Salary History Bans and Healing Scars from Past Recessions*

Joshua Mask†

July 15, 2021

Click here for the latest version

Abstract

In a recession, increased competition forces inexperienced job market entrants to accept lower wages than those who start their careers during an economic boom. Yet despite years of improvement in labor market conditions following a recession, a wage disparity, known as scarring, persists between these cohorts. I use Salary History Ban laws (SHBs) to test whether job mobility for scarred workers is constrained because employers screen on prior compensation. For scarred workers who began their careers during a moderate-to-severe recession, or a 5 percentage point higher state unemployment rate, I find SHBs increase job mobility by 0.6%, hourly wages by 3.4%, and weekly earnings by 5.45% relative to workers who graduated in baseline labor market conditions. These estimates represent a substantial reduction in the original scarring effect and provide evidence this effect partially persists due to salary disclosure.

Keywords: wage scarring, labor discrimination, salary history bans

JEL Codes: J11, J15, J16, J24, J31

*For their valuable guidance, I thank Darren Lubotsky, Ben Ost, Benjamin Feigenberg, Erik Hembre, and Eliza Forsythe. I additionally appreciate feedback from Hannes Schwandt, Jeff Meli, Laura Gee, participants at the Western Economic Association International (WEAI) 95th Annual Conference, and participants at the New York State Economics Association 2020 Annual Conference.

†Department of Economics, Temple University (email: joshua.mask@temple.edu)

1 Introduction

Empirical literature has shown that entering the labor force during a recession results in a negative wage disparity, called wage scarring, that can last decades (Kahn 2010; Oreopoulous et al. 2012). Under adverse labor market conditions, inexperienced job-seekers have more difficulty finding employment. With fewer outside options, salary negotiations strongly favor employers. As a result, these individuals accept lower initial wage offers. However, it unclear why this wage disparity persists years after economic conditions have improved. Two prevailing theories posit differences in human capital accumulation or job search friction as potential mechanisms. Focusing on the latter, I use Salary History Ban laws (SHBs) to test whether job mobility for scarred workers is constrained due to employers screening job-seekers on prior wages. I find SHBs increase job mobility for scarred workers relative to non-scarred workers and reduce the gap in hourly wages and weekly earnings between these workers. This finding contributes to the wage scarring literature because it represents the first evidence, to my knowledge, that wage scarring is partially caused by job search friction related to salary disclosure.

Several US states began passing SHBs in 2017 in an effort to eliminate gender and racial wage disparities. These laws explicitly bar employers from asking job applicants about prior or current compensation during the hiring process. Given that some job applicants may still volunteer this information, there exists a possibility for adverse selection (Agan, Cowgill, and Gee 2020). Despite these adverse selection concerns, an emerging empirical literature has shown that SHBs reduce gender and racial wage gaps (Hansen and McNichols 2020; Sinha 2019; Bessen, Denk, and Meng 2020). One potential explanation is that employers have increasingly responded to SHBs by including target pay information in job postings (Ibid; Sran, Vetter, and Walsh 2020). Increased pay transparency increases information for job-seekers and arguably eliminates the need to discuss salary history in the first place.

To estimate the overall effect of SHBs, I use a difference-in-differences (DiD) empirical strategy that exploits state-by-year variation in SHB enactment on a sample of working-age adults from the Current Population Survey (CPS). I then extend this analysis to account for wage scarring by splitting the sample between "scarred" and "non-scarred" workers using a method proposed by Schwandt and Von Wachter (2019). In DiD event studies with each sample, I find SHBs raise hourly wages and weekly earnings for scarred workers but have no statistically significant effect on compensation for non-scarred workers. I also find SHBs increase job mobility for scarred workers but reduce job mobility for non-scarred workers. This reduction in job mobility may be the result of higher-paid individuals' inability to signal higher wages after SHBs are enacted (Meli and Spindler 2019). These event studies also confirm parallel pre-trends in compensation and job mobility between scarred and non-scarred workers.

I then directly estimate how the effect of SHBs varies for workers affected by early-career job-market conditions using a difference-in-differences-in-differences (DDD) empirical strategy. This model fully interacts the state-by-entry-year unemployment rate[1] with the original DiD model. For workers who started their careers during a moderate recession, or a 5 percentage point higher state unemployment rate, I find SHBs increase job mobility by 0.6%, hourly wages by 3.4%, and weekly earnings by 5.45% relative to workers who graduated in baseline labor market conditions. Additionally, I show these estimates represent a 90% reduction in the original scarring effect for workers with one to five years of experience.

Given that SHBs originated with gender and racial wage disparities in mind,[2] I also test the effect of SHBs on wage scarring between men and women and between whites and non-

[1]This variable, first proposed by Schwandt and Von Wachter (2019), merges state-of-residence, current year, and potential experience in the Current Population Survey with historic state unemployment data to approximate the state-by-entry-year unemployment rate for each person in the sample. This is discussed in detail in Section 4.2.

[2]https://www.nytimes.com/2019/10/22/us/dont-ask-me-about-my-salary-history.html

whites. This represents another contribution to the wage scarring literature as the scarring effect has been found to differ across demographics (Schwandt and Von Wachter 2019). After splitting the sample between male and females, I use the aforementioned DDD estimation strategy and find that SHBs increase job mobility, hourly wages, and weekly earnings for scarred men and women relative to non-scarred men and women, with a smaller effect observed for men. In a separate analysis, I split the sample between whites and non-whites and find that SHBs have a small effect on wage scarring for whites but substantially raise hourly wages and weekly earnings for scarred non-whites. Additionally, I find that scarred whites and non-whites increase job mobility relative to non-scarred whites and non-whites. Finally, I observe increased unemployment-to-employment transitions for scarred whites relative to non-whites.

This study is also the first to my knowledge to explore a potential policy intervention for wage scarring. Traditionally, wage scarring has been thought of as a catch-up scenario. In a perfectly competitive labor market with perfect information, scarred workers transition to higher-paid positions as the economy improves. Policies that target increased job switching therefore might prove useful (Oreopoulos et al. 2012). However, I provide evidence that an additional channel inhibits this process. Although banning salary history disclosure increases job mobility for scarred workers relative to non-scarred workers, much larger relative effects are found with wages and earnings. If compensation growth is partially path dependent through disclosure, then it may not matter as much how often scarred workers switch jobs. Each subsequent job switch would yield a lower increase in wages than their non-scarred counterparts. This suggests that compensation parity between these cohorts may require even more job switching by scarred workers.

Finally, despite my finding that SHBs improve employment and wage outcomes for scarred workers in general, policymakers should also consider the unintended consequences. While

SHBs increase job mobility for scarred workers, I also find they decrease job mobility for non-scarred workers. This could be the result of higher paid individuals (non-scarred workers) being unable to signal market-perceived quality to employers through salary history disclosure (Meli and Spindler 2019). If the benefits of SHBs are partially explained through increased pay transparency, then pay transparency laws may achieve the same desirable effects without harming job mobility for these individuals.

2 Conceptual Framework

2.1 Scarring

Negative consequences associated with entering the labor market during a recession have been well documented. Kahn (2010) shows that US college graduates entering the job market during a recession experience a wage decline that lasts 20 years. A number of other studies have shown similar findings in other countries (Oreopoulus et al. 2012; Liu and Chen 2014; Kondo 2007; Choi, Choi, and Sun 2020; Brunner and Kuhn 2014). The literature also shows substantial heterogeneity within this effect across race and educational attainment. Notably, non-whites and high school graduates exhibit much larger scarring effects (Schwandt and Von Wachter 2019; Hershbein 2012; Altonji, Kahn, and Speer 2016). Inexperience in a job search is also more costly in a recession. Forsythe (2016) shows firms are less likely to hire inexperienced workers during recessions.

However, it is less clear why this disparity persists once labor market conditions improve. The literature focuses on two theoretical themes to explain the long-run nature of wage scarring: human capital accumulation and job search friction. The first and most studied theme, human capital accumulation, posits that individuals entering the labor market during a recession match poorly with their first jobs. With less available jobs and more competition, they are forced to take positions that do not directly involve tasks related to their training. As a result, they accumulate less industry-specific human capital than they would otherwise, resulting in long-term productivity disparities (Kahn 2010). Liu, Salvanes, and Sørensen (2016) find the quality of one's first employer can be a major contributor to the wage scarring gap. Arellano-Bover (2020) shows initial career firm size affects job skill growth and that inexperienced workers match more with smaller firms during economic downturns. In Hagedorn and Manovskii (2013), they "...develop a method to measure match qualities and show empirically that various variables summarizing past aggregate labor market conditions have explanatory power for current wages only because they are correlated with match qualities" (771).

The second theoretical theme, job search friction, is based on contract theory work by Beaudry and Dinardo (1991) and posits that workers experience long-run scarring effects if subsequent job mobility is constrained. Workers that enter employment during a recession take employment contracts that pay lower wages than employment contracts offered during economic boom periods. As the economy improves, this disparity disappears as scarred workers leave lower-paying employment contracts for better-paying ones. However, if these workers cannot change jobs due to search costs, lack of information, or other labor market friction, they will remain in these contracts. Oreopoulous et al. (2012) theorizes this search friction is related to age. With incomplete information, searching for a new job takes time. These costs increase with age and scarred workers may stop changing jobs long before non-scarred workers. Forsythe (2019) shows that within-firm mobility also declines with age. Kwon et al. (2010) finds that individuals who graduate during a recession are also promoted less frequently.

2.2 Salary History Bans

This paper focuses on the second theoretical theme, job search friction, by testing a mechanism not previously considered: employer screening through salary history disclosure. There are many reasons why an employer would want to know a job-seeker's salary history. According to Barach and Horton (2021), "in a competitive labor market, a very recent wage in a similar job is approximately the worker's marginal productivity–precisely what a would-be employer is interested in learning (Kotlikoff and Gokhale 1992; Altonji and Pierret 2001; Lange 2007; Oyer and Schaefer 2011; Kahn and Lange 2014)" (194). Employers also gain an advantage in salary negotiations from this information as they learn more about the job-seeker's reservation wage while the job-seeker remains unaware of the employer's offer expectations.

Survey evidence confirms that many employers do ask job-seekers about their prior salary prior to making an offer. Hall and Krueger (2012) find 47% of respondents in a national representative survey had been asked about past wages at some point in their career. Payscale (2017) also finds that 43% of respondents had been asked about salary history in the past year. If workers accept lower wages during recessions and then disclose this lower wage to potential employers in a subsequent job search, the potential employer might infer they are less productive and not hire them. Therefore, salary history disclosure could plausibly perpetuate scarring effects.

To test how salary history disclosure affects compensation and job mobility for scarred individuals, I exploit variation in the passage of SHB laws. As of June 1, 2021, 15 states have passed SHBs for all employers and 4 states have passed SHBs for public employers. Figure 1 shows the states that have passed SHB laws. States highlighted in green ban state public sector employers and contractors from discussing salary history prior to a job offer, while states highlighted in red ban all employers. Curiously, Wisconsin, highlighted in orange, took the opposite approach and passed a law that prevents local municipalities from passing local SHB laws.[3]

[3]There have also been efforts to ban salary history disclosure on the national level. The Paycheck Fairness Act, which included a provision to ban salary history questions nationwide, was introduced in Congress in

Advocates of SHBs believe salary history disclosure requirements during hiring are discriminatory and perpetuate existing gender and racial wage gaps. They argue that banning the practice would force employers to offer market wages instead of wages tied to prior discrimination.[4] In an experiment using an online job market, Barach and Horton (2021) show that removing salary history information during hiring results in employers evaluating 7% more applicants and hiring workers with 13% lower average past wages. However, given that these findings are derived in a controlled setting, it is still unclear how this law might work in practice.

An emerging theoretical literature offers several predictions on expected outcomes of SHB enactment in a broader setting. One prediction is that if job-seekers typically lie about their salary history when asked to disclose, then this information is not valuable and there would be no effect from banning the practice of salary disclosure (Khanna 2020). Another prediction is that if employers adhere to the law but some job-seekers with higher prior wages continue to volunteer this information, then banning the practice can result in a temporary effect. In this setting, employers infer that any job-seeker who does not disclose salary history is of lower quality. Job-seekers with marginal salary histories may initially refuse to disclose salary history, but they would be increasingly incentivized to do so to avoid discrimination. As more and more job-seekers disclose, the initial effect of the law would unravel (Agan, Cowgill, and Gee 2020). Finally, if employers and job-seekers both stop discussing salary history after SHBs become law, then the law could work similarly to the controlled experiment in Barach and Horton (2021). However, this could also result in unintended consequences as job-seekers with higher salary histories can no longer signal this information to employers (Meli and Spindler 2019).

January 2019 but failed to pass in both chambers. https://www.shrm.org/resourcesandtools/legal-and-compliance/employment-law/pages/congress-considers-nationwide-ban-on-salary-history-inquiries.aspx

[4] https://www.washingtonpost.com/news/on-leadership/wp/2015/04/14/the-worst-question-you-could-ask-women-in-a-job-interview/

Early empirical evidence shows SHBs reduce gender and racial wage gaps (Hansen and McNichols 2020; Sinha 2019; Bessen, Denk, and Meng 2020). These results align closest with the prediction that both employers and job-seekers no longer discuss salary history. However, it is unclear how policymakers could prevent job-seekers from volunteering this information. Changes in employer behavior may offer a more plausible explanation. Sran, Vetter, and Walsh (2020) and Bessen, Denk, and Meng (2020) provide evidence that after SHB enactment, bargaining and screening become too costly and employers post more jobs with target salary information. This provides more information to job-seekers and makes salary history discussions less relevant as employers no longer enjoy an advantage in salary negotiations.

If wage scarring is solely caused by human capital accumulation, then I do not expect a differential effect between scarred and non-scarred workers with either mechanism of strict adherence or changes in job posting behavior. However, both mechanisms would yield differential effects between scarred and non-scarred workers if wage scarring is partially caused by job search friction. With strict adherence to the law, employers lose information on scarred workers' salary history and expand their applicant pool. With changes in job posting behavior, scarred workers gain information and become more likely to apply to new jobs.

3 Data

My primary data source is the January 2013 to May 2021 CPS (Flood et al. 2020). This survey samples roughly 60,000 households each month. Each household is surveyed for four consecutive months, followed by an eight-month period of no surveying. After eight months, surveying resumes and each household is surveyed for an additional four months. The monthly data measure employment outcomes and hours worked. The survey also provides basic demographic information like state, gender, race, and educational attainment. Potential experience is calculated as age minus years of education minus 6. Graduation year (or job-market-entry year) is approximated by subtracting potential experience from the current year. Information on job-to-job transitions is provided for survey periods 2–4 and 6–8. Additionally, unemployment-to-employment transitions can be inferred by observing month-to-month changes in the employment variable.

The Outgoing Rotational Group (CPS-ORG) survey is a supplemental survey conducted in the fourth and eighth survey periods. Respondents are asked whether they are paid hourly or salary, and those paid hourly provide information on hourly wage and weekly earnings. Respondents who are paid salary provide information on weekly earnings. Following the methodology used by the Center For Economic and Policy Research (CEPR)[5] and Schmitt (2003), I create a consistent hourly wage series by dividing weekly earnings for all workers by their average number of hours worked. If hourly wage workers report a higher hourly wage than the wage computed from dividing weekly earnings by average number of hours worked, then the original hourly wage is used. Weekly earnings and hourly wages are also normalized to 2018 dollars using the Consumer Price Index for All Urban Consumers[6] and subsequently converted to logs.

Similar to prior scarring literature (Oreopoulos et al. 2012; Schwandt and von Wachter 2019), I use a cell-based model by aggregating outcomes at the level of current state of residence, job-market-entry year, gender, race, and educational attainment. Cell-level data allow me to work closer to level of variation of my treatment, the staggered state-by-state implementation of SHB laws. Additionally, cells are reweighted based on weights provided by the CPS and CPS-ORG data to reflect population-level estimates. Following Schwandt and von Wachter (2019), I also merge the historical state unemployment rate for each state-

[5] http://ceprdata.org/cps-uniform-data-extracts/cps-outgoing-rotation-group/cps-org-faq/
[6] https://fred.stlouisfed.org/series/CPIAUCSL

by-entry-year group in order to approximate economic conditions at job market entry.

To measure the effects of SHBs on the prime-age working population, I restrict the sample to individuals between the ages of 18 and 45 with at least 1 to 20 years of potential experience. I also drop public sector state workers since these individuals could be affected by public sector SHBs not included in my treatment variable.[7] Table 1 provides a summary table of the sample used in my empirical analysis. The average respondent is 30 years old with nearly 10 years of experience. Twenty-eight percent of the sample possess a high school diploma, while another 36% possess a college degree, and 78% of the sample are white.

In Table 2, I divide the sample by states that implemented an SHB and states that did not. States that pass SHB laws are more educated and more racially diverse. I use a DiD and DDD empirical strategy, so fixed demographic differences across states are less of a concern than how these states trended prior to policy implementation. In Section 5, I provide evidence that trends between SHB and non-SHB states are parallel prior to policy implementation. I also provide evidence of the validity of my DDD design by confirming that separate samples of scarred and non-scarred workers also have parallel trends prior to SHB enactment.

4 Empirical Strategy

4.1 SHB Effect

My empirical strategy relies on the staggered implementation of SHB laws across states to measure the differential effect of SHBs across scarred and non-scarred workers. To first assess how SHB laws affect employment and wage outcomes in general, I employ a two-way

[7]Self-employed workers are also dropped. In Appendix Tables A1 and A2, I show that results are robust to including both self-employed workers and public sector state workers.

fixed effects or DiD model. This specification is similar to other papers in the SHB literature (Hansen and McNichols 2020; Sinha 2019; Bessen, Denk, and Meng 2020) and is meant to provide the reader with a baseline estimate of general effect of SHB enactment using the specific sample data described in Section 3.

Specification 1: $\bar{y}_{g,s,t,e,k} = \alpha + \beta D_{s,t} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}$.

Employment or compensation outcomes, $\bar{y}_{g,s,t,e,k}$, are regressed on an indicator variable, $D_{s,t}$, that takes a value of one if a respondent lives in an SHB state after the ban goes into law and zero otherwise. Table 3 provides an overview of the treated states and the number of observations comprising each pre- and post-period. Treatment starts in the year that each state passes an SHB law,[8] with New York State as the only exception. While a statewide private-sector SHB did not become law in New York State until 2020, New York City and Albany County independently passed a ban in 2017.[9] Given that at least 40% of New York state's population was treated in 2017, I code the state as passing the ban in 2017.[10]

Year and state fixed effects, Φ_t and Φ_s, respectively, are included so that β represents the difference in outcomes in SHB states before and after the ban, after accounting for national trends. Potential experience fixed effects, Φ_e, and predicted year of job-market-entry fixed effects, Φ_k, are included to ensure workers with similar tenure in the labor force are being compared to one another. I also include a vector of demographic-group-level fixed effects, Φ'_g, for gender, race[11], and educational attainment to ensure that individuals with similar demographic characteristics and labor market experience are being compared to one another.

To assess the validity of my DiD empirical strategy, I use an event study to visually inspect

[8] https://www.hrdive.com/news/salary-history-ban-states-list/516662/

[9] https://legistar.council.nyc.gov/LegislationDetail.aspx

[10] In Appendix Tables A.3 and A.3, I show main results are robust to the exclusion of New York.

[11] Race is a dummy variable equal to one if an individual identifies as white and is zero otherwise.

whether there are pre-existing trends between treated and non-treated states that could be driving my results. This estimation is an indirect test of the common trends assumption.

Specification 1a:

$$\bar{y}_{g,s,t,e,k} = \alpha + \sum_{j=-4}^{-2} \beta_j D_{s,t+j} + \sum_{j=0}^{4} \beta_j D_{s,t+j} + \Phi_s + \Phi_t + \Phi_k + \Phi_e + \Phi'_g + \varepsilon_{g,s,t,e,k}.$$

In this estimation, seven DiD estimates (β_j) are estimated for each year relative to the year prior to SHB enactment, t–1. If years t–4, t–3, and t–2 show no statistically significant difference from year t–1, then this provides suggestive evidence that differential pre-existing trends do not exist between these states prior to SHB enactment.

4.2 SHB Effect by Scarring

The second part of my empirical strategy approximates job market entry economic conditions for each respondent. Since the CPS does not contain information on the exact year a person enters the labor market, I use a method proposed by Schwandt and von Wachter (2019). The state-of-residence is combined with the predicted year of job market entry, based on potential experience and the current year, to create a variable called state-by-entry-year. Scarring is then measured using the historical unemployment rate[12] observed for each state-by-entry-year combination. Given this measure doesn't account for individuals who take longer to graduate or move states after they graduate, there are obviously selection concerns with this method. However, the authors test these concerns using an alternate double-weighted estimator that incorporates trends in graduate rates and geographic mobility and confirm these selection concerns have a negligible impact on estimates when using the CPS, the dataset used in this analysis.

[12] http://download.bls.gov/pub/time.series/la/la.data.3.AllStatesS.txt

Figure 2 shows a graph of the range of business cycle activity individuals in the sample faced when they first entered the labor market. Given that the sample is restricted to people with 1–20 years of experience, 1993 would be the earliest year an individual in the data would have entered the labor force. As the figure shows, this range of potential labor market entry years allows me to estimate scarring from peak to trough in the unemployment rate for three recessions: the early 90s recession, the dot-com bubble, and the Great Recession.[13]

The specification for the second part of my empirical strategy is as follows:

Specification 2:

$$\bar{y}_{g,s,t,e,k} = \alpha + \eta_{s,t,k}(D_{s,t} * ue_{s,k}) + \delta D_{s,t} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \Phi_{s,k} + \Phi_{t,k} + \varepsilon_{g,s,t,e,k}.$$

In this specification, the state unemployment rate in each respondent's job market entry year, $ue_{s,k}$, is interacted with the SHB indicator variable, $D_{s,t}$. I also include state fixed effects (Φ_s), year fixed effects (Φ_t), potential experience fixed effects (Φ_e), job-market-entry-year fixed effects (Φ_k), a vector of group fixed effects (Φ'_g),[14] state-by-entry-year fixed effects ($\Phi_{s,k}$), and year-by-entry-year fixed effects ($\Phi_{t,k}$)[15]. Therefore, the interaction coefficient, $\eta_{s,t,k}$, is a DDD estimate of the SHB laws effect on scarred workers relative to non-scarred workers. δ is also reported as a measure of the baseline SHB effect (similar coefficient to Specification 1). For every percentage point increase in the state unemployment rate in a

[13] I limit my analysis to individuals who began their careers between 1993 and 2019. Although my results are robust to the inclusion of the 2020 cohort, there is an ongoing debate in the literature over how much unemployment rates reported by the Bureau of Labor Statistics (BLS) for early-to-mid-2020 reflect true unemployment versus furloughed workers who received Pandemic Unemployment Assistance (PUA).

[14] Specification 2 uses the same vector of group fixed effects as Specification 1.

[15] Results are robust to using individual interaction terms between the entry-year state unemployment rate and each control. I use group interaction terms as this method absorbs additional variation unrelated to changes in SHB laws and scarring.

respondent's job market entry year, $\eta_{s,t,k}$ represents the increase in log wages or log weekly earnings from the passage of SHB laws relative to individuals who started their careers in baseline labor market conditions.

4.3 General Scarring Effect

To understand how my DDD estimates in Specification 2 compare to general estimates of wage scarring, I use the following specification[16]:

Specification 3: $\bar{y}_{g,s,t,e,k} = \alpha + \lambda ue_{s,k} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}$.

This specification estimates the average scarring effect over 20 experience years for workers in the sample. Similar to Specification 1, I control for state fixed effects, year fixed effects, job-market-entry-year fixed effects, potential experience, and group-level fixed effects.[17] λ measures the average scarring effect for every percentage point change in $ue_{s,k}$ for workers with 1–20 years of experience.

I also stratify the scarring effect by experience to assess how the effect is distributed across years of experience. Scarring has an initial effect that slowly decays as the person gains experience, so it is important to understand how experience affects average measures of wage scarring.[18] I use the following specification:

Specification 3a:

[16]This specification is similar to the method proposed in Schwandt and Von Wachter (2019). I additionally add controls for gender and race to be consistent with other specifications in this paper.

[17]Specification 3 also uses the same vector of group fixed effects as Specification 1.

[18]Ideally, I would also want to stratify my DDD estimates by experience to understand how this reversal is distributed across years of experience, but I unfortunately lack statistical power to stratify them in this manner. However, I do have enough power if I divide experience into groups of five years or more. In Table 11, I divide the sample into five-year experience groups and find most of the DDD estimate comes from earlier experience workers. This finding is consistent with estimates in Table 10 that show most of the average scarring effect is also concentrated in this group.

$$\bar{y}_{g,s,t,e,k} = \alpha + \sum_{j=1}^{20} \lambda_j (\Phi_e * ue_{s,k}) + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}.$$

Specification 3a is similar to Specification 3 except that the coefficient for the treatment effect, λ_j, is now stratified across 1 to 20 years of experience. For every percentage point increase in $ue_{s,k}$, λ_1 represents wage losses for workers with 1 year of experience, λ_2 represents wage losses for workers with 2 years of experience, and so on.

5 Results

5.1 Assessing Pre-Trends

Figure 3 tests for parallel pre-trends in the general SHB effect and shows they are parallel for log hourly wages and log weekly earnings. Both graphs in Figure 3 confirm pre-trends are parallel in both log hourly wage and log weekly earnings prior to the policy change. After SHBs are enacted, between periods t and t+4, I observe an increase in both log hourly wages and log weekly earnings for the treated states.

Figure 4 shows that there are also no pre-trends in the outcome variables for my DDD strategy[19]. I find no evidence of pre-trends for log hourly wages, log weekly earnings, or job-to-job transitions for either sample. By overlaying the event studies, I also show there are no pre-existing trends between the samples. Scarred individuals enjoy higher log hourly wage and log weekly earnings gains from the law change than non-scarred individuals. I also observe an increase in job-to-job transitions for scarred individuals and a decrease in job-to-

[19] The sample is first divided into two groups, scarred workers and non-scarred workers. Individuals with state-by-entry-year unemployment rates above the long-run median for that particular state are categorized as scarred, and those with a below-median state-by-entry-year unemployment rate are categorized as non-scarred. I then plot event studies for both non-scarred and scarred groups and overlay them for comparison.

job transitions for non-scarred individuals. This result is possible if non-scarred individuals, who are higher paid on average, are unable to signal their higher wage to employers after SHBs are enacted (Meli and Spindler 2019).

5.2 SHB Effect and Wage Scarring

5.2.1 General SHB Effect (DiD Estimate)

In Table 4, column 1, I estimate the general effect of SHB enactment using a DiD strategy. As noted in Section 4, these results use similar methods to other papers in the SHB literature (Hansen and McNichols 2020; Sinha 2019; Bessen, Denk, and Meng 2020). I provide these results to give the reader a baseline estimate of general effect of SHB enactment. Similar to aforementioned SHB literature, I find the law change induces a positive and statistically significant increase of 1.76% on log hourly wages. SHBs also increase weekly earnings by 2.36% (column 2), meaning workers in SHB states are enjoying higher hourly wages and weekly earnings in general after the bans are enacted. This is plausible as salary negotiations now favor job-seekers. In Table 5, I show SHBs have no general effect on job-to-job transitions and unemployment-to-employment transitions.

While SHBs may have spillover effects that affect workers who do not switch jobs after the law changes, I would expect the effects to be concentrated among those who switch jobs or gain employment after the law changes. Table 6, columns 1 and 2 restricts the sample to observations from individuals who switched jobs during the survey period and shows that most of the effect from SHBs comes from these individuals. In Table 6, columns 3 and 4, I restrict the sample to observations from individuals who went from unemployment-to-employment, and I do not observe any statistically significant effect for this group.

5.2.2 SHB Effect by Scarring (DDD Estimate)

Table 7 shows the results from the DDD strategy. These estimates represent the differential effect of SHB enactment on individuals who started their careers in a recession and represent the main results of this study. Columns 1 and 2, row 2 show that for every percentage point increase in state-by-entry-year unemployment rate, log hourly wages increase by 0.68% and log weekly earnings increase by 1.09% relative to the baseline SHB effect in row 1. In Table 8, column 1, I show that SHBs differentially increase job-to-job transitions by 0.12% for every percentage point increase in the state-by-entry-year unemployment rate over workers who started their careers in baseline economic conditions.

In Table 8, column 4, I show that SHBs differentially increase unemployment-to-employment transitions by 0.04% for every percentage point increase in the state-by-entry-year unemployment rate over baseline estimates. For workers who entered the labor market in a moderate-to-severe recession (5 percentage point increase in the state unemployment rate), this represents differential increases from SHB enactment of 3.4% for hourly wages, 5.45% for weekly earnings, 0.6% for job-to-job transitions, and 0.2% for unemployment-to-employment transitions relative to workers who entered the labor market during baseline labor market conditions.

5.3 Comparison to General Scarring Effect

Tables 7 and 8 show a DDD estimate of the differential effect of SHBs on scarred workers. However, I also want to understand how this estimate compares to general scarring estimates to assess how much SHBs reverse the original scarring effect. In the literature, the initial wage loss from scarring ranges from 1%–2% for every percentage point increase in the job market entry unemployment rate and decays to zero after a period of 15 to 20 years (Kahn 2010; Oreopoulus et al. 2012; Schwandt and Von Wachter 2019; Mask 2020). If SHBs differentially advantage scarred workers, then I expect my DDD estimates in Table 7, columns

1 and 2 will be smaller in magnitude and the opposite sign of general scarring estimates.

To provide an understanding of baseline scarring effects in the absence of SHBs, in Table 9, columns 1 and 3, I show that the average scarring effect for workers with 1–20 years of experience is –0.52% for log hourly wages and –0.88% and log weekly earnings. In comparison to the DDD estimates for log hourly wages and log weekly earnings found in Table 7, columns 1 and 2 respectively, SHB laws appear to completely reverse the average scarring effect and even increase wages for scarred workers. However, these two estimates are likely not directly comparable if the differential effect from SHB laws is concentrated in less experienced workers. The scarring effect is largest in earlier years and decays over time as the worker gains experience.

Table 9, columns 2 and 4 uses Specification 3a from Section 4.3 to show the scarring effect stratified by experience, which for the first experience year is −1.14% for log hourly wages and –2.55% for log weekly earnings for every percentage point increase in the state-by-entry-year state unemployment rate. These estimates are noisier but consistent with initial wage scarring estimates found in Schwandt and Von Wachter (2019).[20] Similar to their findings, I observe that this scarring effect decays with experience and is undetectable after 15 years: the average scarring effect found in columns 1 and 3 over 20 years is much smaller than the initial scarring effect found in columns 2 and 4, row 2.

Given that experience is an important factor in assessing wage scarring, I would ideally like to also stratify my DDD estimates by individual years of experience. However, I lack statistical power to do so using the CPS. As an alternative, I divide the sample into two experience groups: workers with 1–5 years of experience and workers with 5–20 years. Look-

[20]Schwandt and Von Wachter (2019) use 30 years of data to assess scarring so that they can detect the effect beyond 10 years. I use a smaller sample of years (2013–2021) because I am directly testing a policy that started being implemented in 2017.

ing again at scarring averages using Specification 3 from Section 4.3, I find that the average estimated scarring effect is concentrated in workers with 1–5 years of experience and is undetectable in workers with 6–20 years of experience (Table 10, columns 1 and 2). This is because the scarring effect becomes smaller and smaller with every experience year (Table 9, columns 2 and 4), so an average effect between experience years 5–20 would be small and underpowered. Table 11 estimates the DDD empirical strategy on a sample of workers with 1–5 years of experience and a sample of workers with 6–20 years of experience. I confirm the DDD estimate is concentrated within less experienced workers. Comparing the estimates in Table 10, columns 1 and 2 to Table 11, columns 1 and 2, respectively, I find that DDD estimates for 1–5 years of experience group represent a reversal of 90% of the original scarring effect.

I also estimate the general scarring effect for job-to-job transitions and unemployment transitions and find no statistically significant effects.[21] However, it is not necessary that a negative job mobility gap exist for SHBs to induce scarred workers to switch jobs more. If markets are perfectly competitive with full information, then scarred workers should switch jobs more relative to non-scarred workers because they are underpaid. The absence of a positive job mobility scarring gap but the presence of a negative compensation scarring gap suggests that scarred workers are not doing this prior to SHBs.

5.4 Heterogeneity within DDD Estimate

Many SHB laws were written with gender and racial wage disparities in mind. Similarly, Schwandt and von Wachter (2019) show that wage scarring is particularly damaging for non-whites. Therefore, it is important to understand how this law affects gender and race groups differently in the context of scarring.

[21] This separate analysis is omitted for brevity as no effects are reported.

5.4.1 Gender

The event studies in Figure 5 show no evidence of pre-trends in SHB treatment for males in either log hourly wage or log weekly earnings. Similarly, with the overlay, there are also no differences in pre-trends between scarred and non-scarred men either. I observe a statistically significant increase in log hourly wages and log weekly earnings for scarred men from SHB enactment but do not observe this for non-scarred men. Table 12, columns 1 and 2 show that scarred men are advantaged by the law by 0.51% for log hourly wages and 0.74% for log weekly earnings relative to non-scarred men for every percentage point increase in the state-by-entry-year unemployment rate. In Table 12, columns 3 and 4, I observe an increase of 0.1% for job-to-job transitions but no statistically significant effect for unemployment-to-employment transitions for scarred males.

I do find some evidence that scarred women were trending lower than non-scarred women in the pre-period (under Female in Figure 5). Table 13, columns 1 and 2 show that scarred women are advantaged by the law by 0.78% for log wages and 1.35% for log earnings relative to non-scarred women for every percentage point increase in the state-by-entry-year unemployment rate. I also observe evidence of an increase of 0.15% in job-to-job transitions for scarred women relative to non-scarred women as a result of the law change. In terms of magnitude, my estimates show that SHB laws advantage scarred women more than it advantaged scarred men.

5.4.2 Race

In a separate heterogeneity analysis, I split the sample between whites and non-whites, and I find no evidence of pre-trends between scarred workers and non-scarred workers across race (Figure 6). In Table 14, I find no statistically significant difference for scarred whites for log hourly wages, but I do observe an increase of 0.79% for log weekly earnings, 0.12% for job-to-job transitions, and 0.05% for unemployment-to-employment transitions. Non-whites increase log wages by 1.34%, log earnings by 1.85%, and job-to-job transitions by 0.13% for every percentage point increase in the state-by-entry-year unemployment rate (Table 15). Schwandt and Von Wachter (2019) show that general scarring effects are much larger in magnitude for non-white workers. Therefore it is plausible that DDD estimates are much larger for non-whites because of a larger initial scarring effect.

6 Additional Checks on Internal Validity

6.1 Goodman-Bacon (2021)

Estimates from DiD empirical models that exploit policy changes over multiple periods, often called two-way fixed effects (TWFE) models, can be biased if treatment effects are not homogeneous over time. In the context of this study, the principle concern is whether SHB estimates are biased because earlier adopters of SHBs are being compared to late adopters of SHBs. If treatment effects are homogeneous, then these comparisons are still valid. However, if treatment effects are increasing or decreasing over time, then these comparisons can attenuate or exacerbate estimates, respectively.

Goodman-Bacon (2021) proposes a decomposition method and shows that TWFE estimates are a variance-weighted average of each individual difference-in-difference comparisons between treated and non-treated states. Figure 7 illustrates this method by plotting individual treatment and non-treatment group DiD estimates by their magnitude and weighted contribution to the aggregate TWFE estimate. I observe that the aggregate estimate (denoted by the blue line) is mostly driven by treatment versus never treated comparisons. There are a few earlier group treatment versus later group comparisons that yield negative coefficients in magnitude, but they also have very low weights. Conversely, I observe a few later group treatment versus earlier group comparisons that are positive but also have very low weights.

Table 16 shows that most of the aggregate TWFE estimate for log wages comes from a positive average difference-in-difference estimate of 2.5% from comparing treated states to non-treated states ("T vs. Never treated") as the weight on this estimate is 0.907. Both comparisons between groups with different treatment periods show negative coefficients. When earlier treated states post-treatment are compared to later states post-treatment ("Earlier T vs. Later C) and later treated states post-treatment are compared to earlier treated states post-treatment ("Later T vs. Earlier C"), they both show a negative effect on average. For log weekly earnings, Table 17 shows that most of the effect measured in the TWFE estimates comes from comparing treated states to non-stated treated states. However, the average DiD estimate for comparisons between earlier and later treated states are both positive. I can see in Figure 7 that these terms do not contribute much to my overall estimate. Nevertheless, given that they are not approximately 0, I am concerned about the potential for bias and want to test for it. In Section 6.2, I use a new estimator by Callaway and Sant'Anna (2020) to assess the magnitude of this bias.

6.2 Callaway and Sant'Anna (2020)

In their paper, Callaway and Sant'Anna (2020) propose a new estimator when using the staggered treatment in a difference-in-difference framework. They propose an estimator that uses group-time average treatment effects. For example, Oregon, NY,[22] and Delaware all passed SHB laws in 2017. Together these states constitute the 2017 treatment group. Comparisons are then made between this group and non-ban states and pre-treatment SHB states. Under a parallel trends assumption, these estimates would be valid as no other states were treated when they states were treated. The 2018 group, California, Massachusetts, and Vermont, is compared to non-ban states and pre-treatment SHB states. However, the states treated in 2017 are excluded as these comparisons might yield bias. This same process is repeated for

[22]As noted in Section 4.1, I treat NY as passing an SHB in 2017 because of bans passed in NYC and Albany County in 2017.

2019 and 2020 groups, and four estimates are derived. These estimates are then aggregated to a main parameter of interest, the overall effect of participating in the treatment across all groups that have ever participated in the treatment.

Table 18, columns 1 and 3 provides baseline OLS estimates of the SHB laws passing[23]. These estimates are similar to results found in Table 4 with the exception that only state and year fixed effects are included in this specification. Columns 2 and 4 show estimates of SHB laws passing using the estimator proposed by Callaway and Sant'Anna (2020). Since this estimator does not include comparisons between earlier and later treated SHB states, these estimates provide a perfect gauge to assess potential bias. Given that columns 2 and 4 are similar in magnitude and more precise than columns 1 and 3, respectively, it is likely that bias from comparing earlier and later treated SHB states do not change the general conclusions from my OLS estimates.

7 Conclusion

In this paper, I show that SHBs increase job mobility for scarred workers relative to non-scarred workers and reduce the gap in hourly wage and weekly earnings between these workers. However, my analysis also suggests this policy carries unintended consequences. I find, in absolute terms, that job mobility for non-scarred workers is lower after SHBs are enacted. This finding is consistent with Meli and Spindler (2019) that higher-paid workers may be disadvantaged by these laws because they can no longer signal higher perceived productivity from their higher wages. It may be a better policy to simply encourage employers to post salary information without limiting job applicants from volunteering it. Roussille (2020) shows that providing this information removes disparities in initial salary bids between men

[23]These estimates are similar to DiD estimates in Table 4. However, I exclude covariates at this time as the R package used for Callaway and Sant'Anna (2020) estimates, did, requires data aggregated at the state-level instead of the individual-level. However, excluding covariates yields similar results to my general DiD estimates in Table 4 that do use covariates (1.76% versus 2.19% for log wages and 2.36% versus 2.95% for log weekly earnings).

and women, as women increase their bids after learning what the employer intends to pay. Scarred workers appear to react similarly to these gains in information, and such a policy would not limit non-scarred workers from volunteering salary information.

References

[1] Agan, A., Cowgill, B., & Gee, L. K. (2020). "Do workers comply with salary history bans? a survey on voluntary disclosure, adverse selection, and unraveling." AEA Papers and Proceedings (Vol. 110, pp. 215-19).

[2] Altonji, J. G., & Pierret, C. R. (2001). "Employer learning and statistical discrimination." The quarterly journal of economics, 116(1), 313-350.

[3] Altonji, J. G., Kahn, L. B., & Speer, J. D. (2016). "Cashier or consultant? Entry labor market conditions, field of study, and career success." Journal of Labor Economics, 34(S1), S361-S401.

[4] Arellano-Bover, J. (2020). "The Effect of Labor Market Conditions at Entry on Workers' Long-Term Skills." Review of Economics and Statistics, 1-45.

[5] Barach, M. A., & Horton, J. J. (2021). "How do employers use compensation history? Evidence from a field experiment." Journal of Labor Economics, 39(1), 193-218.

[6] Beaudry, P., & DiNardo, J. (1991). "The effect of implicit contracts on the movement of wages over the business cycle: Evidence from micro data." Journal of political Economy, 99(4), 665-688.

[7] Bessen, J. E., Denk, E., & Meng, C. (2020). "Perpetuating Inequality: What Salary History Bans Reveal About Wages." Available at SSRN.

[8] Brunner, B., & Kuhn, A. (2014). "The impact of labor market entry conditions on initial job assignment and wages." Journal of Population Economics, 27(3), 705-738.

[9] Callaway, B., & Sant'Anna, P. H. (2020). "Difference-in-differences with multiple time periods." Journal of Econometrics.

[10] Choi, E. J., Choi, J., & Son, H. (2020). "The long-term effects of labor market entry in a recession: Evidence from the Asian financial crisis." Labour economics, 67, 101926.

[11] Flood S., King, M., Rodgers, R., Ruggles, S. & Warren, J. (2020). Integrated Public Use Microdata Series, Current Population Survey: Version 7.0 [dataset]. Minneapolis, MN: IPUMS, 2020. https://doi.org/10.18128/D030.V7.0

[12] Forsythe, E. (2016). "Why don't firms hire young workers during recessions." University of Illinois.

[13] Forsythe, E. (2019). "Careers within firms: Occupational mobility over the lifecycle." Labour, 33(3), 241-277.

[14] Goodman-Bacon, A. (2021). "Difference-in-differences with variation in treatment timing." Journal of Econometrics.

[15] Hagedorn, M., & Manovskii, I. (2013). "Job selection and wages over the business cycle." American Economic Review, 103(2), 771-803.

[16] Hall, R. E., & Krueger, A. B. (2012). "Evidence on the incidence of wage posting, wage bargaining, and on-the-job search." American Economic Journal: Macroeconomics, 4(4), 56-67.

[17] Hansen, B., & McNichols, D. (2020). "Information and the Persistence of the Gender Wage Gap: Early Evidence from California's Salary History Ban" (No. w27054). National Bureau of Economic Research.

[18] Hershbein, B. J. (2012). "Graduating high school in a recession: Work, education, and home production." The BE journal of economic analysis policy, 12(1).

[19] Kahn, L. B. (2010). "The long-term labor market consequences of graduating from college in a bad economy." Labour economics, 17(2), 303-316.

[20] Kahn, L. B., & Lange, F. (2014). "Employer learning, productivity, and the earnings distribution: Evidence from performance measures." The Review of Economic Studies, 81(4), 1575-1613.

[21] Khanna, S. (2020). "Salary History Bans and Wage Bargaining: Experimental Evidence." Labour Economics, 65, 101853.

[22] Kondo, A. (2007). "Does the first job really matter? State dependency in employment status in Japan." Journal of the Japanese and International Economies, 21(3), 379-402

[23] Kotlikoff, L. J., & Gokhale, J. (1992). "Estimating a firm's age-productivity profile using the present value of workers' earnings." The Quarterly Journal of Economics, 107(4), 1215-1242.

[24] Kwon, I., Milgrom, E. M., & Hwang, S. (2010). "Cohort effects in promotions and wages evidence from Sweden and the United States." Journal of Human Resources, 45(3), 772-808.

[25] Lange, F. (2007). "The speed of employer learning." Journal of Labor Economics, 25(1), 1-35.

[26] Liu, H. Y., & Chen, J. (2014). "Are There Long-Term Effects on Wages When Graduating in A Bad Economy in Taiwan." Asian Economic and Financial Review, 4(10), 1347-1362.

[27] Liu, K., Salvanes, K. G., & Sørensen, E. Ø. (2016). "Good skills in bad times: Cyclical skill mismatch and the long-term effects of graduating in a recession." European Economic Review, 84, 3-17.

[28] Mask, J. (2020). "Consequences of immigrating during a recession: Evidence from the US Refugee Resettlement program." IZA Journal of Development and Migration, 11(1).

[29] Meli, J., & Spindler, J. C. (2019). "Salary History Bans and Gender Discrimination." U of Texas Law, Law and Econ Research Paper, (E587).

[30] Oreopoulos, P., Von Wachter, T., & Heisz, A. (2012). "The short-and long-term career effects of graduating in a recession." American Economic Journal: Applied Economics, 4(1), 1-29.

[31] Oyer, P. (2006). "Initial labor market conditions and long-term outcomes for economists." Journal of Economic Perspectives, 20(3), 143-160.

[32] Oyer, P. & Schaefer, S. (2011). "Personnel Economics: Hiring and Incentives," Handbook of Labor Economics, in: O. Ashenfelter D. Card (ed.),Handbook of Labor Economics, edition 1, volume 4, chapter 20, pages 1769-1823

[33] Payscale. (2017). "The Salary History Question: Alternatives for Recruiters and Hiring Managers."

[34] Roussille, N. (2020). "The central role of the ask gap in gender pay inequality."

[35] Schmitt, J. (2003). "Creating a consistent hourly wage series from the Current Population Survey's Outgoing Rotation Group, 1979-2002." Version 0.9 (August). Washington DC: Center for Economic and Policy Research.

[36] Schwandt, H., & Von Wachter, T. (2019). "Unlucky cohorts: Estimating the long-term effects of entering the labor market in a recession in large cross-sectional data sets." Journal of Labor Economics, 37(S1), S161-S198.

[37] Sinha, S. (2019). "Salary history ban: Gender pay gap and spillover effects." Available at SSRN 3458194.

[38] Sran, G., Vetter, F., & Walsh, M. (2020). "Employer Responses to Pay History Inquiry Bans." Available at SSRN 3587736.

8 Figures

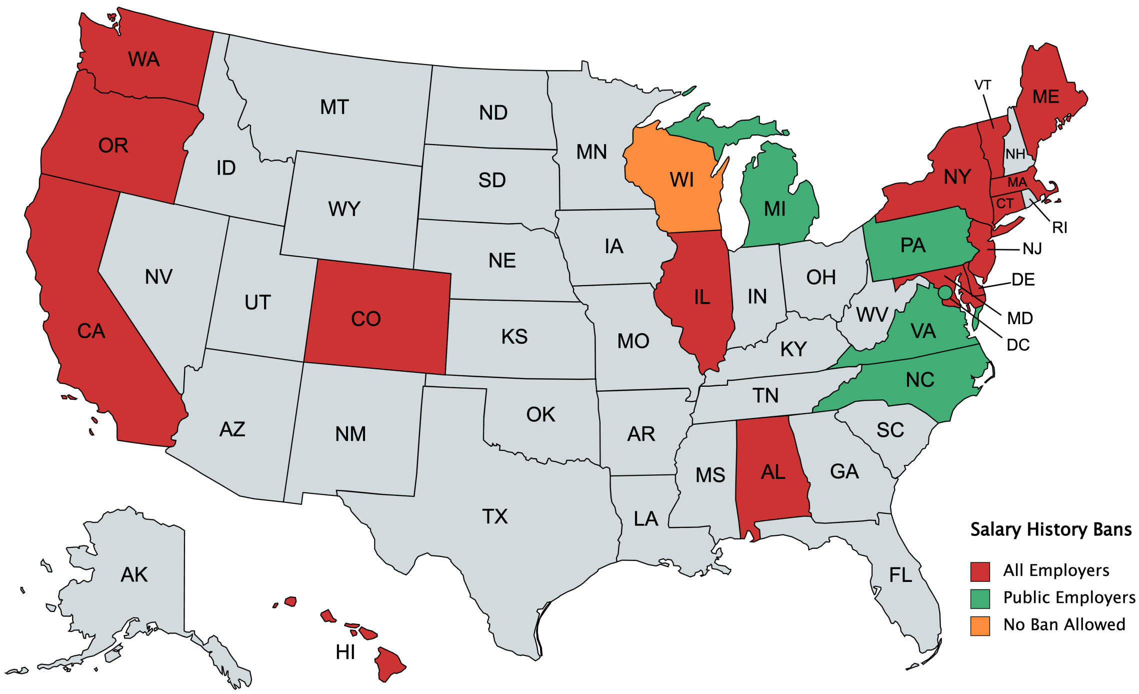

Figure 1: States that have passed Salary History Bans (SHBs)[24]

[24]Data from: `https://www.hrdive.com/news/salary-history-ban-states-list/516662/`

Figure 2: National Unemployment Rate by Job-Market-Entry Period[25]

[25]Data retrieved from https://www.bls.gov/ces/. The range of the x-axis, 1992 to 2019, represents all potential job-market-entry periods covered in the analysis.

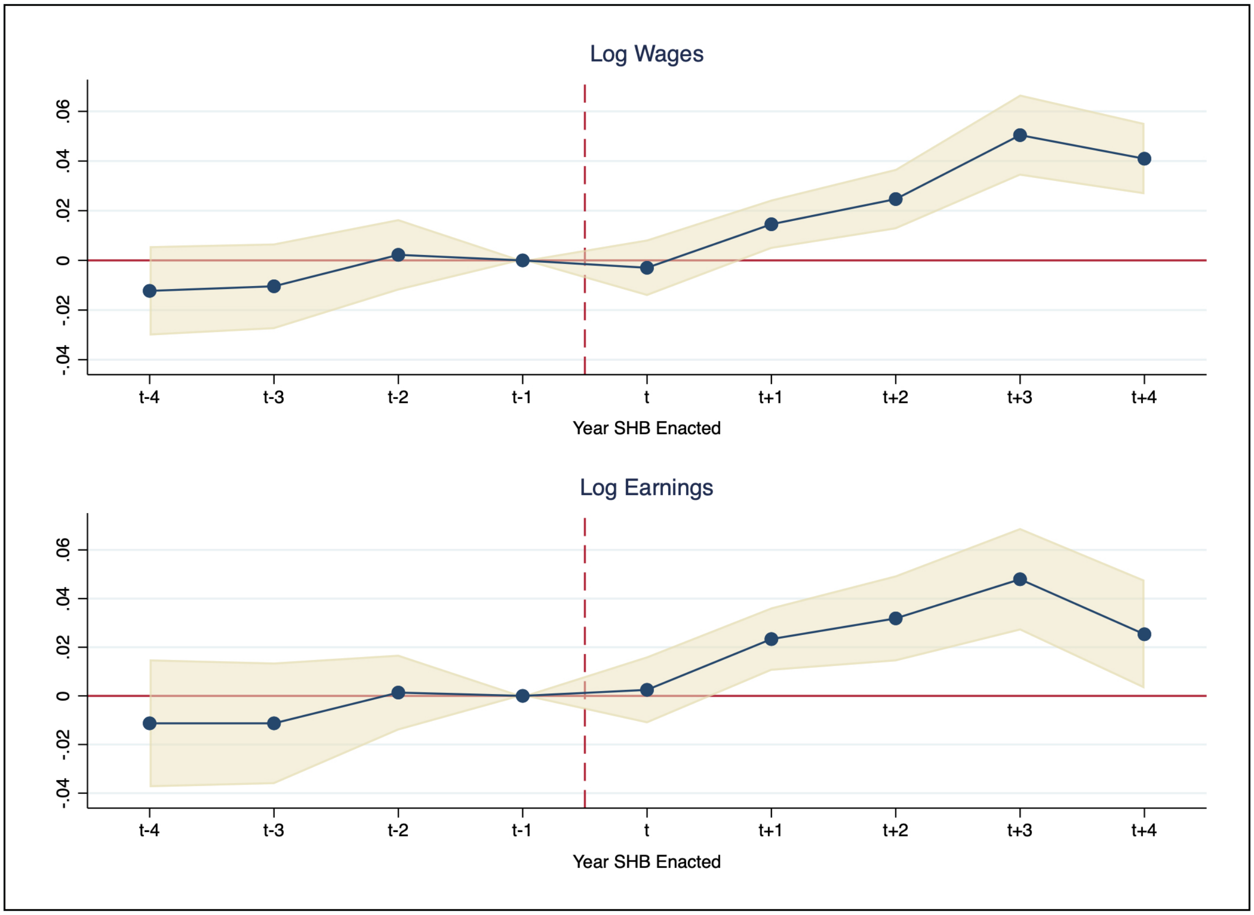

Figure 3: SHB Effect on Log Hourly Wages and Log Weekly Earnings

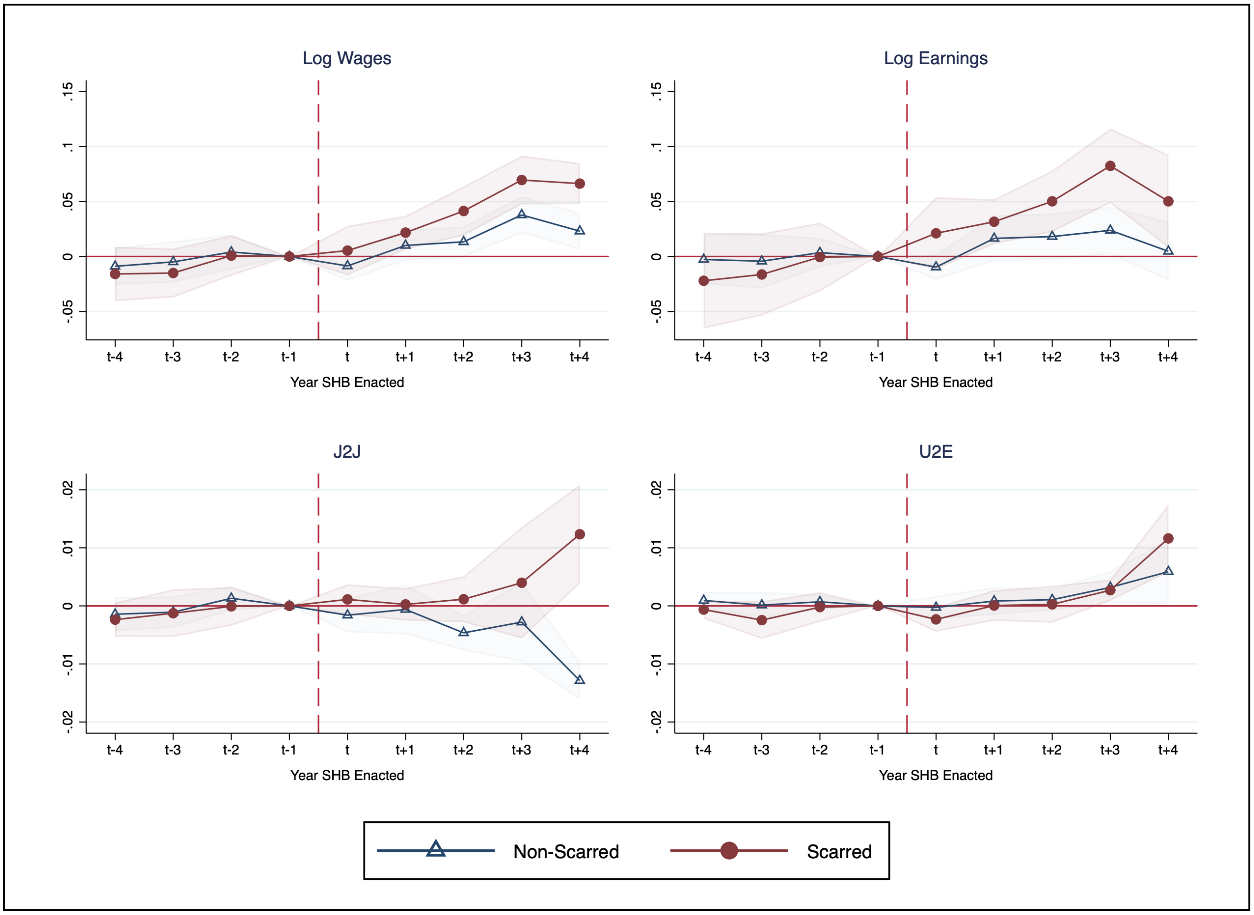

Figure 4: SHB Effect by Scarring[26]

[26]Using potential experience and state combined with historical state unemployment data, I impute the state unemployment rate at job-market-entry for each CPS respondent. Workers who have a state-by-entry-year unemployment rate that is above the long-run median for each state are classified as "scarred." Workers that are below the long-run median for each state are classified as "non-scarred." J2J represents job-to-job transitions and U2E represents unemployment-to-employment transitions.

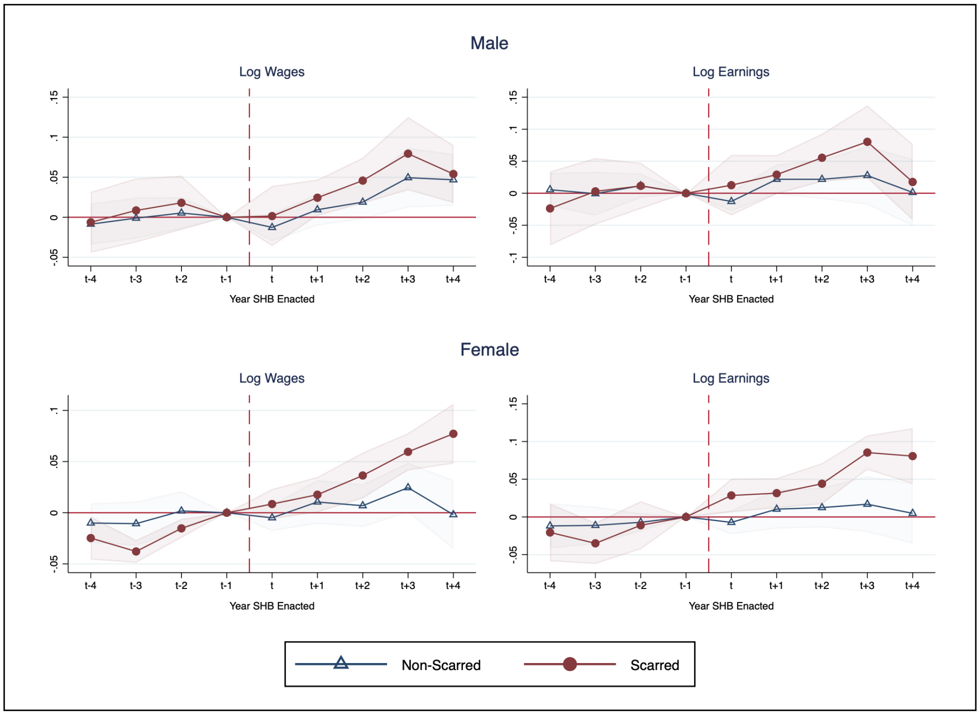

Figure 5: SHB Effect by Scarring and Gender

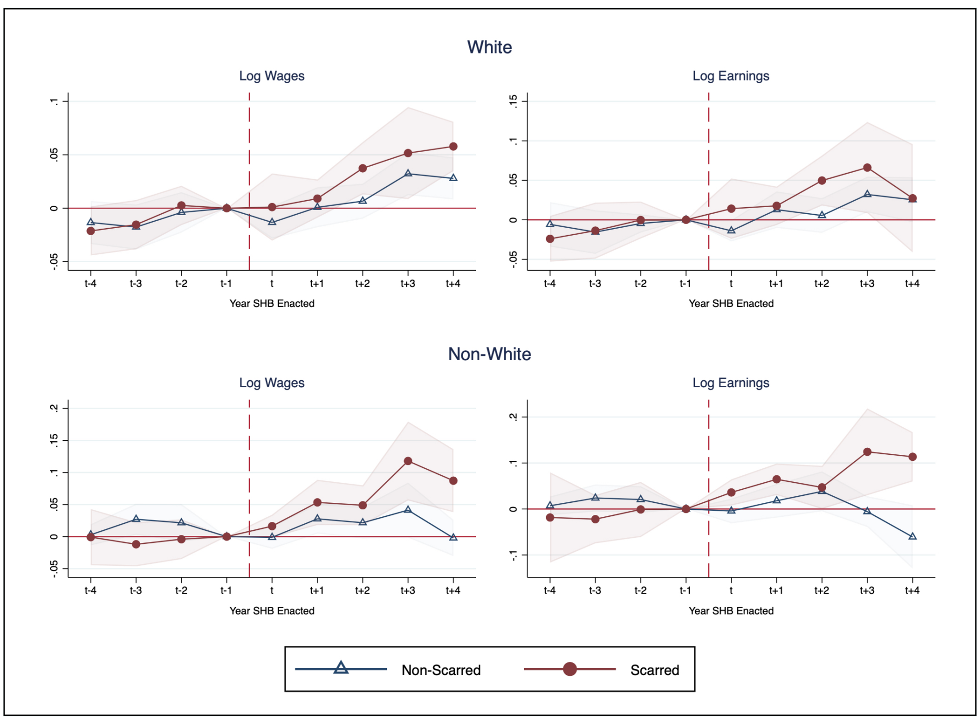

Figure 6: SHB Effect by Scarring and Race

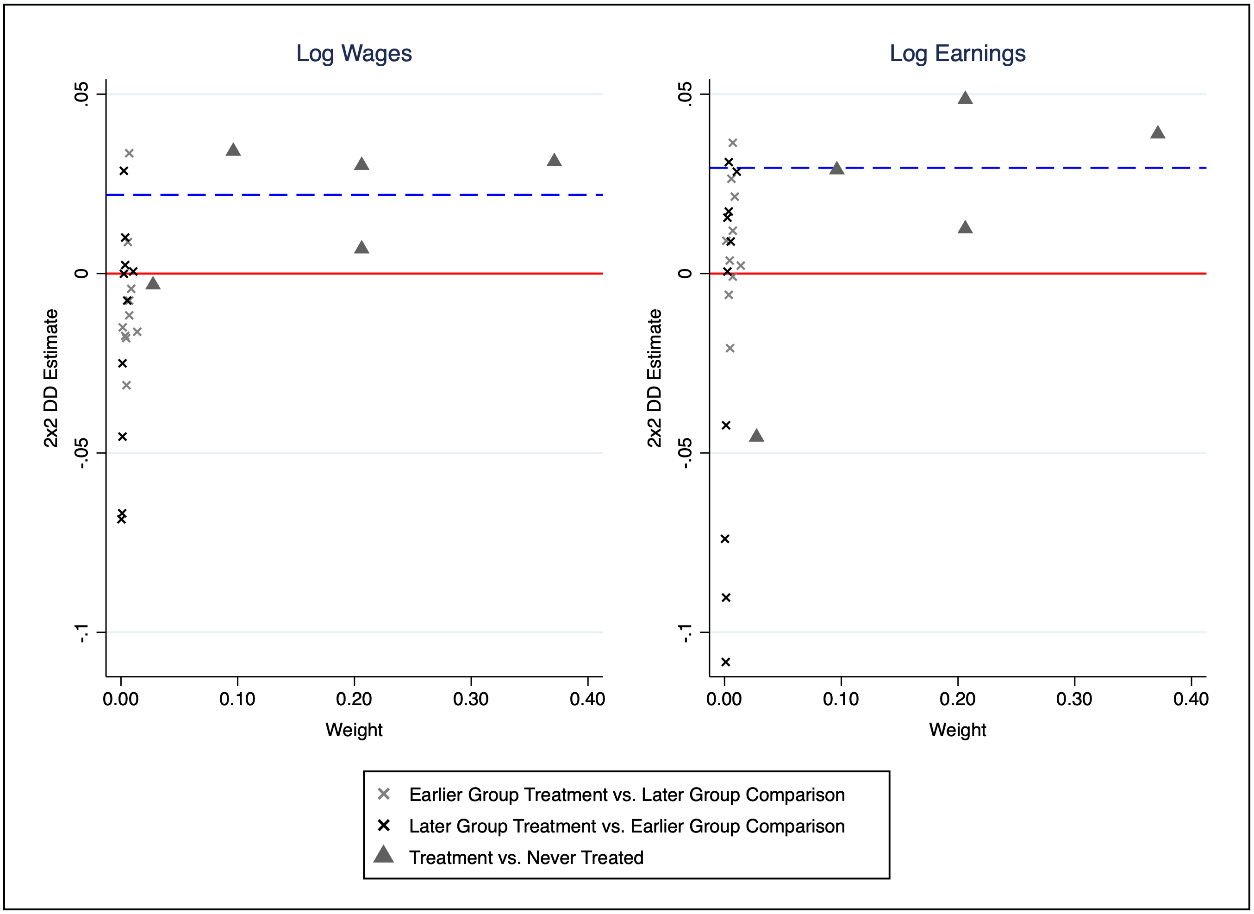

Figure 7: Goodman-Bacon Decomposition[27]

[27]This figure uses a decomposition method proposed in Goodman-Bacon (2021). The empirical strategy used in this paper to measure the effect of SHB laws represents a weighted average of individual difference-in-difference (DID) comparisons across all treated states and time periods used in the analysis. The y-axis the magnitude of each DiD estimate, and the x-axis represents the weight each estimate contributes to the composite effect. Each triangle represents a DiD comparison between treated states and non-treated states. The light grey x's represent DiD comparisons between states that enacted SHB laws earlier to the pre-SHB period in states that eventually pass SHBs. The black x's represent DiD comparisons between states that pass SHBs later and the pre- and post-SHB periods in states that pass SHBs earlier. These later group treatment vs. earlier group comparisons are potentially biased. The estimates using the Callaway-Sant'Anna (2020) method in Table 18, Columns 2 and 4, show the composite DiD effect of SHBs excluding these potentially biased terms.

9 Tables

Table 1: **CPS Sample Summary Table**

	Mean
Age	30.06
% Female	51.20
Potential Experience	9.94
% High School Graduates	27.98
% College Graduates	35.52
% Caucasian	77.93
Observations	3021637

Table 2: **Summary Table by Treatment**

	No Ban States	Ban States
	Mean	Mean
Age	29.99	30.20
% Female	51.24	51.13
Potential Experience	9.96	9.91
% High School Graduates	28.76	26.27
% College Graduates	33.75	39.42
% Caucasian	79.81	73.81
Observations	2075989	945648

Table 3: **Total Observations by Period for Treated States Only**

	Period									
State	<=t-4	t-3	t-2	t-1	**t**	**t+1**	**t+2**	**t+3**	**t+4**	Total
Alabama	15,640	7,093	7,166	7,033	6,798	5,772	2,458	0	0	51,960
California	72,884	35,314	35,086	34,113	32,933	32,172	27,322	11,319	0	281,143
Colorado	33,386	4,932	4,469	4,062	1,639	0	0	0	0	48,488
Connecticut	18,214	3,459	3,496	3,233	3,219	2,813	1,131	0	0	35,565
Delaware	5,268	5,024	4,259	3,961	3,644	3,583	3,127	2,672	1,159	32,697
Hawaii	17,373	5,430	5,311	4,839	4,507	3,866	1,449	0	0	42,775
Illinois	36,042	11,506	10,896	10,216	9,654	8,720	3,240	0	0	90,274
Maine	13,035	2,648	2,725	2,560	2,315	1,972	838	0	0	26,093
Maryland	28,139	4,953	4,841	4,347	3,827	1,621	0	0	0	47,728
Massachusetts	11,387	7,053	8,335	8,294	8,016	7,811	7,320	2,805	0	61,021
New Jersey	30,557	7,160	6,709	6,465	5,675	2,217	0	0	0	58,783
New York	16,640	16,334	15,764	15,743	15,436	14,819	14,711	12,246	4,476	126,169
Oregon	5,425	5,504	5,832	5,764	5,556	5,454	5,524	5,115	2,438	46,612
Vermont	8,907	4,314	4,216	4,041	4,012	3,918	3,331	1,397	0	34,136
Washington	21,068	6,828	7,325	7,094	6,974	6,458	2,673	0	0	58,420
Total	333,965	127,552	126,430	121,765	114,205	101,196	73,124	35,554	8,073	1,041,864

Table 4: **Overall SHB Effect on Compensation (DiD Estimate)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0176***	0.0236**
	(0.0047)	(0.0082)
Observations	101284	102990
Adjusted R^2	0.724	0.718

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 5: **Overall SHB Effect on Job Transitions (DiD Estimate)**

	(1) J2J Changes	(2) U2E Changes
SHB	-0.0002	0.0002
	(0.0007)	(0.0006)
Observations	108007	121046
Adjusted R^2	0.023	0.049

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 6: **Overall SHB Effect on J2J and U2E Compensation**

	J2J		**U2E**	
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
SHB	0.0521***	0.0525**	0.0102	0.0059
	(0.0130)	(0.0177)	(0.0177)	(0.0356)
Observations	22046	23217	14365	15739
Adjusted R^2	0.351	0.342	0.264	0.228

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 7: **SHB Effect on Compensation by Scarring (DDD Estimate)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0126*	0.0158+
	(0.0055)	(0.0086)
SHB × ue_{sk}	0.0068***	0.0109***
	(0.0019)	(0.0023)
Observations	101284	102990
Adjusted R^2	0.726	0.719

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 8: **SHB Effect on Job Transitions by Scarring (DDD Estimate)**

	(1) J2J Changes	(2) U2E Changes
SHB	-0.0011	-0.0006
	(0.0009)	(0.0007)
SHB × ue_{sk}	0.0012***	0.0004*
	(0.0003)	(0.0002)
Observations	108007	121046
Adjusted R^2	0.022	0.047

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 9: **Scarring Effect**

	(1) Log Hourly Wages	(2) Log Hourly Wages	(3) Log Weekly Earnings	(4) Log Weekly Earnings
ue_{sk}	-0.0052***		-0.0088***	
	(0.0012)		(0.0016)	
ue_{sk} × Experience = 1		-0.0114***		-0.0255***
		(0.0034)		(0.0066)
ue_{sk} × Experience = 2		-0.0102***		-0.0209***
		(0.0026)		(0.0041)
ue_{sk} × Experience = 3		-0.0070**		-0.0174***
		(0.0026)		(0.0037)
ue_{sk} × Experience = 4		-0.0081**		-0.0141***
		(0.0027)		(0.0033)
ue_{sk} × Experience = 5		-0.0051*		-0.0073**
		(0.0020)		(0.0026)
ue_{sk} × Experience = 6		-0.0024		-0.0057*
		(0.0018)		(0.0023)
ue_{sk} × Experience = 7		-0.0043*		-0.0057*
		(0.0017)		(0.0024)
ue_{sk} × Experience = 8		-0.0032+		-0.0037+
		(0.0016)		(0.0021)
ue_{sk} × Experience = 9		-0.0054**		-0.0073***
		(0.0017)		(0.0022)
ue_{sk} × Experience = 10		-0.0032		-0.0059+
		(0.0025)		(0.0032)
ue_{sk} × Experience = 11		-0.0092**		-0.0116***
		(0.0030)		(0.0035)
ue_{sk} × Experience = 12		-0.0037		-0.0066+
		(0.0029)		(0.0036)
ue_{sk} × Experience = 13		-0.0052		-0.0058
		(0.0037)		(0.0044)
ue_{sk} × Experience = 14		-0.0097**		-0.0143**
		(0.0035)		(0.0045)
ue_{sk} × Experience = 15		-0.0074+		-0.0118*
		(0.0042)		(0.0052)
ue_{sk} × Experience = 16		-0.0029		-0.0057
		(0.0041)		(0.0049)
ue_{sk} × Experience = 17		-0.0067		-0.0060
		(0.0042)		(0.0051)
ue_{sk} × Experience = 18		-0.0033		-0.0007
		(0.0033)		(0.0044)
ue_{sk} × Experience = 19		-0.0007		-0.0009
		(0.0037)		(0.0042)
ue_{sk} × Experience = 20		0.0003		0.0022
		(0.0038)		(0.0041)
Observations	101284	101284	102990	102990
Adjusted R^2	0.724	0.724	0.718	0.718

Standard errors clustered at the state-by-job-market-entry-year level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 10: **Average Scarring Effect by Experience Year**

	1 to 5 Years of Experience		**5 to 20 Years of Experience**	
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
ue_{sk}	-0.0150***	-0.0232***	-0.0015	-0.0017
	(0.0026)	(0.0035)	(0.0014)	(0.0016)
Observations	25117	25592	76167	77398
Adjusted R^2	0.692	0.701	0.686	0.647

Standard errors clustered at the state-by-job-market-entry-year level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 11: **DDD Estimate by Experience Year**

	1 to 5 Years of Experience		**5 to 20 Years of Experience**	
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
SHB $\times$ ue_{sk}	0.0137**	0.0219*	0.0019	0.0038
	(0.0040)	(0.0087)	(0.0025)	(0.0037)
Observations	25117	25592	76167	77398
Adjusted R^2	0.693	0.701	0.686	0.647

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 12: **SHB Effect by Scarring for Males**

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0057	0.0112	-0.0003	-0.0013
	(0.0076)	(0.0098)	(0.0009)	(0.0011)
SHB × ue_{sk}	0.0051*	0.0074**	0.0010*	0.0006
	(0.0021)	(0.0026)	(0.0004)	(0.0004)
Observations	51681	52551	54995	61487
Adjusted R^2	0.722	0.728	0.025	0.056

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 13: **SHB Effect by Scarring for Females**

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0194***	0.0208*	-0.0020	0.0002
	(0.0052)	(0.0100)	(0.0012)	(0.0007)
SHB × ue_{sk}	0.0078***	0.0135***	0.0015***	0.0002
	(0.0020)	(0.0025)	(0.0004)	(0.0002)
Observations	49603	50439	53012	59559
Adjusted R^2	0.730	0.705	0.017	0.033

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 14: **SHB Effect by Scarring for Whites**

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0124*	0.0153*	-0.0021+	-0.0011+
	(0.0052)	(0.0076)	(0.0011)	(0.0006)
SHB × ue_{sk}	0.0041	0.0079**	0.0012***	0.0005**
	(0.0024)	(0.0027)	(0.0003)	(0.0002)
Observations	61775	62449	63950	67983
Adjusted R^2	0.774	0.768	0.037	0.065

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 15: **SHB Effect by Scarring for Non-Whites**

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0094	0.0123	0.0011	0.0012
	(0.0094)	(0.0144)	(0.0018)	(0.0015)
SHB × ue_{sk}	0.0134***	0.0185***	0.0013+	-0.0001
	(0.0033)	(0.0044)	(0.0008)	(0.0004)
Observations	39509	40541	44057	53063
Adjusted R^2	0.640	0.622	0.015	0.030

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

Table 16: **Goodman-Bacon Decomposition for Log Wages**

DiD Comparison	(1) Weight	(2) Average DiD Estimate
Earlier T vs. Later C	0.062	-0.007
Later T vs. Earlier C	0.030	-0.002
T vs. Never treated	0.907	0.025

Table 17: **Goodman-Bacon Decomposition for Log Weekly Earnings**

DiD Comparison	(1) Weight	(2) Average DiD Estimate
Earlier T vs. Later C	0.062	0.010
Later T vs. Earlier C	0.030	0.009
T vs. Never treated	0.907	0.031

Table 18: **Two-Way Fixed Effects versus Callaway and Sant'Anna (2019)**

	Log Wages		Log Weekly Earnings	
	(1) TWFE	(2) C&S	(3) TWFE	(4) C&S
SHB	0.0219**	0.0245***	0.0295**	0.0284**
	(0.0079)	(0.0059)	(0.0093)	(0.0133)
Observations	459	459	459	459
Adjusted R^2	0.946		0.926	

Standard errors clustered at the state level.

+ 0.1, * 0.05, ** 0.01, *** 0.001

[27]Table 18, Columns 1 and 3 are analogous to Table 4 estimates. I estimate the general SHB effect on the dataset without including additional fixed effects because I want my estimates to serve as a valid comparison to the the Callaway and Sant'Anna (2019) method. The estimation package in R that uses this new estimator, did, requires data to be aggregated at the state level, not the individual level. Table 18, Columns 2 and 4 show the general DiD effect using this new estimation package.

Appendix

Table A.1: **SHB Effect on Compensation (Including State Workers)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0168***	0.0225**
	(0.0043)	(0.0079)
Observations	102452	104131
Adjusted R^2	0.730	0.723

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table A.2: **SHB Effect on Compensation by Scarring (Including State Workers)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0125*	0.0144
	(0.0054)	(0.0086)
SHB $\times$ ue_{sk}	0.0065***	0.0110***
	(0.0019)	(0.0023)
Observations	102452	104131
Adjusted R^2	0.731	0.725

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table A.3: **SHB Effect on Compensation (Excluding NY State)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0194***	0.0261**
	(0.0043)	(0.0079)
Observations	99947	101603
Adjusted R^2	0.725	0.719

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001

Table A.4: **SHB Effect on Compensation by Scarring (Excluding NY State)**

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0166**	0.0212**
	(0.0049)	(0.0073)
SHB × ue_{sk}	0.0052**	0.0093***
	(0.0016)	(0.0020)
Observations	99947	101603
Adjusted R^2	0.727	0.720

Standard errors clustered at the state level.
+ 0.1, * 0.05, ** 0.01, *** 0.001