What is the effect of Salary History Bans on the Labor Supply of Mothers?

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Abstract

This paper examines the impact of Salary History Bans (SHBs) on maternal labor supply. SHBs are policy measures that prevent employers from inquiring about a job applicant's past salary, a practice that perpetuates lower wages for women. While prior research has established that SHBs help narrow the gender pay gap, primarily through increased wages for women, this paper examines the effects of the resulting wage increases on labor supply among mothers. Using labor supply data from the Current Population Survey and leveraging the Callaway and Sant'Anna (2021) method for staggered policy adoption, I construct a pseudo-panel to analyze SHBs' impact across various labor supply outcomes. My findings indicate no significant overall effect on maternal labor supply but indicate an increase in labor force participation among mothers with young children, primarily driven by part-time employment. These results suggest that wage increases incentivize mothers of young children to enter or remain in the labor force.

1 Introduction

Salary History Bans (SHBs) are policy tools that prohibit employers from acquiring and using salary history during any stage of the hiring process. Beginning in 2017, seventeen states across the United States have implemented SHBs. [1] Employers often use salary history as a proxy for productivity, which can exacerbate the gender pay gap; because women's compensation history is generally less competitive than men's [2], relying on compensation history for setting offers of pay

[1]As of September 2023. Additionally, four states and the District of Columbia have implemented public sector Salary History Bans, which apply only to state or local government employers. Henceforth, "Salary History Bans" or "SHBs" will refer specifically to All-Employer SHBs.

[2]This disparity is due to the gender pay gap, which is influenced by various factors, including statistical (Phelps, 1972; Altonji and Blank, 1999; Blau and Kahn, 2017) and taste-based discrimination (Becker, 1971; Arrow, 1973), information gaps and negotiation differences (Cortés, French, Pan, and Zafar, 2024; Card, Cardoso, and Kline, 2016;

can perpetuate lower pay for women. One of the main policy objectives of SHBs, therefore, is to reduce the gender pay gap by addressing this particular source of pay disparity.

Indeed, Salary History Bans have been found to narrow the gender pay gap, and this is largely due to increased earnings for women (Bessen, Meng, and Denk, 2021; Hansen and McNichols, 2020; Sinha, 2022). [3] As SHBs result in increasing wages for women, we can expect women to respond by increasing their labor supply. However, in their employment analysis of the California Salary History Ban using a synthetic control method, Hansen and McNichols (2020) find that there is almost no effect of the California SHB on the employment status of women.

Although specific policies like the California SHB may not significantly impact employment status among all women, broader literature documents that women's labor supply undergoes striking changes in various phases of their life cycle. Women and men exhibit very similar labor supply behavior before parenthood but diverge drastically in their career trajectories once children are born. This point in the life cycle initiates a departure from the labor force for some women and a transition from full-time to part-time work arrangements for others, while some women choose to remain in the labor force. This heterogeneity in labor supply is particularly pronounced among mothers when children are below school-age (under 5 years of age), when parents (usually mothers) face a tradeoff between earnings and high childcare costs.[4] If this is the case, mothers with very young children are likely respond to the resulting wage growth from Salary History Bans by increasing their labor supply at greater rates than women on average. In this paper, I investigate whether this is the case and study the following question: What is the effect of Salary History Bans on the labor force participation of mothers?

Biasi and Sarsons, 2022), limited advancement opportunities (Cardoso and Winter-Ebmer, 2007; Benson, Li, and Shue, 2019), and the motherhood penalty (Blau and Kahn, 2017; Correll, Benard, and Paik, 2007; Glauber, 2018).

[3]In addition, Mask (2023) finds that Salary History Bans increase earnings for people who have scarred wages – a result of initiating their careers during a recession. Overall, the evidence shows that SHBs induce employers to offer higher pay to workers with less competitive salary histories.

[4]Blundell, Dias, and Shaw (2016) find that women with children, both married mothers and single mothers, exhibit the highest labor supply elasticities among all women. Furthermore, among mothers, as Apps, Kabátek, Rees, and van Soest (2016) find in their structural analysis, labor supply of mothers with young children is particularly responsive to wages and cost of childcare compared to other groups of mothers.

I study the impact of state-level Salary History Bans (SHBs) on maternal labor supply, focusing on four outcomes: labor force participation, employment, part-time employment, and full-time employment. I leverage the variation in SHB policy timing across states, applying the Callaway and Sant'Anna (2021) estimator for multiple treatment periods. The analysis utilizes labor supply data from the Current Population Survey's (CPS) Basic Monthly Files (January 2010–March 2020) combined with SHB implementation dates sourced from HR Dive.[5] To construct a state-year-month pseudo-panel, I aggregate individual-level CPS data, first residualizing outcomes by regressing on education and age to address selection biases.

This pseudo-panel approach treats each state-year-month observation as an independent cohort, mitigating issues like attrition and shifting demographics in the CPS data over time. My analysis period ends in February 2020 to exclude disruptions from the COVID-19 pandemic, which altered labor supply behaviors, particularly for mothers. To manage the variation in SHB policy timing and the limited post-SHB implementation observations, I construct a balanced panel that includes only the earliest SHB-adopting states in the treatment group. This approach ensures a uniform number of post-treatment observations for each treated state, reducing potential biases arising from uneven representation in the treatment effect estimates.

The analysis assigns states to treatment and comparison groups based on SHB adoption status: the treatment group consists of states implementing SHBs early, while the comparison group includes states that never adopted SHBs during the analysis period (ending in March 2020). This assignment relies on the assumption that, in the absence of SHB implementation, trends in the labor supply of mothers would have followed parallel paths between treatment and control groups. Recognizing that the parallel trends assumption might not hold perfectly between treated and never-treated states, I conduct a robustness analysis that drops never-treated states entirely, using only not-yet-treated states – those planning to adopt SHBs after March 2020 – as an alternative comparison group. These states may share more similarities with the treated states, offering a more conservative

[5] "Salary history bans: A running list of states and localities that have outlawed pay history questions", 2024 Link to HR Dive

approach to meeting the parallel counterfactual trends assumption.

I aggregate group-time average treatment effects, generated using the Callaway and Sant'Anna method, into (1) an event study and (2) an overall average treatment effect. The results indicate no significant impact of SHBs on the overall labor supply of mothers.

However, among all mothers, those with young children are known to be particularly sensitive to wages (and the cost of childcare), as demonstrated in a structural analysis by Apps, Kabátek, Rees, and van Soest (2016).

These findings suggest that it is fruitful to understand whether there are differential impacts of Salary History Bans on mothers based on the ages of their children. I estimate the effect of SHBs on maternal labor supply for the following three categories of women: (1) the full sample of mothers with children of any age, (2) the subsample of mothers who have at least one child under 5, and (3) the subsample of mothers whose children are between 5 and 18 years old. I find that Salary History Bans have the largest effect on mothers with at least one child under 5; for this group, SHBs increase the labor force participation rate by 2.44 percentage points. Employment rates increase, as well, though not significantly, and most of this increase is driven by the increase in part-time employment rate.

Salary History Bans, by raising women's wages, increase the opportunity cost of non-employment, incentivizing higher labor force participation. Moreover, apart from the earnings, higher wages can also improve access to childcare. Consequently, mothers who are currently out of the labor force are likely to increase their labor supply, while those considering exiting are now more inclined to remain, so as not to forgo these higher earnings. These findings align with the mechanisms discussed, demonstrating that higher wages particularly motivate mothers with young children to enter or remain in the workforce. Furthermore, the increase in part-time work among this group of mothers reveals that barriers to part-time work are lower for mothers with young children. Thus, it is possible that Salary History Bans may influence career trajectories of mothers indirectly by boosting initial engagement with the workforce through part-time work, helping them overcome barriers to full-time employment once they are ready.

There is a nascent body of literature examining the effects of Salary History Bans (SHBs) on labor market outcomes, particularly the gender pay gap. Agan, Cowgill, and Gee (2021) provide experimental evidence that employer compliance with SHBs is influenced by voluntary disclosure behavior, which varies significantly by the job candidate's gender. However, empirical studies using difference-in-differences methods consistently find that SHBs reduce the earnings gap across the United States through increased wages for women (Sinha, 2022; Bessen, Meng, and Denk, 2021). For example, Hansen and McNichols (2020) employ a synthetic control approach to show that California's SHB narrowed the gender pay gap, primarily through increased wages for women. While this research demonstrates SHBs' success in addressing wage disparities, it focuses primarily on employer-side behavior and wage effects.

My research complements this literature by investigating how wage increases, driven by SHBs, influence labor supply outcomes, specifically labor force participation, employment, and hours worked among mothers. Notably, my findings indicate that SHBs increase labor force participation rates among mothers with young children and may also boost part-time employment for this group. These results align with Hansen and McNichols (2020), who find that the narrowing gender pay gap is largely driven by higher wages for women with children aged five and older. However, my findings reveal a distinct effect of SHBs on younger mothers, whose labor supply is more responsive to wage changes.

A potential mechanism for these findings is forward-looking behavior: mothers with young children may adjust their labor supply in response to observed wage gains among women with older children, aiming to secure similar future earnings potential. This suggests that SHBs not only address wage disparities for less elastic labor supply groups (such as mothers with older children) but also incentivize labro force participation among younger, more wage-sensitive mothers. These complementary findings highlight the broader implications of SHBs on labor market dynamics beyond wage outcomes.

This suggests that SHBs have a dual impact: they not only address wage disparities for mothers whose earnings are less sensitive to labor supply adjustments but also actively encourage labor market participation among mothers who are more responsive to wage incentives.

The remainder of the paper is arranged as follows: in Section 2, I provide a background on Salary History Ban policy; in Section 3, I discuss the data and methodology used in my analyses; and in Section 4, I discuss results.

2 Background on Salary History Bans

2.1 Conceptual Framework: Theoretical Considerations and Empirical Evidence

Salary History Bans operate on the premise that, when employers rely on salary history to set offers of pay, initial compensation disparities can be perpetuated from job to job. This practice is particularly concerning for historically underpaid groups, such as women and Black workers, as it can reinforce long-standing pay inequities. Moreover, asking for salary history is a common hiring practice, with experimental evidence from Barach and Horton (2021) revealing that about half of all employers rely on this information during candidate screening. The study also finds that when salary history access is restricted, employers broaden their applicant pool to include candidates with less competitive prior earnings, ultimately hiring successful applicants from this more diverse compensation background. This suggests that SHBs may mitigate portions of gender pay gaps and race pay gaps that stem from the job-to-job perpetuation of lower salaries.

It has been suggested that Salary History Bans could have ambiguous effects on compensation. The literature on statistical discrimination highlights that when employers lack sufficient productivity signals, they may rely on stereotypes based on observable characteristics. For instance, Doleac and Hansen (2020) demonstrate that "Ban the Box" policies, which prevent employers from inquiring about criminal records, have led to increased discrimination against Black male candidates, as employers statistically associate this group with higher rates of criminal history. Similarly, SHBs, which withhold salary history, could theoretically lead employers to assume that women have less competitive past earnings, resulting in lower salary offers.

Nonetheless, empirical research on SHBs has not found evidence to support this theoretical concern. In fact, evidence from empirical suggests that SHBs are effective in reducing the gender pay gap by boosting wages for women, especially those aged 35 and older. This pronounced effect on the gender pay gap for older cohorts stands to reason, as younger cohorts of women and men typically enter the workforce with similar starting salaries. [6]

For younger cohorts of women and men, pay disparities primarily arise later in the career, from the "motherhood gap;" high childcare costs and parental preferences for one parent to spend more time with young children lead many mothers to reduce their labor supply or exit the workforce entirely after having children. In contrast, fathers typically show little change in labor supply, influenced by societal norms and expectations of higher earnings compared to their spouses. Once their children become eligible for public kindergarten, mothers who had reduced their hours or left the workforce are often ready to re-enter or increase their labor supply, marking a transition to a new phase in their life cycle.

Evidence from prior studies indicates that the implementation of Salary History Bans (SHBs) leads to an increase in labor demand for women, as employers make higher wage offers. Consequently, I hypothesize that mothers respond to these wage increases by raising their labor supply. Additionally, mothers who might otherwise consider exiting the labor market are more likely to remain employed, as the opportunity cost of leaving the labor force rises with higher potential earnings.

Furthermore, maternal labor supply responses to SHBs may be more pronounced among mothers with younger children. As discussed above, mothers with at least one child under age 5 are often at a stage in their life cycle where they are prepared to increase their labor supply, after having previously reduced it. For these mothers, wage changes can be particularly salient; given their high

[6] Link

responsiveness to wage fluctuations, they are likely to exhibit the strongest labor supply changes to SHBs. In contrast, mothers whose children are all older than 5 may have already re-entered the labor force, as their childcare costs are relatively lower. The summary statistics in Table 1 support this notion, showing that mothers with older children have higher employment rates than those with younger children. [7] Consequently, we can expect mothers with older children to exhibit a smaller labor supply response to the wage increases prompted by SHBs.

2.2 Policy Background and Rollout

Salary History Bans have their roots in advocacy movements and legislature. Massachusetts is widely recognized as the first state to pass a formal Salary History Ban in August 2016, banning employers from asking about potential candidates' salary histories.[8] Many states quickly followed its example; as of September 2023, All-Employer SHBs have been adopted by 17 states, with many smaller administrative units, such as the city of Cincinnati, Ohio and the county of Lehigh in Pennsylvania, enacting their own SHBs, even in the absence of statewide policies.

There are two main types of Salary History Bans enacted at the state level. The first, known as a Public Sector Salary History Ban (Public SHB), is established by a governor's executive order and restricts only state and local government employers from asking job candidates about their salary history.[9] Four states, Michigan, North Carolina, Pennsylvania, and Virginia, as well as the District of Columbia, have implemented a Public SHB.[10]

In contrast, All-Employer Salary History Bans are enacted through legislation and apply to all employers within the state, prohibiting them from inquiring about salary history from any job can-

[7] For a full discussion of this table, see Section 5.

[8] Link
Link
Although Massachusetts was the first state to pass a Salary History Ban, Oregon was the first state to implement one, in 2017.

[9] Though under any form of Salary History Ban, employers can still access information about a candidate's resume and job history.

[10] In addition, before implementing All-Employer SHBs, Illinois, New Jersey, and New York had also enacted Public SHBs through executive orders.

didate. The broader scope of All-Employer SHBs creates a markedly different policy environment with implications for both the public and private sectors. This study focuses on the All-Employer SHB due to its wider labor market impact and greater popularity (only four states have implemented Public SHBs while having no All-Employer SHBs in effect). Furthermore, the potential ambiguity in Public SHBs' effects on labor market outcomes complicates the analysis and interpretation, prompting the exclusion of states with only a Public SHB from this study.[11]

In Figure 1, each bar represents the Salary History Ban implementation status of an All-Employer SHB implementing state. Each state represented in this figure is one of the seventeen SHB implementing states. When the policy has not yet been implemented, the bars are shaded gray; once the policy is effective in that state, the bar is shaded green. This figure highlights the staggered implementation of SHBs across states, offering an ideal setting for the empirical strategy given by Callaway and Sant'Anna (2021).

Because the analysis period ends in March 2020[12], four of the 17 states that implemented All-Employer SHBs appear as though they never implement the policy in this analysis period. These so-called "eventually-treated" states are important in the robustness analyses that I present in Section 6.

The five earliest SHB implementers, Oregon, Delaware, California, Massachusetts, and Vermont, with SHB implementation dates on or before July 2018 (indicated by the dotted red line), constitute the treatment group in the main analyses presented in this table. In Figure 1, the five earliest implementers are distinguished from the states which implement SHBs between August 2018 and March 2020 using a darker shade of green. In Section 4, I discuss the methodological choice of balancing the panel and using the "5 Earliest Implementers" as the treatment group.

Finally, two states, Michigan and Wisconsin, forbid the enacting of any Salary History Ban within each state. This action effectively maintains the status quo where employers are permitted to ask about salary history. Consequently, both Michigan and Wisconsin are treated similarly to states

[11] I discuss in detail the states excluded from my analysis in Section 4.

[12] Please see Section 4 for a detailed discussion on the time period of the analysis.

that never enact an SHB in this paper.

The following section outlines the data and methodology used in this study.

3 Data

There are two main sources of data I use in my analyses; one for the the treatment variable (Salary History Ban implementation dates) and one for the control and outcome variables.

3.1 Treatment Variable: Salary History Ban Implementation Date

In my analysis, the treatment date refers to the policy's implementation date – that is, the date that the policy goes into effect. A comprehensive running list of implementation dates for each Salary History Ban – covering both Public and All-Employer Bans at state and local levels – is compiled and made available online by HR Dive, a journal covering topics relating to human resources.[13] In the next section, I discuss the data source for the outcome and control variables.

3.2 Labor Supply and Control Variables: Current Population Survey Data

I use data from the Current Population Survey's Basic Monthly Files (CPS), maintained by the Integrated Public Use Microdata Series (Flood, King, Rodgers, et al., 2023), for the period of January 2010-March 2020. The CPS is an unbalanced panel of data with observations at the individual-year-month level. Individual respondents in each household are surveyed over four consecutive calendar months per year for two consecutive years. Individuals are surveyed in the same calendar months in both years; that is, they are surveyed in months 1-4, not surveyed in months 5-12, then surveyed again in months 13-16.[14] In each survey month, survey respondents

[13] "Salary history bans: A running list of states and localities that have outlawed pay history questions", 2024 Link to HR Dive
Salary History Bans are implemented from 2016-present, and HR Dive updates their list regularly. I most recently accessed this list in September 2023; my data is current as of that date.

[14] For example, if an individual is surveyed in December 2013, January 2014, February 2014, and March 2014, they will be surveyed again in December 2014, January 2015, February 2015, and March 2015.

are inquired about their demographics, labor market activity, educational attainment, and living and family arrangements, among various characteristics.

In order to obtain a sample of mothers, I limit the sample to those who are between 22-45 years of age, those who identify as female, and those who have indicated that they have any of their own children, 18-year-old and younger, living with them. The age restriction allows me to focus on mothers who are most likely to be in their prime working years and actively engaged in the labor market.

There are four variables I use to understand labor supply behavior. First, the CPS asks respondents whether they are participating in the labor force, and respondents respond affirmatively or negatively. Second, respondents are asked about their employment status; if individuals indicate that they are working or that they have a job but did not work in the previous week, I consider them to be employed for purposes of my analysis.[15] Finally, respondents are asked, if they are employed, whether they work part-time or full-time. I construct two variables based on individuals' responses.

Control variables used in my analyses are educational attainment and age, both recorded in the CPS. I bin educational attainment into six categories: High School (No Degree), High School Diploma or Equivalent, Some College, Associate Degree, Bachelor's Degree, and Advanced Degree. I incorporate age into my analyses using categorical dummies for each age in years.

In addition, in a heterogeneity analysis, I study policy effects for married and unmarried women, separately. The CPS asks respondents their marital status; if individuals respond "Married spouse present" or "Married, spouse absent," they are treated as married in my analysis; else if they indicate that they are separated, divorced, widowed, or never married/singe, I consider them to be unmarried.

[15] I drop observations where individuals have indicated that they are in the armed forces.

3.2.1 Heterogeneity by Age of Children

The CPS collects detailed information on the number and age of a respondent's own children in the household. Using this, I explore the SHB policy effect on three different groups of mothers: (1) "Any Age" refers to the full sample of mothers with children of any age, (2) "Some Under 5" refers to the subsample of mothers with at least one child under 5, and, finally, (3) "5 and above" refers to the subsample of mothers whose children are all between 5-18 years of age.

I use 5 as the primary cutoff age of children for this analysis, as most 5-year-olds become eligible for public kindergarten. This allows mothers whose youngest children are around this age to transition out of intensive childcare responsibilities and to prepare to re-enter the workforce.

3.3 Aggregating the Outcomes: Pseudo Panel

To understand the effect of Salary History Bans on the labor supply of mothers, I study the effect of SHBs on four outcomes: labor force participation rate, employment rate, part-time employment rate, and full-time employment rate. Accordingly, I aggregate the individual-year-month level data from the CPS (which I treat as a cross section despite its panel nature), to a state-year-month "pseudo-panel." In my preferred specification, I first regress the outcome variable on education bins and categorical age dummies, then aggregate the residuals of the outcomes to the state-year-month level. This approach yields measures of maternal labor force participation rates (LFPR), employment rates, part-time employment rates, and full-time employment rates that have been residualized for individual-level educational attainment and age.

This "pseudo-panel" approach used helps to account for potential biases due to selection into survey response, maintaining the assumption that each state is an independent observation. Specifically, by accounting for individual-level characteristics such as educational attainment and age, this approach ensures that any observed differences in state-level outcomes (labor force participation rate (LFPR), mean weekly hours worked, and employment rate) are more likely to be attributable to the Salary History Ban rather than shifts in the demographic composition of the survey respondents over time.

In the next section, I discuss the methodology used in my analyses.

4 Methodology

4.1 The Callaway and Sant'Anna Estimator

Salary History Bans (SHBs) are state-level policies with implementation dates determined by each state's policymakers. Due to the staggered rollout of SHBs across states, I apply the staggered-implementation policy adoption method developed by Callaway and Sant'Anna (2021).

In line with the parallel trends assumption outlined by Callaway and Sant'Anna (2021), I use two groups as the comparison for treated states: not-yet-treated states (which implement All-Employer SHBs after March 2020) and never-treated states (which never adopt an SHB). This approach assumes that, in the absence of All-Employer Salary History Bans, the trends in maternal labor supply between the treated states and the comparison states would remain "parallel," following the pre-SHB trajectory.

The primary threat to this assumption regarding the comparison group is the potential endogeneity of SHB adoption. Factors influencing a state legislature's decision to implement All-Employer Salary History Bans may also independently affect maternal labor supply trends. For example, states with more progressive policies may be more inclined to adopt SHBs and could also exhibit different maternal labor supply trends. To mitigate this concern, I conduct a robustness check using only the not-yet-treated states as the comparison group. Because these states plan to implement SHBs, this approach provides better control for the factors driving policy adoption and variations in maternal abor supply trends. Nevertheless, my main results are presented using a comparison group that includes both never-treated and eventually-treated states.

Callaway and Sant'Anna's method calculates an average treatment effect on the treated (ATT) for each cohort of states that implemented an SHB in a given time period t, referred to as "SHB cohort

g". [16] Using the potential outcomes framework, each individual ATT can then be expressed as follows:

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

In the equation above, $Y_t(g)$ denotes the labor force participation rate[17] in time period t for a state in SHB cohort g, while $Y_t(0)$ represents the labor force participation rate in time period t for never- or not-yet-treated states. The difference between these rates is averaged across all states in SHB cohort g. This measure, known as the group-time average treatment effect, serves as the fundamental "building block" for all aggregate effects presented in this paper.

Using the group-time average treatment effects, I present both an event plot to illustrate the dynamic treatment effects and an overall ATT, which can be interpreted as the two-period, "before-and-after" treatment effect of the SHB policy.

The event plot aggregates the group-time average treatment effects as follows:

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

Each $\theta_D(e)$ represents the average effect of the treatment of the Salary History Ban at a specific time period, e, after the treatment is first implemented.

To obtain the overall ATTs of Salary History Bans on the labor supply of mothers, the average effect of treatment participation for each group is calculated as follows:

$$\theta_S(g) = \frac{1}{\tau - g + 1} \sum_{t=2}^{\tau} 1\{g \leq \tau\} ATT(g,t)$$

[16] Hereafter, I refer to states that first implement an SHB in time period g "SHB cohort g." For states that are never- or not-yet-treated, g is assigned a value of 0.

[17] This applies equally to the employment rate, part-time employment rate, and full-time employment rate, depending on the outcomes analyzed. For simplicity, I refer only to labor force participation as shorthand. The same procedures are applied to all outcomes.

For each SHB cohort g, $\theta_S(g)$ represents the aggregated treatment effect of Salary History Ban on the labor supply of mothers. It averages the treatment effects across time, providing a summary measure of how the treatment each SHB cohort g over the post-treatment period. All of these cohort-specific effects are further aggregated to an overall treatment effect on the treated, as follows:

$$\theta_S^O(g) := \sum_{t=2}^{\tau} \theta_S(g) P(G = g)$$

$\theta_S^O(g)$s are the estimates reported in the ATT tables. Because I anticipate that the Salary History Ban's increase in wages will induce mothers to increase their labor supply, I expect $\theta_S^O(g)$ to be positive for all labor supply outcomes, and I discuss estimates of this parameter in Section 5.

4.2 Balancing the Panel: 5 Earliest Implementers

Callaway and Sant'Anna recommend the use of a balanced panel in their methodology. Employing a balanced panel helps mitigate biases associated with the staggered timing of policy implementation. Specifically, it addresses potential biases that arise from the rapidly changing composition of states in each period following implementation of SHBs. By maintaining a consistent set of treated states in each period following SHB implementation, I ensure that the treatment effects are not confounded by shifts in the composition of treated states over time, thereby enhancing the credibility and stability of the estimated effects.

I include the five earliest SHB implementers, Oregon, Delaware, California, Massachusetts, and Vermont, in the group of treated states. Balancing the panel to focus exclusively on these five states allows me to observe each state for 19 months during the post-implementation period. Consequently, the eight August 2018-March 2020 implementers are excluded from the sample entirely. I refer to this method of balancing the panel as the balanced panel of "5 Earliest Implementers, " or simply "5 Earliest Implementers." I provide illustrations of the states included in this balanced panel in Figure 1, where I shade the bars representing the post-implementation period of the 5 Earliest Implementers in dark green. In only including these states in the treated group, I impose an "implementation deadline" of July 2018, indicated using the dotted red line. I provide another illustration in Figure **??**, where the 5 Earliest Implementers are indicated using the darker green shading, and the August 2018-March 2020 implementers are given a lighter green shading. This map indicates whether a state is in the sample, and if they are in the sample, whether they are included in the treatment or comparison group. It also includes the reason for exclusion from the sample, or for treatment/comparison status if in the sample.

In the appendix, I provide versions of the main analyses using an unbalanced panel and using a balanced panel of the 11 Earliest Implementers, as well as a map illustrating the states included in the treatment/comparison groups (and the states excluded from the sample).

5 Results

In Table 1, I present descriptive statistics for the three categories of mothers (the full sample of mothers, the subsample of mothers with some children under 5, and the subsample of mothers whose children are 5-18). In this table, I report the means (and standard deviation for the Age variable) of the outcome and control variables using the underlying individual-level data from the Current Population Survey. In the first "panel," or the first four rows, I report means of the four outcome variables in my analyses.

Labor force participation rates are highest among mothers with children aged 5-18 (0.77) and lowest among mothers with at least one child under 5 (0.66). This suggests that mothers of younger children may face constraints or preferences which diminishes their likelihood of participating in the labor force. Similarly, employment rates are highest for mothers with children aged 5-18 (0.73) and lowest for those with younger children (0.61), further suggesting a connection between childcare needs of young children and mothers' labor supply. Part-time employment rates are quite similar across groups (though they are slightly higher for women with at least one child under 5), but full-time employment is noticeably higher among mothers with older children (0.57) compared to those with at least one child under 5 (0.45). Again, this could indicate a greater capacity and/or willingness for mothers with older children to engage in full-time work than mothers with younger children.

Taken together, these descriptive statistics validate that the age of children affects labor supply decisions. Mothers with younger children may face higher childcare demands and higher childcare costs. Furthermore, they may prefer to spend more time with their young children. These baseline differences across these groups of mothers offers insight into how policy effects like SHBs can be different for mothers based on the ages of their children. For instance, mothers with children under 5 may include those whose youngest are approaching public school age, which would reduce childcare demands and facilitate a return to work. Additionally, many mothers in this group weigh the costs of expensive childcare against potential earnings, making them especially responsive to changes in the wage. Thus, wage increases for women driven by policies like Salary History Bans may hold particular salience for mothers with young children.

In the remaining rows of Table 1, I report descriptive statistics for the control variables used in the main analyses, as well as variables included in heterogeneity analyses. The educational attainment distribution is nearly identical for mothers of young children and those with older children. In both groups, the majority hold a bachelor's degree, followed by those with a high school diploma or equivalent. Consequently, if mothers with younger children respond differently to Salary History Bans (and the resulting wage effects) compared to mothers with older children, this difference in response is unlikely to be attributable to educational attainment.

However, mother's age and number of children are correlated; the average age is lowest for mothers with children under 5 (32.39 years) and highest for those with children aged 5-18 (42.09 years).[18] This is expected, as younger mothers are more likely to have younger children. In addition, older mothers may be at different stages in their careers and family lives compared to younger mothers.

[18]In Table 1, I have presented age as a continuous variable; however, age is incorporated in the analysis as a categorical dummy.

Thus, if mothers of younger children respond to the wage effects of Salary History Bans differently than mothers of older children, it is important to consider that some of these differences may stem not only from differences in childcare responsibilities but also from age-related factors, such as more stable employment histories or more work experience and overall greater attachment to the labor force.

Marital status is another relevant factor, with similar proportions of married mothers across both groups (around 0.71-0.72). Since unmarried mothers may exhibit greater labor force attachment than their married counterparts, marital status is an important consideration in labor supply decisions.

Figures 3-6 illustrate the primary findings of this paper, showing event plots generated with the Callaway and Sant'Anna (2021) estimator. These plots display the dynamic effects of SHBs on four labor supply outcomes: labor force participation rate, employment rate, part-time employment rate, and full-time employment rate. Each figure is divided into two panels: Panel A presents analyses for the full sample, while Panel B focuses on mothers with at least one child under 5.

5.1 The Effect of Salary History Bans on Labor Force Participation Rates among Mothers

Figure 3 gives a representation of the dynamic effects of SHBs on labor force participation rates among mothers. In both Panel A and Panel B, the event plots show slight differential pre-trends between treated and comparison states before SHB adoption. However, these differences fluctuate around zero, with confidence bands generally encompassing zero, especially within two years preceding SHB implementation. While these pre-trend differences may suggest minor differences in labor force participation rate trajectories before the policy adoption, they remain relatively small. In general, differential pre-trends suggest that the treatment and comparison states may not follow parallel paths in the absence of SHBs, potentially biasing the estimated effects of the policy on mothers' labor force participation rate; again, however, the pre-trend differences remain small.

In Panel A, using the full sample of mothers with children of any age, labor force participation rate shows minimal change post-SHB implementation. The 95% confidence intervals largely include zero, and so, we cannot reject the null hypothesis that any of the post-treatment effects are statistically significantly different from zero.

In Panel B, which focuses on mothers with at least one child under age 5, labor force participation rate rises sharply in months 7-12 following SHB implementation. This delayed response suggests that it may take time for information about increased wages to disseminate and for mothers to start searching for jobs. These treatment effects are statistically significant, with zero falling outside of the 95% confidence intervals, suggesting that Salary History Bans do, in fact, increase labor force participation rate among mothers with young children. Taken together with the results from Panel A, this finding supports the hypothesis that the wage effects of SHBs may help reduce labor force participation barriers for mothers facing high childcare demands, which makes them more responsive to wage increases than mothers overall.

5.2 The Effect of Salary History Bans on Employment Rates among Mothers

In Figure 4, I present event plots representing the dynamic effect of SHBs on employment rates. In both Panel A and Panel B, there appear to be no differential pre-trends, suggesting that the employment rates in the treatment and comparison states would follow in parallel in the absence of SHBs.

Panel A gives a representation of the dynamic effects of SHBs on employment rates among all mothers. There is a small decrease in employment rate immediately following SHB adoption, and then some fluctuation of the post-SHB treatment effects around zero. The 95% confidence bands include zero in every period, and so, we cannot reject the null hypothesis that any of the post-treatment effects are statistically significantly different from zero.

Panel B, focusing on mothers with at least one child under 5, shows a statistically significant increase in employment rates during months 7-12 post-SHB, similar to the labor force participation results in Figure 3. Again, this delayed response likely reflects the time required for wage changes to disseminate and mothers to respond by seeking new positions, eventually finding employment. These findings suggest that SHBs increase employment rates specifically for mothers with young children. Together with Panel A's results, this supports the conclusion that SHBs may reduce barriers to workforce entry for mothers with high childcare costs, making them particularly responsive to wage increases.

The results in Figures 3 and 4 align, with both indicating that SHBs are particularly effective in reducing labor force participation and employment barriers for mothers with younger children. In the next subsection, I attempt to decompose the effect of SHBs on employment by studying the policy's effects on part-time and full-time employment, separately.

5.3 The Effect of Salary History Bans on Part-Time and Full-Time Employment Rates among Mothers

In Figure 5, I report the event plots representing the dynamic effects of Salary History Bans on the part-time employment rates among mothers. Both Panels A and B show the pre-trends fluctuating around zero, with confidence bands seldom excluding zero, suggesting no differential pre-trends. This suggests that any observed post-SHB treatment effects can be interpreted as the impact of SHB policy implementation.

In Panel A, the effects in each post-SHB adoption period oscillate around zero and are not statistically significant. While the treatment effects in Panel B are not statistically significant, either, they are largely positive, following the same trajectory in the post-SHB period as the event plots in the Panels B of Figures 3 and 4.

In Figure 6, I report event plots that represent the dynamic effects of Salary History Bans on the full-time employment rates among mothers. In both Panels A and B, there appear to be some differential pre-trends. I will proceed with caution in interpreting the post-SHB effects, as the trends in full-time employment are not parallel between treatment and comparison states in the pre-SHB implementation period; this suggests that, absent SHB implementation, they would not be parallel in the post-SHB implementation period, either. These non-parallel trends may be due to regional differences in industry composition, demographic composition, or other pre-existing factors relating to full-time employment among mothers.

In both panels, the estimated treatment effects in the post-SHB implementation period are not significant, with the magnitudes oscillating around zero. This suggests that the Salary History Ban policy does not affect the full-time employment rates among women.

These results align with the baseline differences in part-time employment, reported in Table 1; the part-time employment rate is similar among mothers with younger children (0.17) and mothers, overall (0.16), suggesting that the barriers to part-time employment are not very high for mothers of young children. Therefore, wage effects of policies like Salary History Ban may be more effective at encouraging mothers to take up part-time employment. Meanwhile, the full-time employment rate among all mothers (0.51) much higher than that of mothers with at least one child under 5 (0.45); this suggests that the barrier for mothers with young children to work full-time may be higher, due to high childcare costs or even due to their preference for more flexible hours. Moreover, they may prefer to spend more time with their young children, making them less willing to engage in full-time labor than mothers, overall.

5.4 The Effect of SHBs on Labor Supply among Mothers: Average Treatment Effects on the Treated

I report the ATTs for mothers with children of any age in Table 2. To generate the estimates in the first row, "Specification 1: No Controls," I construct a pseudo-panel by aggregating individual-level employment data to the state-year-month level, yielding labor force participation, employment, part-time employment, and full-time employment rates for each state-year-month. For the pseudo panel used in the second row, "Specification 2: Education Controls," I first regress employment on education bins to obtain residuals, which I then aggregate to the state-year-month level. For estimates in the third row, "Specification 3: Age & Education Controls," I follow the same process, regressing employment on both education bins and categorical dummies for each age in years to generate residuals before aggregation. The column headings specify the outcome used in each analysis. In all of the results reported in this table, I use the balanced panel of 5 Earliest Implementers.

The policy's effects on labor force participation and employment rates are positive, consistent with the hypothesis that SHBs would increase women's labor supply. In my preferred specification, controlling for education and age, implementing an SHB is associated with a 0.485 percentage point increase in labor force participation rate. The estimate generated using Specification 3 in column (2) suggests a slight decrease (-0.000508 percentage points) in the employment rate among mothers in response to SHBs – a nearly zero percent decrease relative to the baseline employment rate of 68%. Moreover, the 95% confidence bands render both effects statistically significantly indistinguishable from zero (standard errors are reported in the parentheses). In my preferred specification, the ATT in column (3) suggests that SHBs are associated with a 0.00368 percentage point increase in part-time employment rate; however, the 95% confidence interval for this estimate includes zero, and so this estimate is not statistically significantly different from zero. Finally, SHB implementation is associated with a decrease in the full-time employment rate by a magnitude of 0.00149 percentage points. This small effect is also statistically insignificant at the 5% significance level (with a standard error of 0.016). Based on the results in this table, the effects of SHBs on labor supply of mothers, overall, are close to zero and statistically insignificant. These results align with the event plots presented in the Panels A of Figures 3-6, where I also do not observe any significant effects of Salary History Bans on the labor supply decisions of mothers, overall.

5.4.1 Policy Effects by Ages of Children

In Table 3, I report the ATTs using the preferred specification for all three categories of mothers (the results reported in the first row are identical to those reported in the last row of the Table 2). The outcome used in each analysis is given in the column heading, and the sample used to generate each estimate is given in the row heading.

The treatment effect of the Salary History Ban is strongest for the group of mothers with some children under 5. The results indicate that SHBs may increase labor force participation rate for this group by 2.44 percentage points. This is a statistically significant effect, with a 95 percent confidence interval of (0.441,4.439). The effect on employment rate is slightly smaller, at an increase of 0.0181 percentage points. With a 95% confidence interval of (-0.0052,0.0414) this effect is not statistically significant – however, it is significant at a 15% significance level. This effect represents an increase in employment, from the baseline level of 61% (see Table 1), of 2.97 percent. The results in Columns (3)-(4) decompose this effect on wages; the estimate of the effect of SHBs on part-time employment rate is 0.0153, and the estimate for full-time employment rate is 0.00281. Though neither effect is statistically significant at the 5% level, it is apparent that most of the effect on the employment rate is driven by the increase in part-time employment rate. This reaffirms the findings from Figures 5 and 6, which also demonstrate that most of the increase in employment comes from the increase in part-time employment.

In the last row, I report ATTs which serve as estimates for the effect of SHBs on the labor supply of mothers with older children. The ATTs are, notably, negative across all outcomes in Sample 3, particularly the labor force participation rate (-0.0113) and employment rate (-0.0160), though with large standard errors (0.00958 and 0.00989), they are "noisy".

The estimates for Sample 3 suggest a few things: first, the employment rate for this group of mothers falls as a result of SHB implementation, despite the wages for women 35+ increasing (Hansen and McNichols, 2020). Future research on this policy could benefit from a more granular breakdown of children's ages within this sample, helping to identify the life stages at which mothers begin to prioritize work-life balance. Moreover, because the change in employment falls by a larger magnitude than the change in labor force participation, it is possible that firms, after a short period, terminate employment of workers in this group. Further research is needed to understand this finding.

5.5 Heterogeneity Analyses

In this section, I examine the sources of labor supply changes driven by the wage effects of Salary History Bans. I consider effects by levels of educational attainment and marital status. Ultimately, I am to determine whether SHBs encourage labor force participation among mothers who are potentially less attached to the workforce. In these heterogeneity analyses, I segment the sample by educational attainment (in Tables 4 and 5) and marital status (in Tables 6 and 7), as the Callaway and Sant'Anna method currently lacks a mechanism for interacting the policy term with heterogeneity variables.

5.5.1 Heterogeneity: Educational Attainment

I present the results of the heterogeneity analysis by educational attainment in Tables 4 for the full sample of mothers and in 5 for mothers with at least one child under the age of 5. The discussion below highlights key findings from Table 4.

For mothers without a high school diploma, the ATT for full-time employment (Column 4) reveals a significant negative effect of -0.0623 percentage points, significant at the 5% level. This indicates that SHBs may be linked to a reduction in full-time employment for this group, potentially suggesting that these mothers are less responsive to wage increases or face significant obstacles that limit their ability to increase full-time work hours. Nevertheless, the interpretation of this result should be approached with caution, as only 10% of the CPS sample falls into this educational attainment category, according to the baseline means in Table 1. Additionally, the small number of observations per state-year-month in the pseudo-panel makes this estimate more vulnerable to outliers, increasing the potential for bias.

For mothers with a high school diploma or equivalent, the ATT for employment (Column 2) is positive (an increase f 0.0363 percentage points) but not statistically significant, indicating a potential but inconclusive increase in labor supply. Meanwhile, for mothers holding a bachelor's degree, the effects across all labor supply outcomes are negative, yet none reach statistical significance. This pattern suggests that SHBs might have a limited impact on highly educated mothers, likely because they are more securely attached to the labor market and less sensitive to wage adjustments brought about by SHBs. Similarly, mothers with an advanced degree exhibit negative ATTs across all columns, none of which are significant, reinforcing the hypothesis that mothers with higher educational attainment generally exhibit lower responsiveness to wage-based policy changes.

Table 5 shows the ATTs for mothers with at least one child under 5. For mothers with a high school diploma or equivalent, the ATT for employment (Column 2) is positive and statistically significant at the 10% level, with a magnitude of 0.0363 percentage points (standard error: 0.0187). This finding implies that SHBs might boost employment for this group, though the persistent issue of small sample sizes warrants a careful interpretation of these results.

Mothers with an associate degree experience significant negative effects on both labor force participation (Column 1) and employment (Column 2), with ATTs of -0.0383 and -0.0400 percentage points, respectively, both statistically significant at the 5% level. However, given concerns over small sample sizes and the susceptibility to outliers, these estimates should also be treated with caution.

For mothers with advanced degrees, the ATTs for labor force participation (Column 1), employment (Column 2), and part-time employment (Column 3) are negative, with the most pronounced effect observed for part-time employment at -0.0780 percentage points, significant at the 10% level. In contrast, the ATT for full-time employment (Column 4) is positive at 0.0238 percentage points, though it is not statistically significant. This may suggest a potential shift from part-time to full-time employment among advanced degree holders with young children, possibly as they transition to more permanent roles reflecting their higher education. Nonetheless, given that advanced degree holders make up only 3% of the baseline sample in Table 1, concerns about sample size and outlier influence remain, requiring a cautious interpretation of these results.

Although SHBs are designed to restrict employer access to candidates' salary histories—-an action particularly relevant for higher-wage, salaried positions—-the results given here are somewhat unexpected. Yet, the limited sample size for several educational attainment categories, combined with the relatively small number of observations per state-year-month in the pseudo-panel, introduces uncertainty and warrants careful consideration when interpreting these ATT estimates.

5.5.2 Heterogeneity: Marital Status

ATT estimates given in Table 6 represent the policy effect of SHBs on the labor supply of mothers, overall. Many of these ATTs are not statistically significant. However, the treatment effects for unmarried mothers are more positive than for married women. This is contrary to prior expectation, as unmarried women tend to be more attached to the labor force than married women however, the decomposition of the employment effect between part-time and full-time employment shed some light – while unmarried mothers appear to be moving from part-time to full-time employment, there appears to be a slight trend in the reverse among married mothers. This suggests that perhaps unmarried women are able to move towards more stable employment as a result of SHBs.

In Table 7, I report ATT estimates for the policy effect of SHBs on the labor supply of mothers with young children. Among unmarried mothers, the ATT estimate for labor force participation is 0.0792 percentage points and is statistically significant at the 1% level (standard error is given in parentheses). This suggests a strong positive effect of SHBs on labor force participation among unmarried mothers with young children, indicating that SHBs may incentivize these mothers to enter or remain in the labor force. There is also a positive and significant ATT of 0.0738 percentage points for employment among unmarried mothers, significant at the 5% level. This result aligns with the increase in labor force participation, suggesting that SHBs may facilitate employment among unmarried mothers. The ATT for part-time employment is 0.0522 percentage points, significant at the 10% level. This indicates that SHBs may encourage part-time work among unmarried mothers, possibly as a flexible entry point into the labor market for women with young children. Meanwhile, there are no notable effects of SHBs on the labor supply among married mothers, with effects small and positive but for Column (4), where the effect on full-time employment rate is slightly negative. None of the ATTs are statistically significant.

The significant positive effects across labor force participation, employment, and part-time employment rates for unmarried mothers suggest that SHBs may provide meaningful support for labor market engagement among this group, especially for part-time work as a flexible option. Again, since unmarried mothers tend to be more attached to the labor force than married mothers, these results are surprising. Married mothers with a working spouse have more flexibility or resources to re-enter the labor force in response to wage increases associated with SHBs, unlike unmarried mothers, who are largely self-reliant.

Perhaps the time horizon of this analysis offers insight into these findings; I study the effect of Salary History Bans on labor supply for 19 periods following the policy adoption. The 19-month horizon could reflect short-term shifts into part-time or flexible employment as unmarried mothers test the job market post-SHB implementation. This early period might be capturing temporary employment decisions rather than sustained full-time employment commitments, especially if unmarried mothers are using these roles to balance financial needs and childcare. However, this does not completely address the vastly larger increase in labor force participation rate among unmarried mothers, and more research is needed to understand the heterogeneous effects between married and unmarried mothers.

A final remark on these heterogeneity analyses: because I am not able to interact the policy "term" with education bins in the case of Tables 4 and 5) and marital status (in Tables 6 and 7, I need to segment the sample into the groups based on educational attainment and marital status. By breaking the sample into discrete groups instead of interacting SHBs directly with these characteristics, this approach provides distinct estimates of the treatment effect for each subgroup without explicitly comparing them within a single model.

6 Robustness Analysis

In the results presented so far, the comparison group includes both never-treated states (those that never implement an All-Employer Salary History Ban) and not-yet-treated states (those planning to adopt an SHB after March 2020, which marks the end of my analysis period). However, there is a concern that the treatment group—consisting of the five earliest implementing states—may differ fundamentally from states with no plans to adopt an All-Employer SHB. To address potential unobservable differences between treated and never-treated groups, Callaway and Sant'Anna suggest using only the not-yet-treated states as a comparison group for robustness. In this section, I present the results of this robustness check, where I use the four eventually-treated states as the sole comparison group, excluding the never-treated states, entirely. I provide a brief discussion of the event plots in Figures 7-10, as well as the ATTs reported in Table A2.

In all Panels A and B of Figures 7-10, the pre-trends no longer appear, with the 95% confidence intervals consistently encompassing zero and the pre-implementation period fluctuating around zero. This suggests that any observed changes in trends during the post-implementation period are most likely attributable to the effects of the Salary History Ban. Both labor force participation rates 7 and employment rates 8 exhibit similar trajectories in the post-implementation period. For mothers overall (Panels A), the effects initially decline but then trend upward, indicating a gradual increase in labor force participation and employment rates. Nevertheless, most of these treatment effects remain negative and are not statistically significantly different from zero. In contrast, for mothers with younger children (Panels B), there is an initial drop following SHB implementation, but the effects rise quickly, turning positive by month 9. Although these treatment effects are not statistically significant, the upward pattern is notably pronounced. The event plots from this robustness analysis corroborate the main analysis results (Figures 3 and 4) and better satisfy the parallel counterfactual trends assumption compared to the original plots.

The event plots illustrating the dynamic effects of SHBs on part-time employment rates among mothers, presented in Figure 9, reveal a clear increase in part-time employment following the policy's adoption. In Panel A, for mothers overall, the effects, although not statistically significant, are consistently positive. In Panel B, for mothers with younger children, the treatment effects are predominantly positive, with those around month 12 reaching statistical significance at the 5% level. This event plot not only corroborates the findings from the main analysis (Figures 5) but also demonstrates a stronger adherence to the parallel counterfactual trends assumption compared to the original plots.

Finally, in Figure 10, I give the event plots that represent the dynamic effects of Salary History Bans on the full-time employment rate among mothers. In both panels, while the post-period treatment effects are not statistically significant, they are mostly negative. This is a slight difference from the main analysis plots, which showed fluctuations of the effects around zero.

The ATTs presented in Table A2 provide an understanding of the impact of SHBs on labor supply using a more stringent parallel trends assumption. For mothers with children aged 5-18, there is a decline in labor force participation, employment, and full-time employment rates. In contrast, mothers with at least one child under 5 experience a modest increase in labor force participation and a more substantial rise in employment rates. This suggests that mothers in this group may find it easier to secure employment. The observed increase in employment is primarily driven by a rise in part-time employment rates, which increase by 0.0255 percentage points. Notably, the part-time employment rate also rises for mothers with older children, while full-time employment rates decrease across both groups.

7 Conclusion

This study provides new insights into the effects of Salary History Bans (SHBs) on the labor supply decisions of mothers, with particular attention to variation by age of children. The findings suggest that SHBs have a nuanced impact on labor supply, primarily encouraging part-time employment and labor force participation among mothers with young children, rather than full-time employment across all mothers. This outcome underscores the role of childcare demands and preferences for flexible work arrangements as influential factors for mothers re-entering or increasing their labor supply in response to wage increases brought on by SHBs.

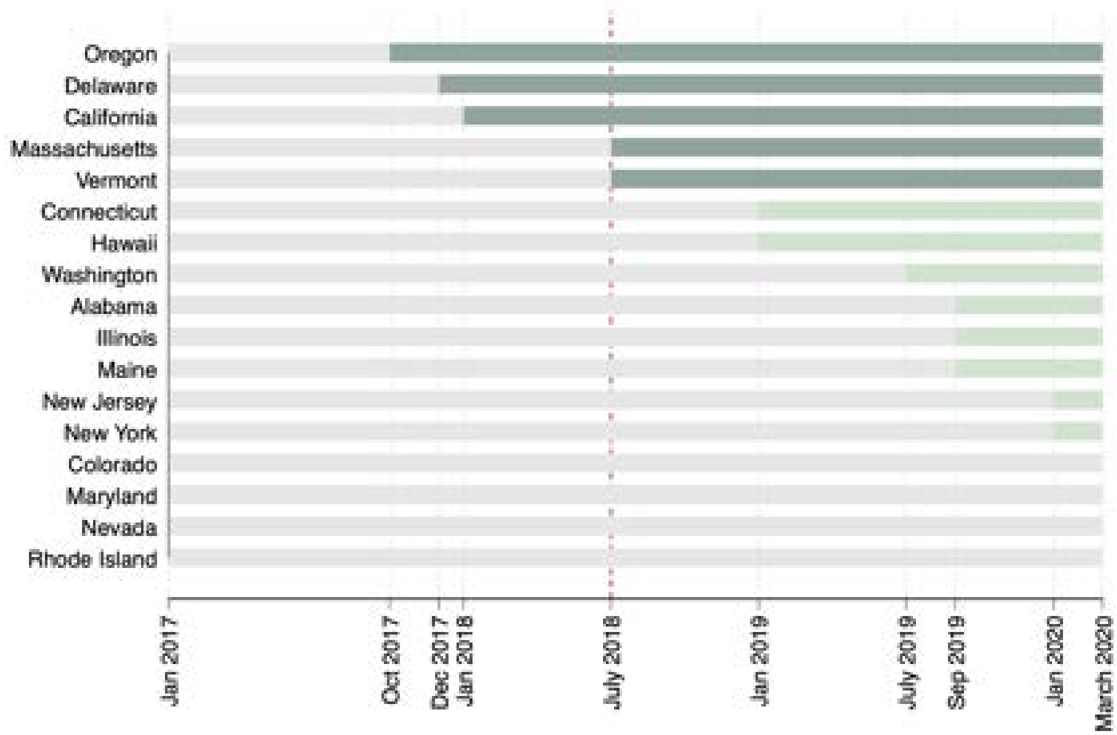
The heterogeneity analyses reveal notable patterns: SHBs appear to modestly increase employment rates for mothers with a high school diploma, particularly those with children under 5, yet have limited effects on highly educated mothers and married mothers, groups traditionally more attached to the labor market. Unmarried mothers, on the other hand, demonstrate a significant rise in labor force participation and employment, suggesting that SHBs may offer crucial wage security that incentivizes labor market engagement in this group.

Although these results align with expectations that SHBs could reduce employment barriers, particularly for those facing high childcare costs, the limited overall impact of SHBs on full-time employment suggests that other factors—such as childcare availability, life cycle stages, and pre-existing labor force attachment—play essential roles in shaping labor supply responses.

Future research can explore in depth the interesting findings relating to older mothers in this paper. Perhaps splitting up the age group of children 5-18 further will reveal the age when mothers start preferring work-life balance over career aspirations, potentially choosing part-time work over full time work. There are also heterogeneity analyses that show interesting patterns

8 Figures and Tables

Figure 1: State-Level All-Employer Salary History Ban Policy Rollout

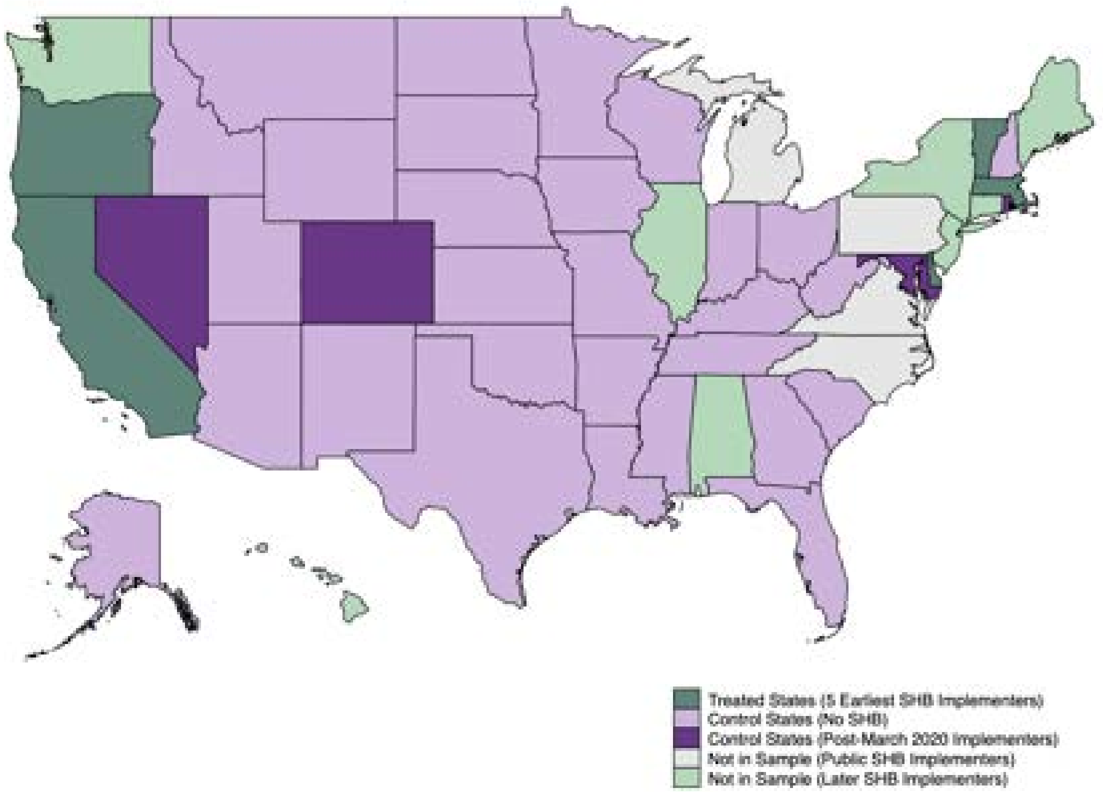


Source: HR Dive

The states given in this figure are those which have implemented a Salary History Ban as of September 2023. Each bar represents the implementation status of the SHB in the respective state through the period of January 2017-March 2020. When the SHB is "off," or not yet implemented, the bars are gray; when the bars are shaded green, it indicates that the policy is effective in that state. The first effective date of any statewide SHB is October 2017, adopted by Oregon.

In order to obtain a balanced panel where each treated state is observed for at least a year (discussed in detail in Section 4), I impose an implementation "deadline" of July 2018, represented by the red dotted line. States which implement SHBs after this deadline (but before March 2020) are not included in my analysis, and their exclusion from the sample is indicated using the lighter green shading. The five states whose implementation dates occur on or before July 2018 constitute the treatment group that I refer to throughout this paper as the "5 Earliest Implementers" (Oregon, Delaware, California, Massachusetts, and Vermont); I distinguish these implementers of SHBs using the darker shade of green. I set the cutoff for the analysis period at March 2020 across all analyses; therefore, Colorado, Maryland, Nevada, and Rhode Island, whose SHB effective date occurs beyond March 2020, appear as control states in all analyses.

Figure 2: All-Employer Salary History Ban Implementation
"5 Earliest Implementers"



Source: HR Dive

The map above represents the status of Salary History Ban implementation in each state (as of September 2023). I also indicate whether each state is part of my sample, and if so, whether it belongs to the treatment or comparison group. The first category, "Treatment States," includes the 5 Earliest Implementers of state-level Salary History Bans (in order to obtain a balanced panel, discussed in detail in Section 4). The "Control States" consist of two subgroups: (1) states that have never enacted an SHB as well as (2) states that have enacted an SHB but plan to adopt the policy after March 2020, the end of my analysis period.

Finally, there are two groups of states excluded from my analyses: first, states that have enacted a Public SHB, due to their significantly different labor market and policy environments; second, to maintain a balanced panel of states and ensure at least twelve months of post-SHB implementation observations, I exclude states that implemented a Salary History Ban between July 2018 and March 2020. I discuss this balanced panel in further detail in Section 4.

Table 1: Descriptive Statistics

	Any Age		Some Under 5		5 and Above	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Labor Force Participation	0.72		0.66		0.77	
Employment	0.68		0.61		0.73	
Part-Time Employment	0.16		0.17		0.16	
Full-Time Employment	0.51		0.45		0.57	
Education Bins						
HS, No Degree	0.10		0.10		0.10	
HS or Equal	0.24		0.23		0.25	
Some college	0.18		0.18		0.18	
Associate Degree	0.12		0.11		0.13	
Bachelors degree	0.33		0.35		0.33	
Advanced degree	0.03		0.03		0.03	
Age	38.66	8.53	32.39	6.26	42.09	7.61
Married	0.71		0.72		0.70	

Source: CPS Basic Monthly Files, 2010-Mar 2020.

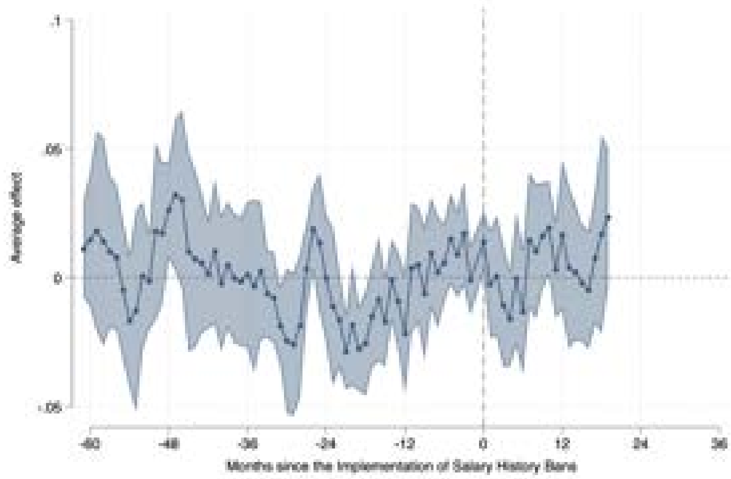
In this table, I report descriptive statistics for each of the categories of mothers. The column headings indicate the samples used to generate descriptives. "Any Age" represents the full sample of mothers – that is, women with children 18 and under living in the household. "Some Under 5" describes the subsample of mothers with at least one child under 5. "5 and Above" is the subsample of mothers whose children are between 5 and 18 years of age. "Some Under 5" and "5 and Above" are mutually exclusive and, when pooled, yield the full sample of mothers ("Any Age"). The number of observations in each of the categories, respectively, are as follows; 906,427; 292,741; 613,686.

The row headings indicate the variables for which descriptive statistics are presented. The first panel, or the first four rows, give descriptives for the four outcome variables used in my analyses. The second panel, consisting of the last 8 rows, presents descriptive statistics for control variables used in the main analyses and for variables included in the heterogeneity analyses.

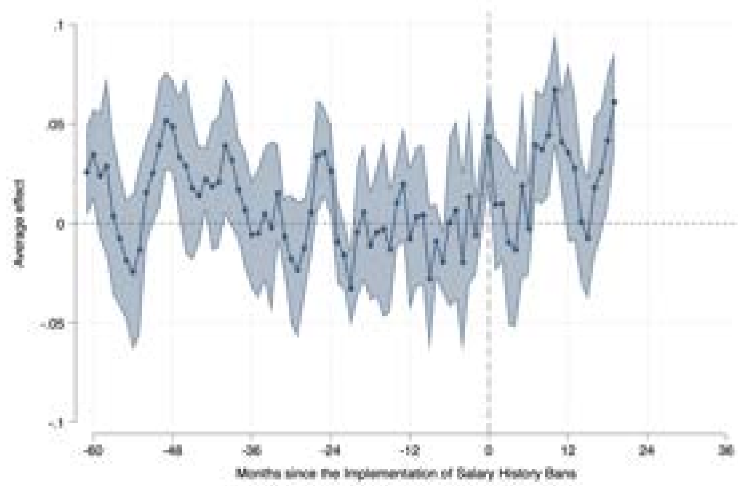
Standard deviations are not provided for the indicator variables – all variables except "Age." In this table, age is presented as a continuous variable; however, age is incorporated as categorical dummies in the analyses rather than as a continuous variable.

Figure 3: Dynamic Effects of Salary History Bans on the
Labor Force Participation Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



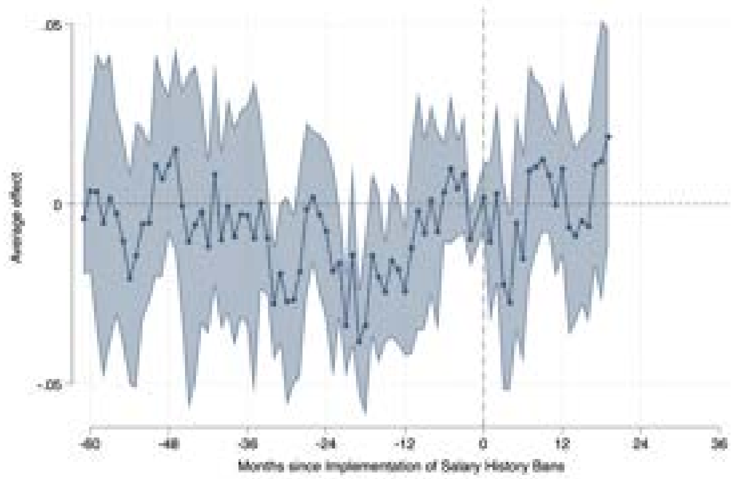
Panel B: Mothers with at least One Child Under 5



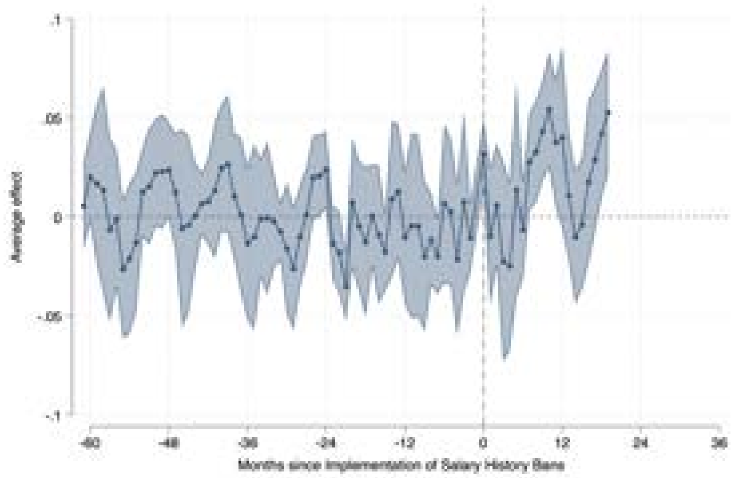
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are (1) states which never implement an SHB as well as (2) states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 4: Dynamic Effects of SHB on the
Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



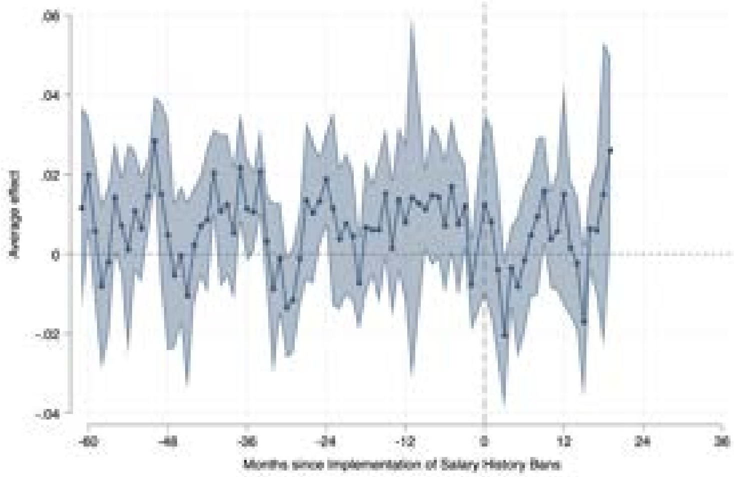
Panel B: Mothers with at least One Child Under 5



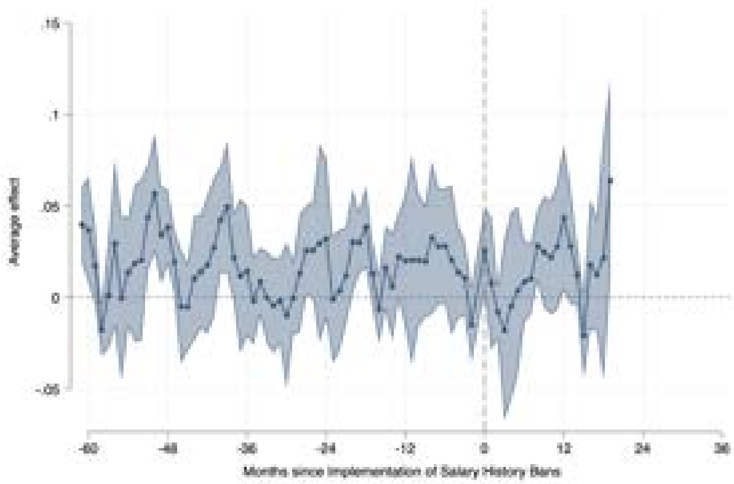
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are (1) states which never implement an SHB as well as (2) states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 5: Dynamic Effects of SHB on the
Part-Time Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



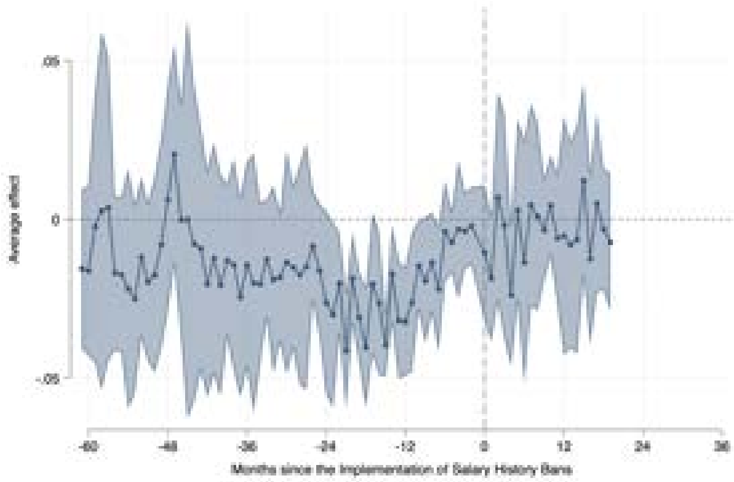
Panel B: Mothers with at least One Child Under 5



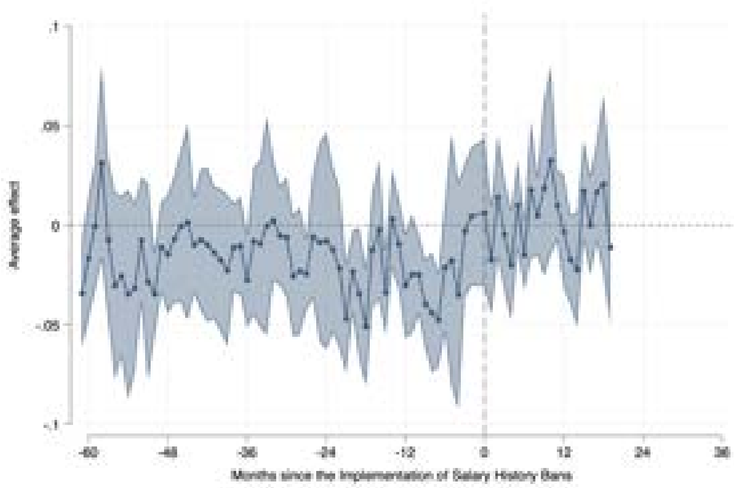
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are (1) states which never implement an SHB as well as (2) states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing part-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 6: Dynamic Effects of SHB on the
Full-Time Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5



Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are (1) states which never implement an SHB as well as (2) states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Table 2: Effect of SHB on the Labor Supply of Mothers
Mothers with Any Children in the Household, 2010-Mar 2020
DiD ATT Estimates from Callaway-Sant'Anna, 2021

	(1)	(2)	(3)	(4)
	Labor Force Participation Rate	**Employment Rate**	**Part-Time Employment Rate**	**Full-Time Employment Rate**
Specification 1: No Controls	0.00728	0.00251	0.00392	-0.00142
	(0.0101)	(0.0107)	(0.00632)	(0.0116)
Specification 2: Education Controls	0.00510	-0.000239	0.00370	-0.00394
	(0.00847)	(0.00875)	(0.00623)	(0.0104)
Specification 3: Age & Education Controls	0.00485	-0.000508	0.00368	-0.00419
	(0.00880)	(0.00913)	(0.00616)	(0.0106)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These reported estimates are average treatment effects on the treated generated using the DiD method put forward by Callaway and Sant'Anna (2021). Results presented in row 1 (Specification 1: No Controls) are estimates using the pseudo-panel obtained by aggregating each outcome to the state-year-month level. Results presented in row 2 (Specification 2: Education Controls) are estimates using the pseudo-panel obtained by first regressing the outcome variables on education bins and then aggregating the residuals to the state-year-month level. Results reported in row 3 (Specification 3: Age & Education Controls) are estimates using the pseudo-panel obtained by first regressing the outcome variables on education bins and age dummies, and then aggregating the residuals to the state-year-month level.

Table 3: Effect of SHB on the Labor Supply of Mothers
Heterogeneity by Age of Children, 2010-Mar 2020
DiD ATT Estimates from Callaway-Sant'Anna, 2021

	(1)	(2)	(3)	(4)
	Labor Force Participation Rate	**Employment Rate**	**Part-Time Employment Rate**	**Full-Time Employment Rate**
Sample 1: Any Age	0.00485	-0.000508	0.00368	-0.00419
	(0.00880)	(0.00913)	(0.00616)	(0.0106)
Sample 2: Some Children Under 5	0.0244*	0.0181	0.0153	0.00281
	(0.0102)	(0.0119)	(0.0145)	(0.00869)
Sample 3: All Children 5-18	-0.0113	-0.0160	-0.00360	-0.0124
	(0.00958)	(0.00989)	(0.0146)	(0.0225)

Standard errors in parentheses, clustered at the state level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These reported estimates are average treatment effects on the treated generated using the DiD method put forward by Callaway and Sant'Anna (2021). All estimates are generated using the pseudo-panel obtained by first regressing the outcome variables on education bins and age dummies, and then aggregating the residuals to the state-year-month level. Results presented in row 1 (Sample 1: Any Age) are estimates using the full sample of mothers with children of any age (under 18). Results presented in row 2 (Sample 2: Some Children Under 5) are estimates using the sub-sample of mothers with any number of children under 5. Results reported in row 3 (Sample 3: All Children 5-18) are estimates using the sub-sample of mothers whose children are all aged 5-18. Samples 2 and 3 are mutually exclusive, and pooled together, constitute Sample 1.

Table 4: Effect of SHB on Labor Supply of Mothers
Average Treatment Effects on the Treated by Education Level, 2010-March 2020
DiD Estimator from Callaway and Sant'Anna, 2021

Education Level:	(1) **Labor Force Participation Rate**	(2) **Employment Rate**	(3) **Part-Time Employment Rate**	(4) **Full-Time Employment Rate**
HS, No Degree	-0.0302	-0.0498+	0.0125	-0.0623**
	(0.0307)	(0.0264)	(0.0212)	(0.0222)
HS or Equal	0.0235	0.0363+	0.0117	0.0245
	(0.0180)	(0.0187)	(0.0154)	(0.0195)
Some College	0.00935	0.00694	0.0161	-0.00912
	(0.0334)	(0.0343)	(0.0121)	(0.0336)
Associate Degree	0.0326	0.0214	-0.0100	0.0314
	(0.0319)	(0.0304)	(0.0148)	(0.0381)
Bachelor's Degree	-0.00438	-0.0146	-0.00644	-0.00812
	(0.0164)	(0.0178)	(0.00619)	(0.0176)
Advanced Degree	-0.0144	-0.0270	-0.0283	0.00130
	(0.0379)	(0.0371)	(0.0604)	(0.0466)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

ATT estimates are generated using the DiD method put forward by Callaway and Sant'Anna, 2021. I use a pseudo panel to obtain these results, first regressing employment on age dummies using individual-level observations, then aggregating the residuals to the state-year-month level. The row headings indicate the sample used to obtain each result; (1) is those who have not completed high school, (2) is those who have completed high school or equivalent degree, (3) is those who have completed some college, but have not finished college, (4) is those who have an associate or 2-year degree, (5) is those who have completed a bachelors degree or equivalent, and (6) is those who have completed an advanced degree. In each of these samples, I include the full sample of mothers (with children of any age). The column headings indicate the outcome used in each analysis. Each analysis is conducted using a balanced panel where the treatment group is comprised of the 5 Earliest Implementers. More details on the balanced panel are given in Section 4.

Table 5: Effect of SHB on Labor Supply of Mothers with Some Children Under 5
Average Treatment Effects on the Treated by Education Level, 2010-March 2020
DiD Estimator from Callaway and Sant'Anna, 2021

Education Level:	(1) **Labor Force Participation Rate**	(2) **Employment Rate**	(3) **Part-Time Employment Rate**	(4) **Full-Time Employment Rate**
HS, No Degree	0.0348	0.00101	0.0193	-0.0183
	(0.0894)	(0.0837)	(0.0427)	(0.0500)
HS or Equal	0.0235	0.0363+	0.0117	0.0245
	(0.0180)	(0.0187)	(0.0154)	(0.0195)
Some College	-0.0176	-0.0177	0.00293	-0.0206
	(0.0558)	(0.0504)	(0.0439)	(0.0346)
Associate Degree	-0.0383*	-0.0400*	-0.0407	0.000735
	(0.0175)	(0.0167)	(0.0324)	(0.0378)
Bachelor's Degree	0.0198	0.00574	0.000748	0.00499
	(0.0325)	(0.0327)	(0.0240)	(0.0195)
Advanced Degree	-0.0489	-0.0542	-0.0780+	0.0238
	(0.0362)	(0.0368)	(0.0417)	(0.0576)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

ATT estimates are generated using the DiD method put forward by Callaway and Sant'Anna, 2021. I use a pseudo panel to obtain these results, first regressing employment on age dummies using individual-level observations, then aggregating the residuals to the state-year-month level. The row headings indicate the sample used to obtain each result; (1) is those who have not completed high school, (2) is those who have completed high school or equivalent degree, (3) is those who have completed some college, but have not finished college, (4) is those who have an associate or 2-year degree, (5) is those who have completed a bachelors degree or equivalent, and (6) is those who have completed an advanced degree. In each of these samples, I include only mothers with at least one child under 5. The column headings indicate the outcome used in each analysis. Each analysis is conducted using a balanced panel where the treatment group is comprised of the 5 Earliest Implementers. More details on the balanced panel are given in Section 4.

Table 6: Effect of SHB on Employment Status of Mothers
Heterogeneity by Marital Status, 2010-March 2020
DiD Estimator from Callaway and Sant'Anna, 2021

	(1)	(2)	(3)	(4)
Marital Status:	**Labor Force Participation Rate**	**Employment Rate**	**Part-Time Employment Rate**	**Full-Time Employment Rate**
Unmarried	0.0228	0.0180	-0.00671	0.0247+
	(0.0165)	(0.0139)	(0.00733)	(0.0143)
Married	-0.00221	-0.00616	0.00693	-0.0131
	(0.0126)	(0.0137)	(0.00725)	(0.0144)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

ATT estimates are generated using the DiD method put forward by Callaway and Sant'Anna, 2021. I use a pseudo panel to obtain these results, first regressing employment on age dummies and education bins using individual-level observations, then aggregating the residuals to the state-year-month level. The row headings indicate the sample used to obtain each result; "Unmarried" represents mothers who were never married, divorced, widowed, or separated, and "Married" represents women who are married. In each of these samples, I include the full sample of mothers (with children of any age). The column headings indicate the outcome used in each analysis. Each analysis is conducted using a balanced panel where the treatment group is comprised of the 5 Earliest Implementers. More details on the balanced panel are given in Section 4.

Table 7: Effect of SHB on Employment Status of Mothers with Some Children Under 5
Heterogeneity by Marital Status, 2010-March 2020
DiD Estimator from Callaway and Sant'Anna, 2021

Marital Status:	(1) **Labor Force Participation Rate**	(2) **Employment Rate**	(3) **Part-Time Employment Rate**	(4) **Full-Time Employment Rate**
Unmarried	0.0792***	0.0738**	0.0522*	0.0216
	(0.0232)	(0.0226)	(0.0255)	(0.0322)
Married	0.00441	0.00101	0.00627	-0.00527
	(0.0180)	(0.0215)	(0.0246)	(0.0113)

Standard errors in parentheses

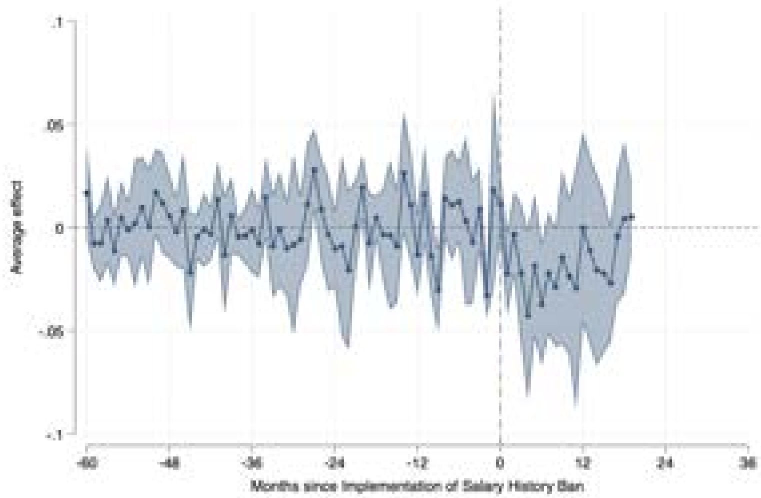
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

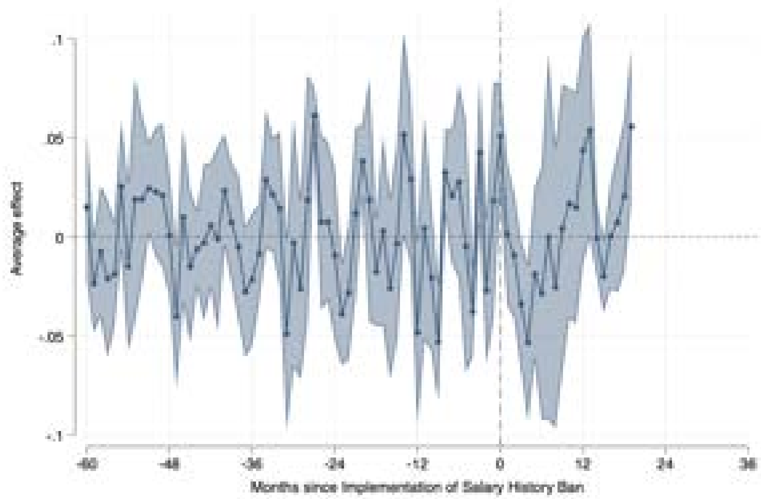
ATT estimates are generated using the DiD method put forward by Callaway and Sant'Anna, 2021. I use a pseudo panel to obtain these results, first regressing employment on age dummies and education bins using individual-level observations, then aggregating the residuals to the state-year-month level. The row headings indicate the sample used to obtain each result; "Unmarried" represents mothers who were never married, divorced, widowed, or separated, and "Married" represents women who are married. In each of these samples, I include only mothers with at least one child under 5. The column headings indicate the outcome used in each analysis. Each analysis is conducted using a balanced panel where the treatment group is comprised of the 5 Earliest Implementers. More details on the balanced panel are given in Section 4.

Figure 7: Robustness Analysis
Dynamic Effects of Salary History Bans on the
Labor Force Participation Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



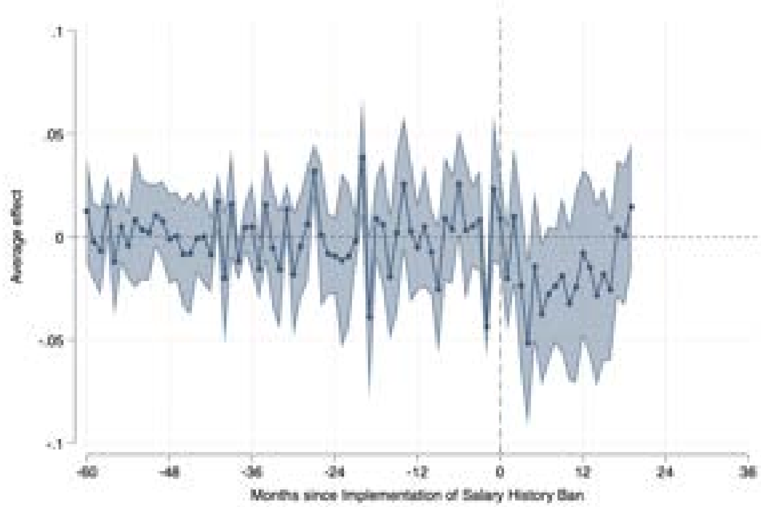
Panel B: Mothers with at least One Child Under 5



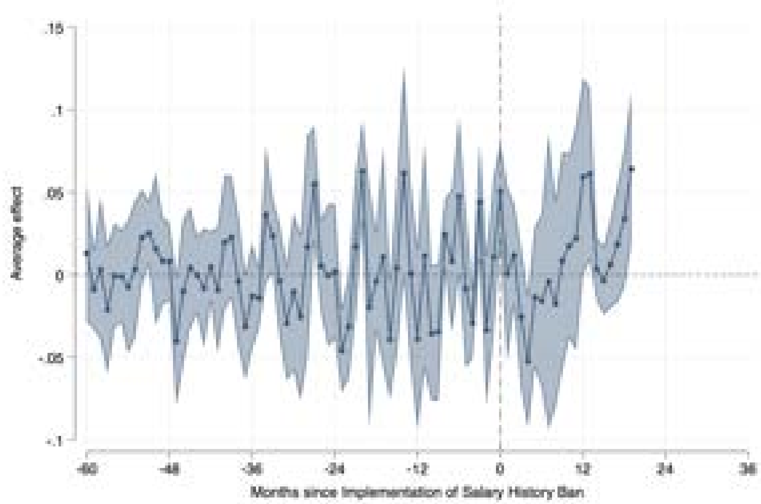
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are only states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 8: Robustness
Dynamic Effects of Salary History Bans on the
Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



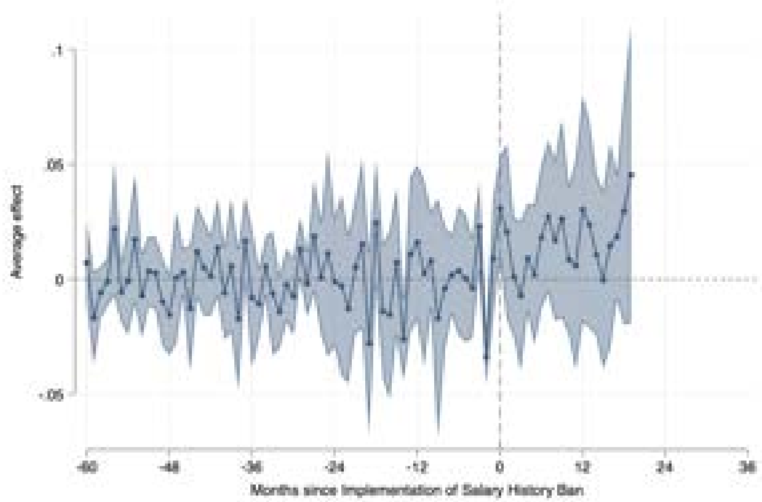
Panel B: Mothers with at least One Child Under 5



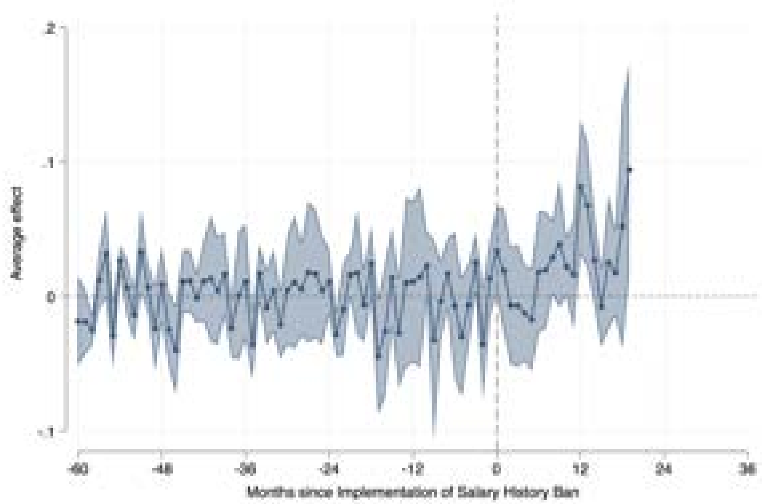
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are only states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 9: Robustness Analysis
Dynamic Effects of Salary History Bans on the
Part-Time Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



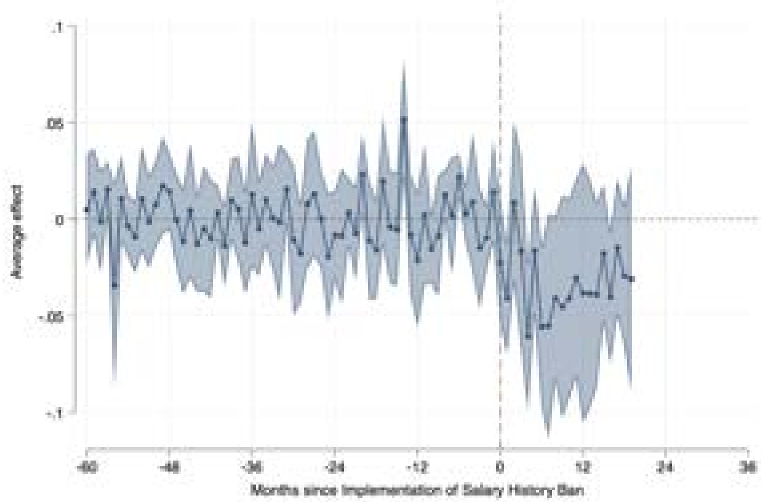
Panel B: Mothers with at least One Child Under 5



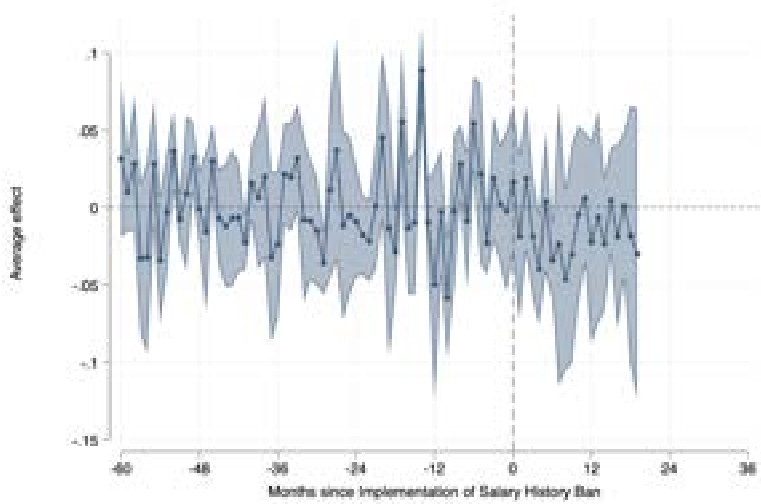
Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are only states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure 10: Robustness
Dynamic Effects of Salary History Bans on the
Full-Time Employment Rate Among Mothers
Event Study Estimates from Callaway-Sant'Anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5



Source: CPS Basic Monthly Files, 2010-Mar 2020. The treated states in these analyses are the "5 Earliest Implementers," and the control states are only states whose SHB implementation dates occur post-March 2020. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Table 8: Robustness
Effect of Salary History Bans on Labor Supply of Mothers
DiD Estimator from Callaway and Sant'Anna, 2021

	(1) **Labor Force Participation Rate**	(2) **Employment Rate**	(3) **Part-Time Employment Rate**	(4) **Full-Time Employment Rate**
Sample 1: Any Age	-0.0164	-0.0166	0.0166	-0.0332+
	(0.0136)	(0.0135)	(0.0147)	(0.0185)
Sample 2: Some Children Under 5	0.00396	0.0113	0.0255	-0.0142
	(0.0151)	(0.0154)	(0.0161)	(0.0193)
Sample 3: All Children 5-18	-0.0307*	-0.0376*	0.0105	-0.0481*
	(0.0154)	(0.0157)	(0.0179)	(0.0241)

Standard errors in parentheses

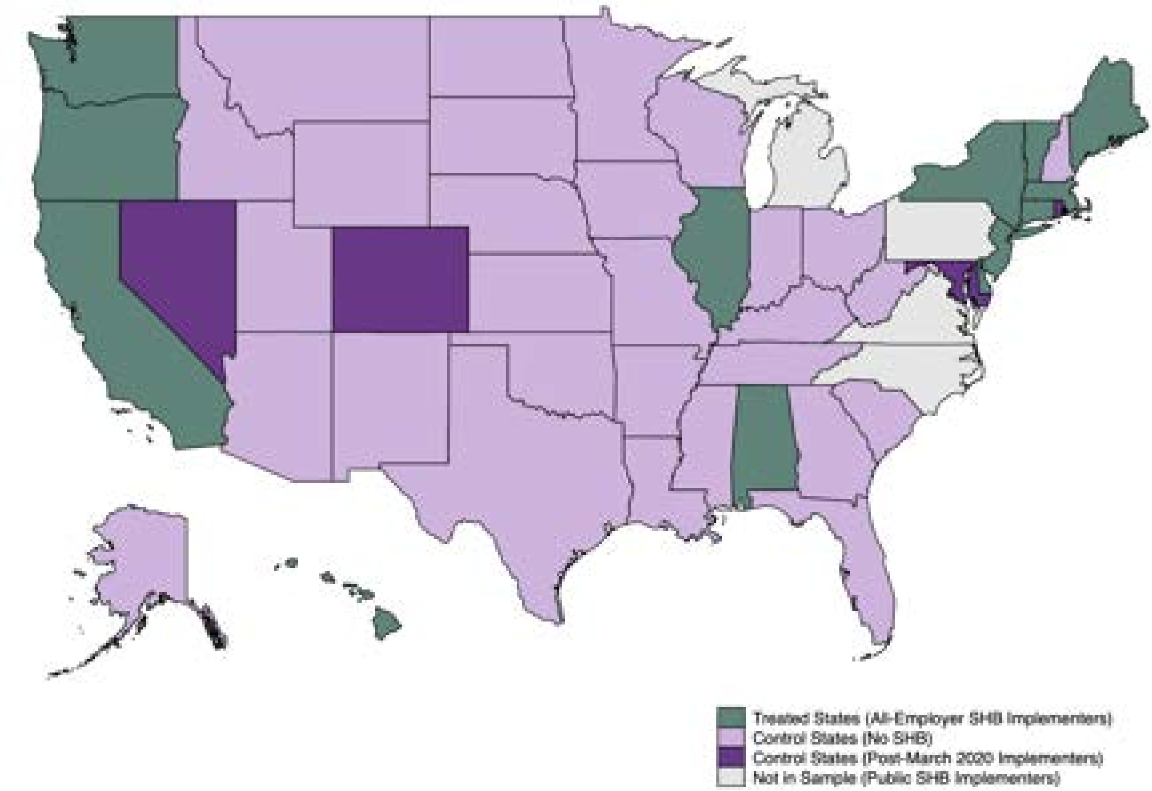
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These reported estimates are average treatment effects on the treated generated using the DiD method put forward by Callaway and Sant'Anna (2021). All estimates are generated using the pseudo-panel obtained by first regressing the outcome variables on education bins and age dummies, and then aggregating the residuals to the state-year-month level. Results presented in row 1 (Sample 1: Any Age) are estimates using the full sample of mothers with children of any age (under 18). Results presented in row 2 (Sample 2: Some Children Under 5) are estimates using the sub-sample of mothers with any number of children under 5. Results reported in row 3 (Sample 3: All Children 5-18) are estimates using the sub-sample of mothers whose children are all aged 5-18. In the above analyses, the treatment group is comprised of the 5 Earliest Implementers; the control group is comprised of states whose Salary History Ban implementation dates are post-March 2020.

A Supplemental Figures and Tables

Figure A1: All-Employer Salary History Ban Implementation
Unbalanced Panel



Source: HR Dive

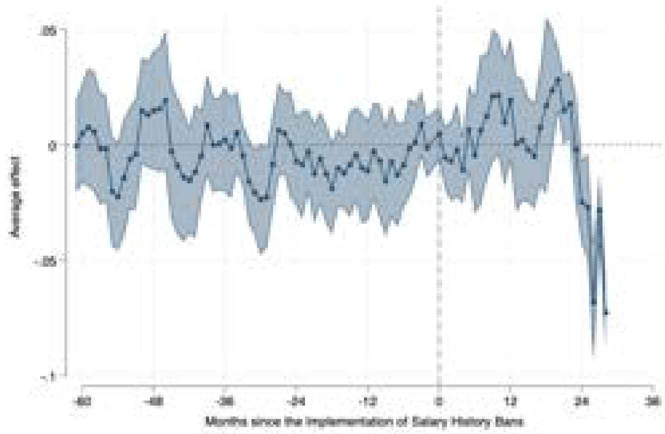
The map above represents the status of Salary History Ban implementation in each state (as of September 2023). I also indicate whether each state is part of my sample, and if so, whether it belongs to the treatment or comparison group. In the analyses that follow in this supplemental section, I use an unbalanced panel, with treatment and control groups that are represented in this map. In the unbalanced panel, earlier implementers are observed in more post-adoption periods than later implementers. I discuss this in further detail in Section 4.

The first category, "Treatment States," includes all states that implement a Salary History Bans as of March 2020. The "Control States" consist of two subgroups: (1) states that have never enacted an SHB as well as (2) states that have enacted an SHB but plan to adopt the policy after March 2020, the end of my analysis period.

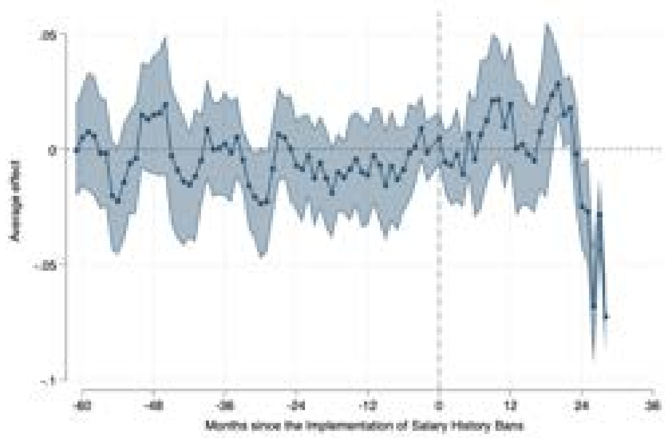
I exclude from the sample those states that have enacted a Public SHB, due to their significantly different labor market and policy environments.

Figure A2: Supplemental Analysis with Unbalanced Panel
Dynamic Effects of Salary History Bans on the
Labor Force Participation Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



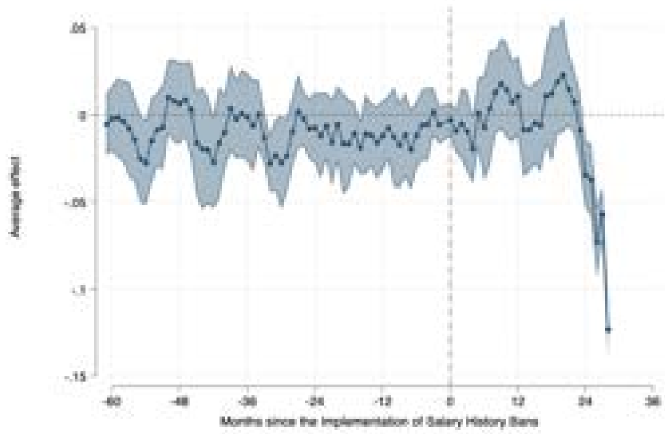
Panel B: Mothers with at least One Child Under 5



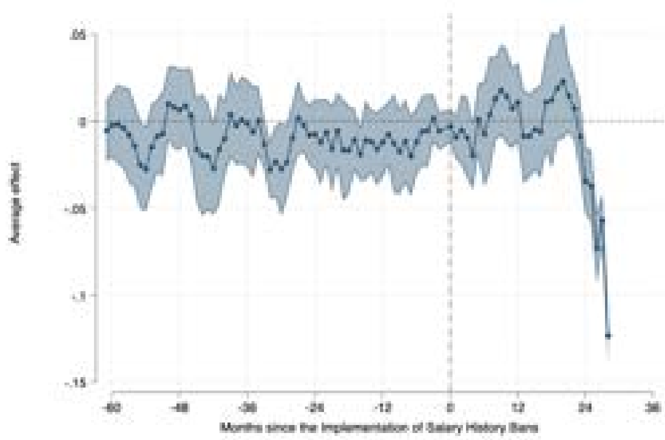
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use an unbalanced panel of states for this analysis, where states that adopt SHBs earlier are observed for a greater number of post-adoption periods than later adopters. See Figure A1 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A3: Supplemental Analysis with Unbalanced Panel
Dynamic Effects of Salary History Bans on the
Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



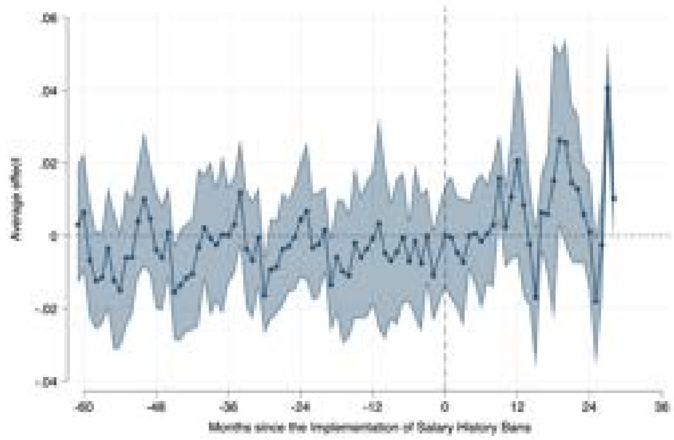
Panel B: Mothers with at least One Child Under 5



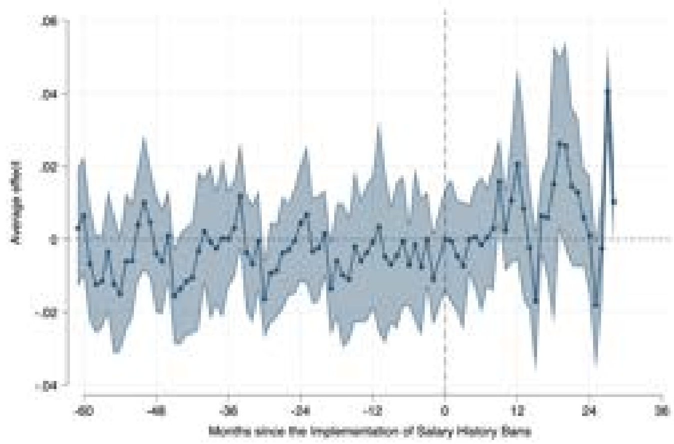
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use an unbalanced panel of states for this analysis, where states that adopt SHBs earlier are observed for a greater number of post-adoption periods than later adopters. See Figure A1 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A4: Supplemental Analysis with Unbalanced Panel
Dynamic Effects of Salary History Bans on the
Part-Time Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



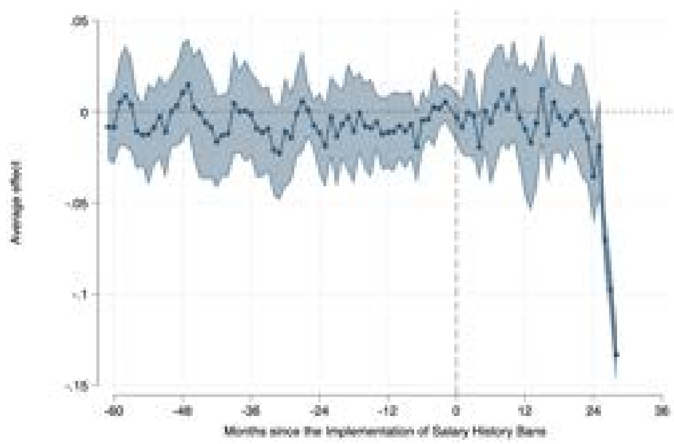
Panel B: Mothers with at least One Child Under 5



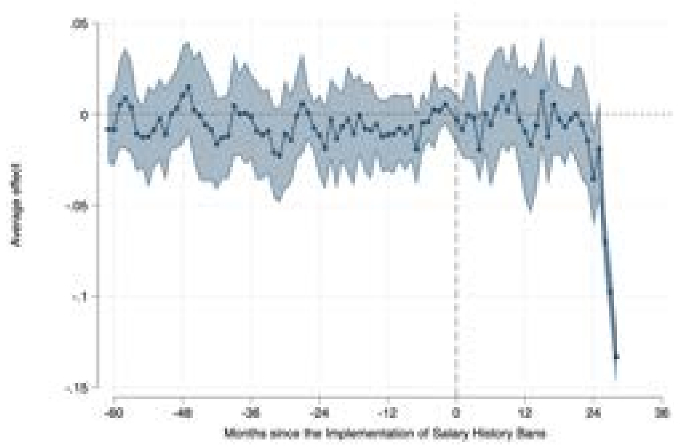
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use an unbalanced panel of states for this analysis, where states that adopt SHBs earlier are observed for a greater number of post-adoption periods than later adopters. See Figure A1 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing part-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A5: Supplemental Analysis with Unbalanced Panel
Dynamic Effects of Salary History Bans on the
Full-Time Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5



Source: CPS Basic Monthly Files, 2010-Mar 2020. I use an unbalanced panel of states for this analysis, where states that adopt SHBs earlier are observed for a greater number of post-adoption periods than later adopters. See Figure A1 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing full-time employment on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Table A1: Supplemental Analysis with Unbalanced Panel
Effect of Salary History Bans on Labor Supply of Mothers
DiD Estimator from Callaway and Sant'Anna, 2021

	(1)	(2)	(3)	(4)
	Labor Force Participation Rate	**Employment Rate**	**Part-Time Employment Rate**	**Full-Time Employment Rate**
Sample 1: Any Age	0.00338	-0.00186	0.00384	-0.00570
	(0.00801)	(0.00835)	(0.00514)	(0.00921)
Sample 2: Some Children Under 5	0.0150	0.0103	0.00911	0.00119
	(0.0114)	(0.0121)	(0.0157)	(0.00960)
Sample 3: All Children 5-18	-0.00589	-0.0116	0.00167	-0.0133
	(0.00789)	(0.00886)	(0.0144)	(0.0209)

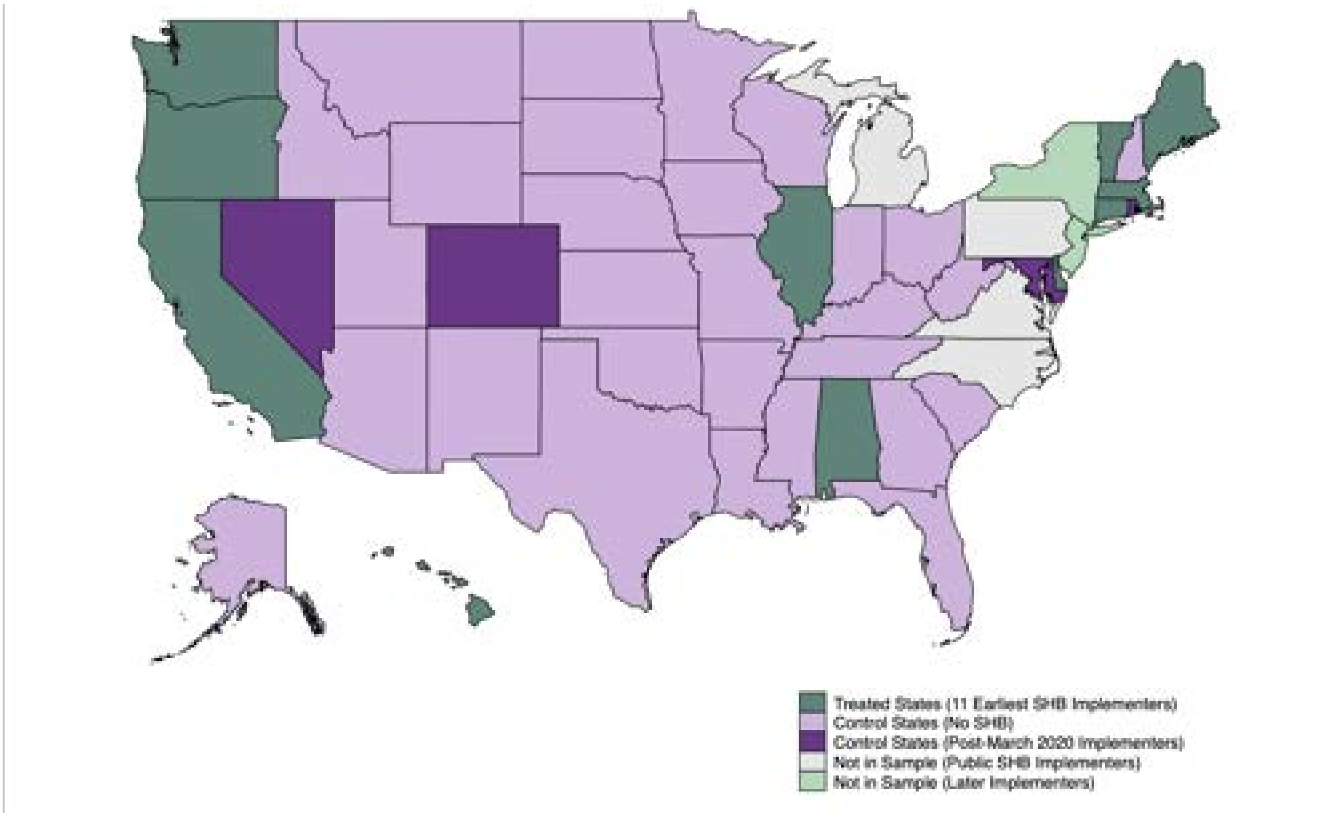
Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020.

These reported estimates are average treatment effects on the treated generated using the DiD method put forward by Callaway and Sant'Anna (2021). I use an unbalanced panel of states for this analysis, where states that adopt SHBs earlier are observed for a greated number of post-adoption periods than later adopters. See Figure A1 for details on which states constitute the treatment and control groups. All estimates are generated using the pseudo-panel obtained by first regressing the outcome variables on education bins and age dummies, and then aggregating the residuals to the state-year-month level. Results presented in row 1 (Sample 1: Any Age) are estimates using the full sample of mothers with children of any age (under 18). Results presented in row 2 (Sample 2: Some Children Under 5) are estimates using the sub-sample of mothers with any number of children under 5. Results reported in row 3 (Sample 3: All Children 5-18) are estimates using the sub-sample of mothers whose children are all aged 5-18.

Figure A6: All-Employer Salary History Ban Implementation
11 Earliest Implementers



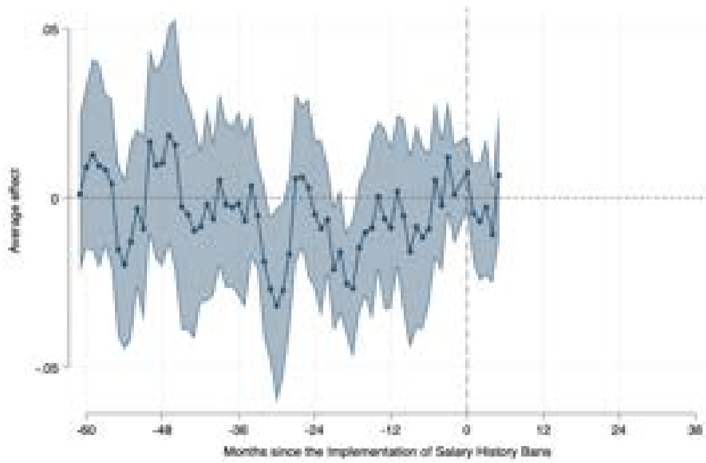
Source: HR Dive

The map above represents the status of Salary History Ban implementation in each state (as of September 2023). I also indicate whether each state is part of my sample, and if so, whether it belongs to the treatment or comparison group. In the analyses that follow in the remainder of this supplemental section, I use a balanced panel, with treatment and control groups that are represented in this map. The first category, "Treatment States," includes the 11 Earliest Implementers of state-level Salary History Bans (in order to obtain a balanced panel, discussed in further detail in Section 4). The "Control States" consist of two subgroups: (1) states that have never enacted an SHB as well as (2) states that have enacted an SHB but plan to adopt the policy after March 2020, the end of my analysis period.

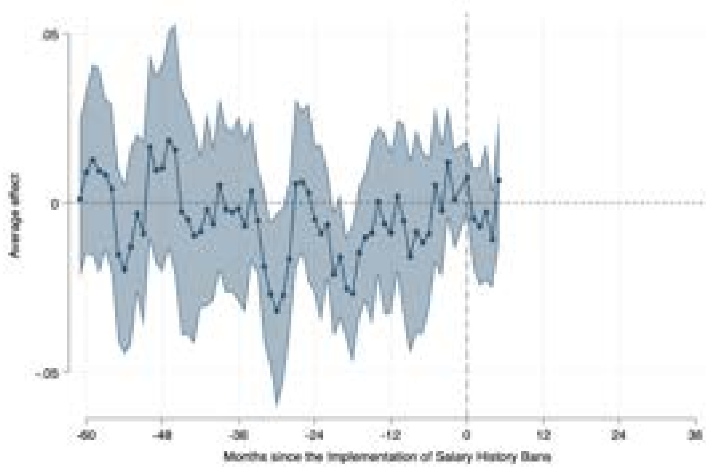
I exclude from the sample those states that have enacted a Public SHB, due to their significantly different labor market and policy environments. In addition, I exclude states that implemented a Salary History Ban between January 2020 and March 2020, as they are not observed in the post-policy adoption period for the requisite minimum of 5 year-months; this is a restriction I impose in order to achieve a balanced panel.

Figure A7: Supplemental Analysis with Balanced Panel of 11 Earliest Implementers
Dynamic Effects of Salary History Bans on the
Labor Force Participation Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



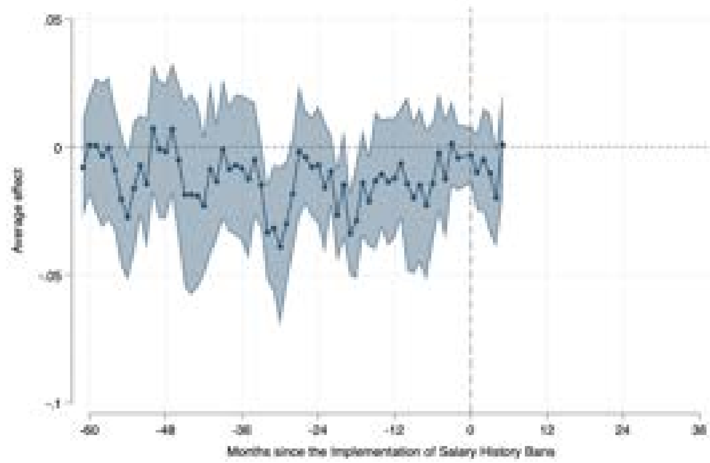
Panel B: Mothers with at least One Child Under 5



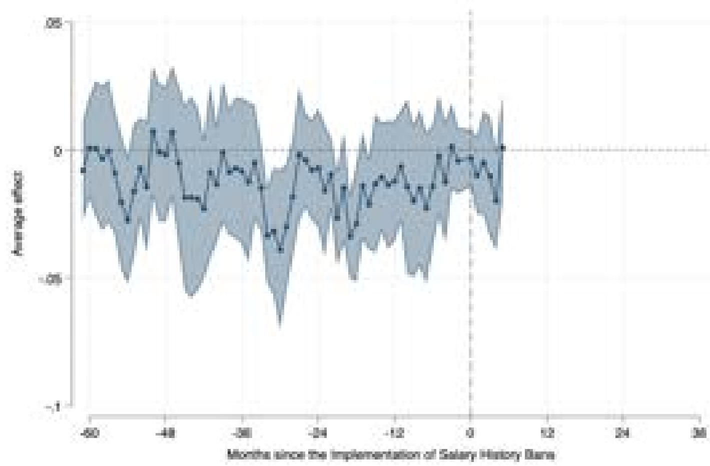
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use a balanced panel of states for this analysis, where the treated group of states are the 11 earliest implementers, and such that I am able to observe these states for a minimum of 5 year-months in the post-policy adoption period. See Figure A6 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A8: Supplemental Analysis with Balanced Panel of 11 Earliest Implementers
Dynamic Effects of Salary History Bans on the
Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



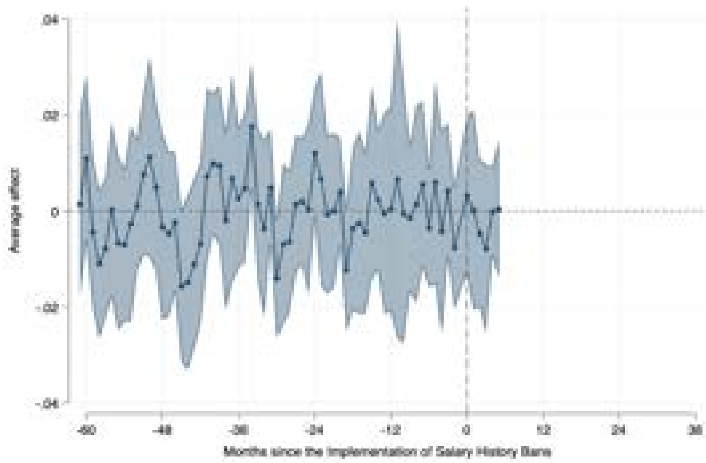
Panel B: Mothers with at least One Child Under 5



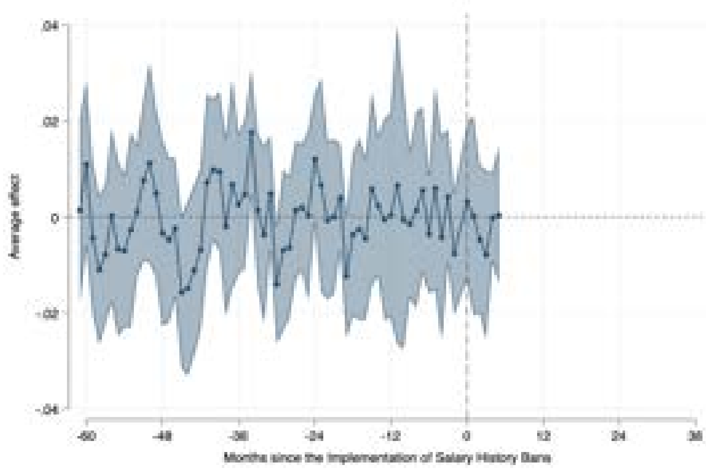
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use a balanced panel of states for this analysis, where the treated group of states are the 11 earliest implementers, and such that I am able to observe these states for a minimum of 5 year-months in the post-policy adoption period. See Figure A6 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A9: Supplemental Analysis with Balanced Panel of 11 Earliest Implementers
Dynamic Effects of Salary History Bans on the
Part-Time Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



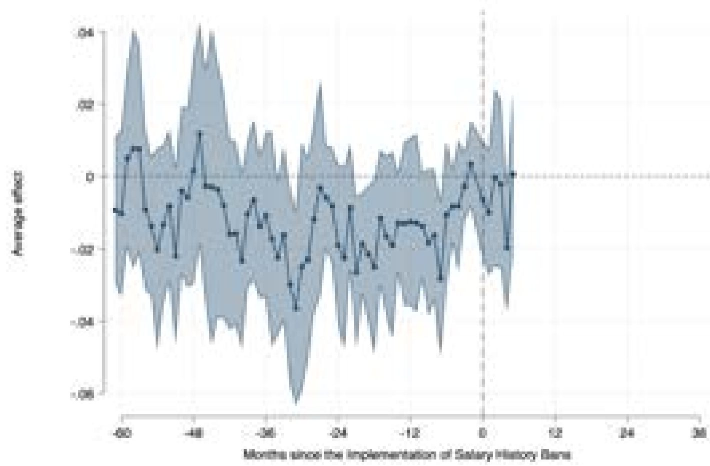
Panel B: Mothers with at least One Child Under 5



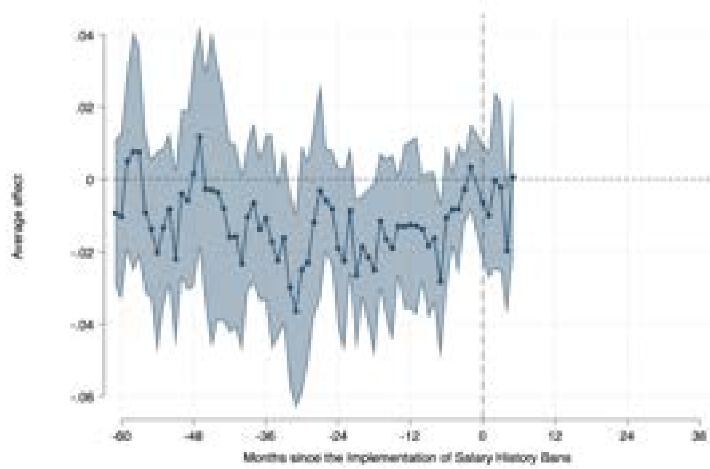
Source: CPS Basic Monthly Files, 2010-Mar 2020. I use a balanced panel of states for this analysis, where the treated group of states are the 11 earliest implementers, and such that I am able to observe these states for a minimum of 5 year-months in the post-policy adoption period. See Figure A6 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Figure A10: Supplemental Analysis with Balanced Panel of 11 Earliest Implementers
Dynamic Effects of Salary History Bans on the
Full-Time Employment Rate of Mothers
Event Study Estimates from Callaway-Sant'anna, 2021

Panel A: Mothers with Children of Any Age



Panel B: Mothers with at least One Child Under 5



Source: CPS Basic Monthly Files, 2010-Mar 2020. I use a balanced panel of states for this analysis, where the treated group of states are the 11 earliest implementers, and such that I am able to observe these states for a minimum of 5 year-months in the post-policy adoption period. See Figure A6 for details on which states constitute the treatment and control groups. Estimates reported in the event studies are generated using the pseudo-panel obtained by first regressing labor force participation on age dummies and education bins, then aggregating the residuals to the state-year-month level.

Table A2: Supplemental Analysis with Balanced Panel of 11 Earliest Implementers
Effect of Salary History Bans on Labor Supply of Mothers
DiD Estimator from Callaway and Sant'Anna, 2021

	(1)	(2)	(3)	(4)
	Labor Force Participation Rate	**Employment Rate**	**Part-Time Employment Rate**	**Full-Time Employment Rate**
Sample 1: Any Age	-0.00182	-0.00787	-0.00150	-0.00637
	(0.00628)	(0.00673)	(0.00505)	(0.00801)
Sample 2: Some Children Under 5	0.000708	-0.00546	-0.00736	0.00190
	(0.0136)	(0.0139)	(0.0144)	(0.0106)
Sample 3: All Children 5-18	-0.00223	-0.00844	0.00496	-0.0134
	(0.00772)	(0.00838)	(0.0127)	(0.0135)

Standard errors in parentheses

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: CPS Basic Monthly Files, 2010-Mar 2020. These reported estimates are average treatment effects on the treated generated using the DiD method put forward by Callaway and Sant'Anna (2021). I use a balanced panel of states for this analysis, where the treated group of states are the 11 earliest implementers, and such that I am able to observe these states for a minimum of 5 year-months in the post-policy adoption period. See Figure A6 for details on which states constitute the treatment and control groups. All estimates are generated using the pseudo-panel obtained by first regressing the outcome variables on education bins and age dummies, and then aggregating the residuals to the state-year-month level. Results presented in row 1 (Sample 1: Any Age) are estimates using the full sample of mothers with children of any age (under 18). Results presented in row 2 (Sample 2: Some Children Under 5) are estimates using the sub-sample of mothers with any number of children under 5. Results reported in row 3 (Sample 3: All Children 5-18) are estimates using the sub-sample of mothers whose children are all aged 5-18.

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