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# Perpetuating Wage Inequality:

# Evidence from Salary History Bans*

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*Abstract: Pay gaps for women and minorities have persisted after accounting for observable differences. Why? If employers can access applicants' salary histories while bargaining over wages, they can take advantage of past inequities, perpetuating inequality. Recently, a dozen US states have banned employer access to salary histories. We analyze the effects of these salary history bans (SHBs) on employer wage posting and pay in a difference-in-differences design. Following SHBs, employers posted wages more often and increased pay for job changers, particularly for women (6.4%) and non-whites (7.7%). Bargaining behavior appears to account for much of the persistence of residual wage gaps.*

(*JEL*: J16, J31, J71, J78)

Keywords: Gender wage gap; salary history ban; labor discrimination, wage inequality, wage bargaining

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# 1. Introduction

Economists have long argued that workers can sometimes escape discrimination by switching jobs (Becker 1971). If a worker is paid below her marginal productivity because her employer discriminates, she can switch to an employer who does not discriminate and thus earn a fair market wage. However, this mechanism might not work if employers can often gain access to job applicants' previous pay histories. Salary history signals the applicant's reservation wage possibly giving the employer a bargaining advantage—job applicants currently suffering from discrimination or other disadvantages may be willing to accept a lower wage offer than other workers with comparable capabilities. Because employers who negotiate with job applicants over pay—as opposed to posting the wage—gain a bargaining advantage from salary history, this information may help perpetuate pre-existing inequities.

Aware of this possibility and frustrated by the stubborn persistence of gender pay gaps, women's advocates have pushed for salary history ban (SHB) legislation that forbids employers from asking for salary histories. Since August 2016, when Massachusetts passed such a law, more than a dozen states and cities have enacted SHB laws or regulations covering private employers (see Table I). The solid line in Figure I shows that nearly a quarter of private-sector workers in the US are now covered by an SHB. It also appears that the SHB may have substantially altered employer behavior. The dashed line shows the share of online help-wanted advertisements that list salary information. That share roughly tripled following the first SHB laws, suggesting that this natural economic experiment might reveal important information about bargaining and wage setting.

This paper explores the relationships between salary history bans and employer behavior regarding wage posting and pay for job changers including, specifically, for women and minorities. We estimate these effects using a differences-in-differences design estimating pay for job changers in treated compared to control states before and after bans were implemented. We further explore differences between demographic groups and we conduct placebo tests and other robustness checks to support identification.

This analysis is important because SHB-related changes in bargaining behavior might reveal the extent to which gender or racial wage gaps are affected by bargaining rather than being the result of productivity-related worker characteristics. Research shows that wage gaps have narrowed in recent decades, especially as human capital differences between groups have been reduced or eliminated.[1] But it is unclear how much of the residual pay gaps—the pay gaps remaining after controlling for observable worker characteristics—are attributable to unobserved worker characteristics that differ by group. Our findings imply that a large portion of these wage gaps is not related to productivity differences between workers.

A number of studies have sought to evaluate the effect of SHBs as a policy intended to reduce the gender wage gap (Sinha 2019; Hansen and McNichols 2020; Sran, Vetter, and Walsh 2020). This paper does not attempt such a policy evaluation. Instead, utilizing the natural experiment, we are able to glean important insights about pay inequities, from both the changes in wage posting behavior of employers in job openings and in the actual equilibrium wages. [2]

We begin with a simple model adapted from Hall and Krueger (2010). In this model, the reservation wage of a job applicant could be less than her marginal productivity for a number of reasons such as search frictions (Burdett and

[1] See, for example, Blau and Kahn (2017)

[2] Our analysis also suggests that it may be too early to properly evaluate the salary history ban as a policy instrument. Although the evidence shows significant impacts on wages, both employer and worker behavior are likely to change over a period of years in reaction to these wage changes.

Mortensen 1998), monopsony, labor market conditions (Mask 2020), or discrimination. Salary history information reveals the applicant's reservation wage. As long as the employer has not previously advertised a salary, the employer can make an offer at this level (or slightly higher) and the applicant will accept. Our model predicts that a salary history ban removes this bargaining advantage, causing: 1) more firms to post wages, 2) increased wages for job changers, and 3) greater wage increases for those workers with the lowest reservation wages. To the extent that certain groups have historically been disadvantaged due to discrimination, these groups will see greater wage gains for job changers.

Our empirical strategy is informed by our model and, in this way, differs from other research on SHBs in several ways. First, Sinha (2019) and Hansen and McNichols (2020) study the impact of SHBs on the gender wage gap. Our model suggests that although the intent of the legislation in most states might have been to improve gender pay equity, the SHB should have effects on some male workers who have low reservation wages. We find evidence of positive wage effects on female workers, but also on non-white male workers and, to a lesser degree, on white male workers who changed jobs.

Second, the model implies that wage effects should apply only to job changers, not to incumbent workers.[3] There are two problems with including incumbent workers in the sample: they dilute the estimates of the treatment effect and they obscure identification. The latter problem arises because passage of SHB legislation is likely endogenous—it may well reflect rising concern within a state

[3] Hansen and McNichols (2020) find that most of their measured effects arise from job changers. Sran et al. (2020) look at job changers, but they include salary history bans that cover public employees only, obscuring identification for private employees. The evidence from the paper is also based on the aggregated Quarterly Workforce Indicators data only. Sinha (2019) does not distinguish between incumbents or job changers. Additionally, Hansen and McNichols (2020) and Sinha (2019) do not provide further evidence from employers' changing hiring behavior.

about gender pay inequity. But that rising concern might also influence employers to increase the relative pay of incumbent women workers independently of a salary history ban. In this case, measuring the treatment on a pooled sample that includes incumbent workers conflates the treatment effect of an SHB with rising general concern about gender pay inequity. We resolve this identification problem in two ways that the other papers do not. First, we measure the net treatment effect on job changers relative to the change in pay for incumbent workers. We do, in fact, find a small but significant increase in the pay of female incumbent workers and a much larger increase for female job changers. Second, we select a comparison group that is likely to have similar general concerns about pay inequities. In particular, we choose controls that are in the same commuting zones as treated workers but across state boundaries.

We find empirical support for each of the main predictions of our model. First, looking at online help-wanted ads, we find a significant rise in the probability that the ads list salary information after SHBs go into effect. Second, we estimate that after an SHB, job-changing workers earn 4% more than comparable job-changers not under SHBs relative to incumbent workers on average. Third, we find even larger increases in the pay of job-changing women (6.4%) and non-whites (7.7%). For these previously disadvantaged groups, the pay increases following an SHB represent a sizeable portion of the residual wage gap measured for job-changing employees, suggesting that most of this gap is related to bargaining differences rather than productivity differences between workers.

Finally, we explore possible secondary effects of the bans. Because SHBs might reduce employer information about worker productivity and consequently produce poor matches, we test whether SHBs affect worker turnover. We find that SHBs do not affect either the rate at which workers change jobs or the characteristics of those workers who switch, suggesting that SHBs do not harm the quality of job matches. We also test whether employers shift hiring to non-SHB states.

Our paper makes several contributions. First, we develop a model of firms' choice between posting wages and bargaining, drawing out the implications for wages. Second, we test the predictions of this model on wages with a robust empirical design using a carefully selected control group, drawing conclusions on the effects on several possible disadvantaged job changer groups by gender and race. Third, we also explore possible non-wage impacts of SHBs including wage posting behavior, firm job advertising rates, and worker rates of changing jobs. These factors might limit the policy effectiveness of salary history bans. Regardless of policy efficacy, our study reveals a mechanism that plays a significant role in perpetuating pay inequities.

The paper proceeds as follows. In Section 2, we describe the institutional background and review the literature on pay gaps and salary history bans. In Section 3, we develop a simple wage formation model. In Section 4 we describe our data and empirical estimation, while the results are presented in Section 5. And in Section 6 we conclude.

# 2. Background

Over the past few decades, the gap between men's and women's wages has been shrinking, especially as human capital differences have disappeared (Blau and Kahn 2017; Goldin 2006; Goldin 2014). Nevertheless, a gap has persisted that is not easily explained by observable worker characteristics. Advocates for the bans argue that the use of salary histories contributes to these persistent gaps.

Salary history bans vary from state to state. Some states explicitly allow for applicants to voluntarily reveal salary history, while others allow for employers to ask for salary history once an employment offer with a proposed salary has been made. The SHB regulations do not mean that an employer never learns of an applicant's past salary but can only learn of salary history after they have made an employment offer or if an applicant offers their history unprompted. The bans do not prevent employers from asking for a "desired" salary. SHBs may cover all employers in a state or only public employers. Nevertheless, all of the bans restrict the information available to employers prior to reaching a bargain.

A related literature looks at other ways information affects wage setting, particularly regarding pay transparency (Mas 2014; Baker et al. 2019; Bennedsen et al. 2019; Cullen and Pakzad-Hurson 2019). But SHBs raise concern about possible adverse information economies. Meli and Spindler (2019) contend that salary history conveys important information about a worker's productivity. In their model, high-earning women can be hurt by the loss of salary history and the policy might even increase the gender wage gap, Moreover, reduced information to prospective employers might induce adverse selection so only low productivity workers change jobs (Greenwald 1986; Sran, Vetter, and Walsh 2020). We look for evidence of productivity and selection effects below.

Our paper also relates to a literature on wage posting and bargaining discussed in the next section.[4]

# 3. A simple model

We wish to set out a model that explains why a salary history ban might motivate some employers to switch from wage bargaining to posting and draw out the implications of that model for wage levels. Several papers have explored differences between occupations, finding that wages tend to be bargained more

[4] This literature includes Brenzel, Gartner, and Schnabel (2014), Brenčič (2012), Ellingsen and Rosén (2003a), and Michelacci and Suarez (2006).

frequently in occupations for more educated workers, presumably because these workers have more heterogeneous tasks and skills (Brenčič 2012; Brenzel, Gartner, and Schnabel 2014; Ellingsen and Rosén 2003b; Hall and Krueger 2010; 2012). But these differences are likely orthogonal to changes over salary history bans. Indeed, we find changes in job posting behavior across all major occupational categories. We abstract away from occupational differences by constructing a model for a single occupation. Below, as a check, we find evidence that SHBs do not significantly change the occupational composition of our sample.

We build on the insights of Hall and Krueger (2010) who develop a straightforward model to explain why some employers post wages for jobs while others choose to bargain individually with workers. The key result of their model is that employers will choose to bargain rather than to post wages when the wage elasticity of labor supply exceeds a certain threshold; they will post wages for elasticities below this threshold. We adapt this model to consider salary history bans. An SHB reduces the bargaining power of employers, increasing the posting-bargaining threshold, thus raising the share of jobs with posted salaries.

Hall and Krueger assume, without a significant loss of generality, that labor supply curves have constant wage elasticities. This assumption implies a specific underlying distribution of reservation wages. Specifically, for each job opening, $j$, in a particular occupation for a particular employer in a particular labor market, the available job applicants differ only in that they have different reservation wages. For the applicants to job $j$, the $i$th worker's reservation wage, $z_i$, is drawn from a distribution as

$$z_i \sim R(z) = z^{\psi_j}, \quad z \in [0,1], \;\; \psi_j \geq 0. \qquad (1)$$

We assume that job seekers encounter employers randomly and one at a time. This makes $\psi_j$ the wage elasticity of the labor supply for job $j$. $R(z;\, \psi_j)$ is a family of distributions/labor supply curves and there is some distribution of elasticities over jobs. This setup corresponds to notions of labor supply elasticity in what Alan Manning (2021) calls "New Classical Monopsony" models such as the model of Card et al. (2018). In labor markets where employers have been competing intensely, we would expect reservation wages to be highly similar in equilibrium. This corresponds to a high elasticity of labor supply, $\psi_j$, where the reservation wages are clustered around 1. We further assume that all workers, regardless of their reservation wages, have the same productivity, $p$.

The employer first decides whether to post a wage or to advertise without listing a wage, leading to bargaining. First, consider wage posting. Prior to encountering an applicant, the employer posts a wage, $w$. The employer does not know the applicant's reservation wage but does know the distribution, $R$. The employer's expected profit per worker is then

$$(p - w)R(w). \tag{2}$$

The employer chooses a wage that maximizes this ex ante expected profit (temporarily suppressing the subscript on $\psi$),

$$w_p = \frac{\psi}{\psi + 1}p. \tag{3}$$

where this is the standard monopsony wage and $\frac{\psi}{\psi+1}$ is the wage markdown. This wage yields expected profit

$$\pi_p = \frac{w_p^{\psi+1}}{\psi}. \tag{4}$$

Now consider bargaining. Here, the employer encounters an applicant and, if the applicant's reservation wage is less than or equal to their productivity, $z_i \leq p$, they bargain. The negotiation can be modeled as a sequential bargaining process where the parties split the surplus, $p - z_i$, with $\gamma(p - z_i)$ going to the employer and $(1 - \gamma)(p - z_i)$ going to the applicant, $0 \leq \gamma \leq 1$. In the case where employers know salary history, the negotiation can be thought of as a sequential bargaining process under complete information (Rubinstein 1982). If, instead, a salary history ban is in place, then the game becomes one of one-sided incomplete information (Myerson and Satterthwaite 1983; Fudenberg and Tirole 1991, 400). Following the theoretical literature, we assume that under incomplete information a bargain might not be reached and, if a bargain is achieved, the employer's share of the surplus, $\gamma$, is less than it would be under complete information, $0 < \gamma_{SHB} < \gamma_{noSHB} \leq 1$.[5] The *i*th worker's wage, conditional on a bargain being concluded, is then

$$w_{bi} = z_i + (1 - \gamma_k)(p - z_i), \qquad k = SHB, noSHB. \tag{5}$$

Since the applicant and employer will only reach a bargain if $z_i \leq p$, the ex ante expected profit is

$$\pi_b = \gamma_k R(p)(p - \bar{z}_b), \qquad \bar{z}_b \equiv E[z|z \leq p] = \frac{\psi}{\psi + 1} p = w_p. \tag{6}$$

The average wage, conditional on employment, is then

$$\overline{w}_b = \bar{z}_b + (1 - \gamma_k)(p - \bar{z}_b) = w_p + (1 - \gamma_k)(p - w_p). \tag{7}$$

Note that this wage is generally higher than the posted wage. Comparing (4) and (6), the employer offering job *j* will choose to bargain if

$$\gamma_k > \left(\frac{\psi_j}{\psi_j + 1}\right)^{\psi_j}. \tag{8}$$

Solving for $\psi_j$, the firm will choose bargaining when

$$\psi_j > \psi^*(\gamma), \qquad \text{where } \psi^*(\gamma) \text{ solves } \gamma = \left(\frac{\psi^*}{\psi^* + 1}\right)^{\psi^*}. \tag{9}$$

[5] For simplicity, and without loss of significant generality, we let $\gamma_{SHB}$ reflect both a probability less than one that a bargain will be concluded and the lower share going to the employer if a bargain is reached. We could add an additional parameter to handle these two aspects separately.

otherwise, the firm will post the wage. A solution will exist for $\gamma > \frac{1}{e}$. Thus, as long as $\gamma$ is not too low, firms will bargain over wages for jobs where the labor supply is elastic and they will post wages for jobs with inelastic labor supply.[6] The intuition for this result comes from the basic tradeoff between posting and bargaining: employers obtain lower wages when they post but only when applicants accept the job. As the labor elasticity of supply gets larger, the difference between the ex-ante expected wage under posting and bargaining shrinks to zero, reducing the advantage of posting. However, the probability that an applicant will accept the posted wage falls. This shifts the employer's choice to bargaining at higher elasticities as long as the employer has sufficient bargaining power. Although we (and Hall and Krueger) developed this result for a specific family of distributions, the result holds more generally as long as a substantial portion of workers reject the monopsony wage at high elasticities of supply.

There is some evidence to support this result. First, if labor markets are tight so that employers compete more intensely (high labor supply elasticity), we might expect less wage posting and vice versa when unemployment is high.[7] Brenzel, Gartner, and Schnabel (2014) find higher unemployment is associated with relatively more wage posting, consistent with the model.

We find similarly that wage posting is negatively associated with labor market tightness (see Appendix Table A7). Second, several papers have used measures of employer concentration in local labor markets as a proxy for market power that should be inversely related to the wage elasticity of labor supply (Rinz 2018; Benmelech, Bergman, and Kim 2018; Azar, Marinescu, and Steinbaum

[6] As $\psi$ increases asymptotically, the employer profits dwindle to zero for both posting and bargaining but bargaining remains more profitable in the limit.

[7] See also (Ellingsen and Rosén 2003b) and (Depew and Sørensen 2013).

2020; Azar et al. 2020).[8] They find higher employer concentration is associated with lower wages. We find that employer concentration is also positively associated with higher wage posting rates (see Appendix Table A7), implying that salary posting rates are greater for lower wage elasticity jobs.

Because an SHB changes the profitability of bargaining, it affects the relative profitability of bargaining versus posting, shifting the boundary of jobs that are posted. We can further distinguish the effect of an SHB on wages across three ranges of supply elasticities:

a) Post before and after SHB. In this range, $\psi_j < \psi^*(\gamma_{noSHB})$, wages remain unchanged at $w_p$.
b) Bargain before, post after SHB. In this range, $\psi^*(\gamma_{SHB}) > \psi_j \geq \psi^*(\gamma_{noSHB})$, equation (7) implies that the average wage falls by $(1 - \gamma_{noSHB})(p - w_p)$. Note that if $\gamma_{noSHB} \approx 1$, this wage decline will be negligible. Our results below are consistent with employers having high bargaining power without an SHB.
c) Bargain before and after SHB. In this range, $\psi_j > \psi^*(\gamma_{SHB})$, the average wage rises by $(\gamma_{noSHB} - \gamma_{SHB})(p - w_p)$.

From this setup, we can draw several implications for our empirical analysis about what happens when an SHB decreases $\gamma$. Assuming that the distribution of jobs by elasticity remains fixed, a decrease in firm bargaining power with an SHB means:

1. More jobs will be posted with salaries. Since $\left(\frac{\psi^*}{\psi^*+1}\right)^{\psi^*}$ is decreasing in $\psi^*$, equation (8) means that a decrease in $\gamma$ implies an increase in $\psi^*$.

[8] Manning (2021, 10) notes that in some search models higher employer concentration could represent a more competitive market.

More jobs will then fall into the range where posting is preferred to bargaining.

2. Average pay of job changers will rise for those jobs that bargain over wages. If $\gamma_{noSHB}$ is sufficiently large (close to 1), the average wage of all new jobs will rise. This is because the decline in wages in group (b) can be arbitrarily small, while the wages among group (c) jobs increase.
3. Assuming that the supply elasticity of jobs is uncorrelated with their productivity, the average posted wage will increase. This is because the jobs in group (b) have higher supply elasticities hence smaller markdowns than the jobs in group (a), all else equal.
4. Bargained wages will rise the most for those workers with the lowest reservation wages. Looking at equation (5), the change in the bargained wage for individual $i$ is $\Delta w_{bi} = -(p - z_i) \cdot \Delta\gamma$. This means that the increase in bargained wages will be greatest for the individuals with the lowest $z_i$. To the extent that certain groups suffer from depressed current wages, those groups should see larger increases in their wages under a switch to an SHB.

These implications provide hypotheses that can be empirically tested. However, the model has made some strong assumptions that might not hold empirically. Critically, the model assumes that the distribution of jobs and the distribution of reservation wages remain unchanged after an SHB. In the long run, however, these assumptions are likely to be untenable. If SHBs raise wages, then multi-state employers might shift their hiring to non-SHB states. If certain groups of employees particularly benefit from SHBs, these groups might change jobs more frequently, altering the distribution of reservation wages. Nevertheless, these effects on the extensive margin might take some time to occur. If so, then we can make reliable hypothesis tests and also gain lower-bound estimates of the effects of salary history information. Below we conduct tests on the composition of job changers and on the location of firm hiring to check for shifts along the extensive margin. We do not find economically significant shifts with only a few groups being statistically significant, suggesting that our short-run estimates hold under the model assumptions.

Also, the model assumes that employers know each worker's marginal productivity. It is possible, however, that salary histories might convey information about applicants' marginal productivities. In that case, a salary history ban might lead to a greater rate of bad matches, higher job turnover, and lower productivity. Below we also look for evidence on changes in turnover rates and productivity. We do not find significant evidence of change, suggesting that at least in the short run, productivity concerns do not affect our estimates.

Finally, the model assumes that without salary history information, employers' bargaining surplus falls from $\gamma_{noSHB}$ to $\gamma_{SHB}$. However, for discriminated groups, employers under an SHB might still practice *statistical discrimination*. For example, an employer under an SHB won't know a woman's previous salary but will know that women have been paid less than men on average. This statistical knowledge might well improve the firm's bargaining outcome so that it receives a share of the surplus that falls between $\gamma_{noSHB}$ and $\gamma_{SHB}$ for female job applicants. This possibility implies that the estimates we obtain for the impact of SHBs on disadvantaged groups may well be lower bound estimates of the extent to which salary history information perpetuates inequities for these groups.

# 4. Empirical analysis

## 4.1.Data

Our two main data sources are job advertisements collected by Burning Glass Technologies (BG), and survey microdata from the Basic Monthly Current

Population Survey (CPS). BG is a software company that scrapes and deduplicates the near-universe of online job advertisements.[9] A previous analysis of BG shows this dataset accounts for 60-70% of all job openings and 80-90% of openings requiring a bachelor's degree or more (Carnevale, Jayasundera, and Repnikov 2014). BG data includes the advertised wage (if any), firm name, industry, occupation, required education and experience, requested skills, and geographic location of the job at the state, county, and metropolitan statistical area.[10]

Our BG sample spans from January 2010 to December 2018. We omit job advertisements that are missing a firm name, are in the public sector, are part time, or are internships. Additionally, we require non-missing education and experience fields. In total, about 41 million postings meet these criteria.[11]

The CPS is a monthly survey that is jointly conducted by the Bureau of Labor Statistics and the United States Census Bureau. Participants are surveyed for four months consecutively, drop out of the sample for eight months, and then are interviewed again for four months. The survey reaches about 60,000 households per month. Our sample contains monthly data from January 2013 to

[9] For a detailed discussion of the representativeness of job posting data, see the appendix of Hershbein and Kahn (2018)

[10] We also classify commuting zones based on FIPS county codes, imputing some commuting zones based on county populations within the state. Firm names are cleaned by Burning Glass, though we cleaned firm names again, removing common identifiers such as "Inc." and "Ltd." and then applying a fuzzy matching algorithm. Occupations are provided up to 6-digit SOC codes, with better coverage at higher levels of aggregation. Industries are provided up to the 6-digit NAICS level, with better coverage at higher levels of aggregation. Advertised salaries are sometimes given as a single number and sometimes a range. We created three variables from salary advertisements. The first is a dummy variable indicating the presence of a salary advertisement of any kind. The second is an indicator for if the salary advertisement is given as a range. Finally, the natural log of average salary was calculated.

[11] These 41 million observations do not appear to be systematically different in terms of education or experience from the observations that do not meet these criteria.

February 2020.[12] In addition to a range of worker characteristics,[13] wage, weekly earnings[14], and hours worked are reported in the outgoing rotation groups, months 4 and 8. Our sample contains 1.1 million observations with wage or earnings data. However, when we limit the sample to control and treatment groups and look at demographic subgroups, the effective sample is much smaller. We provide tests below to demonstrate that these analyses have sufficient statistical power.

Importantly, the CPS asks if respondents are working for a new employer in months 2-4 and 6-8 in the survey. We use this information to determine whether workers in the outgoing rotation groups (months 4 and 8) have changed jobs during the last three months. Approximately 52,000 of our wage observations are for workers who changed employers during the three-month window.[15]

## 4.2. Control group

In the ideal experiment for our study, we would randomly assign firms to be under a salary history ban while allowing others to seek salary history. We could then compare salary posting rates and the wages of job changers between these two groups. But the actual passage of state SHB laws is not random. Factors that could have led to SHB laws—such as general concern about the gender wage

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[12] We further restrict the sample to include only respondents aged to 16-65, full-time workers, and those working in the private sector.

[13] The basic monthly CPS contains demographic information, education, occupation, industry, and job status. For ease of comparison with the Burning Glass data, Census definitions of occupation and industry were converted to their Standard Occupation Code (SOC) and North American Industry Classifications System (NAICS) equivalents, respectively.

[14] Earnings in the CPS are top-coded, with different top codes for hourly and annual earnings. Hourly earnings are top coded at \$99.99 for usual hours worked < 29 and \$2885.07/hours worked for those with usual hours worked > 29. Less than 1% of observations are top coded at either weekly or hourly wage levels. When normalized to annual earnings, 0.67% of observations are top coded. Excluding top-coded observations does not significantly alter our results.

[15] To control for business cycle effects, we also add a measure of labor market tightness by state-month. We follow Moscarini and Postel-Vinay (2016) in defining labor market tightness as the ratio between Job Openings and Labor Turnover Survey (JOLTS) statewide openings for the non-farm sector and the state unemployment rate.

gap—might also lead employers to adjust women's wages independently of the SHB. To assuage concerns about selection into SHB laws and unobserved heterogeneity, for both data sets, we construct a comparison group that consists of counties not covered by SHBs, but in the same labor market areas (commuting zones) as treated counties. Commuting Zones were defined beginning in the 1980s to better delineate labor markets by grouping counties using a hierarchical cluster analysis and the Census Bureau's "journey to work" data.[16] A county is more similar to its cross-state counterpart than a randomly chosen one. Appendix Figure A1 shows an example of a commuting zone consisting of treated and untreated counties. Adjacent counties likely have similar sentiments regarding the gender wage gap and other factors possibly related to the passage of SHB laws. Other studies have taken a similar approach to eliminating selection bias using adjacent counties or state line boundaries to create control and treatment groups (see for example, Dube, Lester, and Reich 2010, Card and Krueger 1994). This is a conservative approach that might understate the measured treatment effects because labor market competition might cause comparison group firms to post wages or raise offers to women. In the Appendix, we explore other control group definitions using synthetic controls with algorithmically defined weights.

Also, not all respondent county codes are reported in the CPS. In the analysis below, we only include control group observations where county information is reported. In Appendix Table A5, we explore alternative control groups where we include non-reporting counties in adjacent states, all observations in adjacent states, and all non-treated states. These alternative

[16] The county groupings of commuting zones are slightly adjusted every 10 years. We selected the commuting zones defined in 1990 and utilized Dorn's crosswalk file to map counties to commuting zones (Autor and Dorn 2013). For more details on the construction of Commuting Zones, see Tolbert and Sizer (1990).

control group choices generate higher treatment coefficients than our more conservative assignment.

# 5. Results

## 5.1.Salary posting

We study the propensity to advertise salary using a standard extended difference-in-difference specification:

$$P_{ist} = \alpha_s + \beta_t + \gamma \cdot \mathbf{1}[t \geq \tau_s] \cdot \mathbf{1}[s \in T] + \delta X_{ist} + \epsilon_{ist}. \qquad (10)$$

where $P_{ist}$ is 1 if ad *i* lists salary in state *s* at time *t*, and 0 otherwise. $\alpha$ and $\beta$ are state and time fixed effects, $X_{ist}$ is a vector of controls,[17] and $\epsilon_{ist}$ is the error term. $\gamma$ is the estimate of the treatment effect, treatment occurring when the state belongs to the set of treated states, *T*, and the observation occurs after the effective date of the SHB, $\tau_s$.

The first column in Table II estimates a treatment effect using our treatment and comparison groups. Errors are clustered by state, the primary unit for the assignment of treatment.[18] The estimate is about 3 percentage points and is highly significant.

Figure II shows event study coefficients for a comparable regression plotted against the quarter relative to the ban with a 95% confidence interval.[19] The rate of posting increases sharply the quarter after the ban goes into effect.

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[17] The controls include labor market tightness, experience required (and squared experience), education required, county, firm, and occupation.

[18] Seven counties in New York state enacted SHBs prior to the statewide ban for all employees. These represent only 1% of the observed treated workers.

[19] The coefficients $\gamma_{t-\tau}$ are obtained from regressing a dummy variable for posting, *P*, $P_{ist} = \alpha_s + \beta_t + \sum_{\substack{t-\tau \\ t-\tau\neq -1}} \gamma_{t-\tau} \cdot \mathbf{1}(t \geq \tau_s) \cdot \mathbf{1}(\mathrm{s} \in \mathrm{T}) + \delta X_{ist} + \epsilon_{ist}$ where $\tau_s$ is the quarter when the ban went into effect and *X* are control variables. The coefficients are omitted for the first quarter observed in the data and for the quarter immediately before the ban.

There are no significant pre-event trends, although perhaps a slight negative anticipation effect can be seen the quarter before the ban. This implies that wage posting rates for the control group were not trending differently from the treatment group prior to the bans. This provides support for the assumption that wage posting rates for treatment and control group trend in parallel, making the control group a plausible counterfactual.

We also support our identification by using two placebo tests. First, in several states, the SHBs that were enacted only covered government jobs. If our measured treatment effect were driven by general concerns about the gender wage gap, a "zeitgeist effect", then we should see a change in job posting by private employers following a state ban on salary histories for public sector employers. Column 2 of Table II shows results for control and treatment groups selected for public SHBs. It shows no such effect. Second, if such factors were behind our result, then we would expect to see an increase in job posting after the salary history bans were *enacted* but before they came into *effect*.[20] Column 3 repeats the regression of column 1, adding a treatment effect after the SHB was enacted but before it came into effect. The enacted date effect is actually negative and statistically significant, consistent with the anticipation effect seen in the event study. These tests address concerns about policy endogeneity and spillovers from public sector SHBs.[21]

Although we find an economically significant treatment effect of around 3 percentage points in our baseline estimation, this is quite a bit smaller than the nearly 25 percentage point jump in salary posting rates seen in Figure I. This may

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[20] The mean lag from enactment to effect is 205 days in our sample.

[21] Aside from the shown placebo tests, we also performed a separate endogeneity check using the first- and second-order residual gender wage gap and measures of state political ideology (constructed by Richard Fording) as predictors for a state adopting the SHB. The coefficients are not statistically or economically significant.

stem from our attempt to measure the direct effect of the SHB on job posting in the affected states using a conservatively selected control group. However, there may be a substantial indirect or contamination effect as well. That is, employers not subject to the ban might nevertheless change their hiring behavior because of changing attitudes. For example, the enactment of SHBs in some states may have encouraged human resource professionals to voluntarily avoid the use of salary histories or to switch to job posting. Or multi-state firms may change policies company-wide after encountering an SHB in one state. In the Appendix, we show results from using different control groups with synthetic control analyses of California's SHB. These support the notion of a substantial indirect effect of SHBs on job posting.

Finally, columns 4 and 5 of Table II explore whether the SHB changed the salaries advertised conditional on being posted. Consistent with the model, the SHB is associated with a small but statistically insignificant increase in the average log salary posted (column 4) and no change in the size of the range of salaries posted (column 5).[22] While employers may change their behavior in terms of posting wages, it does not appear that they adjust the characteristics of posted wages.

## 5.2. Pay of job-changers

In our model, changes in bargaining power both induce firms to post salaries for more jobs and to pay higher wages for job changers. To the extent that such differences in bargaining power drive differences in posting rates across states, we should expect states with higher posting rates to also pay job changers more. Using our sample of matched counties, Column (1) of Table III

[22] The dependent variable is the maximum salary advertised minus the minimum divided by the minimum.

demonstrates a significant positive association between log annual earning of job changers with the monthly share of job ads that post salary information per state.[23]

We can see the impact of an SHB in a crude way by looking at the unconditional change in wage realized by workers who change jobs. For a subset of the CPS outgoing rotation groups, we observe the hourly wages of workers who have changed employers during the last three months and we can also observe their hourly wages a year earlier. We calculate that for job changers not under an SHB, the unconditional mean hourly wage is 3.9% higher than the year-earlier wage, but for job changers under an SHB, the increase is 7.9%. This difference, 4%, is large and statistically significant.[24]

It is possible that SHB states tended to have some other factor that affected earnings. We can further control for such possible trends using a difference-in-differences-in-differences design (DDD). Since we assume that the SHB affects the pay of job changers but not of incumbent workers,

$$Y_{ist} = \alpha_s + \beta_t + \gamma \cdot \mathbf{1}[i \in N] \cdot \mathbf{1}[t \geq \tau_s] \cdot \mathbf{1}[s \in T] + \delta X_{ist} + I + \epsilon_{ist}$$
$$I = \mu \cdot \mathbf{1}[t \geq \tau_s] \cdot \mathbf{1}[s \in T] + \rho \cdot \mathbf{1}[i \in N]. \tag{11}$$

where $Y_{ist}$ is log annual earnings for individual *i* in state *s* at time *t*. Here, the treatment effect is estimated for workers belonging to the set of job changers, *N*, in SHB states, after the effective date of the SHB. We include other interaction terms to capture baseline effects for job changers and treated workers.[25] This regression is run on the full outgoing rotation group sample and is shown in Column 2. The estimated treatment effect is larger (8.0%) and there does seem to

[23] All regressions include controls for experience, experience squared, education, union coverage, marital status x gender, child in household, industry, county, occupation, month and year.
[24] The probability value of a t-test is .035.
[25] The third level of difference here between job changers and incumbents intends to take away contamination in the treated states coming from general concerns of pay equality that affect both two groups independently of the bans.

be a significant positive effect among incumbent workers (4.3%), which may come from the general concerns on equality in the treated states.[26] If we subtract the coefficient on incumbents from the coefficient on job changer, we obtain a *net* effect of 3.8% for the increase in annual earnings for job changers solely coming from SHBs. Column (3) repeats the estimation on county pairs only, which provide an additional level of control. The treatment effect for incumbent workers is smaller and only marginally significant, but the net effect is very similar to that of the full sample. Figure III reports the event study charts corresponding to this regression. Once again, there do not appear to be significant pre-trends but a significant increase in pay following an SHB. Column (4) repeats the regression of column (3), but with log hourly wage as the dependent variable. We have a very similar estimate of the treatment effect and net effect.

One concern is that these estimates might be biased if the composition of job switchers changes under an SHB. For instance, Meli and Spindler (2019) argue that highly paid women will be less likely to change jobs under an SHB. We further explored whether changes in the composition of job changers might affect our results. We compared the year-earlier pay of workers who changed jobs in months 6 to 8 of the survey depending on whether the worker was covered by an SHB or not. Calculating Mincer-type residuals for the earlier year, job changers under an SHB tended to come from jobs with slightly higher pay on average, contrary to Meli and Spindler. However, the difference was small and not statistically significant.[27] Consistent with the model, workers who earned relatively more in their previous job benefit less from an SHB than do lower paid

[26] The regression also includes non-interacted dummy variables for incumbents and job changers (not shown).

[27] The Mincer equation controlled for experience, experience squared, union membership, part time status, marital status x gender, motherhood, county, occupation, industry, education, month and year. The difference in the log residual was .023 (.026).

workers.[28] This implies that our estimates of the treatment effect on pay are slightly smaller than they would be otherwise because of this small shift in the composition of job changers.

The above estimates stand up to a variety of robustness checks (see Appendix). To correct for possible state-specific trends, Table A2, column 1 shows an estimation with state-by-year fixed effects. Column 2 explores whether the effect is consistent across different states, here grouped by cohort of the dates the SHB went into effect. This is important because California represents over half of the treated observations. If anything, the treatment effect appears to grow larger over time. Also, Table A4 reports estimates of Table III using Coarsened Exact Matching to balance the characteristics of the treatment and control groups.

Finally, we also test our findings on an alternative dataset, the Quarterly Workforce Indicators from the Census (see also Sran et al. (2020)). Although this is aggregate data, it can be obtained for a sufficient number of cells to run our basic difference-in-differences regression using our treatment and control counties. In Table A8 in the Appendix, we find an SHB treatment effect of 3.0% for job changers.

Is the size of our treatment effect implausibly large? Previous studies have found big changes in pay when relevant information is revealed for university faculty and CEOs (Mas 2016; Baker et al. 2019). More directly comparable, Barach and Horton (2020) conduct a field experiment and find an even greater difference, 9%, when salary history is suppressed. Moreover, our estimated treatment effect should be thought of as a lower bound to the Local Average Treatment Effect because it is an Intent to Treat. Not all prospective applicants choose to keep their salary histories private; some voluntarily reveal their

[28] Job changers with above-median Mincer residuals saw their pay change by -.025 (.020); job changers with below median residuals saw their pay change .107 (.015).

histories thus providing employers with a bargaining advantage. Agan et al. (2020) survey 504 Americans in the labor force and find that 58% of them are "compliers" of the SHB regardless of the firm's behavior. This suggests that the Local Average Treatment Effect is larger than the estimates shown in Table III.[29]

## 5.3. Gender and Minority Groups

The model suggests that individuals with low reservation wages should see the greatest pay gains from a salary history ban. Consequently, groups of individuals who might have experienced discrimination or other disadvantages should see gains. Table IV explores the relationship between SHBs and wages for several groups. It again uses the DDD specifications for workers of different groups possibly subject to discrimination, distinguishing between job-changing and incumbent workers as well as selecting a control group highly similar to the treatment group.

In the first column of Panel A, we see that job-changing women earn 9.9% more under an SHB. Job-changing men also earn significantly more (3.5%), suggesting that SHBs reach beyond gender inequities, as our model suggests. SHBs also have a significant effect on the pay of incumbent women, consistent with a general concern about gender equity affecting incumbents; there is not a significant effect on the pay of incumbent men. As above, the difference between the coefficient on female job changers and the coefficient on female incumbents demonstrates a 6.4% net effect of SHBs. We can compare this to the residual wage gap for female job changers. From the baseline effects in the table, female job changers earn 14.3% less than male job changers on average, after taking observables into account. This implies that on net, SHBs reduce the gender wage

[29] Dividing our reduced form estimates by 0.58, job changers under SHB thus see a 7.9% increase in hourly wages and 6.9% increase in annualized salary.

gap for female job changers by $\frac{6.4}{14.3} = 45\%$. That is, almost half of the residual gender wage gap is accounted for by differences in bargaining behavior under SHBs. The bottom row of each panel lists this ratio for each group. To ensure that a general concern about gender inequity is not driving these changes, we run a placebo test in column 2. Here, the events studied are SHBs that cover only public employees. Presumably concerns about gender equity have promoted the passage of these SHBs, but they do not cover the workers in our sample at private employers. The effects of these placebo events are not significant either economically or statistically, suggesting that our results are not driven by a general concern about gender inequity.

The model implies that some male job changers should see pay gains following an SHB, including workers in possibly disadvantaged groups. Panel B explores treatment effects for non-white workers of both genders (column 3) and for non-white male workers separately (column 4). Non-whites job changers earn substantially more after an SHB, seeing a 12.7% increase in wages. But there is a significant 4.9% increase in pay for incumbent non-whites, suggesting a general concern about racial pay inequities that might be correlated with the SHB events. After subtracting this background effect, the net effect of an SHB is a 7.7% increase for non-white workers. Column (4) repeats the exercise, but only for male workers. Non-white male job changers experience an 11% increase in wages relative to white male job changers with a 6.3% net effect. These net effects are highly significant, and they account for a substantial share of the residual pay gaps. These findings suggest that these groups might, indeed, be disadvantaged, perhaps because of discrimination.[30]

[30] Although salary history bans may have been intended primarily to benefit women, they appear to play a substantial and positive role for other disadvantaged groups, consistent with our model.

One possible concern with these estimates is that the sample size of job changers for the observed groups might be too small to provide reliable statistical estimates. In the Appendix (Table A10), we conduct power tests finding sufficient statistical power for the sub-samples analyzed in Table IV.[31] In addition, Table A8 shows results using the QWI which generate similar effect sizes for women and Black workers (compared to non-white workers).

Finally, these estimates of treatment effects might be understated because of statistical discrimination. If employers do not know salary histories under an SHB, they might *infer* previous salaries by the applicant's identity, for example, making lower offers to female or non-white applicants.[32] To the extent that such statistical discrimination occurs, our estimates of treatment effects for these groups understate the true level of inequality perpetuated by the use of salary histories.

## 5.4. Changes on the Extensive Margin

Our estimates are based on a model that assumes a stable distribution of jobs and of job applicants. But given real wage effects, some workers might be more likely to change jobs than others and some employers might shift their hiring to states without salary history bans. Also, to the extent that salary history reveals information about the applicant's productivity, firms might make better matches if they knew the applicant's salary history. Poorer quality matches under an SHB could lead to higher worker turnover that causes lower productivity. These secondary effects might confound our estimates or lead to possible adverse

[31] Subsamples looking at Black and Hispanic workers, have less statistical power, hence we excluded them.

[32] Such behavior would violate the Equal Pay Act. There is, however, some evidence that "ban the box" legislation, which suppresses employer access to information on felony convictions or credit checks, increased statistical discrimination against minorities (A. Agan and Starr 2018; Doleac and Hansen 2020; Bartik and Nelson 2019).

effects. (Greenwald 1986; Meli and Spindler 2019; Sran, Vetter, and Walsh 2020).

Above we found that higher paid workers were slightly more likely to change jobs, although this difference was not statistically significant. Table V provides additional evidence that SHBs appear to have little effect on job turnover or on the composition of job changers. The table reports the rate of job switching for the control and treatment groups overall and for subgroups. The third column reports the difference with standard error. Overall, turnover is slightly less in the treatment group at a weakly significant level. Nor does the composition of job changers appear to change substantially for the various subgroups shown.[33]

As a robustness check we also tested employee turnover using the QWI (see Appendix Table A9), finding no significant overall effect and statistically significant but economically small increase in turnover for women. These findings suggest that SHBs do not result in higher job turnover arising from poorer matches. Nor do we find evidence of a change in productivity. In Appendix Table A2, column 3, we treat state GDP per worker in a DID regression with year and state fixed effects. Labor productivity does not seem to change with an SHB. One possibility is that under an SHB, employers substitute other sources of information about worker productivity. We tested whether SHBs are associated with higher skill requirements listed in the ads (see Appendix Table A6). We find that SHBs are associated with higher levels of education required, experience required, and the number of skills required, although the coefficients are not large. Information about these specific skill measures appears to at least partially substitute for salary history information.

[33] More specifically, the t-test result for the overall sample in row 1 is significant at 10% level, result for services & healthcare support occupation is significant at 5% level, and result for 31-49 age group is significant at 1% level. For the other groups, the differences are not significant.

We also tested the possibility that firms, faced with higher labor costs under an SHB, might choose to switch their hiring to non-SHB states. Table A3 in the Appendix shows difference-in-differences regressions of the log of the number of online help-wanted ads over states by month. Column 1 reports the results for just ads of multi-state firms; Column 2 reports for all firms. Instead, of decreasing, help-wanted ads appear to rise slightly under an SHB although the effect is not statistically significant.

All told, in our data salary history bans do not appear to be associated with substantial changes in job turnover, the composition of the workforce, or labor market demand. These factors do not appear to bias our estimates of treatment effects. However, it is quite possible that there have not been significant effects on the extensive margin because our time window post-SHB is short, just a year or two. Larger changes along the extensive margin may occur in the future. This means that although we can draw robust conclusions about the role of salary histories in wage setting and bargaining, the impact of SHBs as a policy instrument may well require additional years of evidence to properly evaluate.

# 6. Conclusion

Salary histories reveal information about job applicants' reservation wages to employers, giving employers a bargaining advantage. Correspondingly, salary history *bans* reveal evidence about the frequency with which employers have exploited this information and the magnitude of the advantage it provided them. Our evidence suggests that this advantage has been an important factor perpetuating wage inequality, especially for women and non-whites.

We find that following SHBs, employers increase the rate at which they post salaries in online help wanted ads. This suggests that SHBs remove a benefit of wage bargaining, inducing some firms to post wages instead. The national share of online help wanted ads listing salary information increased by around a quarter of all ads following the introduction of SHBs in a dozen states. Since employers in other states were not under direct pressure to avoid using salary histories and since not all employers under SHBs would switch to advertising salaries, this suggests that, as a lower bound, a quarter of employers might have exploited salary history information.

And the implied benefit to employees is large. The wages of job-changing workers subject to SHBs rose 3.9% on average compared to equivalent job changers in other states. Apparently, workers who have had low reservation wages earn relatively more when they switch jobs under a salary history ban and less when they switch without one.

Also, we find particularly strong effects of SHBs on groups subject to historical discrimination. Workers' pay—and hence their current reservation wages—can be less than their marginal productivity for a variety of reasons, including search frictions, different bargaining capabilities, monopsony power, and discrimination. But groups historically subject to discrimination appear to be particularly affected by pay inequities. Following SHBs, the pay of job-changing women rose about 6.4% and the pay of job-changing non-whites rose about 7.7% on average compared to control group job changers, after netting out general changes in pay.

Moreover, the estimated treatment effect of SHBs for these groups is large compared to the residual wage gaps that remain after controlling for observable characteristics. Over the last decades, average wage differences between men and women or between non-whites and whites have narrowed as education and experience differences have shrunk or even been reversed. However, persistent pay gaps still remain, and it is unclear whether these are due to discrimination, to unobserved differences in worker characteristics that affect their productivity, or to something else. Our analysis suggests that at least half of the residual wage gap for job-changing women disappears under an SHB, implying that at least half of the residual gender wage gap cannot be attributed to differences in worker productivity. The bargaining process appears to account for an even larger share of the residual wage gap for non-whites.

The differences erased by SHBs are not necessarily caused by individual discrimination—for instance, they might reflect group differences in negotiating propensity. But salary histories enable a form of *institutional* discrimination. Even if employers do not individually discriminate, the use of salary histories appears to perpetuate the effects of past discrimination or other group inequities.

The large effects we estimate might seem surprising because the salary history ban is a rather mild restrictive policy. For instance, as previously noted, SHBs do not prevent employers from asking applicants their desired salary nor do they prevent applicants from volunteering past salary history (Agan, Cowgill, and Gee 2020). Perhaps the impact of SHBs despite these limitations speaks to the depth of pay inequities that are unrelated to productivity. In any case, the effect size compares well with experimental evidence (Barach and Horton 2020).

As a policy directed to address pay inequities, salary history bans appear to have had a positive effect in our sample. However, our effects are limited to a short time window and adverse effects might develop over a longer time period. Nor do our data speak to workers' wage trajectories after they are hired or about the effectiveness of this policy in a less-than-booming economy. While the overall effectiveness of salary history bans at correcting pay inequities appears to be promising, definitive conclusions await further research. Nevertheless, we have identified a major mechanism that appears to perpetuate inequality and our analysis implies that the persistent pay gaps remaining for women and non-whites are not mainly about unmeasured productivity differences. Our results make clear that informational concerns may be key to designing more equitable policies.

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# 8. Tables

Table I. Statewide Salary History Bans

| State | Passed | Effective | Employers Covered | Groups Mentioned |
|---|---|---|---|---|
| Massachusetts | 8/1/16 | 7/1/18 | All | Gender |
| New York | 1/9/17 | 1/9/17 | Public Only | Gender |
| Puerto Rico | 3/8/17 | 3/8/17 | All | Gender |
| Oregon | 5/22/17 | 10/6/17 | All | Protected Classes |
| Delaware | 6/14/17 | 12/14/17 | All | Gender |
| California | 10/12/17 | 1/1/18 | All | None |
| District of Columbia | 11/17/17 | 11/17/17 | Public Only | None |
| New Jersey | 1/16/18 | 2/1/18 | Public Only | Gender |
| Hawaii | 1/19/18 | 1/1/19 | All | Gender |
| Vermont | 5/11/18 | 7/1/18 | All | None |
| Connecticut | 5/22/18 | 1/1/19 | All | None |
| Pennsylvania | 6/6/18 | 9/4/18 | Public Only | Gender |
| New Jersey | 1/14/19 | 1/1/20 | All | None |
| Illinois | 1/15/19 | 1/15/19 | Public Only | Gender |
| North Carolina | 4/2/19 | 4/2/19 | Public Only | Gender |
| Maine | 4/12/19 | 9/17/19 | All | Gender |
| Washington | 4/25/19 | 7/28/19 | All | Gender |
| Colorado | 5/22/19 | 1/1/21 | All | Gender |
| Alabama | 5/30/19 | 9/1/19 | All | Race, Gender |
| New York | 6/15/19 | 1/6/20 | All | None |
| Virginia | 6/20/19 | 7/1/19 | Public Only | None |
| Illinois | 7/31/19 | 9/29/19 | All | Gender, "Other Protected Characteristics" |

Note: This table shows the states with salary history bans. Our main analysis only includes SHBs that cover private employers. In addition to these statewide bans, New York City instituted a ban on 10/31/2017, and in New York State bans were put in effect by Albany County 12/31/2017, Westchester County 7/9/2018, and Suffolk County 6/30/2019.

Table II. The Effect of Salary History Ban on Firm Salary Posting

Dependent variable = 1 if help wanted ad contains salary information, 0 otherwise.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Base | Public SHB Placebo Test | Enacted Date Placebo Test | Mean Ln Salary | Salary Range (pct) |
| Post-SHB | 0.027 | -0.004 | 0.025 | 0.010 | 0.001 |
| | (0.009) | (0.003) | (0.009) | (0.007) | (0.006) |
| Enacted Date | | | -0.009 | | |
| | | | (0.003) | | |
| Observations | 17,530,375 | 5,490,708 | 17,530,375 | 1,486,575 | 1,486,575 |
| R-squared | 0.449 | 0.408 | 0.449 | 0.676 | 0.429 |

Note: This table shows the extended Diff-in-Diff (DD) results of the effect of SHBs on firms' salary posting and salary offered. The data are from the near universe job board microdata in US compiled by Burning Glass Technologies. Errors are clustered by state in parentheses. Sample include online help wanted ads for counties that are eventually treated and for control counties and excludes ads for interns, part-time jobs, public sector employers and employers where no firm is listed (likely recruiters). Column 2 has a different sample with treatment and control groups defined for states with SHBs that cover only public sector employees. All regressions include controls for labor market tightness, experience required, experience squared, education required, firm, county, occupation, month and year.

Table III. Salary History Bans and Pay by Job Changers and Incumbents

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable (log) | Annual Earnings | Annual Earnings | Annual Earnings | Hourly Wage |
| Sample | County Pairs | Full Sample | County Pairs | County Pairs |
| Treatment effects | | | | |
| Incumbent x State Posting Rate | 0.242 | | | |
| | (0.133) | | | |
| Job Changer x State Posting Rate | 0.474 | | | |
| | (0.140) | | | |
| Incumbent x Post-SHB | | 0.043 | 0.026 | 0.028 |
| | | (0.009) | (0.012) | (0.012) |
| Job Changer x Post-SHB | | 0.080 | 0.065 | 0.064 |
| | | (0.010) | (0.012) | (0.015) |
| Baseline | | | | |
| Job Changer | -0.055 | -0.033 | -0.032 | -0.030 |
| | (0.008) | (0.003) | (0.006) | (0.006) |
| Observations | 313,668 | 1,041,923 | 337,700 | 330,289 |
| R-squared | 0.554 | 0.547 | 0.553 | 0.507 |
| **Net effect for job changers** | 0.232 | 0.038 | 0.039 | 0.036 |
| | (0.050) | (0.006) | (0.007) | (0.005) |

Note: Column 1 of this table presents a correlation test of the state salary posting rate and the annual earnings of individuals. Column 2-4 shows the DDD estimation results of the effect of SHBs on wages. The net effects are the differences between the coefficients on job changers and the coefficients on incumbents to control for other factors that affect both groups in the treated states. The dataset is from the Current Population Survey. Errors are clustered by state in parentheses. This sample includes private sector employed workers in control and treatment groups. All regressions include controls for experience, experience squared, education, union coverage, marital status x gender, child in household, industry, county, occupation, month and year. Job changers are determined by those in outgoing rotation groups who report that they changed employers in the previous 3 months. Additionally, the same analysis was run omitting top-coded salaries in the CPS; results were highly similar.

Table IV. Salary History Bans and Log Annual Earnings by Groups

| **Panel A: Male / Female** | | |
|---|---|---|
| | (1) | (2) |
| Sample: | All | Placebo |
| **Treatment effects** | | |
| Male **x** Incumbent **x** Post-SHB | 0.017 | 0.001 |
| | (0.011) | (0.007) |
| Male **x** Job Changer **x** Post-SHB | 0.035 | 0.039 |
| | (0.010) | (0.024) |
| Female **x** Incumbent **x** Post-SHB | 0.035 | -0.006 |
| | (0.014) | (0.007) |
| Female **x** Job Changer **x** Post-SHB | 0.099 | 0.020 |
| | (0.019) | (0.024) |
| **Baseline effect (gap)** | | |
| Female **x** Job Changer | -0.143 | -0.154 |
| | (0.008) | (0.012) |
| Observations | 337,700 | 186,846 |
| R-squared | 0.555 | 0.563 |
| **Net Effect for Female Job Changers** | 0.064 | 0.025 |
| | (0.012) | (0.024) |
| Net Effect / Gap for Job Changers | 45% | 16% |

| **Panel B: White / Non-White** | | |
|---|---|---|
| | (3) | (4) |
| Sample: | All | Males Only |
| **Treatment effects** | | |
| White **x** Incumbent **x** Post-SHB | 0.020 | 0.012 |
| | (0.013) | (0.010) |
| White **x** Job Changer **x** Post-SHB | 0.045 | 0.015 |
| | (0.013) | (0.010) |
| Non-White **x** Incumbent **x** Post-SHB | 0.049 | 0.047 |
| | (0.012) | (0.013) |
| Non-White **x** Job Changer **x** Post SHB | 0.127 | 0.110 |
| | (0.020) | (0.030) |
| **Baseline effect (gap)** | | |
| Non-White x Job Changer | -0.089 | -0.109 |
| | (0.012) | (0.023) |
| Observations | 337,700 | 171,379 |
| R-squared | 0.555 | 0.548 |
| **Net Effect for Non-White Job Changers** | 0.077 | 0.063 |
| | (0.015) | (0.023) |
| Net Effect / Gap for Job Changers | 87% | 58% |

Note: This table shows the DDD results of SHBs on annual earnings by group. Errors are clustered by state in parentheses. Sample includes private sector employed workers in control and treatment groups from the Current Population Survey. Column 2 uses SHB laws that covered only public employees as a placebo treatment; the other columns use SHB laws covering all employees. Non-white is defined as any respondent who does not identify as white in the CPS. Job changers are determined by those in outgoing rotation groups who report that they changed employers in the previous 3 months. All regressions include controls for experience, experience squared, education, union coverage, marital status x gender, child in household, industry, county, occupation, month and year. Not shown are baseline effects interacting male/female (white/non-white) with job-changer/incumbent

Table V. Changes in job switching rates with and without SHB

*Share of workers who have begun a new job during the last 3 months*

| | (1) | (2) | (3) | |
|---|---|---|---|---|
| | Control Group | Treatment Group | Difference | |
| All | 0.046 | 0.044 | -0.002 | (0.001) |
| Male | 0.046 | 0.044 | -0.002 | (0.001) |
| Female | 0.047 | 0.045 | -0.002 | (0.001) |
| **Occupation** | | | | |
| Management | 0.038 | 0.042 | 0.004 | (0.003) |
| Professional | 0.043 | 0.041 | -0.002 | (0.002) |
| Services & healthcare support | 0.055 | 0.049 | -0.006 | (0.003) |
| Sales | 0.047 | 0.045 | -0.002 | (0.003) |
| Office & administrative | 0.045 | 0.042 | -0.003 | (0.003) |
| Production, constr., transport | 0.049 | 0.047 | -0.002 | (0.002) |
| **Age** | | | | |
| 15-30 | 0.062 | 0.06 | -0.002 | (0.002) |
| 31-49 | 0.043 | 0.039 | -0.004 | (0.002) |
| 50-65 | 0.036 | 0.037 | 0.001 | (0.002) |

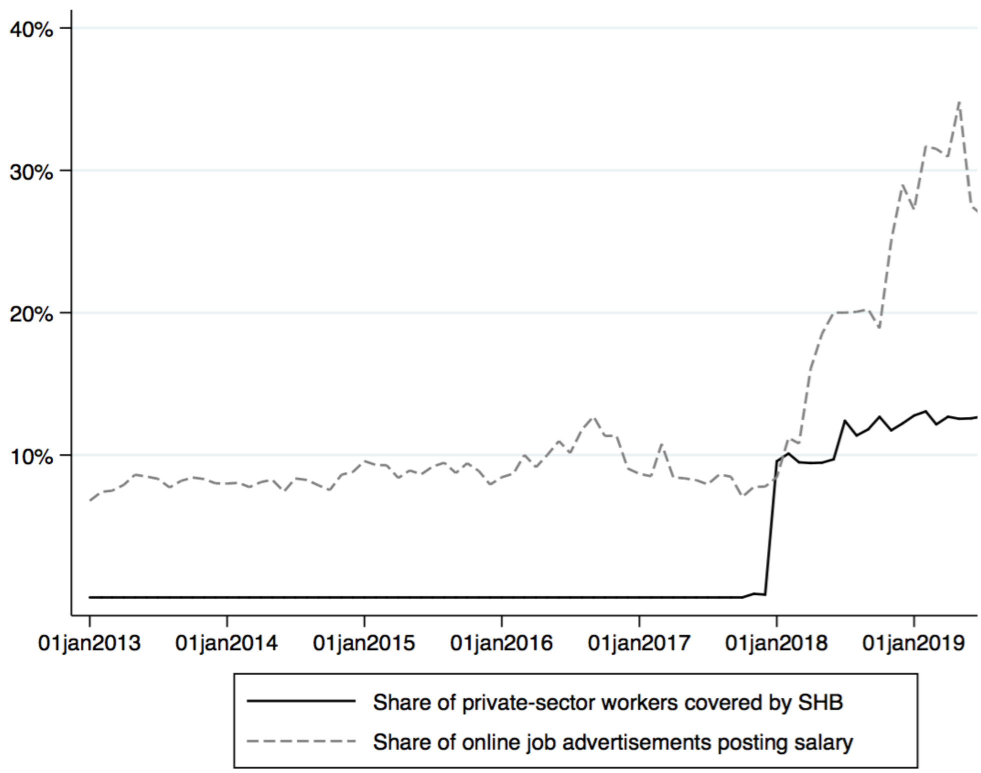
Note: This table reports the comparison of the share of job switchers in the treatment and control group. The third column report the difference with standard error. Data drawn from the Current Population Survey. Sample includes private-sector employed workers in control and treatment groups.

# 9. Figures

Figure I

Coverage of Salary History Bans and Online Job Posting

Source: Current Population Survey; Burning Glass




Note: This figure shows share of private-sector workers covered by a salary history ban policy in the United States and the share of online job advertisements that posted a salary or salary range. Shortly after the first salary history bans went into effect the share of job ads that posted wages nearly tripled.

Figure II

Event study of online salary posting


Note: This figure shows an event study of the probability of posting a salary in an online job advertisement. Compared to figure I these shares may seem low, but the specification controls for county, education, experience, experience squared, occupation, and firm name. There may be a slight anticipation effect in the quarter relative to the ban. All subsequent quarters show a statistically significant increase in the rate of salary posting.

Figure III

Event Study of Job Changer Salaries


Note: This event study shows the log annual earnings of job changers from the Current Population Survey. Job changers are determined by answers to a question asking if the respondent has begun working for a new employer in survey months 2-4 and 6-8. There is no clear pre-trend in the four quarters leading up to a salary history ban and a clear and statistically significant increase starting in the quarter following these bans.