The Effect of Salary History Bans on Labor Market Reattachment

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October 25, 2025

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Abstract

Intended to reduce wage disparities and promote equity in hiring, Salary History Bans (SHBs) prohibit employers from inquiring about job applicants' current or previous salary during the hiring process. I first develop a search-and-matching model showing that information frictions introduced by SHBs lengthen unemployment duration and generate higher reemployment wages for workers who voluntarily reveal their past salaries, while non-revealers earn lower wages. Then, I examine the impact of these bans on unemployment duration, weekly earnings, and job transitions among displaced workers, by employing a difference-in-differences approach that leverages the staggered implementation of SHBs across states. The empirical analysis shows that these policies increased unemployment durations by approximately 19 percent and reduced the probability of full-time employment by 4.8 percentage points, both driven by impacts among women. I also find a negative and statistically significant effect on women's wages, widening the gender pay gap. These findings highlight the potential effects of SHBs on gender equity in the labor market and job-matching efficiency.

Keywords: Salary History Ban, Unemployment Duration, Wage Disparity
JEL Codes: J08, J31, J78

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I am deeply grateful to Michael Bates, Victor Ortego-Marti, and Joseph Cummins for their invaluable guidance and support. I also thank Ruoyao Shi, Sarojini Hirshleifer, Jang-Ting Guo, Bree Lang, Matthew Lang, Robynn Cox, and seminar participants at the XVII International Conference on Economics at the World Economic Research Institute, the 2025 All-California Labor Economics Conference at Stanford University, the UC Riverside Applied Micro Brown Bag, and the UC Riverside Applied Economics Colloquium for their thoughtful feedback and suggestions.

1. Introduction

Recent estimates place the gender pay gap between 10 percent and 25 percent (Goldin (2014); Blau and Kahn (2017)). Since prior wages may embed bias, employers' use of this information when determining job offers can amplify existing pay disparities. While past wages may provide insights into a worker's productivity or outside opportunities, their use in hiring can disadvantage women and historically underrepresented minorities whose past earnings may reflect discrimination, leading to history-dependent wage growth and wider inequality (Bessen et al. (2020); Sinha (2022); Mask (2023)). This concern has prompted policymakers to institute Salary History Bans (SHBs), which prevent employers from inquiring about job applicants' current or previous salary during the hiring process. As of May 2025, 22 states and 24 local jurisdictions in the United States have passed SHBs.[1]

In this paper, I study the effect of SHBs on the efficiency of labor market dynamics among displaced workers. First, I develop a search-and-matching model based on Diamond (1982), Mortensen (1982), and Pissarides (1985) (DMP) to analyze the mechanisms through which SHBs affect the labor market outcomes and provide predictions regarding the strength and direction of the effects. The implementation of SHBs introduces an information friction in the labor market that is expected to prolong unemployment spells. While SHBs prohibit employers from asking job applicants about their previous earnings, they do not restrict job applicants from revealing their salary information voluntarily. This design of the policy generates heterogeneity among workers by compelling them to choose whether to reveal their salary history voluntarily. Incorporating volunteer disclosure as a channel into the theoretical framework by specifying SHB as a parameter that increases disclosure costs for workers, the model generates predictions regarding how information restrictions may alter unemployment duration and wages. Workers endogenously decide whether to disclose their salary history based on match-specific costs of revelation, such as the risk of perpetuating past low wages or limiting future bargaining flexibility. The model predicts that as disclosure cost increases, market tightness declines, leading to longer unemployment durations.[2] Beyond this aggregate effect, the mechanism generates distinct wage outcomes across workers depending on their disclosure choices. As disclosure costs rise, workers who choose to reveal their prior salary experience higher reemployment wages, while those who withhold this information face wage declines. Consistent with the literature documenting that men are more likely to disclose their salary information than women, the model's predictions for revealers are therefore more indicative of men's outcomes, whereas those for non-revealers are more representative of women's. This mechanism thus provides a potential explanation for gender differences in labor market outcomes arising from variation in disclosure behavior.

I empirically examine the effect of SHBs on unemployment duration and wages, assessing the mechanisms highlighted in the theoretical model and providing evidence on how policy interventions affect the labor market trajectories of displaced workers. In order to estimate the effects of SHBs on unemployment duration, I exploit the staggered implementation of the bans and employ a semiparametric Cox proportional hazards model

1. The count of local jurisdictions includes the District of Columbia and Puerto Rico.
2. Market tightness is defined as the ratio of vacancies to unemployed workers.

within a difference-in-differences and event study framework. Since unemployment duration measures the time until the event of job finding, survival analysis is ideal for studying the effects of SHBs on unemployment duration. It accounts for censored observations, the timing of the event, and the distribution of durations. Using data from the Displaced Workers Supplement (DWS) of the Current Population Survey (CPS), the empirical analysis reveals that SHBs increased unemployment durations by approximately 19 percent. This corresponds to an increase in unemployment duration of 3.32 weeks and a loss of \$2,478, on average.[3] While I do not find a significant impact on unemployment durations of men, I find that following the bans, women experienced a 37.6 percent increase in unemployment duration, corresponding to an increase of approximately 6.8 weeks and a loss of \$4,292, on average.[4] The results are consistent across various analytical methods and robust to the inclusion of different control variables.

Since the adverse results on unemployment duration are driven primarily by effects observed among women, this paper also conducts an analysis on the effect of SHBs on earnings for the purpose of cost-benefit analysis. To estimate the causal effect of SHBs on the weekly earnings and gender pay gap, I leverage the staggered adoption of SHBs across states over time using event study and difference-in-differences approaches. I find that there is no significant effect of SHBs on average earnings. In contrast, the estimates suggest a statistically significant 14.5% relative decline in women's earnings and a marginally significant 9.52% relative increase in men's earnings. In addition, I find that the gender pay gap increased by 12.8% after the bans. These results reveal the policy-generated inefficiency in how the labor market reallocates displaced workers, and highlight the gender-specific effects of SHBs and the adverse consequences for the group the policy intended to protect.

In recent years, laws regulating the availability of information relevant to hiring and pay decisions have become increasingly prevalent. These transparency laws aim to promote fairer labor market practices and shift the informational balance from employers to workers by requiring firms to disclose previously private information, such as salary ranges, while allowing workers to withhold certain personal information, such as criminal records or credit histories. The literature on pay transparency laws that require employers to disclose salary information in job postings finds no evidence of adverse effects and instead documents wage increases driven by increased competition (Frimmel et al. (2022); Skoda (2022); Arnold et al. (2022)). On the other hand, studies examining policies that limit employers' ability to obtain specific information from workers, with the goal of encouraging fair hiring practices, have pointed out several unintended consequences. For instance, Ban the Box policies prevent employers from obtaining criminal records of job applicants and are designed to prevent statistical discrimination against ex-offenders. The literature that studies the effect of Ban the Box policies finds that while Ban the Box improves employment in high-crime neighborhoods, it exacerbates racial discrimination and worsens the labor market outcomes of black workers (Shoag and Veuger (2016), Agan and Starr (2018), Doleac and Hansen (2020)). Similarly, the literature that studies the

3. This is found by multiplication of increase in unemployment duration, 3.32 weeks, by the mean of current weekly earnings.

4. This is found by multiplication of increase in unemployment duration, 6.8 weeks, by the mean of current weekly earnings.

policies that ban employers from checking job applicants' credit histories finds that the job-finding rates increased for financially distressed job seekers but decreased for black job seekers (Bartik and Nelson (2016), Friedberg et al. (2021)). Hence, while these policies may provide better labor market outcomes for the main intended beneficiaries, they often yield detrimental results for those who are statistically or demographically similar to them.

While SHBs belong to the broader class of policies that restrict employers from obtaining information from job applicants during the hiring process, they differ from other such policies in two key aspects. First, salary discussions and negotiations usually take place at an early stage of hiring and are common practices across occupations (Sinha (2022)). Second, by not preventing job applicants from voluntary disclosure, the policy creates an alternative signal that workers can use, prompting employers to draw inferences about workers, even among those whose behavior remains unchanged (Agan et al. (2020)).

Since SHBs restrict the employers' access to workers' compensation history, these bans raise the costs of evaluating applications and alter employers' job posting behavior. The studies on the effect of SHBs on employer behavior find that SHBs led to an increase in the number of job postings and that postings are more likely to include salary information (Bessen et al. (2020), Sran et al. (2020)). In addition, field experiments provide evidence that employers who do not have access to applicants' pay history evaluate more applicants more intensively and that they have negative beliefs about the applicants who do not reveal their salaries (Agan et al. (2021), Barach and Horton (2021)). Conversely, having their compensation history hidden appears to be beneficial for workers. The studies on the workers' labor market outcomes document higher earnings (Bessen et al. (2020), Mask (2023)) and lower gender pay gap (Sran et al. (2020), Hansen and McNichols (2020), Sinha (2022)) following SHBs. Moreover, the applicants are less likely to voluntarily disclose their pay history after the bans, especially women (Agan et al. (2020), Sinha (2022)).

This paper contributes to the literature in three aspects. First, this is the first study to evaluate the impact of SHBs on unemployment duration. The estimation results suggest that, following the policy, the job finding rate of workers has decreased by 19 percent, indicating inefficiency in the labor market. This finding aligns with prior works on pre-employment credit checking bans, which report that the bans lower the job-finding rates of black job seekers by $7\% - 16\%$ and increase the job-finding rates of financially stressed job seekers by about 28 percent (Bartik and Nelson (2016), Friedberg et al. (2021)). A decrease in job finding rate has relevant implications for policy design. Longer unemployment durations can signal potential mismatches between employers and workers, skill depreciation, economic instability, or barriers to job access for certain groups, and have significant economic and social costs, such as lower economic productivity, lower future earnings, and mental health impacts (Gregory and Jukes (2001), Pollak (2013), Schmieder et al. (2016), Cohen et al. (2025)). Moreover, additional welfare analysis in this paper indicates a decrease in women's earnings and a widened gender pay gap following SHBs, suggesting that prolonged unemployment duration is less likely to be associated with workers' choices, and that the gender heterogeneity could be a contributing factor to observed differences.

Second, this is the first study to assess the impact of a recent pay transparency policy on displaced workers. Since SHBs restrict employers from inquiring about prior salaries, their most direct relevance is for individuals who have previously held jobs. Displaced workers, who are often in a vulnerable position, are therefore a particularly affected group (Schmieder and Von Wachter (2010), Schmieder et al. (2023)). Displaced workers consistently face long-term economic consequences, with studies showing persistent earnings losses, difficulty finding full-time employment, and reduced wage rates that can last for years after job loss (Farber and Gibbons (1996), Von Wachter et al. (2009), Lachowska et al. (2020), Hyman et al. (2024)). In addition, market efficiency can be examined through people in transition, since their movement reveals how smoothly reattachment occurs. Displaced workers provide a reliable signal of how well the market is functioning, as they are among those in transition. Moreover, since these workers did not leave their jobs voluntarily, their separations are not considered to be a strategic response to the policy. Hence, SHBs are not expected to affect their labor supply at the extensive margin. Compared to individuals who voluntarily separate from employment, likely because they anticipate better opportunities elsewhere, displaced workers are expected to experience inferior outcomes. Therefore, displaced workers are a distinct group to study, as they represent a negatively selected group within the population, and this may explain why my results, showing lower earnings for women and a wider gender pay gap after SHBs, diverge from previous studies, which focus on broader populations and often find positive effects.

Third, to my knowledge, this is the first study that incorporates the unique aspect of SHBs, voluntary disclosures, into a standard search-and-matching framework. While Meli and Spindler (2019), Agan et al. (2020), and Sinha (2022) also provide theoretical implications of SHBs, their models do not account for search frictions, and thus cannot capture unemployment duration dynamics that arise from the matching process. My model generates predictions that are consistent with the empirical patterns observed in my analysis. Specifically, I find that as disclosure costs increase, the share of workers who voluntarily reveal their salary history (*revealers*) decreases, unemployment duration rises, wages of *revealers* increase, and those of *non-revealers* decline. While my empirical results also indicate a rise in unemployment duration following SHBs, the data constraints prevent testing disclosure decisions and their impacts. On the other hand, previous empirical studies on voluntary disclosure shed light on the interpretation of my results. Sinha (2022) finds that applicants became less likely to voluntarily disclose their pay history after employers could no longer prompt them, with a decline approximately two percentage points larger among women. Agan et al. (2020) find that workers who disclose their salary history, regardless of whether an SHB is in effect, are more likely to be men; whereas workers who do not disclose their salary history only when an SHB is in effect are more likely to be women. These findings are consistent with my model's prediction that *non-revealers* experience adverse wage effects, and with my empirical finding that women's wages decline after the ban.

Overall, this paper shows that SHBs, while designed to reduce wage discrimination and improve labor market outcomes for women and minorities, can generate unintended consequences. By documenting longer unemployment durations and lower reemployment wages for displaced women, the analysis reveals inefficiencies in labor market reattachment and highlights the gender-specific effects of transparency policies.

The rest of this paper is structured as follows. Section 2 provides the background and further details regarding SHBs. Section 3 presents the theoretical framework and helps interpret the empirical findings. Section 4 describes the data. Section 5 discusses the empirical strategy. Section 6 presents the empirical results and Section 7 concludes.

2. Background

In recent years, a growing body of transparency laws and wage equity regulations has emerged to address inequities regarding how workers are evaluated, compensated, and promoted within firms both during hiring and within the workplace. These policies build on existing federal protections, including the Equal Pay Act, Title VII of the Civil Rights Act, the Lily Ledbetter Fair Pay Act, and subsequent transparency measures that collectively aim to promote pay equity and limit discriminatory pay practices. More recently, states have introduced a series of hiring and compensation reforms that further regulate what information employers may consider and how wages are determined. Ban the Box laws delay inquiries into criminal history until later stages of hiring, with the intention of improving employment access for individuals with prior convictions. Restrictions on pre-employment credit checks limit the use of personal financial histories that may reflect socioeconomic disadvantage rather than job-relevant characteristics. Pay transparency laws require employers to disclose salary ranges in job postings or during the hiring process, increasing the accessibility of compensation practices and reducing information asymmetries between employers and applicants. Together, these policies reflect a broader shift toward limiting the role of personal background in hiring and improving fairness in wage negotiation and job access.

One of the most recent policies in this area is the adoption of Salary History Bans (SHBs), which represent a more recent approach focused specifically on the information used to set pay during hiring. SHBs prohibit employers from requesting or requiring an applicant's pay history during the hiring process. In addition, employers may not obtain this information from other sources, such as public databases, hiring agencies, or the applicant's former employer. Employers may verify wage history only after extending an employment offer with compensation terms. On the other hand, the laws do not prevent applicants from voluntarily disclosing their previous pay, so disclosure remains a choice made by workers.

A central motivation behind SHBs is the concern that past wages may reflect historical inequities. If a worker received a lower wage in a prior job due to discrimination, occupational segregation, or weaker bargaining power, reliance on past pay when setting new offers may carry these disparities forward across jobs and over time. By weakening the direct link between past and current wages, SHBs aim to reduce the persistence of wage inequality and the history dependence of wage trajectories (Tanenbaum (2019), Wong (2019)). Although some policymakers argue that these laws fail to consider functional needs of businesses by bringing incomplete information into the hiring process, and have the risk of harming small businesses and nonprofits, SHBs have become increasingly prevalent (Graumlich and Sedivy (2024)). Within the United States, SHBs have been enacted in 22 states, 6 counties, 16 cities, as well as in the District of Columbia and Puerto Rico, as of May 2025.

The scope and enforcement of SHBs vary across jurisdictions. Most of the states enforces SHBs for both public and private sector employers. On the other hand, Pennsylvania, Michigan, and North Carolina requires SHBs only for public sector employers. New York, New Jersey, and Illinois initially applied the ban only to public sector employers, but later extended the coverage to all employers. There are also cities and counties that have implemented SHBs, even though the states they belong do not.[5]

Beyond variation in coverage, states also differ in the legislative context in which the SHB was enacted. In some states, the SHB was implemented separately from other policy changes, whereas in others it was bundled with additional employment regulations. In these cases, the accompanying policies often relate to pay equity, anti-discrimination, salary transparency, or wage-range disclosure. However, there is no uniform set of additional policies. In some instances, the primary objective was to introduce the SHB while also making modifications to existing laws.

Figure A1 illustrates the geographic distribution of SHBs across the United States. Table B1 and Table B2 present the effective dates of these policies at the state and local levels. Table B10 summarizes the relevant legislation in treated states and indicates whether the ban was adopted on its own or bundled with other policy changes. Table B11 lists the states by whether the SHB was bundled with other policies or introduced separately. These tables highlight variation in the timing, scope, and legislative context of SHB adoption, which provides the basis for the empirical analysis that follows.

3. A Simple Theoretical Model

I develop a continuous-time search-and-matching framework augmented with information choice to study labor market dynamics of a change in policy. The model builds on the standard search-and-matching framework of Diamond (1982), Mortensen (1982), and Pissarides (1985) (DMP). Unemployed workers receive a flow benefit b, meet firms at a Poisson rate $f(\theta)$, where θ denotes market tightness, and decide whether to reveal their salary history (R) or not (N) upon being matched with a firm. Revealing entails a cost $\mu\alpha$, where μ is a policy parameter common to all workers, and α is an idiosyncratic worker-specific cost drawn from distribution $G(\alpha)$. The parameter α captures worker-specific heterogeneity in the cost of revealing one's salary history. This cost can arise from several sources: revealing may expose workers to anchoring effects, where employers base offers on past rather than market wages; pay compression, which perpetuates previously low earnings; or bias introduction, as employers may infer productivity or reservation wage through the disclosed wage. In addition, disclosure may entail a privacy cost and reduce future bargaining flexibility by constraining the negotiation range. Together, these factors make salary revelation more costly for some workers than others, giving rise to heterogeneity in information disclosure decisions.

Employed workers earn a wage w_i, where $i \in \{R, N\}$, and are subject to exogenous job separation at rate s. Firms post vacancies at a cost k, match with job applicants at rate $q(\theta)$, and encounter a revealer with endogenous probability ρ. Filled jobs generate outputs y_R and y_N for revealers and non-revealers, respectively. Workers' value functions are given by $U(\alpha)$ in unemployment:

$$rU(\alpha) = b + f(\theta)\max\left\{\, W_R(\alpha) - U(\alpha) - \mu\alpha,\ W_N - U(\alpha)\,\right\} \tag{1}$$

5. Wisconsin prohibited SHBs in April 2018. This prohibition stated that local governments may not intervene the information that employers use during the hiring process. Michigan also initially prohibited SHBs in June 2018; however, later changed its policy and adopted SHBs for public sector employers in January 2019.

Workers' value functions are given by $W_R(\alpha)$ and W_N in employment with a revealing or non-revealing match, respectively:

$$rW_R(\alpha) = w_R(\alpha) + s\big(U(\alpha) - W_R(\alpha)\big) \tag{2}$$

$$rW_N = w_N + s\big(U(\alpha) - W_N\big) \tag{3}$$

Firms' value functions are J_R and J_N for a filled job with a revealer or non-revealer:

$$rJ_R(\alpha) = y_R - w_R(\alpha) + s\big(V - J_R(\alpha)\big) \tag{4}$$

$$rJ_N = y_N - w_N + s\big(V - J_N\big) \tag{5}$$

Let V be the value function of a vacancy:

$$rV = -k + q(\theta)\left[\int_0^{\alpha^*} (J_R(\alpha) - V)\, dG(\alpha) + \int_{\alpha^*}^{\infty} (J_N - V)\, dG(\alpha)\right], \tag{6}$$

where α^* is the revealing threshold.

Wages are determined endogenously through Nash bargaining, where the worker has bargaining weight β. Nash bargaining conditions are given as the following:

$$\beta\,(J_R(\alpha) - V) = (1-\beta)\big(W_R(\alpha) - U(\alpha) - \mu\alpha\big) \tag{7}$$

$$\beta\,(J_N - V) = (1-\beta)\big(W_N - U(\alpha)\big). \tag{8}$$

Assuming free entry in the market for vacancies, $V = 0$, wages, $w_R(\alpha)$ and w_N, and market tightness, θ, satisfy the following in equilibrium:

$$w_R(\alpha) = \frac{\beta(r + s + f(\theta))y_R + (1-\beta)(r+s)[b + \mu\alpha(r+s)]}{r + s + \beta f(\theta)} \tag{9}$$

$$w_N = \frac{\beta(r + s + f(\theta))y_N + (1-\beta)(r+s)b}{r + s + \beta f(\theta)} \tag{10}$$

$$\rho y_R + (1-\rho)y_N = b + (r+s)\mu\rho\bar{\alpha} + \frac{(r + s + \beta f(\theta))}{(1-\beta)q(\theta)}k, \tag{11}$$

where $\bar{\alpha}$ is the expected value of α given that the worker reveals, $\bar{\alpha} = \mathbb{E}[\alpha \mid \alpha \leq \alpha^*]$.

Next, I examine the comparative statics. Since the policy parameter μ scales the cost of revealing and is common to all workers, an SHB being enacted is considered as an increase in μ. Proposition 3.1 summarizes the prediction of the model, indicating that an SHB increases unemployment duration and has differential impacts on wages based on revealing types.

Proposition 3.1 *In equilibrium, an increase in the common disclosure cost, μ, yields (i) lower market tightness and longer unemployment duration, $\frac{d\theta}{d\mu} < 0$, (ii) higher wages for revealers, $\frac{dw_R(\alpha)}{d\mu} > 0$, and (iii) lower wages for non-revealers, $\frac{dw_N}{d\mu} < 0$.*

4. Data

In this paper, I utilize the Displaced Workers Supplement (DWS) from Current Population Survey's (CPS) (Flood et al. (2023)) and as a robustness check, I also use the Survey of Income and Program Participation (SIPP) dataset.

4.1 Displaced Workers Supplement (DWS)

The analysis draws on data from the Displaced Workers Supplement (DWS) of the Current Population Survey (CPS) for the period January 2012–January 2024. CPS is a nationally representative monthly survey of approximately 60,000 households that collects information on demographics, employment, job search and unemployment, and education. CPS provides a rotating unbalanced panel dataset. The households are interviewed for four consecutive months, leave the sample temporarily for eight months, and then are interviewed again for four consecutive months. After eight months of interview, they leave the survey permanently.

DWS is administered as part of the CPS and includes respondents who are considered to be displaced, defined as those who lost or left a job due to plant closure, insufficient work, or abolition of position or shift. The survey includes information on demographic characteristics such as gender, race, age, education, location, and labor market outcomes of both the displaced and the current job, such as employment status, earnings, public and private sector, full time status, unemployment duration, industry and occupation. DWS is conducted biennially each January throughout the sample period of this study. The structure of CPS and DWS results in each individual being interviewed for the DWS only once. Table B4 summarizes the distribution of individuals based on whether they lost or left a job and whether they are classified as displaced workers.

In this paper, I restrict my sample to the civilian non-institutionalized population who reported the years ago they last worked at the displaced job and not above the age 65 at the time they were displaced. The full sample consists of 18,300 individuals. The year in which they were displaced ranges from 2009 to 2023. Table B5 reports the reason individuals lost or left their displaced jobs.

For the unemployment duration analysis, I use the self-reported response to the question asking "the number of weeks not working between the end of the displaced job and the beginning of their next job", namely, the continuous variable *dwwksun*.

For the wage analysis, I use both the current wage and the wage earned at the displaced job, using 2009 as the base year. I drop individuals with top-coded wages or missing either wage. Although hourly wage is available in the data, it is reported by only a small number of respondents. Therefore, I conduct the analysis using weekly earnings.

4.2 Survey of Income and Program Participation (SIPP)

As a robustness check, I replicate the main results using the 2018–2024 waves of the Survey of Income and Program Participation (SIPP), a longitudinal dataset that offers several advantages for analyzing labor market adjustment following displacement. SIPP provides detailed monthly labor market and income histories, and allows for a detailed observation of employment spells, job transitions, and timing of reemployment outcomes.

5. Empirical Strategy

5.1 Unemployment Duration

To estimate the effects of SHBs on unemployment duration, I exploit the staggered implementation of the bans and employ a Cox proportional hazards model. Formal definitions of the survival and hazard functions, as well as the Cox model specification, are provided in Appendix C.

The specification closely follows Friedberg et al. (2021):

$$\log[h_{irt}(\tau)] = \log[h_0(\tau)] + \beta \cdot SHB_{rt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \tau_t \tag{12}$$

where $h_{irt}(\tau)$ is the weekly unemployment exit hazard for the displaced person i who has been unemployed in region r for τ weeks, SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t, $\mathbf{X}_{irt}$ represents controls for sex, race, age, education category, and marital status. δ_r and τ_t are location and displaced year fixed effects, respectively. The standard errors are clustered at the location level. The results are presented in Table 1.

The coefficient β reflects the impact of SHBs on the log of the job-finding hazard rate, controlling for individual characteristics and location and displaced year fixed effects. As Kroft and Notowidigdo (2016) and Friedberg et al. (2021) point out, since log of unemployment duration is approximately equal to the inverse hazard rate, $-\beta$ can also be interpreted as the change in the log of unemployment duration.

The following specification is used to conduct an event study analysis of the effect of SHBs on the job-finding hazard rate for displaced workers, capturing the dynamic impacts across event time relative to policy implementation:

$$\log[h_{irt}(\tau)] = \log[h_0(\tau)] + \sum_{l \in [-5^+, +5^+] \setminus \{-1\}} \beta_i \cdot SHB_{rl} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \tau_t \tag{13}$$

where $h_{irt}(\tau)$ is the weekly unemployment exit hazard for the displaced person i who has been unemployed in region r for τ weeks. The event time index l denotes years relative to the policy implementation year, where all event years earlier than $l = -4$ are grouped into a single bin, and all years later than $l = 4$ into another bin. SHB_{rl} is an indicator for the Salary History Ban being in effect in region r in event year l, $\mathbf{X}_{irt}$ represents controls for sex, race, age, education category, and marital status. δ_r and τ_t are location and displaced year fixed effects. The standard errors are clustered at location level. The event study results are displayed in Figure 3.

5.2 Earnings

I leverage the staggered adoption of SHBs across states over time using a difference-in-differences framework to estimate the causal effect of these policies on earnings. I estimate the following specification:

$$y_{irt} = \alpha + \beta \cdot SHB_{rt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \tau_t + \epsilon_{irt}, \tag{14}$$

where y_{irt} is the log of current wages of the displaced person i located in region r, in calendar year t; SHB_{rt} is an indicator for the Salary History Ban being in effect in region

r in year t. $\mathbf{X}_{irt}$ represents controls for sex, race, a polynomial (cubic) in age in displaced year, education category, marital status, a polynomial (cubic) in tenure in the displaced job, full time status in the displaced job; sector, industry, and occupation in the displaced job, and δ_r and τ_t are location and year fixed effects. The standard errors are clustered at state level. The results are presented in Table 2.

which provides corrected event-study coefficients that avoid comparisons to already treated units and allow treatment effects to evolve flexibly over time.

Recent work has shown that standard two-way fixed effects estimates can be biased in settings with staggered adoption and heterogeneous treatment effects. To address this concern, I also employ the difference-in-differences estimator of Callaway and Sant'Anna (2021), which estimates group-time average treatment effects by comparing each treated cohort to units that have not yet adopted the policy. This approach allows treatment effects to vary across adoption cohorts. In addition, I use the event-study estimator of Sun and Abraham (2021), which avoids contamination from already treated regions and produces cleaner estimates of the policy's effect over time. The results are presented in Table B18 and Table B19.

Since gender plays a significant role in the context of SHBs, I also examine their effect on the gender pay gap following Sinha (2022) and Hansen and McNichols (2020). I estimate the following specification:

$$\begin{aligned} y_{irt} = \alpha &+ \beta_1 \cdot SHB_{rt} \cdot Female_i + \beta_2 \cdot SHB_{rt} + \beta_3 \cdot Female_i \\ &+ \beta_4 \cdot Female_i \cdot Treated_r + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \tau_t + \epsilon_{irt}, \end{aligned} \tag{15}$$

where y_{irt} is the outcome variable of the displaced person i located in region r, in calendar year t; SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t. $Female_i$ is a female dummy. $Treated_r$ is a dummy variable if region r ever implements the ban. $\mathbf{X}_{irt}$ represents controls for race, a polynomial (cubic) in age in displaced year, education category, marital status, a polynomial (cubic) in tenure in the displaced job, full time status in the displaced job; sector, industry, and occupation in the displaced job, and δ_r and τ_t are location and year fixed effects. The standard errors are clustered at the state level. The results are presented in Table 3.

5.3 Earnings by Prior Pay Level

SHBs may affect workers differently depending on their wage history. In the absence of the ban, workers with low previous pay could be more likely to receive offers that anchor on their prior wage, potentially perpetuating pay penalties that do not reflect their productivity. By contrast, workers with high previous pay can benefit from having their past wage observed, and their past wage can lead to higher offers.

If SHBs raise the earnings of individuals with low previous pay without significantly reducing the earnings of those with high previous pay, this would suggest a more efficient allocation of wages and a reduction in history-dependent wage distortions. If, however, bans reduce the role of wage history as a productivity signal or alter workers' disclosure behavior, this may introduce inefficiencies in the matching process and weaken sorting across jobs.

I examine whether the effect of SHBs on earnings differs between individuals with high and low previous pay. To distinguish between these two groups, I estimate an expected wage at the displaced job as a function of observable characteristics. Specifically, I regress the log of wages at the displaced job on educational attainment, tenure at the displaced job, industry and occupation at the displaced job, full-time status, sector, and state and year fixed effects. Using the estimated coefficients, I predict the wage that each individual would be expected to earn given these characteristics. I then classify individuals as having low previous pay if their actual wage at the displaced job falls below the predicted wage, and as having high previous pay otherwise. Specifically, I define Low paid_{iD} as the following:

$$\text{Low paid}_{iD} = \begin{cases} 1, & \text{if } \log(w_{iD}) < \widehat{\log(w_{iD})}, \\ 0, & \text{if } \log(w_{iD}) \geq \widehat{\log(w_{iD})}. \end{cases} \tag{16}$$

where w_{iD} denotes the wage at the displaced job for individual i. In this framework, I refer to individuals with Low $\text{paid}_{iD} = 1$ as having low previous pay and those with Low $\text{paid}_{iD} = 0$ as having high previous pay.

After assigning individuals to low and high previous pay based on their predicted wage, I examine whether SHBs affect these two groups differently. The empirical strategy exploits the staggered timing of policy adoption across states. I estimate the following specification:

$$\begin{aligned} y_{irt} = \beta_0 + \beta_1 \cdot SHB_{rt} \times \text{Low paid}_{irt} + \beta_2 \cdot SHB_{rt} \times \text{High paid}_{irt} \\ + \beta_3 \times \text{Low paid}_{irt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \tau_t + \epsilon_{irt} \end{aligned} \tag{17}$$

where where y_{irt} is the log of current wages of the displaced person i located in region r, in calendar year t; SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t. *Low paid*, as defined in (16), is an indicator for whether the individual's past wage is below the predicted wage based on observable characteristics. *High paid* indicates that the past wage is above the predicted wage based on observable characteristics, and equals one when Low $\text{paid}_{iD} = 0$. $\mathbf{X}_{irt}$ represents controls for sex, race, a polynomial (cubic) in age in displaced year, education category, marital status, a polynomial (cubic) in tenure in the displaced job, full time status in the displaced job; sector, industry, and occupation in the displaced job, and δ_r and τ_t are location and year fixed effects. The standard errors are clustered at state level. The results are presented in Table 4.

5.4 Job Transitions

5.4.1 Full Time Employment

I leverage the staggered adoption of SHBs across states and employ a difference-in-differences framework to examine their effect on the probability of full-time employment among displaced workers. The following logit specification is estimated:

$$\log\left(\frac{p_{irt}^{full}}{1 - p_{irt}^{full}}\right) = \alpha + \beta \cdot SHB_{rt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \eta_t + \epsilon_{irt} \tag{18}$$

where p_{irt}^{full} is the probability that displaced worker i in region r and displaced year t is employed full-time at the current job, SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t, $\mathbf{X}_{irt}$ represents controls for sex, race, age, and education category, and δ_r and η_t are location and year (displaced year) fixed effects. Standard errors are clustered at the state level. The results are presented in Table 5.

5.4.2 Other Job Transition Outcomes

Number of Jobs Changed: The following Poisson regression model is used to analyze the effect of SHBs on the number of jobs changed by the displaced workers:

$$\log(\mu_{irt}) = \alpha + \beta \cdot SHB_{rt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \eta_t + \epsilon_{irt} \tag{19}$$

where μ_{irt} is the expected number of jobs changed since being displaced, for the displaced person i who has been unemployed in region r at time t, SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t, $\mathbf{X}_{irt}$ represents controls for sex, race, age, and education category, and δ_r and η_t are location and year (displaced year) fixed effects, respectively. I also include the number of years since displacement as an exposure variable to account for differences in the time during which individuals could change jobs. Standard errors are clustered at the state level.

Private Sector Employment: The following logit specification is used to analyze the effect of SHBs on the probability that the current job is in the private sector among displaced workers:

$$\log\left(\frac{p_{irt}^{\text{private}}}{1 - p_{irt}^{\text{private}}}\right) = \alpha + \beta \cdot SHB_{rt} + \mathbf{X}_{irt}\boldsymbol{\gamma} + \delta_r + \eta_t + \epsilon_{irt}, \tag{20}$$

where p_{irt}^{private} denotes the probability that displaced worker i in region r and displaced year t holds a private sector job at the current job. SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t, $\mathbf{X}_{irt}$ represents controls for sex, race, age, and education category, and δ_r and η_t are location and year (displaced year) fixed effects. Standard errors are clustered at the state level. The results for the number of jobs changed and the probability of private-sector employment are presented in Table B20.

6. Empirical Results

6.1 Unemployment Duration

6.1.1 Kaplan- Meier Survival Curves

Figure 1 and Figure 2 present the Kaplan- Meier survival curves for men and women in the treated states, before and after the implementation of SHBs. While the survival curves of men and women are not statistically different before the bans (log-rank test $p = 0.94$), the survival curves of men and women are statistically different after the bans (log-rank test $p = 0.03$). The upward shift in the survival curves of women compared to men, suggests that the unemployment duration increased for women after the ban.

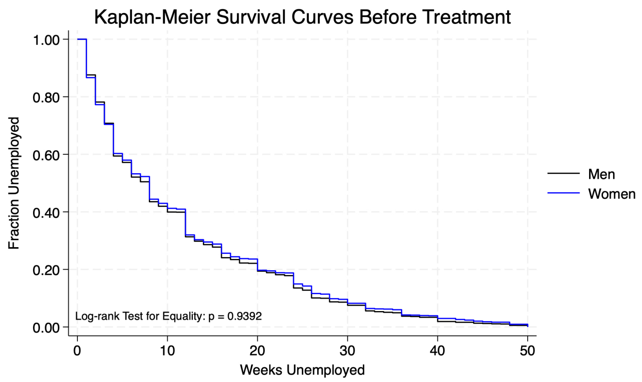

Figure 1: Non-parametric Survival Curves
Before Bans

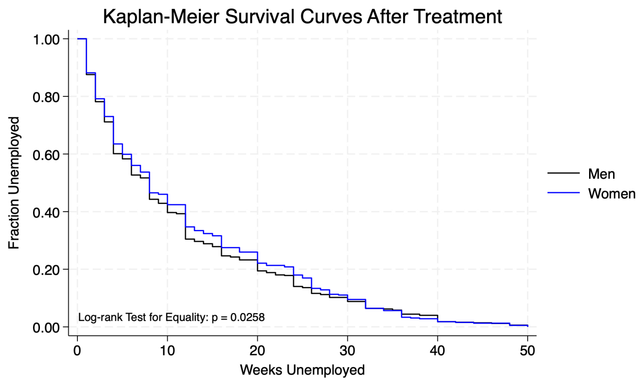

Figure 2: Non-parametric Survival Curves
After Bans

6.1.2 Baseline Results

Table 1 presents the estimates from semi-parametric Cox proportional hazard model in Equation (12). Panel A presents the results based on the sample that includes all states with SHBs, regardless of whether the policy applies to both the private and public sectors or only to the public sector. Panel B restricts the sample to states where the policy covers both sectors, excluding those where the ban applies only to the public sector.

The results in Panel A indicate that SHBs decreased the job finding hazard by 19 percent, which corresponds to an increase in unemployment duration by 19 percent. This implies an increase of approximately 3.32 weeks in unemployment duration, based on the average unemployment duration of 17.46 weeks in the sample. While there are no significant effect of the bans for men, the bans increased the unemployment duration of women by 37.6 percent, which suggests an increase of approximately 6.8 weeks in unemployment duration for women. Panel B shows similar results as in Panel A, with a slightly stronger effect. This may indicate that the impact of bans is primarily driven by those covering both sectors and that policies applying only to the public sector may have a limited effect.

Table 1: The Impact of Bans on Job-Finding Hazard

	All	Men	Women
Panel A: All States			
SHB	-0.190***	-0.0583	-0.376***
	(0.0575)	(0.0754)	(0.0779)
Mean (in weeks)	17.46	17.01	18.08
Observations	9,906	5,728	4,178
Panel B: Both-Sectors States			
SHB	-0.214***	-0.0823	-0.396***
	(0.0579)	(0.0783)	(0.0832)
Mean (in weeks)	17.33	17	17.83
Observations	8,906	5,175	3,731
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12). The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. *All States* in Panel A refers to the states that implement the policy for either both private or public sectors or only public sector. *Both-Sectors States* in Panel B refers to the states that implement the policy for both private or public sectors. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

6.1.3 Event Study

Figure 3 presents the event study graph, depicting the dynamic effects of the policy estimated from Equation (13). The event study graph reveals an upward trend in the coefficients during the pre-policy period, though none of the lead estimates are statistically significant. This suggests no strong evidence in the pre-policy period. In contrast, a downward trend is observed following the policy implementation. Significant effects appear in two to four periods after the policy implementation.

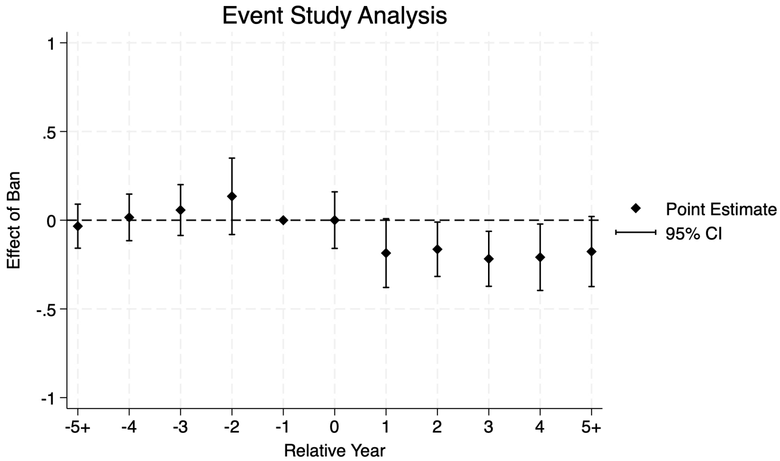

Figure 3: Event study of the impact of bans on the job-finding rate

6.1.4 Additional Analysis and Robustness Checks

Additional Control Variables: I extend Equation (12) by including additional control variables. These controls account for variation in unemployment benefit generosity and prior job conditions. Specifically, I control for the displaced job's characteristics, the number of weeks of regular unemployment insurance benefits, and to account for the effects of pandemic-related federal UI programs, I control for early termination of Pandemic Unemployment Assistance (PUA) and the Federal Pandemic Unemployment Compensation (FPUC) benefits and its interaction with a 2021 displacement indicator. The results are presented in Table B6.

Sectoral Variation in Policy Scope: Since there are states that implement the policy for both private and public sector, and there are states that implement the policy only for public sector, I distinguish between these two types of treatment in the analysis. To do so, I modify the equation (12) as follows:

$$\begin{aligned} \log[h_{irt}(\tau)] = \log[h_0(\tau)] &+ \beta_1 \cdot SHB_{\text{Both Sectors Ban}_{rt}} + \beta_2 \cdot SHB_{\text{Public Sectors Ban}_{rt}} \\ &+ X_{irt}\gamma + \delta_r + \tau_t, \end{aligned} \tag{21}$$

where $h_{irt}(\tau)$ is the weekly unemployment exit hazard for the displaced person i who has been unemployed in region r for τ weeks, $\text{SHB}_{\text{Both Sectors Ban}_{rt}}$ is an indicator for the bans being in effect in the states that implement the policy for both private or public sectors, $\text{SHB}_{\text{Public Sectors Ban}_{rt}}$ is an indicator for the bans being in effect in the states that implement the policy only for public sectors. $\mathbf{X}_{irt}$ represents controls for sex, race, age, education category, and marital status. δ_r and τ_t are state and displaced year fixed effects, respectively. The standard errors are clustered at the state level. The results are presented in Table B7.

Other Major Policies: To ensure that the estimated effects of SHBs are not confounded by the influence of other major policies regarding fair hiring, I also control for Ban-the-Box and pre-employment credit checking bans. I estimate the following equations:

$$\log[h_{irt}(\tau)] = \log[h_0(\tau)] + \beta_1 \cdot SHB_{rt} + \beta_2 \cdot BBT_{rt} + X_{irt}\gamma + \delta_r + \tau_t \tag{22}$$

$$\log[h_{irt}(\tau)] = \log[h_0(\tau)] + \beta_1 \cdot SHB_{rt} + \beta_2 \cdot CCB_{rt} + X_{irt}\gamma + \delta_r + \tau_t \tag{23}$$

$$\log[h_{irt}(\tau)] = \log[h_0(\tau)] + \beta_1 \cdot SHB_{rt} + \beta_2 \cdot BBT_{rt} + \beta_3 \cdot CCB_{rt} + X_{irt}\gamma + \delta_r + \tau_t, \tag{24}$$

where $h_{irt}(\tau)$ is the weekly unemployment exit hazard for the displaced person i who has been unemployed in region r for τ weeks, SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t. BBT_{rt} is an indicator for the Ban-the-Box policies being in effect in region r in year t. CCB_{rt} is an indicator for the pre-employment credit checking bans being in effect in region r in year t. $\mathbf{X}_{irt}$ represents controls for sex, race, age, education category, and marital status. δ_r and τ_t are location and displaced year fixed effects, respectively. The standard errors are clustered at the location level. The results are presented in Table B9.

Bundled vs Separate Implementation: The analysis also accounts for variation in legislative context. In some states, the SHB was introduced independently, whereas in others it was bundled with additional employment regulations. I separate these two types of policy adoption and examine the estimated effects of SHBs. The results are presented in Table B12.

Local Policy Variation: Several SHBs were introduced not only by states but also by cities and counties, which creates within-state variation in policy exposure. To account for this, I conduct the analysis at the metropolitan area (MSA) level. This local variation makes it possible to study job search and hiring within the local labor market, where job search and hiring interactions are concentrated. I re-estimate the baseline specifications using MSA identifiers, with fixed effects defined accordingly. The results of the MSA-level analysis are presented in Table B13.

Parametric Regression Models: As a robustness check, I estimate parametric survival models, specifically the Weibull and Exponential models. The parametric models impose a specific functional form on the baseline hazard, as opposed to the Cox model.
Weibull Regression Model:

$$\log[h_{irt}(\tau)] = \log(\lambda) + (\rho - 1)\log(\tau) + \beta \cdot SHB_{rt} + X_{irt}\gamma + \delta_r + \tau_t, \tag{25}$$

where $h_{irt}(\tau)$ is the weekly unemployment exit hazard for the displaced person i who has been unemployed in region r for τ weeks, λ is the scale parameter, and ρ is the shape parameter. SHB_{rt} is an indicator for the Salary History Ban being in effect in region r in year t, $\mathbf{X}_{irt}$ represents controls for sex, race, age, education category, and marital status. δ_r and τ_t are state and year fixed effects.

Exponential Regression Model: An Exponential regression model is a special case of the Weibull model when the shape parameter $\rho = 1$. Modifying equation (25),

$$\log[h_{irt}(\tau)] = \log(\lambda) + \beta \cdot SHB_{rt} + X_{irt}\gamma + \delta_r + \tau_t. \tag{26}$$

The results are presented in Table B14.

Survey of Income and Program Participation (SIPP): I evaluate the robustness of the main findings to the choice of dataset by re-estimating the baseline model using the 2018–2024 waves of the Survey of Income and Program Participation (SIPP). Table B15 reports the results for all involuntary separations, while Table B16 presents the estimates restricting the sample to workers displaced due to plant closure, slack work, and abolished shift. The full sample of involuntary separations captures the policy's impact in a broader set of displacement circumstances. The restricted sample follows the classification in the CPS Displaced Worker Supplement (DWS), which categorizes plant closure, slack work, and abolished shift as displacement reasons that arise from shifts in firm or industry conditions rather than from worker-specific factors.

6.2 Earnings

6.2.1 Baseline Results

Table 2 presents the estimates from the model specified in Equation (14). Panel A presents the results based on the sample that includes all states with SHBs, regardless of whether the policy applies to both the private and public sectors or only to the public sector. Panel B restricts the sample to states where the policy covers both sectors, excluding those where the ban applies only to the public sector.

The results indicate that while there is no significant effect on average earnings overall, the estimates reveal a positive and marginally significant effect for men, of 9.52%, and a negative and statistically significant effect for women, of 14.5% ($p < 0.05$).

Table 2: The Impact of Bans on log(Current Weekly Earnings)

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All States						
SHB	-0.0326	-0.0304	0.0807	0.0952*	-0.140*	-0.145**
	(0.0492)	(0.0463)	(0.0586)	(0.0545)	(0.0722)	(0.0705)
Mean (in log)	6.2	6.2	6.33	6.33	6.04	6.04
Observations	7,364	7,364	4,121	4,121	3,243	3,243
Panel B: Both-Sector States						
SHB	-0.0307	-0.0288	0.109*	0.120**	-0.185**	-0.185**
	(0.0572)	(0.0551)	(0.0620)	(0.0572)	(0.0847)	(0.0817)
Mean (in log)	6.19	6.19	6.31	6.31	6.04	6.04
Observations	6,591	6,591	3,716	3,716	2,875	2,875
Log(previous wage)	No	Yes	No	Yes	No	Yes
Previous job controls	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results are coefficient estimates based on the regression specified in equation (14). The outcome variable is log(Current Weekly Earnings). *All States* in Panel A refers to the states that implement the policy for either both private or public sectors or only public sector. *Both-Sectors States* in Panel B refers to the states that implement the policy for both private or public sectors. Demographic control variables are sex, race, a polynomial (cubic) in age in displaced year, education category, and marital status. Previous job controls are a polynomial (cubic) in tenure in the displaced job, full time - part time status in the displaced job; sector, industry, and occupation in the displaced job. Standard errors, in parentheses, are clustered at state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

6.2.2 Gender Pay Gap

Table 3 presents the estimates from the model specified in Equation (15). The main coefficient of interest, *SHB* $\times$ *Female*, is negative and marginally significant, indicating that the policy widened the gender pay gap by 12.5%. The coefficient on *SHB*, which captures the effect of the policy on men's earnings, is not statistically significant. The coefficient on *Female* is negative and statistically significant, reflecting the baseline gender pay gap. The interaction term *Female* $\times$ *Treated*, shows no significant effect, indicating that there are no baseline differences in gender pay gap between treated and control states.

Table 3: The Impact of Bans on Gender Pay Gap

	All States		Both-Sector States	
	(1)	(2)	(3)	(4)
SHB x Female	-0.125*	-0.128*	-0.160*	-0.155**
	(0.0729)	(0.0668)	(0.0890)	(0.0761)
SHB	0.0275	0.0318	0.0447	0.0453
	(0.0505)	(0.0448)	(0.0546)	(0.0480)
Female	-0.166***	-0.111***	-0.157***	-0.102***
	(0.0355)	(0.0364)	(0.0363)	(0.0373)
Female x Treated	-0.0128	0.0158	-0.0480	-0.0156
	(0.0478)	(0.0451)	(0.0443)	(0.0434)
Log(previous wage)	No	Yes	No	Yes
Previous job controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Mean (in log)	6.2	6.2	6.19	6.19
Observations	7,364	7,364	6,591	6,591

Notes: Results are coefficient estimates based on the regression specified in equation (15). The outcome variable is log(Current Weekly Earnings). The coefficient on "SHB x Female" reflects the policy's impact on the gender pay gap. The coefficient on "SHB" captures the policy's effect on men. The "Female" coefficient represents the baseline gender pay gap, and the interaction term "Female x Treated" indicates the baseline difference in the gender wage gap between the treated and control states. Demographic control variables are sex, race, a polynomial (cubic) in age in displaced year, education category, and marital status. Previous job controls are a polynomial (cubic) in tenure in the displaced job, full time - part time status in the displaced job; sector, industry, and occupation in the displaced job. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

6.3 Earnings by Prior Pay Level

Table 4 reports how the effect of SHBs varies by previous pay and gender and presents the estimates from the model specified in Equation (17). The estimates for *SHB* × *Low paid* indicate that the effect of SHBs differs across gender. In the full sample, the coefficient is small and statistically insignificant, suggesting no average effect for previously low paid workers as a whole. Among men, the effect is also small and not statistically significant. In contrast, for women, *SHB* × *Low paid* is negative and statistically significant at the ten percent level, with an estimated magnitude of about 17 percent, indicating that among women with low previous pay, those in states with an SHB in place earn about 17 percent less than observationally similar women with low previous pay in states without the ban. For individuals with high previous pay, there is no significant effect of SHBs in the full sample or among women, as the interaction coefficients are small in magnitude and statistically insignificant in both cases. By contrast, among men with high previous pay, the coefficient on *SHB* × *Low paid* is positive and significant at the ten percent level, indicating that men whose past wages exceeded their predicted wage earn approximately 12.6 percent higher current wages when the ban is in effect. *Low paid* reflects the baseline difference in current wages between individuals who were previously low paid and those who were previously high paid in the absence of an SHB. In the full sample, the coefficient on *Low paid* is negative, which is consistent with persistence in wage penalties from the displaced job. The estimates imply that previously low paid workers earn between 10 to 12 percent lower current wages when no SHB is in place. Taken together, these results suggest that SHBs do not eliminate wage disadvantages associated with prior low pay and may even reinforce them for women, while benefiting men with high previous pay.

Table 4: The Impact of Bans on Log(Current Weekly Earnings)

	All	Men	Women
SHB x Low paid	-0.0177	0.0662	-0.169*
	(0.0459)	(0.0620)	(0.0883)
SHB x High paid	-0.0423	0.126*	-0.123
	(0.0724)	(0.0734)	(0.107)
Low paid	-0.117***	-0.111	-0.107**
	(0.0403)	(0.0713)	(0.0453)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	7,364	4,121	3,243

Notes: Results are coefficient estimates based on the regression specified in equation (17). *SHB* takes the value of 1 for treated regions in post-treatment period, and 0 otherwise. *Low paid* equals 1 if the individual's past wage was below the fitted value from the first-stage wage regression, and 0 otherwise. *High paid* equals 1 if the individual's past wage was above the fitted value. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

6.4 Job Transitions

Table 5 presents the estimates from the model specified in Equation (18), which examines the effect of SHBs on the probability of full-time employment among displaced workers. The coefficient for the full sample indicates that the implementation of an SHB is associated with a statistically significant decrease in the likelihood of being reemployed in a full-time position. The logit coefficient of -0.294 corresponds to an average marginal effect of -0.048, implying that displaced workers in states with an SHB in effect are about 4.8 percentage points less likely to be employed full-time relative to those in states without the ban, holding other factors constant. When the sample is separated by gender, the estimated effect is negative for both men and women, but only statistically significant for women. For women, the marginal effect is -0.065, indicating a reduction of about 6.5 percentage points in the probability of full-time employment following the introduction of the ban. Overall, the findings suggest that SHBs are linked to lower probability of full-time employment following displacement, and this reduction appears more pronounced for women.

Table 5: The Impact of Bans on Probability of Full-Time Employment

	All	Men	Women
SHB	-0.294***	-0.189	-0.332**
	(0.0961)	(0.148)	(0.148)
Marginal Effect	-0.048	-0.025	-0.065
	(0.017)	(0.020)	(0.030)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	11,490	6,624	4,866

Notes: Results show coefficient estimates from logistic specification in Equation (18) and corresponding average marginal effects. The dependent variable, "Full-Time Employment", equals one if the individual's current job is full-time. *SHB* is the difference-in-differences indicator for Salary History Ban implementation. All regressions include demographic controls, year fixed effects, and state fixed effects. Demographic control variables are sex, race, age, and education. Standard errors, in parentheses, are clustered at the state level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7. Conclusion

This paper analyzes the effects of SHBs on labor market reattachment of displaced workers. Using variation in the timing and scope of policy adoption across states and local jurisdictions, the analysis provides new evidence on how restricting employers' access to prior wage information shapes job search outcomes after a job loss. The results show that while these policies were introduced with the intention of reducing discrimination, they also generated unintended consequences for displaced workers, documenting that the introduction of SHBs is associated with longer unemployment durations and reduced likelihood of full-time reemployment, with particularly pronounced effects for women. In addition, women experience lower reemployment wages under the policy, while men experience either no change or slight relative gains. The policy is also associated with a widening of the gender pay gap among displaced workers. A further analysis shows that the wage effects depend on previous pay levels. Women who had lower prior wages experience marginally significant declines in reemployment wages when the ban is in place, whereas men with higher prior wages tend to experience wage gains. As a result, SHBs widen existing gender wage disparities among displaced workers and amplify differences between workers with high and low past earnings.

The magnitude of these adverse effects depends on the scope of the bans. States that extend the policy to both public and private sector employers experience stronger declines in job-finding rates, suggesting that broader transparency constraints can magnify information frictions in hiring. In contrast, states that introduced SHBs alongside other transparency reforms, such as pay range disclosure requirements, exhibit more muted effects. This pattern highlights the importance of the institutional environment in shaping how employers and workers adjust to changes in the information set available during hiring.

The findings have important implications for policy design. SHBs limit the information available to employers during the hiring process, and this reduction in transparency increases information frictions in wage bargaining and screening. The voluntary disclosure margin further shapes these frictions, since workers differ in their willingness to reveal past wages. As predicted by the search-and-matching model presented in this paper, differences in disclosure behavior help explain why men and women experience the policy differently and provide a potential channel for the gender differences observed in labor market reattachment and reemployment wages. These findings emphasize the importance of evaluating how transparency regulations and information frictions interact with job search behavior and hiring dynamics. Given that the resulting inefficiencies fall disproportionately on workers who were already in more vulnerable labor market positions, this study underscores the importance of designing policies that promote fairness without inadvertently harming employment prospects. Future work could further examine several dimensions that remain beyond the scope of this study. The effectiveness of SHBs depends in part on enforcement, and future research could investigate employers' compliance when prior wage information remains accessible through public records or informal networks. It would also be valuable to explore differences across sectors and occupational groups in the effects of the bans, as industries vary in the extent of wage bargaining, the degree of wage dispersion.

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Appendix

A. Figures

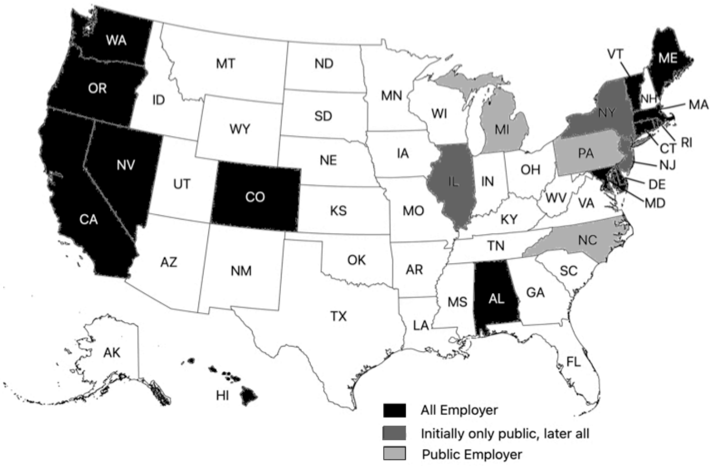

Figure A1: Map of Salary History Ban Implementation in the US

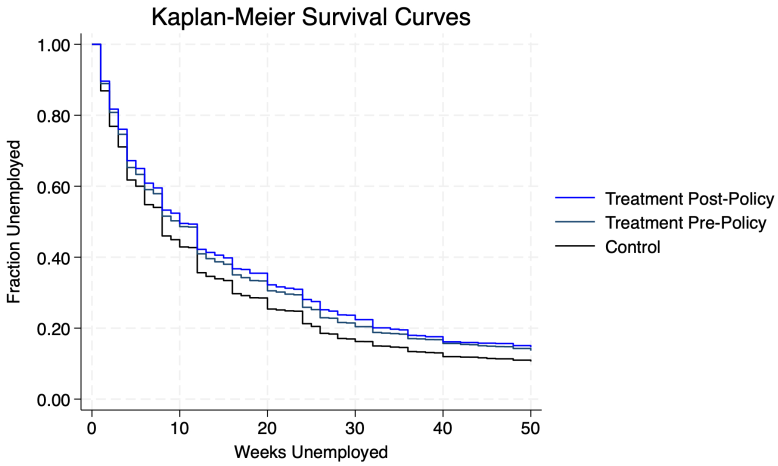

Figure A2: Survival Curves

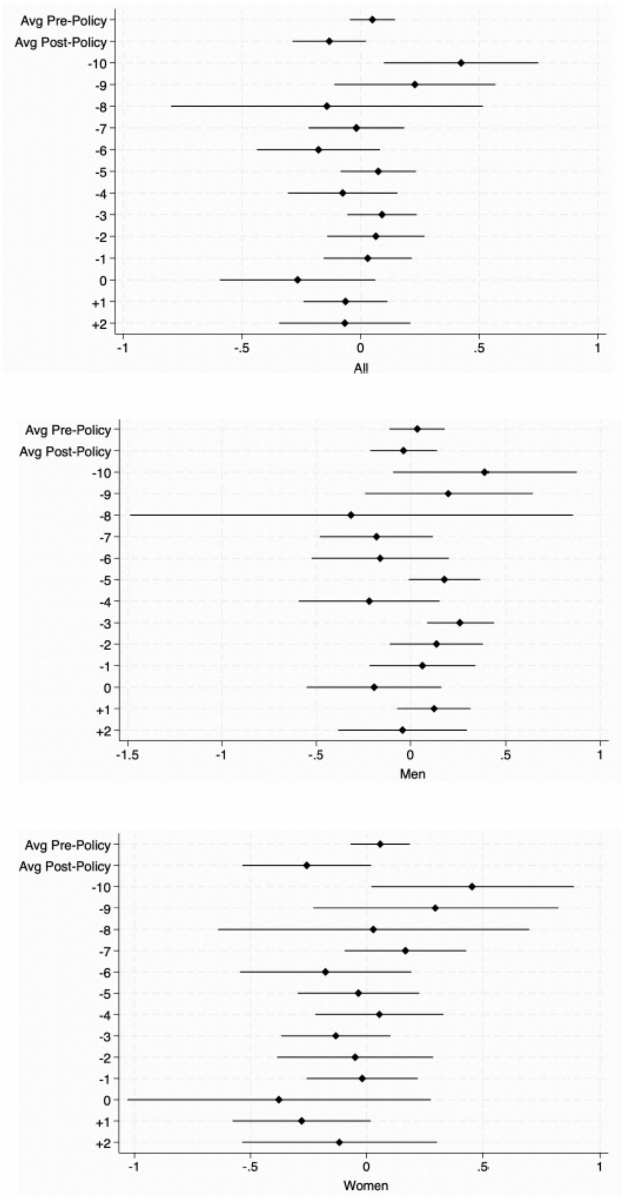

Figure A3: Event-Study Estimates of the Effect of SHBs on log(Current Weekly Earnings)
Notes: Estimates are obtained using the difference-in-differences framework proposed by Callaway and Sant'Anna (2021). The three graphs show dynamic treatment effects relative to the year of policy implementation for all individuals, men, and women, respectively. "Avg Pre-Policy" denotes the mean effect across pre-treatment periods. "Avg Post-Policy" denotes the mean effect across post-treatment periods.

B. Tables

Table B1: SHB Effective Dates as of May 2025 (in chronological order)
States and Non-States

State	**Effective Date**	**Public/All**	**Notes**
New York (NY)	Jan 2017	Public	Extended to all in Jan 2020
Oregon (OR)	Oct 2017	All	
Delaware (DE)	Dec 2017	All	
California (CA)	Jan 2018	All	
New Jersey (NJ)	Feb 2018	Public	Extended to all in Jan 2020
Massachusetts (MA)	Jul 2018	All	
Vermont (VT)	Jul 2018	All	
Pennsylvania (PA)	Sep 2018	Public	
Connecticut (CT)	Jan 2019	All	
Hawaii (HI)	Jan 2019	All	
Michigan (MI)	Jan 2019	Public	Banned SHBs in Jun 2018
Illinois (IL)	Jan 2019	Public	Extended to all in Sep 2019
North Carolina (NC)	Apr 2019	Public	
Washington (WA)	Jul 2019	All	
Virginia (VA)	Jul 2019	Public	
Maine (ME)	Sep 2019	All	
Alabama (AL)	Sep 2019	All	
Maryland (MD)	Oct 2020	All	
Colorado (CO)	Jan 2021	All	
Nevada (NV)	Oct 2021	All	
Rhode Island (RI)	Jan 2023	All	
Minnesota (MN)	Jan 2024	All	
Non-State	**Effective Date**	**Public/All**	**Notes**
Puerto Rico (PR)	Mar 2017	All	
District of Columbia (DC)	Nov 2017	Public	Extended to all in June 2024

Source: Author's compilation based mainly on data from HRDive, and verified using official state legislation sources.

Table B2: SHB Effective Dates as of May 2025 (in chronological order)
Counties and Cities

Counties	**Effective Date**	**Public/All**	**Notes**
Albany (NY)	Dec 2017	All	
Westchester (NY)	Jul 2018	All	
Richland (SC)	May 2019	Public	
Suffolk (NY)	Jun 2019	All	
Montgomery (MD)	Aug 2019	Public	
Lehigh (PA)	June 2024	All	Two cities are exempt
Cities	**Effective Date**	**Public/All**	**Notes**
New Orleans (LA)	Jan 2017	Public	Put more restrictions in Oct 2019
Pittsburgh (PA)	Jan 2017	Public	
New York City (NY)	Oct 2017	All	
Salt Lake City (UT)	Mar 2018	Public	
Chicago (IL)	Apr 2018	Public	
Louisville (KY)	May 2018	Public	
Kansas City (MO)	Jul 2018	Public	Extended to all in Oct 2019
San Francisco (CA)	Jul 2018	All	
Atlanta (GA)	Feb 2019	Public	
Jackson (MS)	Jun 2019	Public	
Columbia (SC)	Aug 2019	Public	
St. Louis (MO)	Mar 2020	Public	
Cincinnati (OH)	Mar 2020	All	
Toledo (OH)	Jun 2020	All	
Philadelphia (PA)	Sep 2020	All	
Columbus (OH)	Mar 2024	Private	

Source: Data obtained from HRDive.

Table B3: MSAs with More Than One State

MSA	States
MSA is a treated region	
Allentown-Bethlehem-Easton	***PA-NJ***
New York-Newark-Jersey City	***NY-NJ-PA***
Philadelphia*-Camden-Wilmington	***PA-NJ-DE***
Portland-Vancouver-Hillsboro	***OR-WA***
Virginia Beach-Norfolk-Newport News	***VA-NC***
Springfield	***MA-CT***
Worcester	***MA-CT***
MSA is a control region	
Augusta-Richmond County	**GA**-**SC**
Chattanooga	TN-**GA**
Clarksville	TN-**KY**
Duluth	MN-WI
Evansville	IN-**KY**
Fargo	ND-MN
Fayetteville-Springdale-Rogers	AR-**MO**
Fort Smith	AR-OK
Huntington-Ashland	WV-**KY**-**OH**
La Crosse	WI-MN
Memphis	TN-**MS**-AR
Minneapolis-St Paul-Bloomington	MN-WI
Omaha-Council Bluffs	NE-IA
MSA is a mixed region	
Boston-Cambridge-Newton	***MA***-NH
Boston-Cambridge-Quincy	***MA***-NH
Charlotte-Concord-Gastonia	***NC***-**SC**
Chicago*-Naperville-Elgin	***IL***-IN-WI
Cincinnati*	**OH**-**KY**-IN
Columbus	**GA**-***AL***
Davenport-Moline-Rock Island	IA-***IL***
Hagerstown-Martinsburg	***MD***-WV
Kansas City*∗	**MO**-KS
Kingsport-Bristol	TN-***VA***
Louisville*	**KY**-IN
Myrtle Beach-Conway-North Myrtle Beach	**SC**-***NC***
St. Louis*	**MO**-***IL***
South Bend-Mishawaka	IN-***MI***
Washington-Arlington-Alexandria	***DC***-***VA***-***MD***-WV
Winchester	***VA***-WV
Youngstown-Warren-Boardman	**OH**-***PA***
Norwich-New London	***CT***-RI
Providence-Fall River-Warwick	RI-***MA***
Rochester-Dover	NH-***ME***

Note: The bolded and italicized states indicate statewide implementation, while the bolded states represent those where only some regions have enacted the law. An MSA is classified as a control region if (*i*) all states within the MSA are control states, or (*ii*) some states within the MSA have implemented the law in certain regions, but none of those regions border the MSA. An MSA is classified as “mixed” if the MSA consists of (*i*) treated states and control states, or (*ii*) treated states and the states that have implemented the law only in certain regions but those regions do not border with the MSA, or (*iii*) control states and the states that have implemented the law only in certain regions and those regions borders with the MSA.

* The city implements the law.

*∗ Kansas City, MO implements the law while Kansas City, KS does not.

Table B4: Lost or Left Job and Displaced Status

Number of Workers	Displaced	Not Displaced	Total
Lost or left job	18,485	15,976	34,461
Did not lose or leave job	0	512,627	512,627
Total	18,485	528,603	547,088

Notes: Data are from the CPS Displaced Worker Supplement (DWS). "Displaced" indicates job loss due to plant closing, position elimination, or lack of work.

Table B5: Reason Lost or Left Job

	Number of Displaced Worker
Plant/company closed down or moved	5,602
Plant/company operating but lost/left job because of insufficient work	7,397
Plant/company operating but lost/left job because position or shift abolished	5,301
Total	18,300

Source: CPS Displaced Worker Supplement (DWS).

Table B6: The Impact of Bans on Job-Finding Hazard
Additional Control Variables

	(1)	(2)	(3)	(4)
SHB	-0.190***	-0.191***	-0.189***	-0.182***
	(0.0575)	(0.0560)	(0.0551)	(0.0562)
UI Early x 2021	No	No	No	Yes
Regular UI weeks	No	No	Yes	Yes
Previous job controls	No	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	9,906	9,906	9,906	9,906

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12). All states, states that implement the policy for either both private or public sectors or only public sector, are included. The mean of unemployment duration is 17.46 weeks. The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. "UI Early" is an indicator for whether the state terminated the Pandemic Unemployment Assistance (PUA) and the Federal Pandemic Unemployment Compensation (FPUC) early, data obtained from Holzer et al. (2021). "UI Early x 2021" is the interaction of "UI Early" with an indicator for whether the individual was displaced in 2021. "Regular UI weeks" is the maximum duration of unemployment benefits, data obtained from the U.S. Department of Labor. Previous job controls are the weekly earnings, tenure, full-time status, and the class at the previous job. Demographic control variables are sex, race, age, education category, and marital status. Unknown variables are assigned a random number, and a dummy for indicating missing value is added to the regression. Standard errors, in parentheses, are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table B7: The Impact of Bans on Job-Finding Hazard
Sectoral Variation in Policy Scope

	All	Men	Women
$SHB_{\text{Both Sectors Ban}}$	-0.210***	-0.0826	-0.391***
	(0.0585)	(0.0769)	(0.0832)
$SHB_{\text{Public Sectors Ban}}$	-0.112	0.0353	-0.318**
	(0.0983)	(0.111)	(0.147)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	9,906	5,728	4,178

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from the Equation (21). The mean of unemployment duration is 17.46 weeks. The coefficient for $SHB_{\text{Both Sectors Ban}}$ represents the change in the log of the job-finding hazard rate for the states that implement the policy for both private or public sectors. The coefficient for $SHB_{\text{Public Sectors Ban}}$ represents the change in the log of the job-finding hazard rate for the states that implement the policy only for public sectors. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Effective Years of Salary History Bans, Ban the Box, and Credit Checking Bans

State	**Salary History Bans**	**Ban the Box**	**Credit Checking Bans**
Alabama (AL)	2019	-	-
Arizona (AZ)	-	2017	-
California (CA)	2018	2010	2012
Colorado (CO)	2021	2012	2013
Connecticut (CT)	2019	2010	2011
Delaware (DE)	2017	2014	2014
District of Columbia (DC)	2017	2011	-
Georgia (GA)	-	2015	-
Hawaii (HI)	2019	1998	2009
Illinois (IL)	2019	2014	2011
Indiana (IN)	-	2017	-
Kansas (KS)	-	2018	-
Kentucky (KY)	-	2017	-
Louisiana (LA)	-	2016	-
Maine (ME)	2019	2019	-
Maryland (MD)	2020	2013	2011
Massachusetts (MA)	2018	2010	-
Michigan (MI)	2019	2018	-
Minnesota (MN)	2024	2009	-
Missouri (MO)	-	2016	-
Nebraska (NE)	-	2014	-
Nevada (NV)	2021	2017	2013
New Hampshire	-	2020	-
New Jersey (NJ)	2018	2014	-
New Mexico (NM)	-	2010	-
New York (NY)	2017	2015	-
North Carolina (NC)	2019	2020	-
North Dakota (ND)	-	2019	-
Ohio (OH)	-	2015	-
Oklahoma (OK)	-	2016	-
Oregon (OR)	2017	2015	2010
Pennsylvania (PA)	2018	2017	-
Rhode Island (RI)	2023	2013	-
Tennessee (TN)	-	2016	-
Utah (UT)	-	2017	-
Vermont (VT)	2018	2015	2012
Virginia (VA)	2019	2015	-
Washington (WA)	2019	2018	2007
Wisconsin (WI)	-	2016	-

Source: Doleac and Hansen (2020), Avery and Hernandez (2019), and Friedberg et al. (2021).

Table B9: The Impact of Bans on Job-Finding Hazard
Other Major Policies

	(1)	(2)	(3)
SHB	-0.194***	-0.201***	-0.203***
	(0.0539)	(0.0648)	(0.0602)
BTB	Yes	No	Yes
CCB	No	Yes	Yes
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	9,906	9,906	9,906

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate, based on regression results from Equations (22), (23), (24), corresponding to columns (1), (2), (3), respectively. All states, states that implement the policy for either both private or public sectors or only public sector, are included. The mean of unemployment duration is 17.46 weeks. The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. BTB is an indicator for the Ban-the-Box policies being in effect, CCB is indicator for the pre-employment credit checking bans being in effect. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table B10: Relevant State Statutes and Salary History Ban (SHB) Provisions

State	**Relevant Legislation**
California	California Equal Pay Act; California Labor Code (SHB added later, AB 168 and AB 2282); California Fair Employment and Housing Act
Colorado	Colorado Antidiscrimination Statute; Equal Pay for Equal Work Act (includes SHB)
Connecticut	Connecticut Labor Statute (SHB added later); Connecticut Human Rights Act
Delaware	Delaware Antidiscrimination Act; Delaware Wage Payment and Collection Act; General Provisions of Delaware's Labor Code (SHB added later)
D.C	D.C. Wage Transparency Law, D.C. Human Rights Act of 1977, D.C Salary History Ban (SHB)
Hawaii	Hawaii Equal Pay Law (included SHB), Hawaii Wage and Hour Law
Illinois	Illinois Equal Pay Act of 2003 (SHB was added later), Illinois Equal Wage Act, Wages of Women and Minors Act, Illinois Human Rights Act
Maine	Maine Equal Pay Law (included SHB), Maine Human Rights Act (SHB)
Maryland	Maryland Equal Pay for Equal Work Law (included SHB, HB123), Maryland Anti-Discrimination Law
Massachusetts	Massachusetts Equal Pay Law (included SHB), Massachusetts Anti-Discrimination Law
Michigan	Michigan Pay Transparency Law, Michigan Equal Pay Law, Michigan Civil Rights Act, Executive Directive No. 2019-10 (introduced SHB)
Nevada	Nevada General Anti-Discrimination Law, Nevada Equal Pay Law, Nevada Salary History Ban Law (SB293, included SHB)
New Jersey	New Jersey Law Against Discrimination, Diane B. Allen Equal Pay Act, Executive Order No. 1 (introduced SHB), New Jersey Salary History Ban (A1094, SHB for all sectors)
New York	New York Equal Pay Law, New York Human Rights Law, Executive Order 161 (introduced SHB), New York Salary History Ban (S6549, A5308 SHB)
North Carolina	North Carolina Equal Employment Practices Act, Executive Order No. 93 (introduced SHB)
Oregon	Oregon General Anti-Discrimination Law, Oregon Equal Pay Law (included SHB)
Pennsylvania	Pennsylvania Equal Pay Law, Pennsylvania Human Relations Act, Executive Order 2018-18-03 (SHB)
Vermont	Vermont Fair Employment Practices Law, Vermont Salary History Ban (H.294, SHB)
Virginia	Virginia Equal Pay Law, Employment Equity Initiative, SB370 - HB990
Washington	Washington Equal Pay Law (HB 1696, SHB), Washington Law Against Discrimination

Source: Author's compilation based on official state legislations.

Table B11: States Introducing Salary History Bans (SHBs) Separately or as Bundled Legislation

Introduced Separately	**Bundled with Other Legislation**
California	Alabama
Connecticut	Colorado
Delaware	Hawaii
District of Columbia	Illinois
Michigan	Maine
New Jersey	Maryland
New York	Massachusetts
North Carolina	Nevada
Vermont	Oregon
	Pennsylvania
	Virginia
	Washington

Source: Author's compilation based on official state legislations.

Table B12: The Impact of Bans on Job-Finding Hazard
Bundled vs Separate Implementation

	Bundled States	Separated States
SHB	-0.151	-0.294***
	(0.0937)	(0.0436)
Demographic controls	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	6,888	7,094

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12). The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. Bundled States and Separated States are given in the Table B11. Demographic control variables are sex, race, age, and education category. Standard errors, in parentheses, are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table B13: The Impact of Bans on Job-Finding Hazard
MSA Analysis

	(1)	(2)
SHB	-0.162***	-0.177***
	(0.0628)	(0.0618)
Demographic controls	Yes	Yes
Year FE	Yes	Yes
MSA FE	Yes	Yes
Observations	9,203	9,203

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12). The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. In column 1, the entire MSA is considered treated if any part of it, including a single city, implemented the law, reflecting the assumption that policies can spillover across the metropolitan region. In column 2, only the portions of the MSA located within the state that passed the law are considered treated, reflecting the fact that the law applied strictly at the state level. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at MSA level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B14: The Impact of Bans on Job-Finding Hazard
Parametric Regression Models

	Weibull Regression Model	Exponential Regression Model
SHB	-0.217***	-0.252***
	(0.0654)	(0.0744)
Demographic controls	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
$log(\rho)$	-0.158***	-
	(0.00855)	-
Constant	-2.356***	-2.849***
	(0.0849)	(0.110)
Observations	9,906	9,906

Notes: Results are coefficient estimates from the parametric Weibull and Exponential regression model of the job-finding hazard rate based on regression results from Equations (25) and (26), respectively. The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. All states, states that implement the policy for either both private or public sectors or only public sector, are included. The mean of unemployment duration is 17.46 weeks. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at the state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B15: SIPP
The Impact of Bans on Job-Finding Hazard
All Involuntary Separations
Censored at 52 weeks

	All	Men	Women
SHB	-0.0152	0.0958*	-0.159***
	(0.0395)	(0.0578)	(0.0585)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	8,329	4,234	4,117

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12), using data from SIPP. The sample includes respondents who were involuntarily separated from their jobs. The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. All regressions include demographic control variables, and year- month and state fixed effects. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B16: SIPP
The Impact of Bans on Job-Finding Hazard
Plant closed, slack work, abolished shift
Censored at 52 weeks

	All	Men	Women
SHB	0.0431	0.145	-0.139*
	(0.0514)	(0.0934)	(0.0810)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	4,971	2,561	2,424

Notes: Results are coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate based on regression results from Equation (12), using data from SIPP. The sample includes respondents who were involuntarily separated from their jobs. The coefficient for SHB represents the change in the log of the job-finding hazard rate after the ban. All regressions include demographic control variables, and year- month and state fixed effects. Demographic control variables are sex, race, age, education category, and marital status. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B17: The Impact of Bans on log(Current Weekly Earnings)
Simple Difference-in Differences Regression
Panel Structure

	All	Men	Women
SHB	-0.0593	0.0798	-0.210**
	(0.0478)	(0.0496)	(0.0917)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	14,728	8,242	6,486

Notes: Results are coefficient estimates based on the regression specified in equation (14). The data are transformed into a panel structure for analysis. The outcome variable is log(Current Weekly Earnings). Demographic control variables are sex, race, a polynomial (cubic) in age in displaced year, education category, and marital status. Previous job controls are a polynomial (cubic) in tenure in the displaced job, full time - part time status in the displaced job; sector, industry, and occupation in the displaced job. All regressions include year and state fixed effects. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B18: The Impact of Bans on log(Current Weekly Earnings)
Callaway and Sant'Anna Approach

	All	Men	Women
SHB	-0.1306*	-0.0294	-0.2753**
	(0.0754)	(0.0882)	(0.1305)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	6,578	3,604	2,880

Notes: Results are coefficient estimates based on the regression specified in equation (14). Estimates are obtained using the group-time average treatment effects estimator proposed by Callaway and Sant'Anna (2021). The outcome variable is log(Current Weekly Earnings). Demographic control variables are sex, race, a polynomial (cubic) in age in displaced year, education category, and marital status. Previous job controls are a polynomial (cubic) in tenure in the displaced job, full time - part time status in the displaced job; sector, industry, and occupation in the displaced job. All regressions include year and state fixed effects. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B19: The Impact of Bans on log(Current Weekly Earnings)
Sun and Abraham Approach

	All	Men	Women
SHB	-0.1017	0.01213	-0.2438*
	(0.0762)	(0.0828)	(0.1394)
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	14,728	8,242	6,486

Notes: Results are coefficient estimates based on the regression specified in equation (14). Estimates are obtained using the event-study estimator proposed by Sun and Abraham (2021). The outcome variable is log(Current Weekly Earnings). Demographic control variables are sex, race, a polynomial (cubic) in age in displaced year, education category, and marital status. Previous job controls are a polynomial (cubic) in tenure in the displaced job, full time - part time status in the displaced job; sector, industry, and occupation in the displaced job. All regressions include year and state fixed effects. Standard errors, in parentheses, are clustered at state level.
*** p<0.01, ** p<0.05, * p<0.1.

Table B20: The Impact of Bans on Other Job Transition Outcomes

	All	Men	Women
Panel A: Number of Jobs Changed			
Poisson regression coefficient	-0.0384	0.0245	-0.124**
	(0.0300)	(0.0380)	(0.0527)
Marginal Effect	-0.037	0.025	-0.110
	(0.028)	(0.039)	(0.044)
Observations	16,663	9,273	7,390
Panel B: To Private Sector			
Logit regression coefficient	-0.263*	-0.235	-0.277
	(0.152)	(0.230)	(0.212)
Marginal Effect	-0.020	-0.014	-0.026
	(0.013)	(0.015)	(0.021)
Observations	14,058	7,984	6,063

Notes: Panel A reports estimates from the Poisson regression model specified in Equation (19) for the number of jobs changed since displacement. Panel B reports estimates from the logit specification in Equation (20) for the probability that the current job is in the private sector. “Number of Jobs Changed” is the number of jobs changed since being displaced. “To Private Sector” is a dummy variable equal to one if the respondent moved into a private-sector job (from either a private or public job previously), and zero if they moved into or remained in the public sector. All regressions include demographic controls, year fixed effects, and state fixed effects. Demographic control variables are sex, race, age, and education. Standard errors, in parentheses, are clustered at the state level.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B21: The Impact of Bans on Other Variables

	Moved Since Job Loss	Received Unemp. Benefits	Unable to work due to pandemic
Logit coefficient	0.0735	0.248***	-0.0206
	(0.140)	(0.0619)	(0.687)
Marginal Effect	0.007	0.055	-0.001
	0.014	0.014	0.049
Demographic controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	17,254	16,857	1,637

Notes: Results show coefficient estimates from logistic regressions and average marginal effects. "Moved Since Job Loss" is a dummy variable for moving to a different city/county since job loss. "Received Unemp. Benefits" is a dummy variable for receiving unemployment benefits. "Unable to work due to pandemic" is a dummy variable for being unable to work due to COVID-19 pandemic. Demographic control variables are sex, race, age, and education. Standard errors, in parentheses, are clustered at the state level.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

C. Survival Model Definitions and Notations

This section summarizes standard definitions and notation for survival analysis, following Cameron and Trivedi (2005).

Let $T > 0$ denote the duration of unemployment. The survivor function gives the probability that an individual remains unemployed beyond time t:

$$S(t) = Pr(T > t). \tag{C27}$$

The hazard function is the instantaneous probability of exiting unemployment, conditional on still being unemployed at time t.

$$h(t) = \frac{f(t)}{S(t)} = -\frac{dln(S(t))}{dt}. \tag{C28}$$

I estimate a Cox proportional hazards model, a commonly used semi-parametric approach to survival analysis. The model assumes the hazard can be expressed as

$$h(t|\mathbf{x}, \beta) = h_0(t)\phi(\mathbf{x}, \beta), \tag{C29}$$

where $h_0(t)$ is the baseline hazard, which has an unspecified functional form, while the functional form of $\phi(\mathbf{x}, \beta)$ is fully specified. The most common specification for $\phi(\mathbf{x}, \beta)$ is the following:

$$\phi(\mathbf{x}, \beta) = exp(\mathbf{x}'\beta). \tag{C30}$$

Substituting equation (C30) into the hazard function yields

$$log[h(t|\mathbf{x}, \beta)] = log[h_0(t)] + \mathbf{x}'\beta. \tag{C31}$$