the empirical findings to theories of how employers choose between wage posting and wage bargaining protocols.

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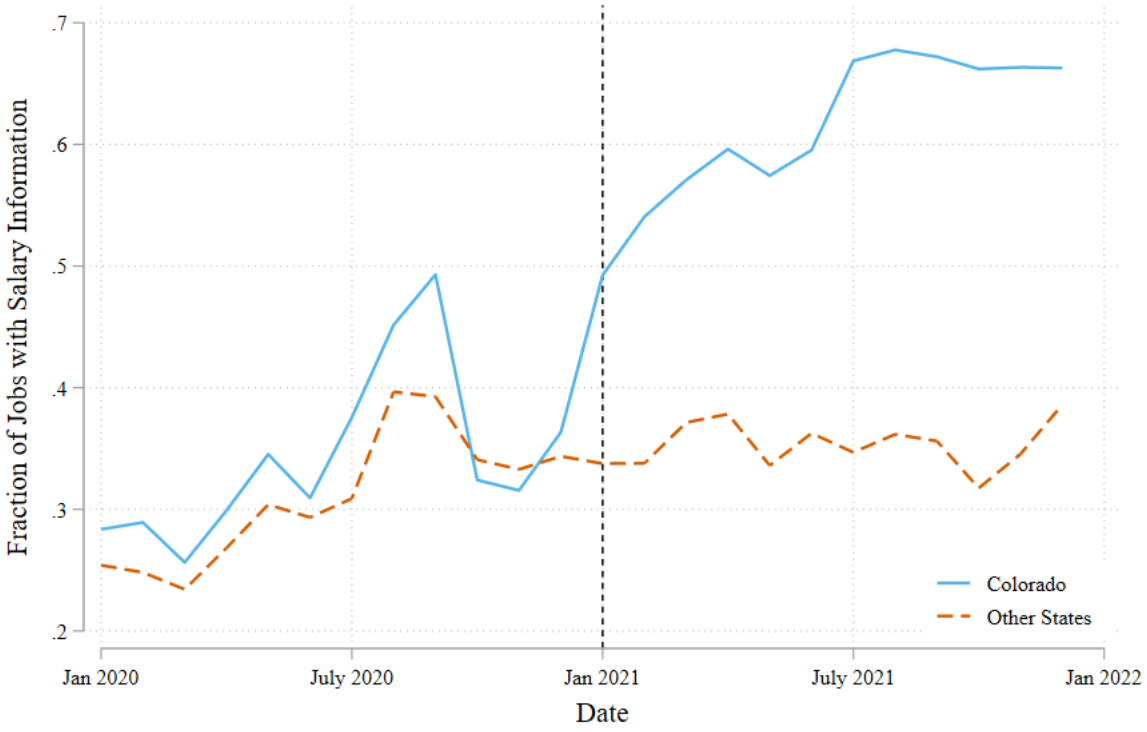
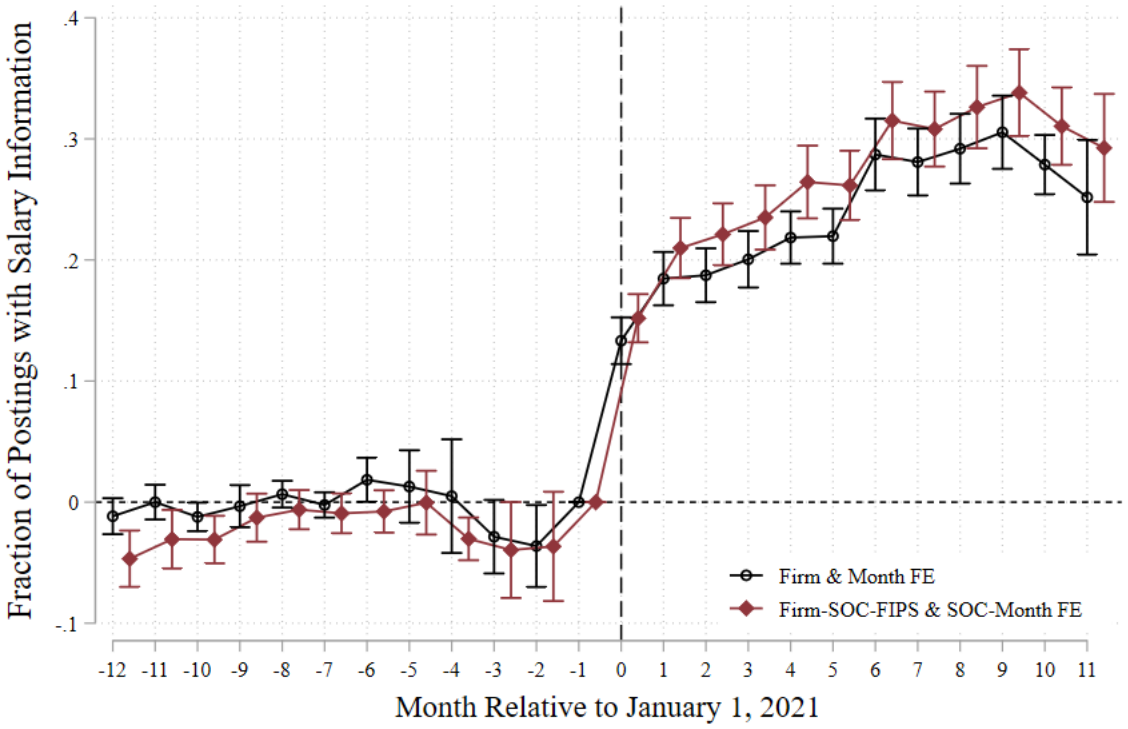
Figure 1: Fraction of Postings with Salary Information by State



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

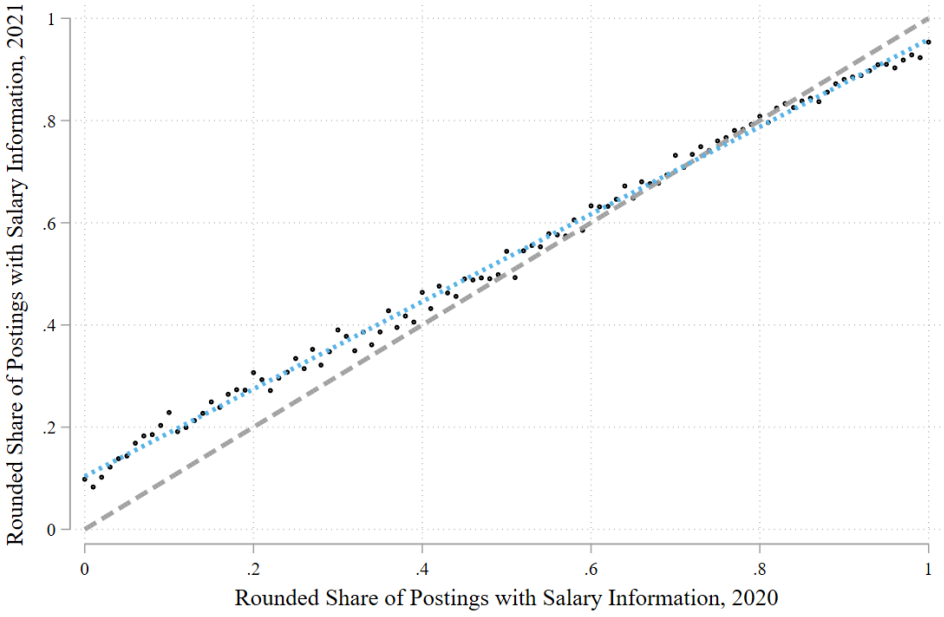
Figure 2: Impact of Pay Transparency Law on Fractions of Postings with Salary Information



Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information. The blue hollow circles include specifications that control for firm fixed effects and month fixed effects. The red 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: Share of Postings with Salary Information 2021 vs 2020, by Employer

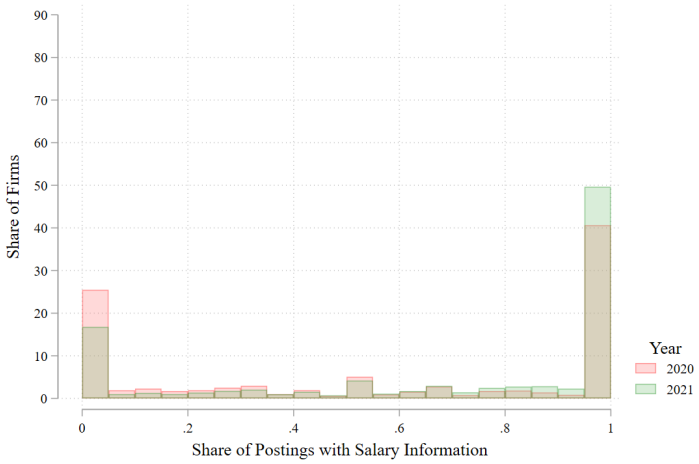
(a) Colorado



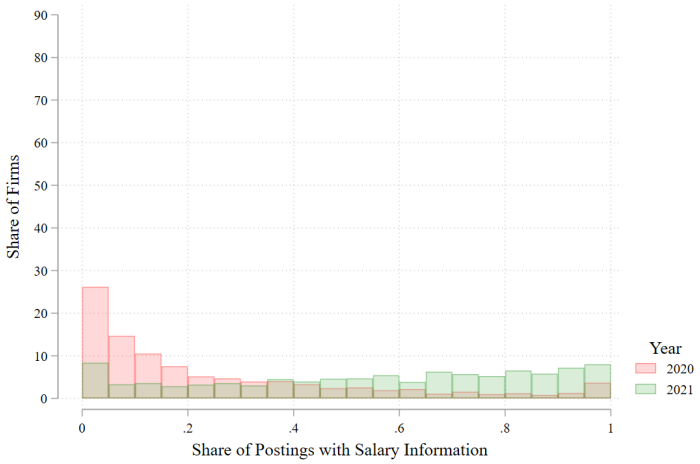
(b) Other States

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.

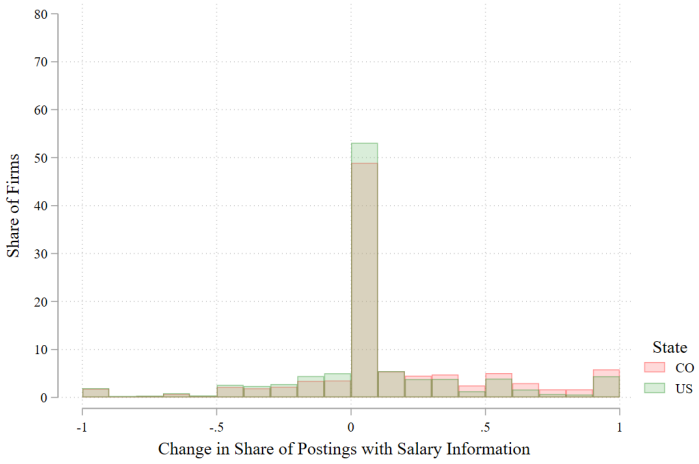
Figure 4: Distribution Postings with Salary Information, 2020 vs 2021



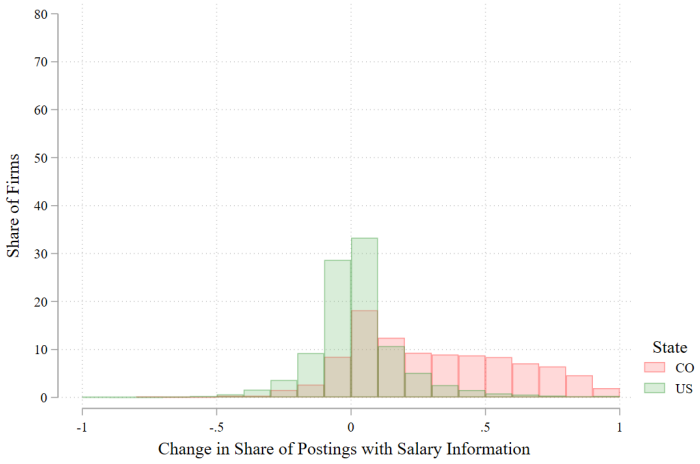
(a) Colorado, 10-100 Postings



(b) Colorado, >100 Postings



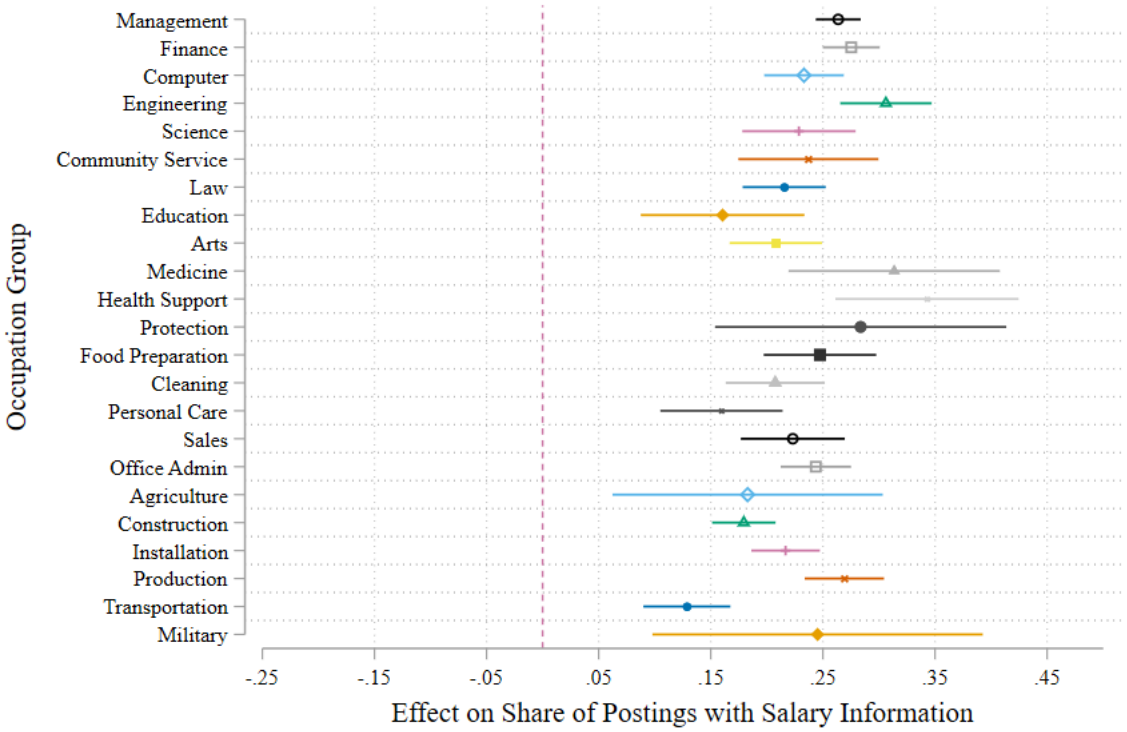
(c) Colorado vs. U.S., 10-100 Postings



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

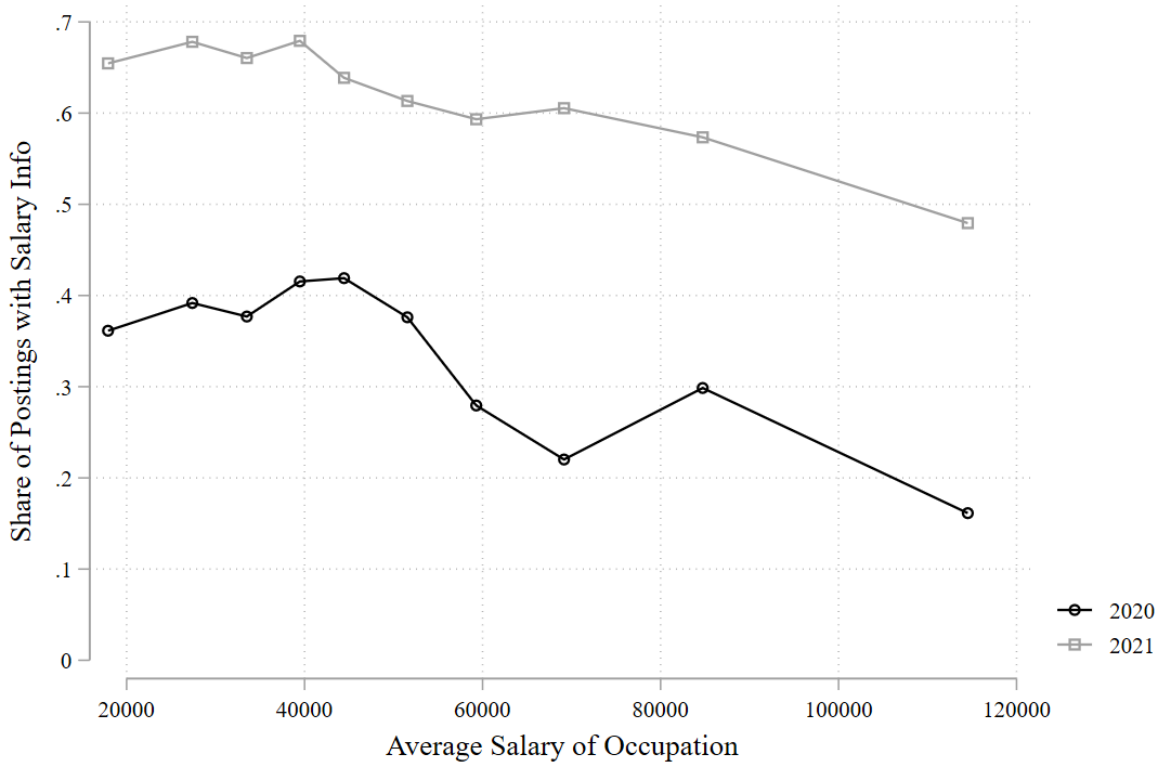
Note: Panel (a) plots the distribution of employers in Colorado by the share of their postings with salary information, separately for 2020 and 2021. The sample is restricted to employers that post jobs in Colorado in both years, with between 10-100 postings in 2020. Panel (b) plots an analogous figure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

Figure 5: Impact of Pay Transparency Law on Fractions of Postings with Salary Information, by Occupation



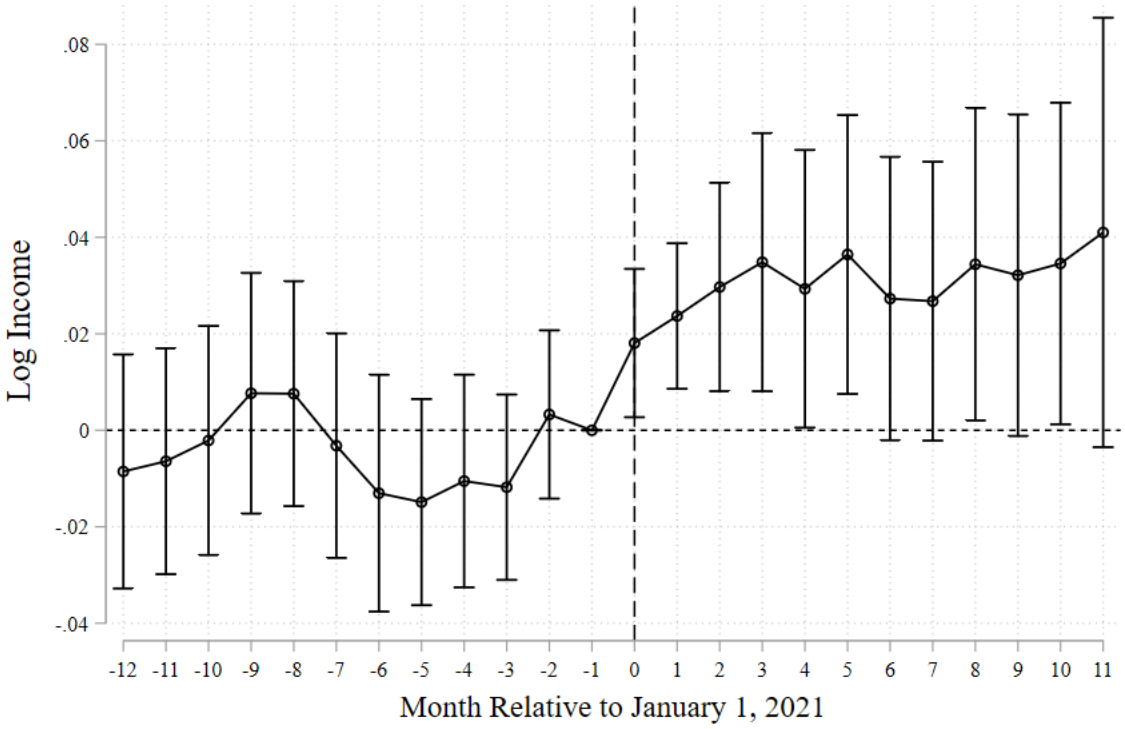
Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for each 2-digit SOC occupation code, following the specification in Equation 1. 95 percent confidence intervals clustered at the firm level are displayed.

Figure 6: Transparency by Wage of Occupation, within Colorado



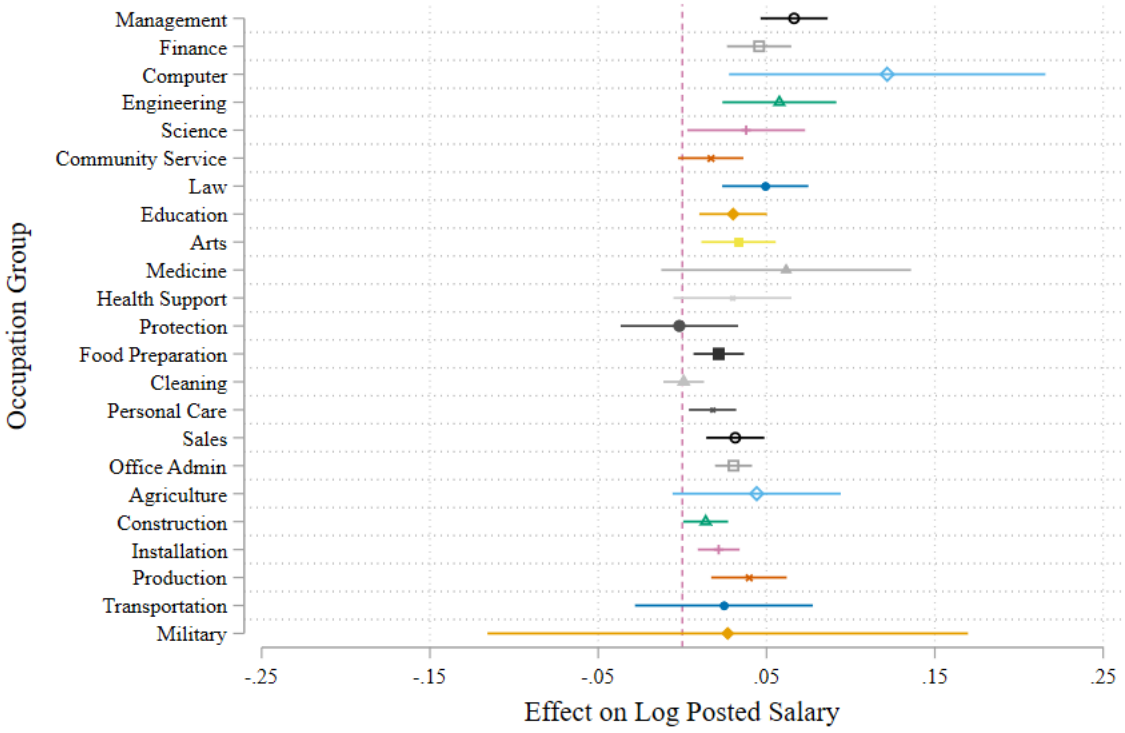
Note: The figure plots the average probability that a job posting has salary info, as a function of the average salary of the posting's 5-digit SOC code computed from the 2015-2020 ACS. The postings are aggregated over deciles of average salary across occupations.

Figure 7: 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 8: Impact of Pay Transparency Law on Log Posted Salary, by Occupation



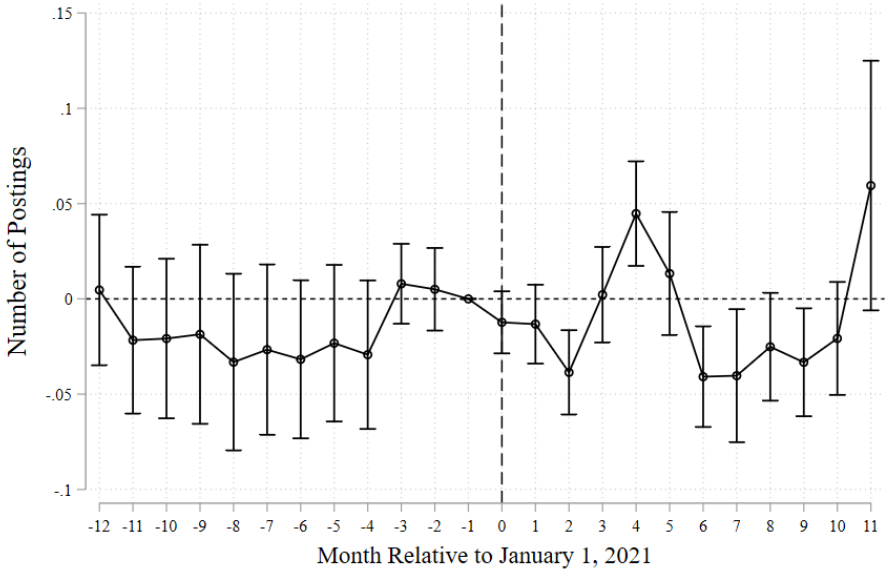
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary, separately for each 2-digit SOC occupation code. 95 percent confidence intervals clustered at the firm level are displayed.

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.

Figure 10: Impact of Pay Transparency Law on Number of Job Postings

(a) State Trends



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

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

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

	Colorado	Other States
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
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 Burning Glass Technologies dataset with non-missing location, employer, and occupation information.

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

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

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

Table 3: Effect of Transparency Law on Posted Wages

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

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

Table 4: Effect of Transparency Law on the Range of Posted Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post · Colorado	.035	.044	.037	.044	-.002	-.001	-.008
	(.008)	(.009)	(.005)	(.005)	(.006)	(.006)	(.004)
Outcome	Log(Max)	Log(Max)	Log(Min)	Log(Min)	Log(Max/Min)	Log(Max/Min)	Log(Max/Min)
Sample	All	Above MW	All	Above MW	All	Above MW	All
Firm FE							X
Time FE							X
Firm-SOC-FIPS FE	X	X	X	X	X	X	
SOC-Time FE	X	X	X	X	X	X	
N	14,465,056	8,611,123	14,465,056	8,611,123	14,465,056	8,611,123	19,901,376

Note: This table displays difference-in-difference estimates that compare postings in Colorado to other US states, before and after 2021. The odd columns use the full data sample and the even columns restrict the sample to only firm-SOC-FIPS with an average wage above $14/hr in 2020. Standard errors are clustered at the firm level.

Table 5: 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.

Table 6: Effect of Transparency Law on Number of Job Postings

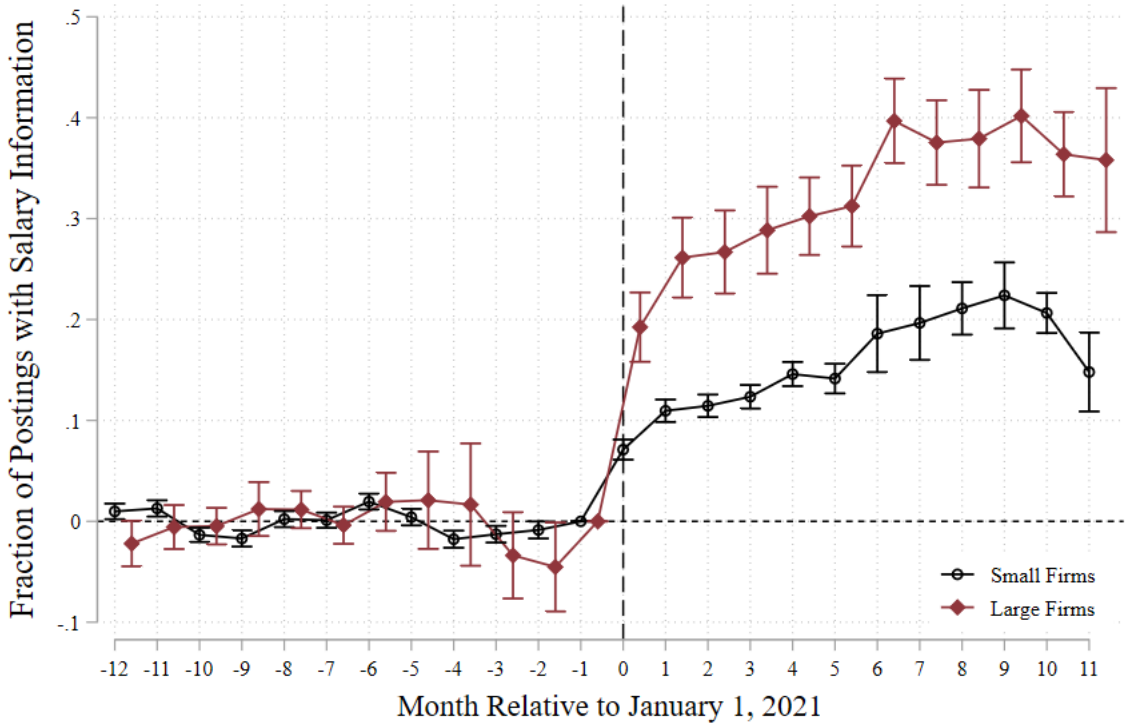
	(1)	(2)	(3)	(4)	(5)
Post · *Colorado*	-.004	-.004	-.004	.005	-.009
	(.034)	(.049)	(.018)	(.012)	(.055)
Sample	All	Above Median	Below Median	High Trans.	Low Trans.
Firm-SOC-FIPS FE	X	X	X	X	X
SOC-Time FE	X	X	X	X	X
Avg. postings in 2020	1.691	1.743	1.583	1.344	1.947
N	21,811,166	14,929,637	6,881,529	9,311,615	12,498,404

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

Appendix: For Online Publication

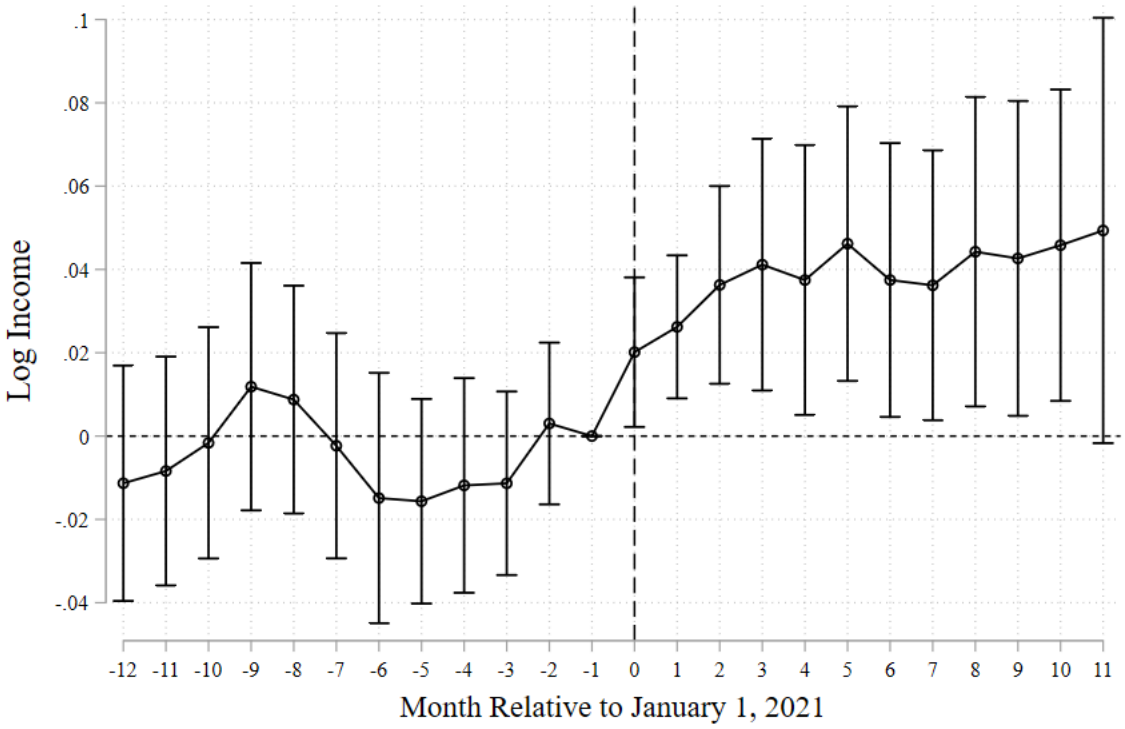
Appendix A. Additional figures and tables

Appendix Figure 1: Impact of Pay Transparency Law on Fractions of Postings with Salary Information, by Size

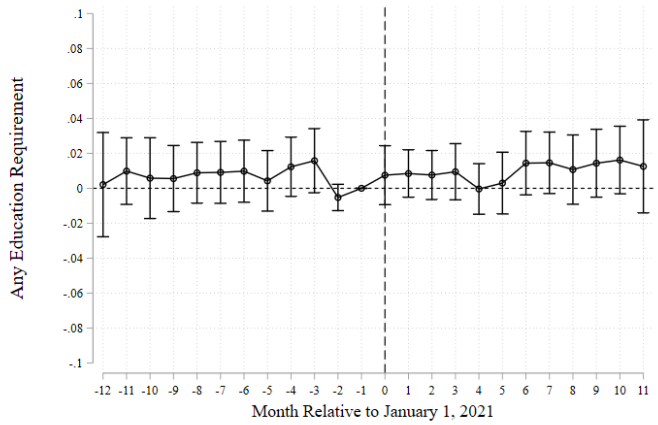


Note: This figure estimates the impact of the pay transparency law in Colorado on the fraction of job postings that contain salary information, separately for firm-states with fewer than 100 posting in 2020 and firm-states with more than 100 postings in 2020.

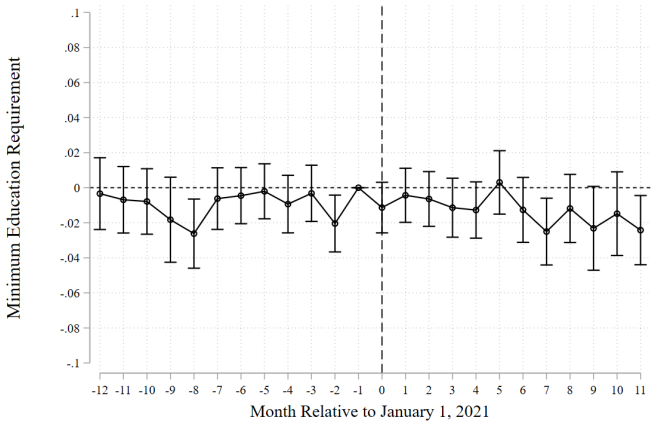
Appendix Figure 2: Impact of Pay Transparency Law on Log Posted Salary for Jobs with Wage ≥ $14/hr in 2020



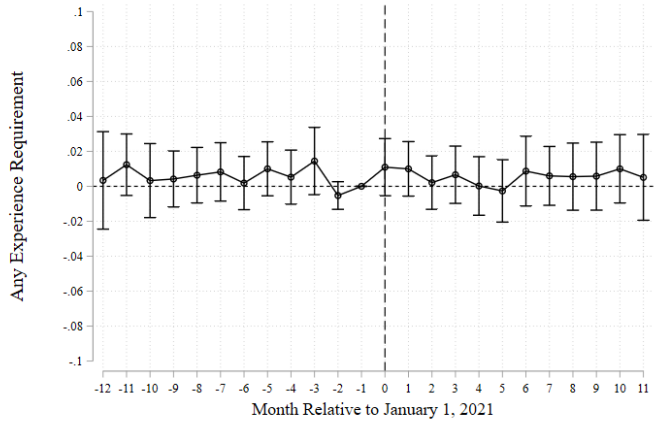
Note: This figure estimates the impact of the pay transparency law in Colorado on the logarithm of the expected salary following the specification in Equation (2). The sample is restricted to firm-SOC-FIPS with an average wage of at least $14/hr in 2020. This specification controls for firm-SOC-FIPs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



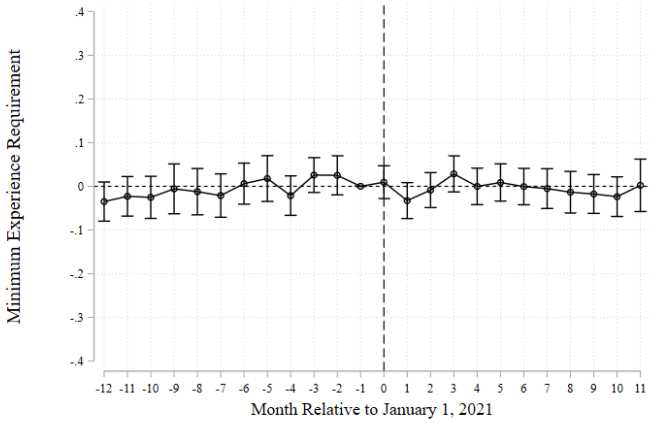
(a) Any Edu Requirement



(b) Min Edu Requirement



(c) Any Exp Requirement

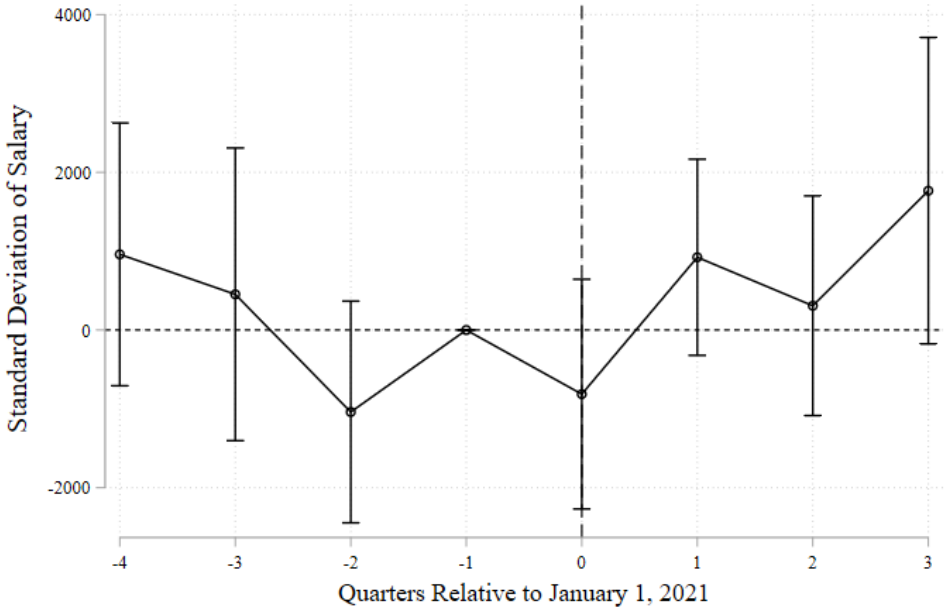


(d) Min Exp Requirement

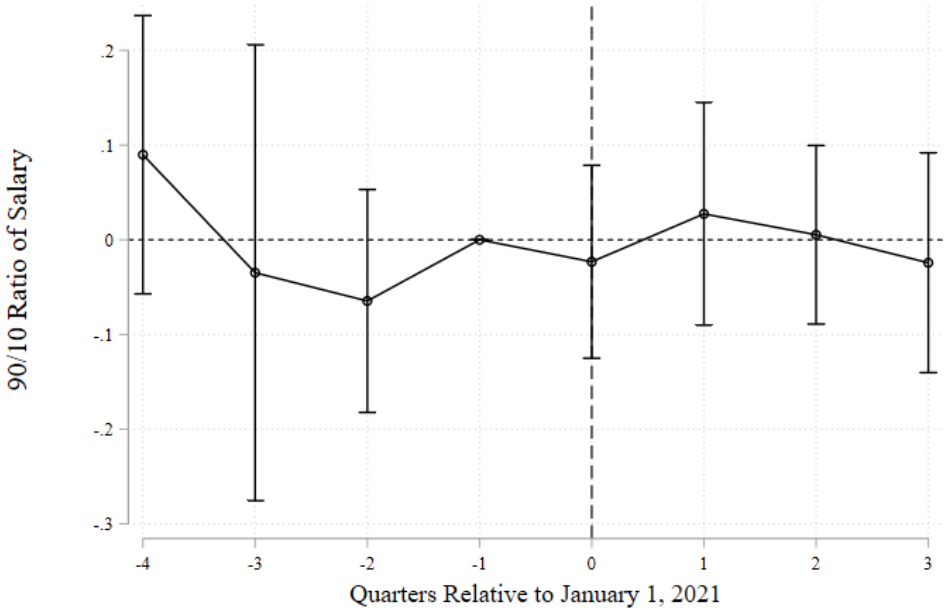
Appendix Figure 3: Effect of Pay Transparency Law on Education and Experience Requirements

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.

Appendix Figure 4: Impact of Pay Transparency Law on Dispersion of Posted Salaries within County-Occupation



(a) Standard Deviation



(b) 90/10 Ratio

Note: This figure plots estimates of Equation (3) using the sample of FIPS-SOC with at least 10 job postings each month, and restricted to firms that had salary info for all job postings in 2020. The outcome variable in panels (a) and (b) are the standard deviation of salaries within FIPS-SOC and the ratio of the 90th and 10th salary percentile, respectively.

Appendix Figure 5: Impact of Pay Transparency Law on Number of Job Postings vs. Impact on Share of Postings with Salary Info, by Occupation

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

Appendix Table 1: Number of Observations

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

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

Appendix Table 2: Effect of Transparency Law on Salary Compression

	(1)	(2)	(3)	(4)	(5)
Standard Deviation	1026.043	574.863	452.029	433.656	449.131
	(301.805)	(404.608)	(563.244)	(721.726)	(772.685)
90/10 ratio	.096	.081	-.061	-.072	-.042
	(.025)	(.023)	(.041)	(.049)	(.051)
Sample					
Baseline SD	17080	17261	22794	24540	18558
Baseline 90/10	1.697	1.638	2.602	2.769	2.198
Transparent 2020		X	X	X	X
At least 10 postings			X	X	X
Below 50% Trans.				X	
Above 50% Trans.					X
N	1,772,439	993,028	134,563	94,825	39,206

Note: This table displays difference-in-difference estimates that compare the level of wage compression within the same occupation, state, and quarter 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 state-firm-occupations that were already transparent in 2020. Column (3) further restricts the sample to only fip-soc-quarter with at least 10 postings. Column (4) keeps only state-soc-fips with average share transparent in 2020 $< 50\%$ based on column (3). Column (5) keeps only state-soc-fips with average share transparent in 2020 $\geq 50\%$ based on column (3).