Combating Inequality with Transparency? Evidence from Colorado

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Abstract

Does giving workers more information on wages reduce wage differentials? We leverage a recent law passed in Colorado requiring employers to list the expected wages in job listings to study this question. Using a synthetic control approach, we find no causal evidence of a reduction in the gender gap in Colorado. On the contrary, our analysis suggests that the gender pay gap between men and women *increased* in Colorado after passage of the law, although this finding is not significant at the 5% level. We conduct additional correlational analysis on potential mechanisms through which the law could have impacted the gender pay gap (directed search, bargaining, and signal informativeness). Overall, our correlational evidence is consistent with male workers taking advantage of the increased transparency in postings more than female workers, which could have adversely affected the policy's capacity to reduce the gender gap. Our paper highlights the importance of ensuring information policy is properly targeted to its intended beneficiaries.

JEL Codes: D83, J16, J31, J38, J63, K31

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1 Introduction

In the last few years, laws in several areas of the United States have been passed that require employers to give an estimated pay range when posting job ads. Similar to previous laws requiring greater transparency in some aspect of worker wages in other parts of the world, the motivation for the first state-wide law targeting salary information in postings, Colorado's Equal Pay for Equal Work Act ("The Colorado Law"), was to reduce gender pay gaps (Colorado Department of Labor & Employment, 2024a). Proponents passing similar legislation in other states and locales in the years since have cited similar concerns over fairness and pay gaps. The states that have passed transparency in posting laws include: Colorado (1 January 2021), California (1 January 2023), Washington (1 January 2023), New York State (17 September 2023), Hawaii (1 January 2024), and Illinois (1 January 2025). Four other states have instead passed laws requiring pay transparency upon request or at some point in the hiring process: Maryland (1 October 2020), Connecticut (1 October 2021), Nevada (1 October 2021), and Rhode Island (1 January 2023). Additionally, other, more local, transparency laws came into effect in areas of New Jersey, New York, and Ohio in 2022, including New York City (effective 1 November 2022).[1] 13 other states (and DC) have proposed similar bills since 2020. Finally, national versions of these bills were also proposed in Congress in March 2023: The Salary Transparency Act and Pay Equity for All Act (Marfice, 2024).

The Colorado law, specifically, requires all employers to include salary information in all job listings.[2] The requirement for all employers to post expected

[1]The New Jersey and New York laws require transparency in posting, while the ones in Ohio require transparency upon request when extending a job offer.

[2]The Colorado Department of Labor lists only four exceptions: 1) The listing is part of a non-competitive promotion, 2) The listing is for acting, interim, or temporary work, 3) The opening is a confidential replacement of a current employee and 4) The opening is for fully-remote out-of-state positions for employers based in Colorado who both have no physical site and have less than fifteen employees (Colorado Department of Labor & Employment, 2024b).

salary information for all jobs is a very significant change from the status quo on modern job boards. For example, in their first-quarter 2011 sample from Chicago and Washington CareerBuilder.com, Marinescu and Wolthoff (2020) estimate that only 20% of jobs contain salary information. Similarly, Banfi and Villena-Roldán (2019) find that only 13.3% of job ads on their Chilean job board (www.trabajando.com) contain salary information.

Due to the recency of these efforts, there has not been much formal analysis of the effects of these laws. The notable exception is Arnold, Quach and Taska (2022), which focuses on studying the effects of the initial Colorado law on job postings. Its authors find that the law increased posted salaries by 3.6 percent and the fraction of employers listing salaries by 30 percent. However, they also note that since their data source, Burning Glass Technologies, only includes information on job postings, they are unable to study other outcomes such as changes in realized worker salaries.

This paper aims to contribute to this literature by answering two questions:

1. Have the recent pay transparency laws been effective in their goal of reducing (gender) differentials in worker pay?
2. If the laws did affect pay gaps, through which mechanisms did they do so?

We only focus on answering these questions for Colorado in the main body of our paper since it has the most available data and because doing so eases the task of constructing a good control group for our treatment. However, we also try a different method to measure the aggregate average effect of posting wage transparency laws in all treated states, which can be found in appendix B. Overall, we do not find a significant effect of this type of legislation on the gender pay gap of newly hired workers using either method.

The rest of this paper is organized as follows. Section 2 gives an overview of the literature. Section 3 discusses our data. Section 4 discusses our empirical method and evaluates the plausibility of its main assumptions. Section 5 gives our main results. Section 6 discusses mechanisms through which the law could have affected our results and gives suggestive evidence related to these mechanisms. Section 7 concludes.

2 Literature Review

We contribute to a very active literature analyzing the effects of legislation designed to increase transparency in worker wages. We highlight some notable recent work from this literature in this section. An interesting facet of the papers cited is that they each focus on different types of pay transparency. That is, there have been many attempts to enact policies that make pay more transparent in the workplace, and these efforts have targeted different segments of the labor market over time.

Böheim and Gust (2021) and Gulyas, Seitz and Sinha (2023) each use different research designs to study a policy requiring firms to produce internal reports on the gender gap in their workers' wages, which became effective in Austria in 2011. Both papers find that the policy did not reduce the gender wage gap or affect worker wages. The policy of interest was a law that required employers to provide employees with reports on the annual income of current employees by gender and occupation group, but did not require making this information available to the public (Gulyas et al., 2023).

Cullen and Pakzad-Hurson propose a wage transparency and bargaining model in their 2022 paper. As part of this paper, they find that laws that prevent employers from punishing workers for sharing wage information lead to 2% lower wages overall in US states that enacted these pay transparency laws (Cullen and Pakzad-Hurson, 2023).

Mas (2017) studies the effect of a 2010 mandate in Calfornia which disclosed the salaries of municipal workers to the public online. The author finds that this led to a 7% decrease in compensation and a 75% increase in quit rates among top management (Mas, 2017). Similarly, Perez-Truglia (2020) studies the effects of Norwegian tax records becoming accessible online in 2001. This change made looking up any other citizen's income (already possible through an in-person request process) very accessible, thereby effectively disclosing all salaries to interested individuals in Norway. The author finds that this change reduced measures of happiness and life satisfaction between the rich and poor by 29% and 21%, respectively. Finally, in Baker, Halberstam, Kroft, Mas and Messacar (2023), the authors study laws in Canada that allowed the public access to the salaries of individual faculty working at public Canadian universities. They find strong evidence that the laws reduced the gender gap in impacted faculty salaries by about 20 to 40 percentage points.

We also contribute to a large literature in economics analyzing the magnitude and causes of differentials in worker pay. For a recent literature review, see Blau and Kahn (2017).

3 Data Sources and Summary Statistics

Our data on worker outcomes come from the Longitudinal Employer-Household Dynamics (LEHD) database published by the US Census Bureau. Specifically, we use the Quarterly Workforce Indicators (QWI) to measure earnings, employment, and hires by state, sex, and industry. The QWI are constructed from several administrative data sources and cover over 95% of all private sector jobs in the United States (US Census Bureau, 2022) — earnings data comes from unemployment insurance earnings data, while employer industry and location information comes from the Quarterly Census of Employment and Wages (QCEW). Worker data comes from a number of administrative datasets.

Individuals may be counted more than once if they hold multiple jobs in a given quarter. Unlike statistics tabulated from firm or person-level data, the QWI source data are unique job-level data that link workers to their employers. Because of this link, labor market data in the QWI is available by worker age, sex, educational attainment, and race/ethnicity.

We use these data to examine earnings and employment for all workers that are working in a given quarter, along with earnings and employment for "new" workers, that is, workers that are working for an employer in a given quarter and were not working for that employer in the previous four quarters. Worker earnings are computed in the QWI by summing quarterly earnings and dividing by the number of workers. Our data spans the period from the second quarter of 2011 to the final quarter of 2023. That is, from roughly ten years before the implementation of the Colorado Law up until the most recent quarter for which we have data for all states in our sample.[3]

Key summary statistics for our final sample are given in the table below. We end up with 41 states from which we can construct a synthetic control (see next section) after dropping all states which are missing observations or which receive at least partial treatment over the sample period.[4]

[3]There are only six states which have some QWI data for the first quarter of 2024. Our synthetic control method requires the panel data to be strongly balanced, so we cannot use these observations. Similarly, we exclude the first quarter of 2011, since doing so would require us to drop a state missing observations for only that quarter (Massachusetts) completely from our sample.

[4]We currently have no method by which to study states partially treated with local laws. For states other than Colorado which received full treatment, we do try to estimate an average effect of treatment. This analysis can be found in appendix B. The states dropped due to being at least partially treated during our sample period include California, New Jersey, New York, Ohio, and Washington. Additionally, Alaska, Michigan, and North Carolina were dropped because they were missing several periods of data.

Variable Description	Colorado	Donor Pool
Panel A: Pre-treatment		
Average Male Earnings	3,527.36	3,108.91
	(405.51)	(617.42)
Average Female Earnings	2,462.28	2,085.80
	(321.45)	(479.55)
Average Earnings Gap	1,065.08	1,023.11
	(111.13)	(288.80)
Panel B: Post-treatment		
Average Male Earnings	5,119.58	4,109.68
	(337.58)	(698.16)
Average Female Earnings	3,703.33	2,966.74
	(280.56)	(591.09)
Average Earnings Gap	1,416.25	1,142.95
	(108.95)	(246.07)
Pre-treatment Observations	39	1,599
Post-treatment Observations	12	492
Number of States	1	41
Sample Period	2011 Q2 - 2023 Q4	

Table 1: Summary statistics. Donor pool numbers are for all states that could contribute to the synthetic control, whether or not they actually received positive weights in the synthetic control's construction. Earnings are average monthly earnings of newly hired workers in a quarter using QWI data.

We also use data on job postings from Lightcast to measure patterns in employer posting behavior. Lightcast constructs these data using their extensive scrapes of over 65,000 online sources (Lightcast, 2024). Examining employer posting behavior is useful for getting suggestive evidence on the underlying mechanisms which could explain our main sythetic control result (see section 6).

Finally, we use datasets from the American Community Survey (ACS), Bureau of Economic Analysis (BEA), Local Area Unemployment Statistics (LAUS), and the *New York Times* (NYT) to get covariates with which to construct our synthetic control.[5] The specific measures we use from these

[5]The New York Times data is available publicly at https://github.com/nytimes/covid-19-data. The other three datasets are also public and can located easily at the appropriate government websites.

datasets are discussed in section 4.2.1.

4 Empirical Strategy

4.1 Synthetic Control Estimation Method Description

We assess the efficacy of the Equal Pay for Equal Work Act by comparing the change in the gender gap in new hire earnings in Colorado with a synthetic control constructed from the gender gaps of other states.[6] The method of synthetic control was popularized in economics by Abadie, Diamond and Hainmueller (2010). It relies on comparing the treated group with a control created by weighting an average of untreated observations from a "donor pool." The classic form of the method seeks to find a combination of weights W that minimize the distance

$$\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)} \tag{1}$$

where X_1 is a matrix of observed characteristics and pre-treatment outcome variables for the treated group and where X_0 is the same for the observations in the donor pool. V is a symmetric, positive semidefinite matrix which lets different variables receive different weights depending on their power to predict the outcome variable (Abadie, Diamond and Hainmueller, 2011). We use the default setting for V in our chosen statistical package, where it attempts to minimize the mean square prediction error in the outcome variable in the pre-treatment period (Abadie et al., 2011; Wiltshire, 2022).[7] Specifically, we make use of the user-written *allsynth* package in Stata to implement this method

[6] By "new hire," we mean the hiring of workers who had not previously worked at the firm hiring them. That is, we exclude recall hires, which we expect to be less impacted by changes in formal job postings.

[7] For more detail on the theory behind the classical synthetic control method, see Abadie et al. (2011) and Abadie et al. (2010).

(Wiltshire, 2022). This package allows us to implement the synthetic control method and placebo tests discussed in Abadie et al. (2010).

Finally, we note that recent work in the synthetic control literature has proposed adjustments to the basic synthetic control method to overcome cases where the basic method is unable to build a synthetic control that matches the characteristics of the treated unit well (Abadie and L'Hour, 2021; Ben-Michael et al., 2021). While the package we use offers the functionality to implement these adjustments (Wiltshire, 2022), we do not find that it makes a meaningful difference to our results and thus only present results using the classic method previously described.

4.2 Identifying Assumptions: Synthetic Control

In this section, we discuss the identifying assumptions needed for the causal interpretation of our synthetic control results.

4.2.1 Similarity of Donor Pool to Treated Group

The key assumption of the synthetic control method is that the weighted synthetic control forms a good approximation for what the treated group would have looked like had it not undergone treatment. To assess the plausibility of this assumption, we show the similarity between Colorado, the treatment group, and a synthetic control in both outcome variables and relevant predictive covariates.

For all of our specifications, we construct the synthetic control using a rich set of covariates gathered from various datasets.[8] These are:

1. Average demographic characteristics of a state's population (race, sex, education, marital status) from ACS data.

[8]For the data described in this section, all measures from the ACS and BEA are matched using the year of the current quarter. The NYT measures are from the first month of each quarter, and the LAUS data (average quarterly unemployment rate) averages the three LAUS monthly rates over the current quarter for each state.

2. Economic characteristics of a state (labor force size, real personal income per capita, average quarterly unemployment rate) from ACS, BEA, and LAUS data.

3. A states' cumulative number of cases and deaths due to COVID-19 from NYT data.

The graph below compares our treatment and synthetic control groups in the outcome variable using data from all periods before treatment. The vertical dashed line marks the first quarter of 2021, the quarter when the law became effective.

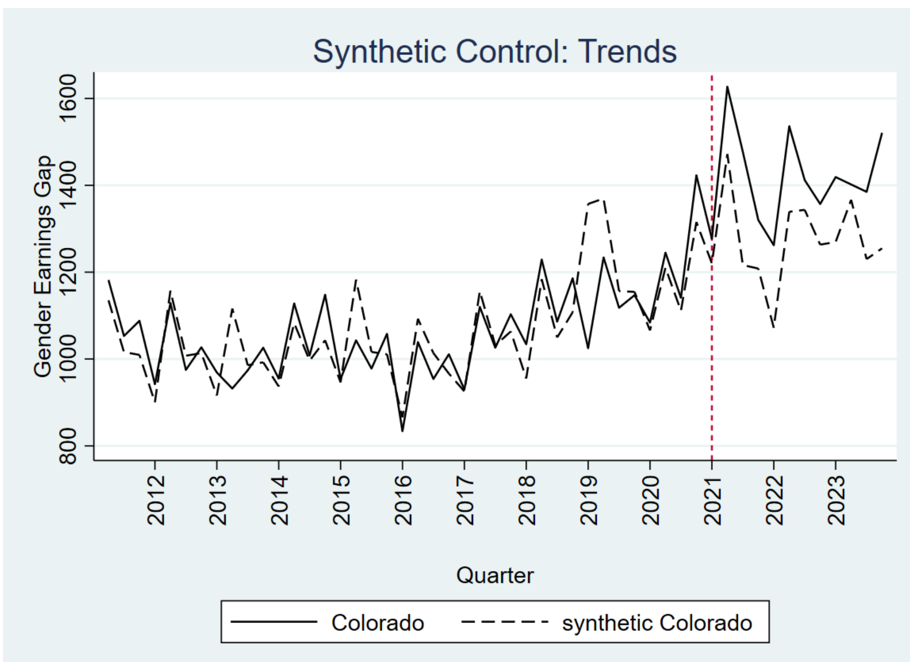

Figure 1: Synthetic control trends. The graphed synthetic control was constructed using data all periods before treatment. The dashed vertical line indicates the first quarter of 2021 when the Colorado law became effective.

Table 2 below displays the balance of predictors for Colorado and the synthetic control. Overall, the synthetic control appears to match Colorado very closely in most predictor values.

Predictor	Colorado	Synthetic Colorado
Gender Earnings Gap	1,065.08	1,067.09
Real Personal Income Per Capita	52,119.44	51,761.55
Percent White	82.89	77.83
Percent Black	4.10	7.84
Percent Married	40.83	39.93
Percent Male	50.32	49.69
Percent with High School Only	23.90	25.61
Percent with Four Years of College Only	18.13	16.19
Percent with Postgraduate Education	10.20	9.16
Percent in Labor Force	68.02	68.86
Average Quarterly Unemployment Rate	4.94	4.54
COVID-19 Cases Per 100,000	1.61	1.85
COVID-19 Deaths Per 100,000	.02	.02

Table 2: Predictor balance. ACS measures (demographics, education) include general population of all ages.

4.2.2 Other Assumptions

Other important assumptions for the interpretability of the synthetic control method include the "no anticipation" assumption and the "Stable Unit Treatment Value Assumption" (SUTVA). The first indicates that the treated group does not act differently in anticipation of treatment. Prior work by Arnold et al. (2022) found that there was no indication of anticipation from employers in the fraction of postings with salary information.[9]

The second assumption requires that the potential outcomes of any particular unit are not affected by the treatment status of any other unit. This could be violated if, for example, the passage of this law in Colorado prompted employers or workers to enter or leave the state in a way that impacted the relative gender gap in other states. Since past work by Arnold et al. (2022) did not find a

[9]We are currently working on developing ways to validate this assumption for worker behavior. However, since there is no evidence that employers changed their behavior prior to being required to make a change, we note that workers considering applying for jobs would only make a change if both cognizant of the law and willing to wait to apply for or accept currently available jobs until the law became effective.

significant change in postings in Colorado after the law, we do not expect this assumption to violated (as there is no evidence employers moved postings out of Colorado to avoid complying with the law).[10]

5 Results

5.1 Synthetic Control Composition

The states in the donor pool which received positive weights in the synthetic control are listed in the table below, along with their weights.

State	Weight
Minnesota	.524
Utah	.264
District of Columbia	.089
Vermont	.061
Hawaii	.053
North Dakota	.005
Oregon	.004

Table 3: Synthetic control: Composition from donor pool states.

5.2 Estimated Effect

Figure 1 shows that our synthetic control has a similar trend to Colorado over the treatment period, but at a lower level. That is, we do not find evidence of a reduction in the gender earnings gap. On the contrary, the point estimate for the gender earnings gap in Colorado is higher than that of the synthetic control for all treated periods. The table below displays value of the gap between these point estimates, along with the p value from the placebo tests (discussed in the next section).

[10] We are currently working with Lightcast and J2J flow data (another subset of the LEHD data) to further validate the plausibility of this assumption for the next draft of this paper.

Quarter	Gap	p
2021 Q1	56.57	.6905
2021 Q2	155.24	.4524
2021 Q3	260.68	.1667
2021 Q4	111.63	.1667
2022 Q1	189.90	.1667
2022 Q2	196.94	.1190
2022 Q3	68.48	.1429
2022 Q4	93.61	.1667
2023 Q1	149.50	.1667
2023 Q2	35.47	.1905
2023 Q3	154.90	.2143
2023 Q4	266.11	.1905

Table 4: Synthetic control: Main results. "Gap" refers to the difference in the outcome variable (the gender earnings gap) between Colorado and the synthetic control.

In Section 6, we identify suggestive patterns indicating that female earnings and the proportion of newly hired employees who are women were negatively correlated with increased salary visibility at the industry level. While not establishing causality, this pattern suggests that men may have benefited more from the policy than women, which runs counter to the policy's goal of greater pay equality.

5.3 Placebo Tests

Inference for synthetic control is typically conducted with in-place placebo tests using units in the donor pool. Essentially, the idea is to see whether the synthetic control procedure would produce the observed effect even if the state considered "treated" were randomly assigned. These placebo tests are shown graphically in the figure below.

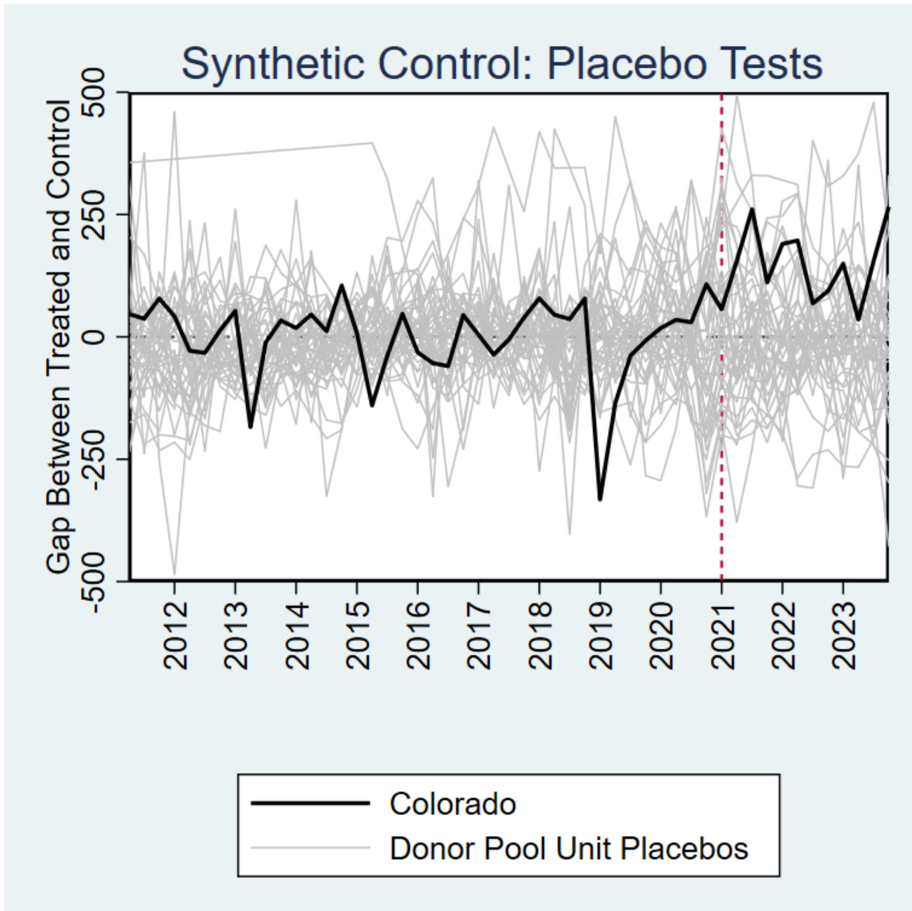

Figure 2: Placebo tests for synthetic control. The vertical axis refers to the difference between the value of the gender earnings gap in the "treated" state (either Colorado or an untreated state from the donor pool) and the gender earnings gap in its synthetic control.

We can see visually that the gap between Colorado and its synthetic control is higher than the majority of gaps using placebo states. However, as shown in table 4, the p values are too high to indicate a statistically significant difference at the 5% level.

5.4 Hours and Earnings

Here we would also like to note that our measure of worker pay from the QWI is average monthly earnings, not hourly wages. That is, we are unable to directly measure how labor supply is potentially changing for the individuals used to generate our primary outcome variable. If, for example, the law narrowed the gap in worker hourly pay, but women simultaneously decided to work less on average, our outcome variable could show no change in the pay gap.[11]

To account for this limitation of the data, we use the CPS to measure the annual average weekly hours worked by gender. Figure 3 below graphs the difference in average weekly hours between men and women for Colorado and states which contribute to the synthetic control (see table 3). We do not observe an abnormally large shift in working hours for Colorado during the treatment period, and the gaps in working hours generally seem fairly stable over time.

[11] We still believe the QWI is a better source for our purposes than other datasets such as the CPS, because it includes the near-universe of worker earnings and especially because it is able to distinguish between the earnings of newly hired workers and all workers.

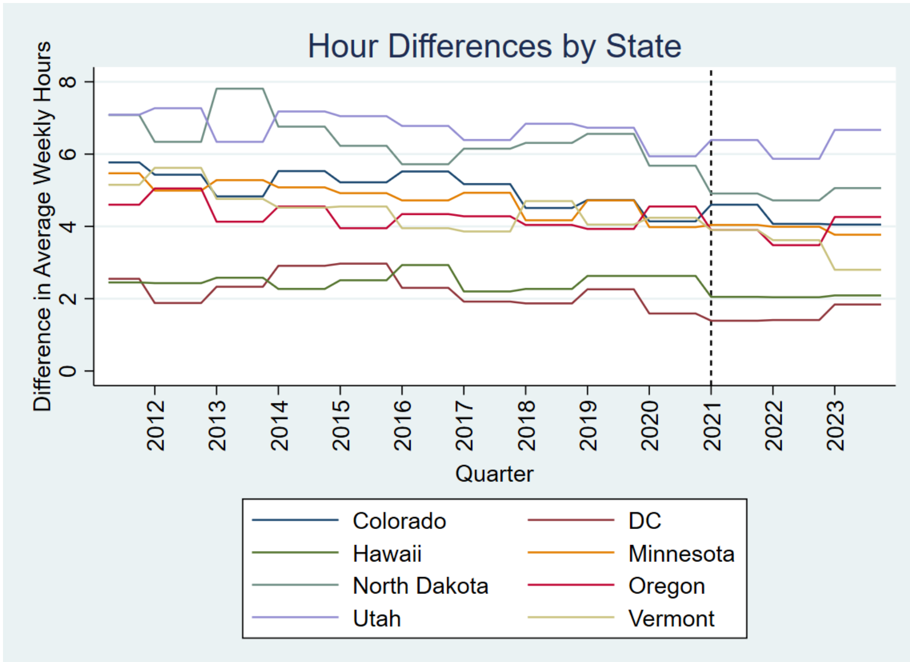

Figure 3: Trends in the difference in weekly hours worked for women and men. Y axis is each state's difference in average weekly hours reported by male and female workers (average male hours - average female hours). Displayed states include Colorado and states which contributed to the synthetic control used for the main analysis.

Additionally, we perform a synthetic control analysis using average weekly hours from the CPS to infer average hourly wages from the average monthly earnings data. The details of this analysis are available in appendix A. Overall, the results of this analysis are very similar to the results of our main analysis, so we do not find any evidence that our main results are driven by changes in working hours.

6 Mechanisms

In this section, we discuss reasons why mandating pay transparency might or might not affect the gender wage gap according to economic theory and past findings in the literature.

6.1 Directed Search

One way that greater transparency in postings could reduce the gender pay gap would be by inducing more directed search behavior in female workers. That is, if employers post informative wage ranges, female workers in lower-paying occupations or sectors become better suited to direct their searches to higher-paying jobs. Further, as the supply of workers to the higher-paying jobs increases and the supply of workers remaining in or applying to lower-paying jobs decreases, differences between pay between jobs should decrease. Note, however, that this only applies to jobs for which a worker could qualify. That is, if the barriers to entry (e.g., degree type, having preferred skills, applicant confidence) are too high for workers at low-paying jobs to transition to high-paying jobs, then revealing the salary of high-paying offers will not reduce pay differences. Additionally, if workers in low-paying jobs do not search widely enough to see the newly-revealed wages, there may also be no effect.

There has been some recent empirical evidence on directed search and pay transparency in postings that supports this mechanism. Using data from an online job board in Chile, Banfi and Villena-Roldán (2019) show that jobs with higher salaries attract more applications from workers. They also find that jobs which have higher hidden salaries also receive more applications, although this effect is smaller than for jobs with visible salaries.[12]

To examine the effect of increased visibility on the sex ratio of new hires to jobs in Colorado, we first use Lightcast data to get the change in the percentage of active postings with expected salary information by NAICS 2-digit sector between the first quarters of 2021 and 2020 (that is, by subtracting the share of

[12]For the job board studied, the website requires employers to attach expected salary information internally, even if choosing not to display it to applicants.

postings with wages in Q1 2020 from the same measure for Q1 2021). We then we use QWI data to see the change in the sex ratio of workers hired into each sector for the same change in quarters. Figure 4 below plots this relationship.

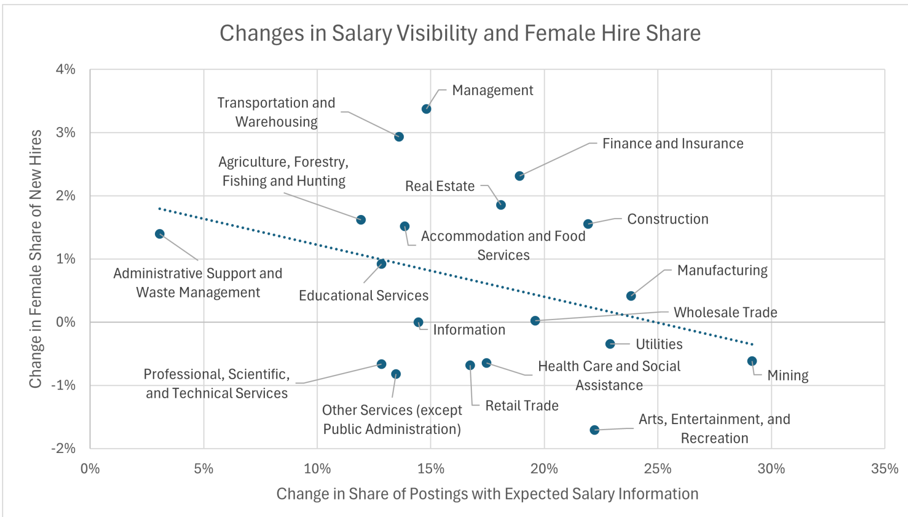

Figure 4: Industry-level changes in the number of job postings with salary information and the share of new hires that are female. Units are percent change from year ago (percent change in 2021Q1 relative to 2020Q1).

Overall, we do not see evidence that sectors which increased transparency attracted a higher share of female workers. If anything, the correlation appears negative. Therefore, we do not have evidence that sectors which implemented greater transparency attracted more female workers than their status quo.

6.2 Bargaining

Suppose that firms who were newly required to post wages all chose to post informative wage ranges that reflected the average appointed wages of new hires. Assuming no change to the composition of the workers applying to each job or the value of labor to the firm, we would expect this change to reduce the variance in new worker wages. That is, workers who would have felt less confident asking for wages above the lower bound of the posted wages should now feel safe asking for higher wages, while the firm should feel more comfortable denying higher wages to workers who would have asked above the posted range.

A large literature in experimental economics has documented that women are less competitive and more risk-averse than men (see Croson and Gneezy (2009) for a literature review). Several papers have cited this as a potential explanation for differences in gendered pay and career choice (e.g. Dohmen and Falk (2011), Buser et al. (2014)). This finding has also been supported in the field (Flory, Leibbrandt and List, 2014). Further, surveys such as those conducted by Glassdoor have found that men are more likely than women to ask for raises (Glassdoor Team, 2021). Relevant to the bargaining context, Roussille (2024) uses an online platform for full-time engineering jobs and finds that women with comparable resumes to men ask for 2.9% lower salaries on average. The author also finds that companies offer women 2.2% lower salaries in their initial bids compared to comparable men. To the extent that this carries over to bargaining behavior, we would expect reducing the effect of bargaining to reduce the gender pay gap.

While cannot directly observe how bargaining behavior changed in our data, we can see if there is a relationship between female earnings and transparency adoption at the aggregate level. If becoming more transparent aided female bargaining relatively more than male bargaining and this effect was large, we would expect to see a positive relationship between these two outcomes.[13] To do this, we find the percent change in monthly female earnings by sector using the QWI data. Figure 5 below plots this measure against the percent change in postings with earnings information, described in the previous section.

[13] We note, however, that the change in earnings could also be affected by other factors, such as changes in application behavior affecting a position's competitiveness.

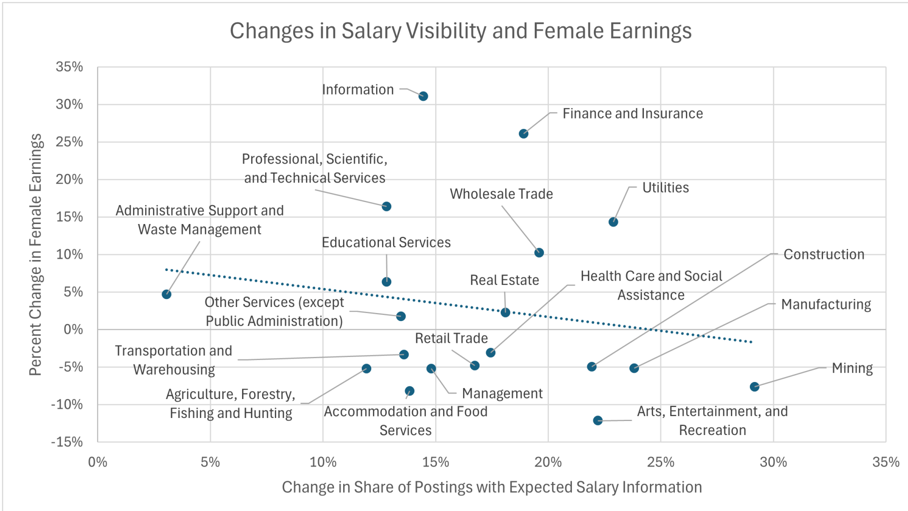

Figure 5: Industry-level changes in the number of job postings with salary information and the average earnings of newly-hired female workers. Units are percent change from year ago (percent change in 2021Q1 relative to 2020Q1).

If anything, female earnings are negatively related to salary information visibility in postings across industries. Therefore, our result that there was no detectable closure in the gender wage gap could be because revealing wages did not significantly aid female bargaining.

6.3 Signal Informativeness

The previous two mechanisms were described based on the idea that employers newly reporting wage ranges are doing so in a way that meaningfully gives workers information on the posting's wage distribution (where the variance in the realized wage is presumably determined by applicant match quality). However, the wage range could be uninformative to workers for a few reasons. First, workers could simply not believe the posted range. Second, the wage range could be too broad to be informative. Third, employers could post wage ranges that are too low or too high relative to the wage they actually expect to pay.

Here, we mention the analysis already completed by Arnold et al. (2022) on the impact of the Colorado law on wage ranges. Its authors found no evidence that newly visible wage ranges were wider than already visible wage ranges or that already visible wage ranges became wider after the law. Therefore, it seems unlikely that changes in wage ranges affected signal informativeness.

While we do not have enough data to study whether workers' beliefs in the credibility of the posted wage information changed, we can study whether posted wages became more or less indicative of realized wages before and after the policy. To do this, we plot median posted annual wages from the Lightcast data against mean annualized earned monthly wages from the QWI data in the figure below.[14]

[14]Unfortunately, we do not currently have a way to compare means to means or medians to medians with our current data since our Lightcast and government sources report different measures of centrality. However, we are working on getting an estimate of the means from the Lightcast side of the data for the next draft of this paper.

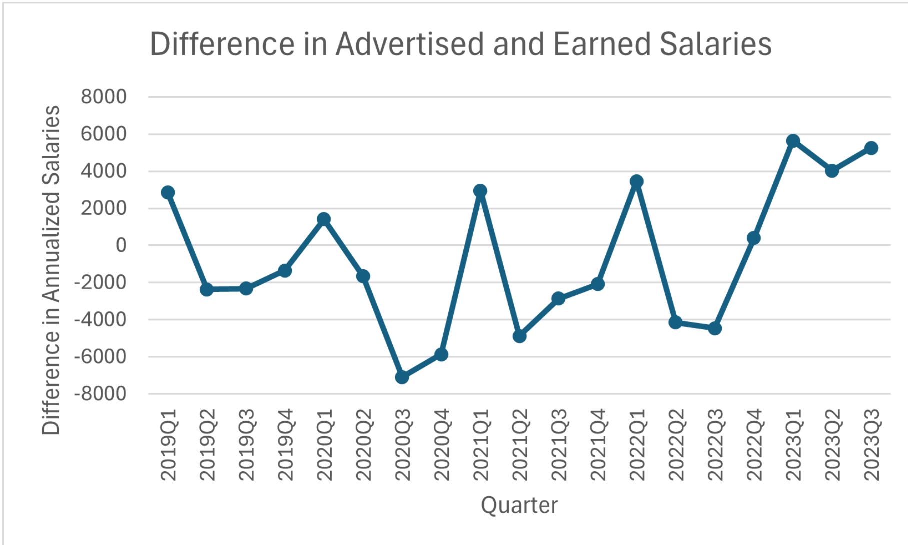

Figure 6: Difference in posted and realized quarterly wages for Colorado. Reported difference is median posted wages from the Lightcast data minus average realized wages from the QWI data. For QWI data, monthly earnings are multiplied by 12 to get annualized amounts.

Overall, the distance between advertised and earned salaries seems fairly similar before and after the law became effective. When combined with Arnold et al. (2022)'s evidence on wage range changes, we do not find any suggestive evidence that the law meaningfully changed the informativeness of advertised wage ranges.

7 Conclusion

In this paper, we attempted to determine whether Colorado's Equal Pay for Equal Work Act was effective in its stated goal of closing gender pay gaps. While the act contains many provisions similar to past legislation aiming to reduce pay equity in the workplace, it was the first state-wide law in the United States to cover all job postings, regardless of employer or job. We therefore focus on this aspect of the legislation and attempt to assess its impact on the wage of employees who were first-time hires. We find that the law did not have a significant effect on reducing the gender gap in new worker earnings. Next, we examined mechanisms by which the law could have reduced gaps in worker pay: directed search, bargaining, and signal informativeness. We did not find any patterns which would lead to a smaller gender wage gap. On the contrary, the mechanisms studied give suggestive evidence for why the policy could have increased the gender pay gap.

While we are careful to note that placebo tests do not support statistical significance in our main result, we may well wonder why the policy did not lead to a clear reduction in gender pay differentials as intended. One mechanism that we were unfortunately unable to examine in this study was the role of how workers searched for jobs. For instance, we are unable to comment on the extent to which female workers in lower-paying occupations and sectors saw newly-visible higher-salary openings and adjusted their application behavior. As mentioned in an earlier section, including more information on job postings can only help workers if they see the postings with the new information. This study therefore highlights the potential value of future work dedicated to studying how workers search and how information policy can be designed to target those who would best benefit from the information.

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A Hourly Wage Synthetic Control Analysis

For our main analysis, we directly use the monthly earnings measures for newly hired workers from the QWI. In this section, we instead construct a measure of average hourly wages by dividing each gender group's average monthly earnings measures by the product of its corresponding average weekly hours worked measure from the CPS and 4.345, the average number of weeks in a month.[15] We then use this as our outcome variable and repeat our synthetic control analysis as before.

The synthetic control group composition, predictor balance, and effect estimates are given in tables 5, 6, and 7 below, respectively.

Notably, the selection of states into the synthetic control group is very similar as when using monthly earnings directly, with the largest difference being that North Dakota goes from receiving a small positive weight to receiving 0 weight. Otherwise, the states included remain the same, with Minnesota still receiving a majority of the weight.

Similar to our main analysis, we find no evidence that the law narrowed pay gaps; for each quarter following treatment, the gap between Colorado and its synthetic control is positive, although the difference is never significant at the 5% level.

State	Weight
Minnesota	.548
Utah	.177
Vermont	.143
District of Columbia	.063
Hawaii	.034
Oregon	.034

Table 5: Synthetic control, hourly wages: Composition from donor pool states.

[15] Note that this proxies the average hours worked by new hires with an estimate of the average hours worked by all workers.

Predictor	Colorado	Synthetic Colorado
Gender Wage Gap	3.99	4.03
Real Personal Income Per Capita	52,119.44	51,984.87
Percent White	82.89	80.67
Percent Black	4.10	6.86
Percent Married	40.83	40.37
Percent Male	50.32	49.70
Percent with High School Only	23.90	26.25
Percent with Four Years of College Only	18.13	16.41
Percent with Postgraduate Education	10.20	9.17
Percent in Labor Force	68.02	68.82
Average Quarterly Unemployment Rate	4.94	4.52
COVID-19 Cases Per 100,000	1.61	1.69
COVID-19 Deaths Per 100,000	.02	.02

Table 6: Predictor balance. ACS measures (demographics, education) include general population of all ages.

Quarter	Gap	p
2021 Q1	.15	.9762
2021 Q2	.56	.7619
2021 Q3	1.05	.3571
2021 Q4	.36	.4048
2022 Q1	1.06	.2619
2022 Q2	1.13	.2381
2022 Q3	.21	.2619
2022 Q4	.40	.3095
2023 Q1	.76	.3333
2023 Q2	.26	.3571
2023 Q3	.70	.3333
2023 Q4	1.33	.2143

Table 7: Synthetic control: Hourly wage results. "Gap" refers to the difference in the outcome variable (the gender wage gap) between Colorado and the synthetic control.

B Multiple State Analysis

For our main analysis, we only examine the effect of the Colorado law. We do this since Colorado's law became effective two years before the law of any other state, and so it has the most available data for analysis. Further, it is most straightforward to construct a synthetic control for a single state with a single time of treatment. In this section, we attempt an additional analysis which aims to get an aggregate average effect of pay transparency laws across all states for which we can get any data. To do this, we use a method for difference-in-differences with multiple treatments introduced by Callaway and Sant'Anna (2021).

B.1 Estimator Description

For this analysis, we use the Callaway and Sant'Anna (2021) staggered difference-in-difference estimator to measure the effect of the state laws on worker outcomes.[16] Specifically, this estimator attempts to estimate *group-time average treatment effects* Callaway and Sant'Anna (2021). That is, for the group of individuals who receive treatment in period g, the average treatment effect at time t is

$$ATT(g,t) = E[Y_t(g) - Y_t(0)|G_g = 1]$$

While these group-time average treatment effects can be reported separately for each treatment group, we instead use two types of aggregation in reporting our main results to ease visualization and interpretation.

First, the "simple" aggregator:

$$\theta_S^O := \sum_{g=2}^{T} \frac{1}{T-g+1} \sum_{t=2}^{T} \mathbf{1}\{g \leq t\} ATT(g,t) P(G=g)$$

where T is the number of time periods in the sample. This aggregation measures the average effect of treatment participation among all ever-treated groups (Callaway and Sant'Anna, 2024). It has the advantage of giving a single number

[16]We make use of the *csdid* command in Stata to implement this estimator.

which makes for an easily-interpretable result.

Second, the "dynamic" aggregator:

$$\theta_D(e) := \sum_{g=2}^{T} \mathbf{1}\{g + e \leq T\} ATT(g, g + e) P(G = g | G + e \leq T)$$

which gives the average effect for units treated for e periods (Callaway and Sant'Anna, 2024). This aggregation is used for the coefficients shown in figure 10. It provides a sense of the lasting impacts of the transparency laws.

B.2 Selection of Treatment and Control Groups

The treated states are those that passed transparency laws which became effective over the sample period. These are Colorado, Washington, and California. Although New York state passed a similar state-wide policy which went into effect in the fall of 2023, it was excluded from the sample because New York City had passed a similar, more local law the year prior. Similarly, New Jersey and Ohio were excluded from both treated and control groups since each experienced partial treatment in the form of more local transparency laws. Following Cullen and Pakzad-Hurson (2023), we use all states never treated prior to 2024 as a control group. Only four states (Alaska, Michigan, Mississippi, and North Carolina) were dropped due to a lack of available data.

B.3 Identifying Assumptions: Difference-in-differences with multiple treatments

The validity of our estimator depends on the following assumptions, the formulas for which are taken directly from Callaway and Sant'Anna (2021).[17] In this

[17] Callaway and Sant'Anna also describe an additional assumption needed if using not-yet treated observations in the control group. This is that there must be parallel trends between treated and not-yet treated observations if using not-yet treated observations in the control group. However, we do not currently do this, instead only using never treated units. Therefore,

section, we describe how each of these apply to our context.

B.3.1 Irreversibility of Treatment

$$D_1 = 0 \text{ almost surely (a.s.)}$$
$$\text{For t} = 2, \ldots, T, D_{t-1} = 1 \implies D_t = 1 \text{ a.s.}$$

This assumption requires that once an observation becomes treated, it remains treated thereafter. Since none of the laws requiring greater transparency were repealed over the course of our sample, this assumption should be satisfied in our sample.

B.3.2 Limited Treatment Anticipation

There exists a known $\delta \geq 0$ such that

$$E[Y_t(g)|X, G_g = 1] = E[Y_t(0)|X, G_g = 1] \text{ a.s.}$$
$$\forall g \in G, t \in 1, \ldots, T \text{ such that } t < g - \delta$$

That is, if treated states anticipate treatment, there must be some known limit to this anticipation. Since we look at worker outcomes, this assumption would be violated if worker behavior changed in an undetectable way in advance of the laws coming into effect (e.g. waiting to apply for jobs for some unknown number of months ahead of the January 2021 in Colorado anticipation of more wages being visible afterwards). It could also be affected by employers changing in anticipation of the policy, since employer behavior could affect worker outcomes.[18]

our current analysis does not require this assumption.

[18] Currently, we are only able to test for changes in employer behavior, but we are planning to add additional analysis explicitly examining changes in worker behavior in a future draft.

Arnold et al. (2022) suggests that (in Colorado's case), trends appear fairly parallel for employer posting behavior prior to transparency law implementation with untreated states. We show in the graphs below that trends in posted wages do not differ greatly between treatment and control groups either immediately before the 2021 Colorado law or immediately before the 2023 California and Washington laws.[19] The first graph shows trends in levels, whereas the second shows trends in percent changes.

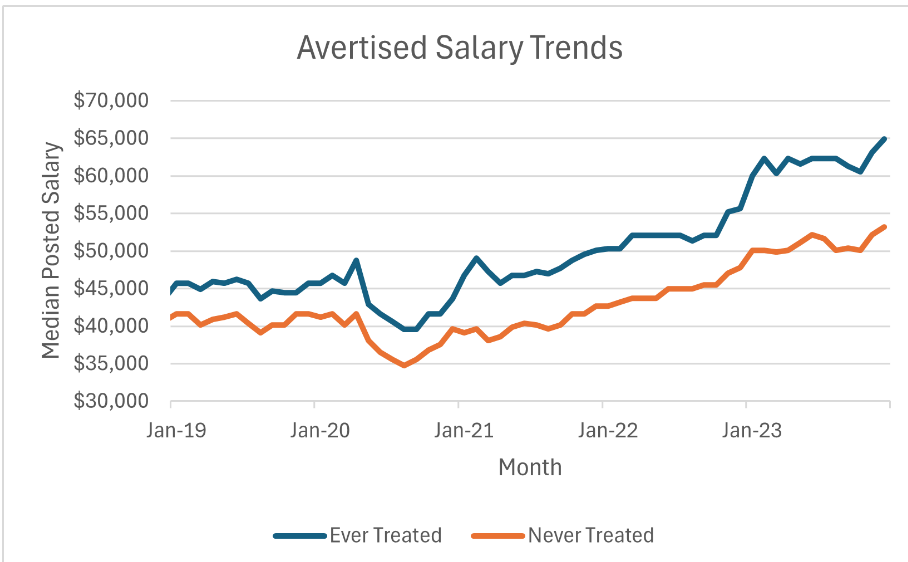

Figure 7: Median advertised annual wages, levels.

[19] These graphs are taken from existing summary methods made available by Lightcast. Because of this, the displayed trends use median advertised wages rather than an estimate of the mean. The median should be sufficient to show a change in the direction of posted wages, but we note that our main results using the QWI are based on average wages, not medians. Because of this, we instead use an estimate of average posted wages when making direct comparisons between Lightcast and QWI data, as in our section on evaluating changes to signal informativeness (see section 6.3).

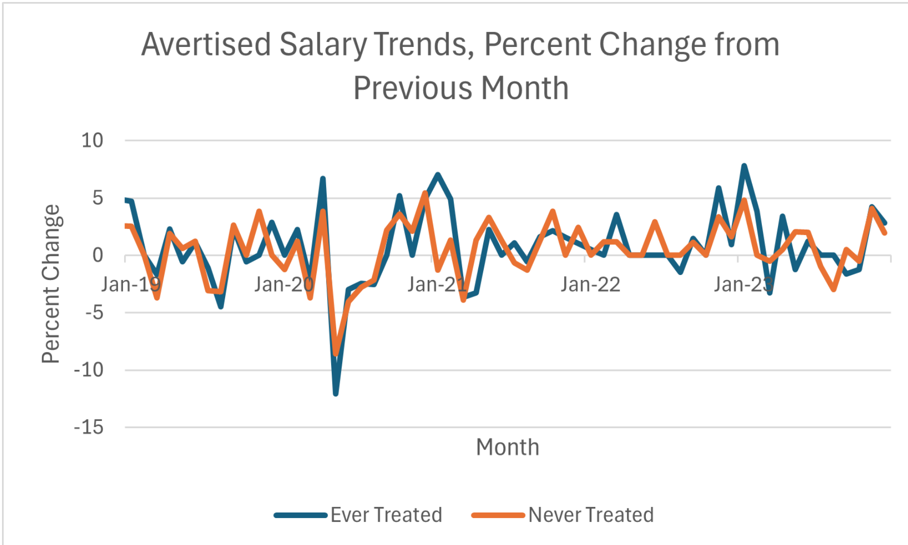

Figure 8: Median advertised annual wages, change from previous month.

B.3.3 Conditional Parallel Trends Assumption

For each $g \in G$ and $t \in 2, \ldots, T$ such that $t \geq g - \delta$,

$$E[Y_t(0) - Y_{t-1}(0)|X, G_g = 1] = E[Y_t(0) - Y_{t-1}(0)|X, C = 1] \text{ a.s.}$$

where C is an indicator equal to 1 if observations are in the never treated group. That is, counterfactual trends must be parallel between treated observations and never treated observations, conditional on included covariates.

The following graph illustrates the trends in our main outcome of interest between treated and control states.

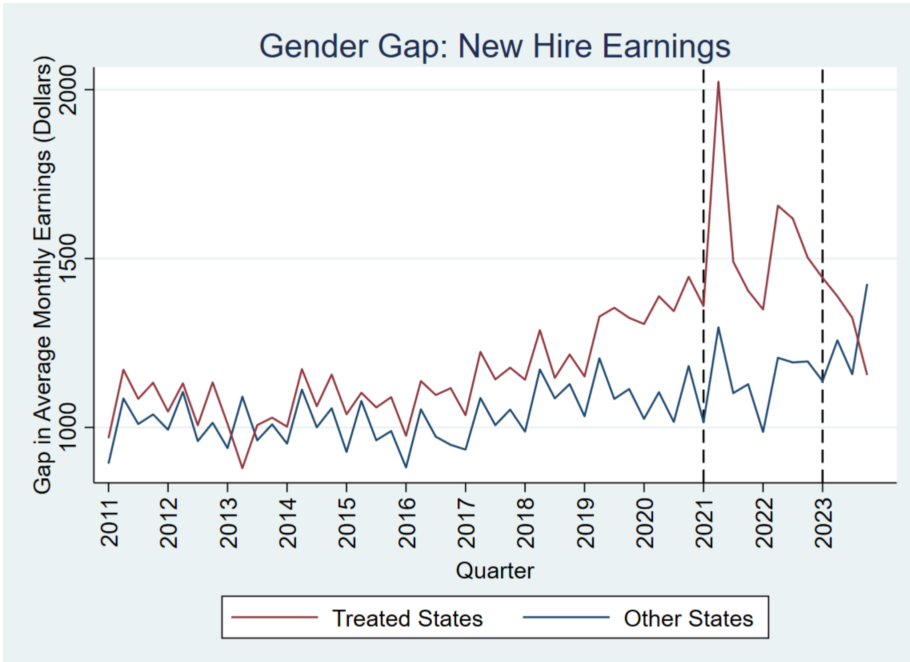

Figure 9: Gender gap in average monthly earnings of newly hired workers. The vertical axis displays male average earnings minus female average earnings. The dashed vertical lines mark the effective dates of the pay transparency laws.

Additionally, we note that figure 10 presented in the results section displays the estimated coefficients for the aggregate average treatment effect on the treated both before and after treatment. The coefficients do not differ significantly from 0 in the periods before treatment, so we do not have evidence that pre-treatment trends differ between the two groups.

B.4 Multiple State Results

In this section, we present our main results. First, the table below gives the simple aggregation results of the average effect of treatment on the treated.

Outcome Variable	ATT	Standard Error	Observations
Gender Gap in New Hire Earnings	-96.5667	97.4635	2,249

Table 8: Simple aggregation results.

That is, we estimate that the gap in earnings in treated states was about $100 smaller on average following the policy than it would have been otherwise, although this effect is not statistically significant. The graph below instead looks at the average treatment effect on the treated by length of exposure to treatment.

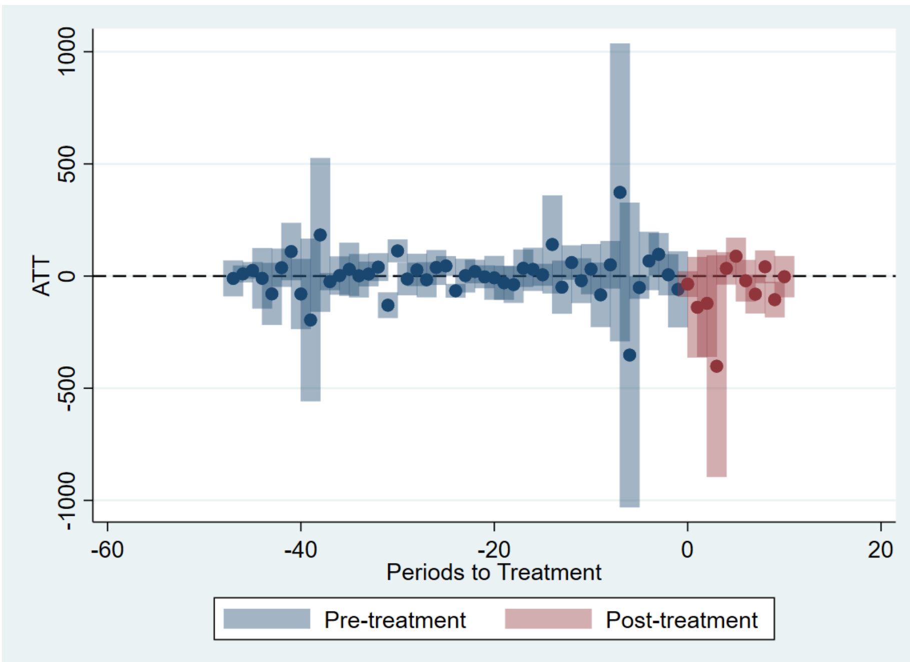

Figure 10: Dynamic effects. Vertical axis represents the average treated effect on the treated for the gender gap in monthly earnings.

Here we see that the treatment effect is most negative in the few periods after treatment. Therefore, we do not have evidence of an enduring impact of this legislation on the gender gap.[20]

[20]Note, however, that only Colorado has enough observations to last for more than the first few quarters after treatment. It is therefore unsurprising that there is no negative effect in the later quarters, since, as we show in the main body of the paper, the gender gap in earnings was higher in Colorado than in other states during the treatment period.