Combating Inequality with Transparency?
Evidence from Colorado

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Abstract

Does giving workers more information on wages help eliminate 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. 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 more advantage of the increased transparency in postings than female workers, which could have had an attenuating effect on 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 which 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, 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. As of the time of writing, 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).

 Specifically, the Colorado law requires all employers to include salary infor-
mation in all job listings.[2] The requirement for all employers to post expected

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[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 yet. 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 attempting to answer 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 this question 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 for the interested reader in appendix A. 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 method and evaluates the plausibility of its main assumptions in our setting. 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 policy attempts to make pay more transparent in the workplace, and these policy efforts have targeted different segments of the labor market over time.

Böheim and Gust (2021), 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. They both find that the policy did not reduce the gender wage gap or affect worker wages. The law studied in this case required employers to provide their 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 pay transparency leads to 2% lower wages overall in US states enacting pay transparency laws (Cullen and Pakzad-Hurson, 2023). In this case, the studied laws prevent employers from retaliating against the sharing of wage information among

coworkers.

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. The final paper we cite studying policies which publicize worker wages is Baker, Halberstam, Kroft, Mas and Messacar (2023). Its authors study laws in Canada which allowed the public to access 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) subset of the data to measure the pay and percentage of hired workers for each state. The LEHD data is constructed from several administrative data sources and covers over 95% of all private sector jobs in the United States (US Census Bureau, 2022).

We use these data to examine relevant worker outcomes over the period from the first quarter of 2011 to the final quarter of 2023. That is, from ten years before the implementation of the law being analyzed 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]

Variable Description	Colorado	Donor Pool
Panel A: Pre-treatment		
Average Male Earnings	3509.9250	3133.9500
	(415.3684)	(640.5601)
Average Female Earnings	2448.5000	2102.3506
	(329.1174)	(494.1152)
Average Earnings Gap	1061.4250	1031.5994
	(112.0675)	(292.8368)
Panel B: Post-treatment		
Average Male Earnings	5091.7273	4150.0089
	(342.0009)	(724.3049)
Average Female Earnings	3684.4545	2997.6718
	(288.8973)	(604.4042)
Average Earnings Gap	1407.2727	1152.3370
	(108.673)	(250.6823)
Pre-treatment Observations	40	1640
Post-treatment Observations	11	451
Number of States	1	41
Sample Period	2011 Q1 - 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

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[3]At the time of writing, 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.

[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 A.

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 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

$$\|X_1 - X_0 W\|_V = \sqrt{(X_1 - X_0 W)'V(X_1 - X_0 W)} \tag{1}$$

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[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.

[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.

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

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

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. The main way to assess the plausibility of this assumption is to show similarity between the treatment group and 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.

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.

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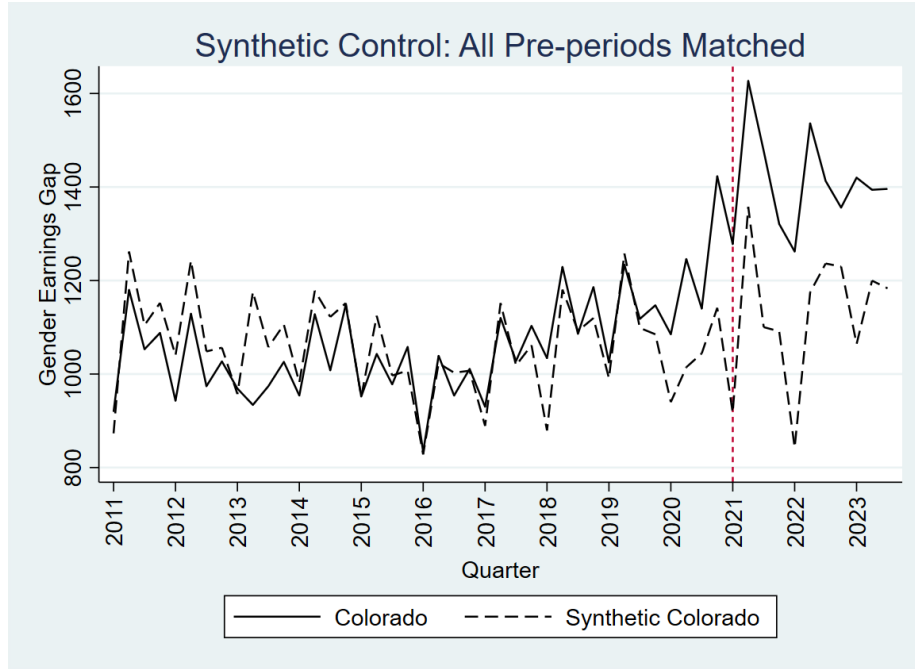
Figure 1: Synthetic control trends. The graphed synthetic control was constructed using data all periods before treatment. The dashed vertical line indicates the first quarter of 2021 when the Colorado law became effective.

While this specification uses the most available data and still includes the variables related to a state's COVID-19 exposure as predictors, we find that it does not match Colorado very well in the periods immediately before treatment. Further, since the COVID-19 pandemic remained in effect at the time of treatment, matching on the periods immediately before treatment is especially important for our analysis.

Therefore, for our primary specification, we construct the synthetic control using only using data from 2020. Although this increases the distance between the synthetic control in the periods before 2020, it also greatly lessens the distance between the two in 2020.
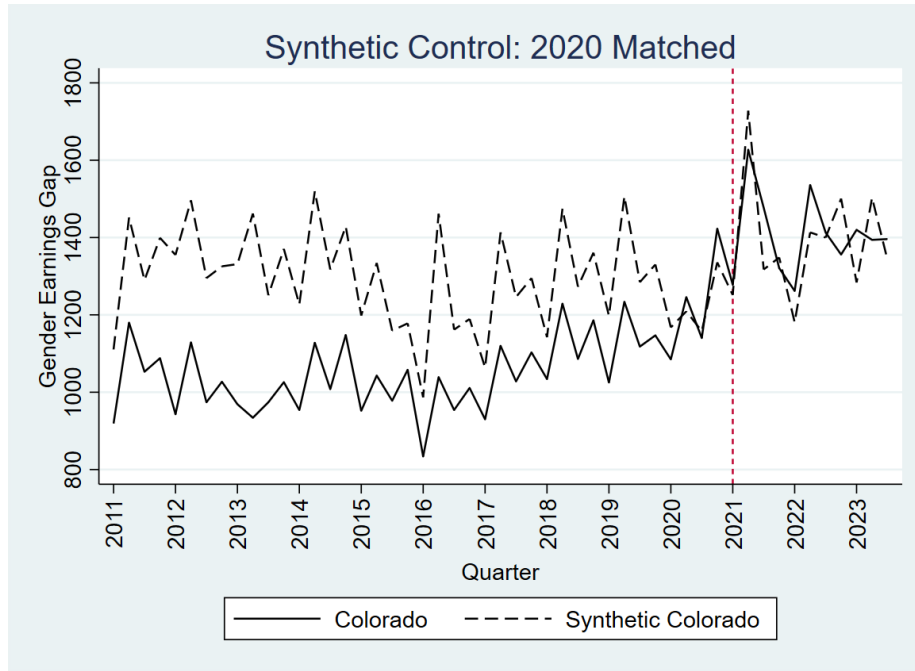
Figure 2: Synthetic control trends. The graphed synthetic control was constructed using 2020 observations only. The dashed vertical line indicates the first quarter of 2021 when the Colorado law became effective.

Table 2 below displays the balance of predictors for the treated group and the 2020-based synthetic control. Overall, the synthetic control appears to closely match Colorado in predictor values.

Predictor	Colorado	Synthetic Colorado
Gender Earnings Gap	1223.5	1217.71
Proportion White	.7184762	.7747408
Proportion Black	.0397822	.050637
Proportion Married	.4109083	.407588
Proportion Male	.5047681	.5009144
Proportion with Some College	.1816456	.1885262
Proportion with Four Years of College	.2030027	.185296
Proportion with Postgraduate Education	.1192622	.1109941
Real Personal Income Per Capita	59863	61068.91
Proportion in Labor Force	.5488477	.5445037
Average Quarterly Unemployment Rate	6.783333	6.779092
COVID-19 Cases Per Capita	.1785285	.183774
COVID-19 Deaths Per Capita	.005774	.0056428

Table 2: Predictor balance. The "Proportion with Some College" measure includes individuals with more than a high school education but less than four years of college.

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 the passage of this law in Colorado caused many employers or workers to enter or leave the state in a way that impacted the relative gender gap in other states, for example. Since past work by Arnold et al. (2022) did not find a significant change in postings in Colorado after the law, we do not

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[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.

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
District of Columbia	.063
Hawaii	.092
Massachusetts	.061
New Hampshire	.338
North Dakota	.258
Vermont	.189

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

5.2 Estimated Effect

Figure 2 shows that our synthetic control is very close to Colorado over the treated period. That is, we do not find evidence of a reduction in the gender earnings gap. If anything, the gender earnings gap seems higher in Colorado for most of the treated periods. The value of the gap between Colorado and its synthetic control for each of the treated periods is shown in the table below, along with the p value from the placebo tests (shown in the next section).

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[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.

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Quarter	Gap	p
2021 Q1	26.516	.952381
2021 Q2	-100.833	.8095238
2021 Q3	159.566	.7380952
2021 Q4	-27.521	.8095238
2022 Q1	81.344	.8095238
2022 Q2	122.722	.8095238
2022 Q3	12.942	.8333333
2022 Q4	-144.608	.8333333
2023 Q1	136.278	.8333333
2023 Q2	-108.879	.8333333
2023 Q3	54.26	.9047619

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 find suggestive patterns that female earnings and the share of newly hired employees who are women were somewhat negatively correlated with increases in salary visibility at the industry level. That pattern, although not causal, would be more suggestive of men benefiting more from the policy than women than the reverse.

5.3 Placebo Tests

Inference for synthetic control is typically conducted with in-place placebo tests using units in the donor pool. These placebo tests are shown graphically in the figure below.
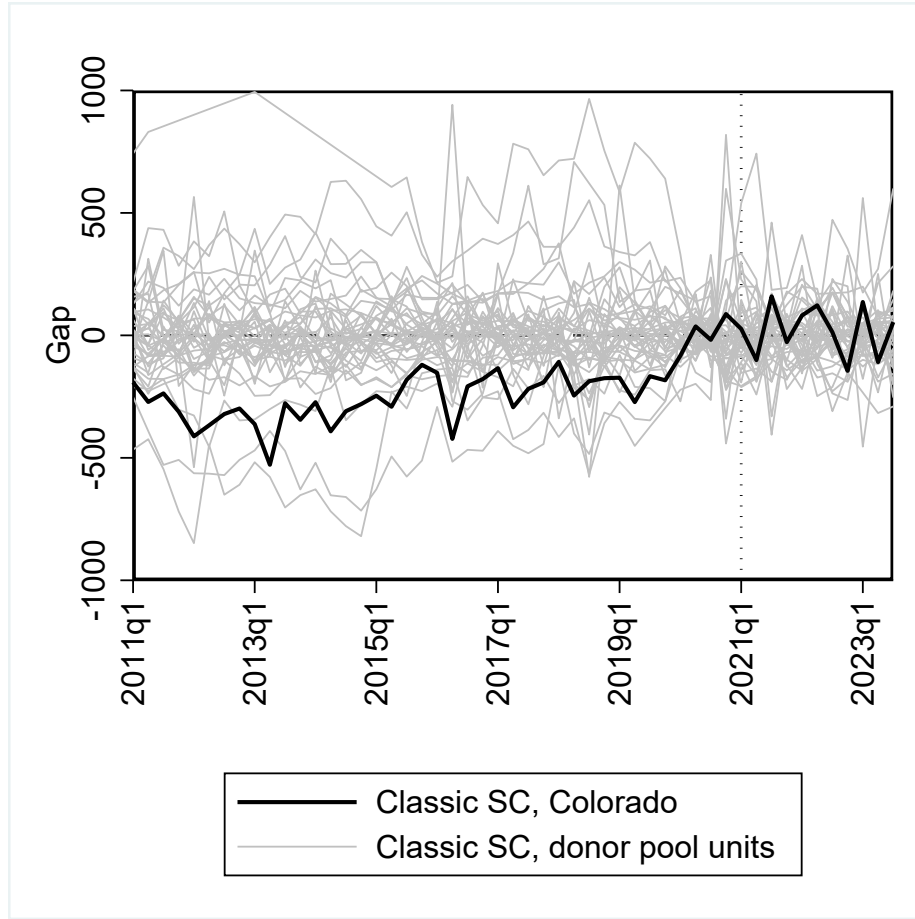
Figure 3: Placebo tests for 2020-based synthetic control. Here "Gap" refers to the difference between the value of the gender earnings gap in Colorado and the gender earnings gap in the synthetic control (or placebo) for a given quarter.

We can see visually that the gap between the selected synthetic control and Colorado is very similar to the majority of gaps using placebos, so our main result does not indicate a significant difference. This finding is reflected in the p values given in table 4.

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. We also present additional analyses as they become relevant to studying the proposed mechanisms.

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 crowded in lower-paying occupations or sectors should be more able 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 motivates the relevance of this explanation as a potential mechanism of interest. Using data from an online job board in Chile, Banfi and Villena-Roldán (2019) find evidence 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 more

minor that for jobs with visible salaries.[11]

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 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.
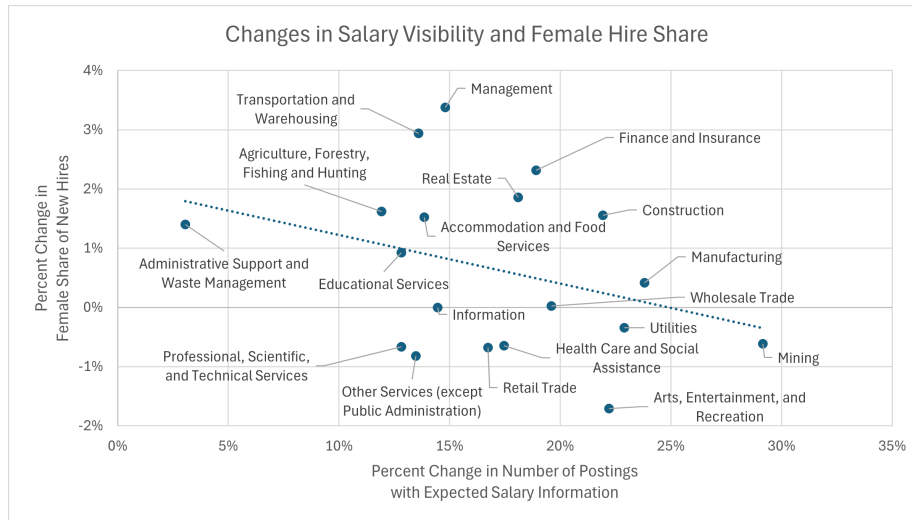

Figure 4: Industry-level changes in the number of job postings with salary information and the share of newly hires that are female. Units are percent change from year ago (percent change in 2021Q1 relative to 2020Q1).

Overall, we do not see evidence that sectors which increased transparency more received 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.

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

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.[12] 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.
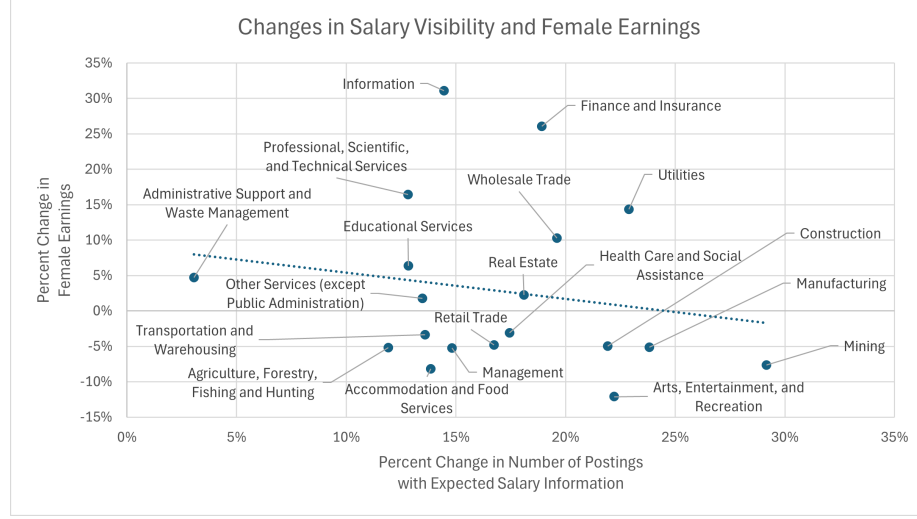

Figure 5: Industry-level changes in the number of job postings with salary information and the average earnings of newly-hired female workers. Units are percent change from year ago (percent change in 2021Q1 relative to 2020Q1).

If anything, female earnings decrease with how much a sector changed its salary information visibility in postings. 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

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

in the realized wage is supposedly 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 on wage ranges. They found no evidence that newly visible wage ranges were wider than already visible wage ranges or that visible wage ranges became overall wider after the law. Therefore, it seems unlikely that changes in wage ranges affected signal informativeness.

While we do not have enough data in this study whether workers' beliefs in the credibility of the posted wage information changed, we can study whether posted wages became less or more 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.[13]

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[13]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.
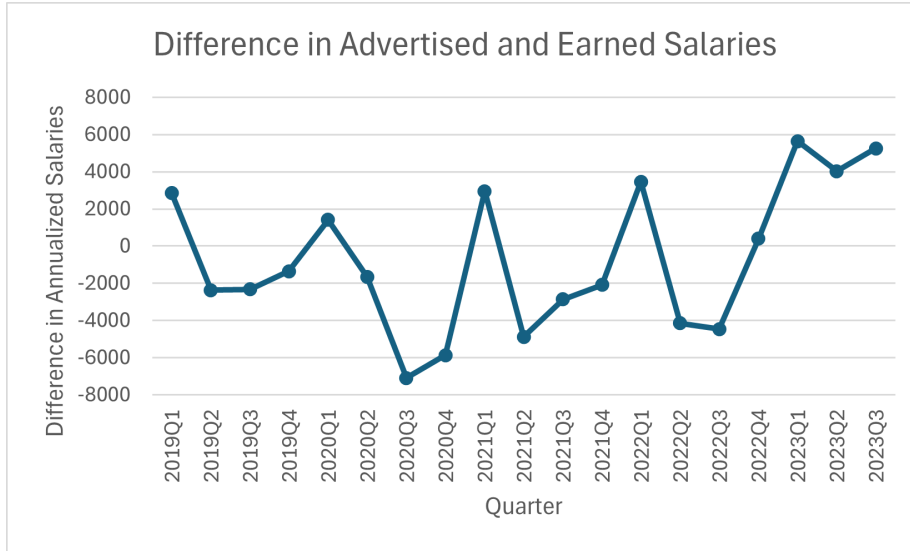
Figure 6: Difference in posted and realized quarterly wages for Colorado. Reported difference is median posted wages from the Lightcast data minus average realized wages from the QWI data. For QWI data, monthly earnings are multiplied by 12 to get annualized amounts.

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 attempting to address pay equity in the workplace, it was the first state-wide law in the United States to require all employers to list expected salary ranges for all employees.

We therefore focus on this aspect of the legislation and attempt to assess its impact on the wage of employees who began working at firms for the first time (hires that were not recalls). We estimate that the laws had a positive effect on average on the gender gap in new worker wages in the post-treatment period for treated states, but this effect was not significant. We also examined mechanisms by which the law could have reduced gaps in worker pay and 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 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 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).

A.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.[14] 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.

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

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 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.

A.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.

A.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).[15] In this section, we describe how each of these apply to our context.

A.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.

A.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

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[15]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, our current analysis does not require this assumption.

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.[16]

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.[17] The first graph shows trends in levels, whereas the second shows trends in percent changes.

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[16]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.

[17]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).
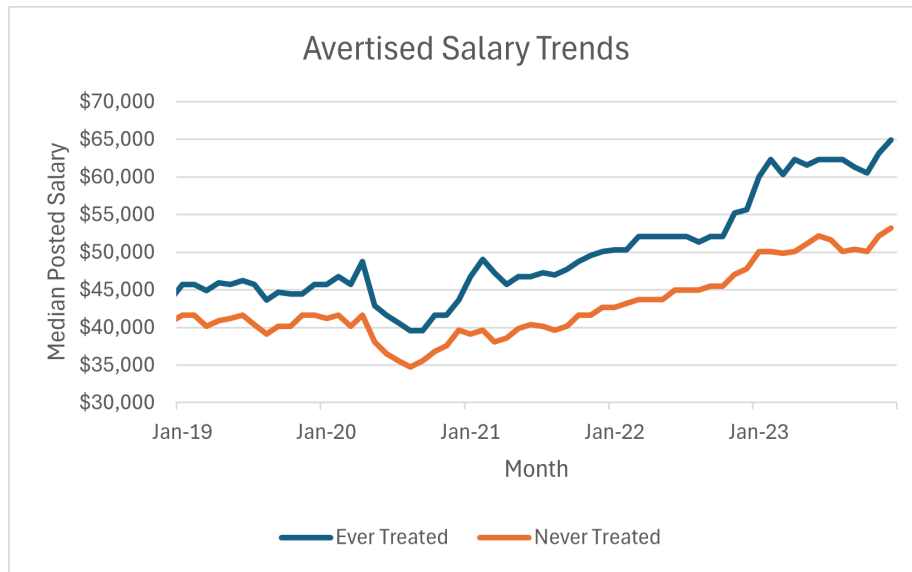
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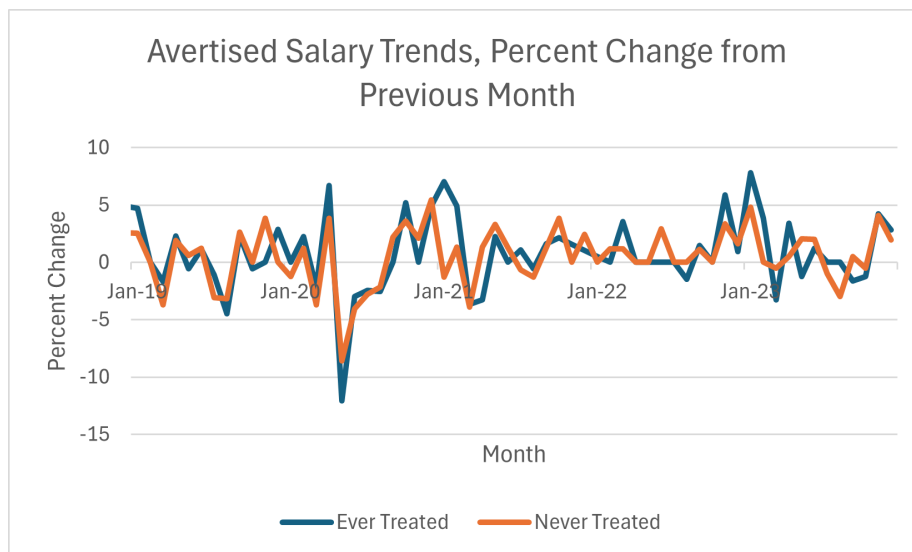Figure 7: Median advertised annual wages, levels.

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

A.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.
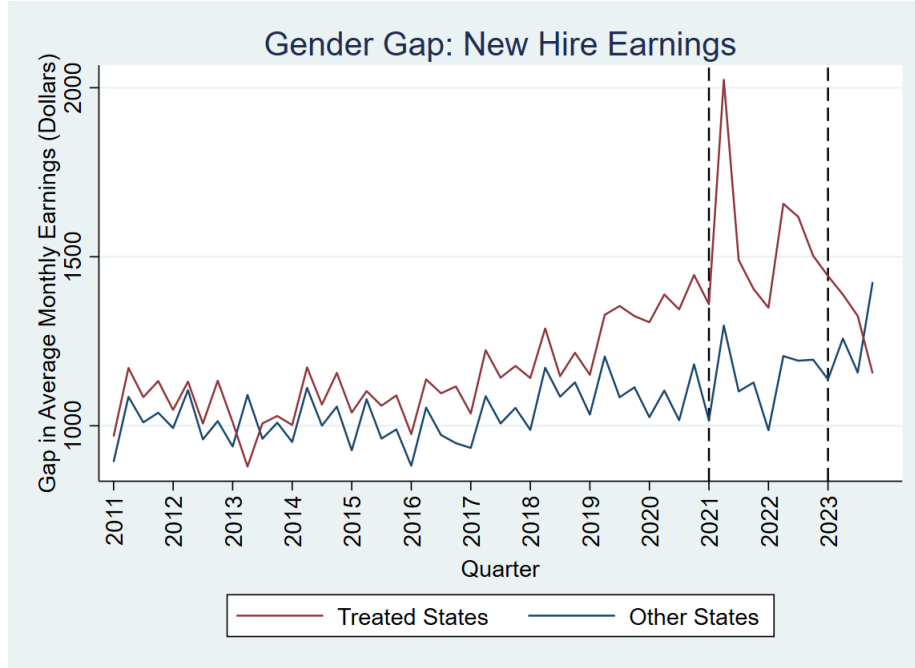

Figure 9: Gender gap in average monthly earnings of newly hired workers. The vertical axis displays male average earnings minus female average earnings. The dashed vertical lines mark the effective dates of the pay transparency laws.

Additionally, we note that figure 10 presented in the results section displays the estimated coefficients for the aggregate average treatment effect on

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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.

A.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 5: 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.
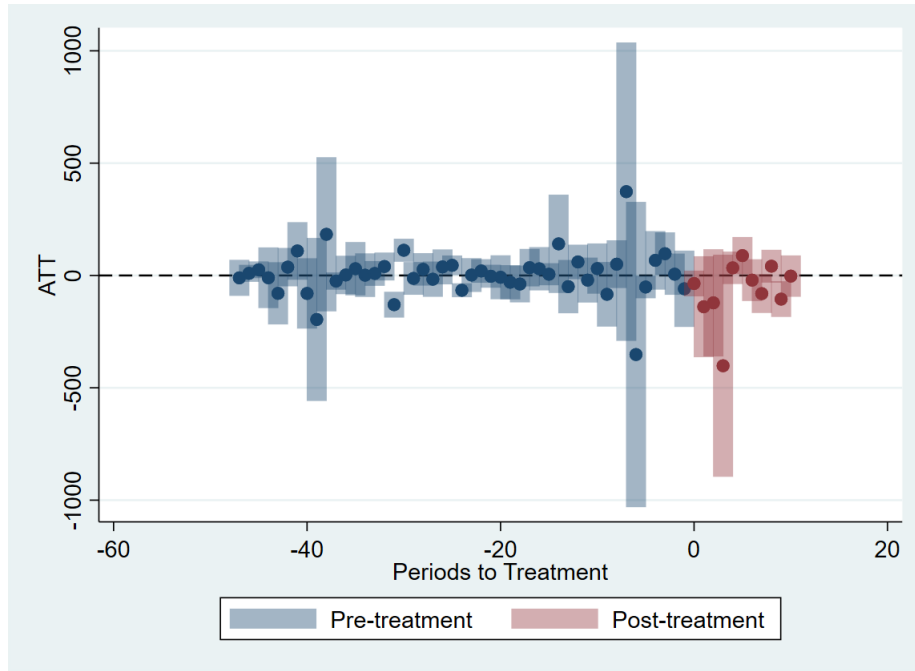
Figure 10: Dynamic effects. Vertical axis represents the average treated effect on the treated for the gender gap in monthly earnings.

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.[18]

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[18]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.