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 (7.6%). Bargaining behavior appears to account for much of the persistence of residual wage gaps.

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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 provides information about 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 bans were implemented and for a short time afterwards. We further explore differences between demographic groups, and we measure the effects on the composition of job changers.

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 aim to glean important and comprehensive insights about pay inequities, from both the changes in wage posting behavior of employers in job openings and in the actual equilibrium wages. We are interested in the short-run reaction to SHBs because they reveal aspects of wage gaps yet we recognize that dynamic effects may alter the long run equilibrium needed for policy analysis.

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 Mortensen 1998), monopsony, labor market conditions (Mask 2020), or discrimination. Salary history information provides information about 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

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

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 three 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. 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 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 control group that is likely to have similar general concerns about pay inequities. In contrast, Sinha (2019) and Hansen and McNichols (2020) pool incumbent workers and job changers in measuring the gender wage gap, obscuring identification, although Hansen and McNichols note that most of their measured effects arise from job changers. Sran et al. (2020) look only at job changers, so they are unable to net out effects that are common to incumbent workers.[2]

Third, we look at how salary history bans affect the composition of the workforce. A variety of mechanisms, including adverse selection and better information flows, might affect which workers choose to change jobs. Then the measured wage changes might reflect changes in the composition of job changers rather than changes in pay for comparable workers. While some of the other papers discuss adverse selection, they do not estimate whether highly paid workers actually change jobs less often.

We find empirical support for each of the main predictions of our model and little concern about changing composition in the short run. 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 3.8% 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 (7.6%). 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 find that salary history bans have little effect on the composition of job changers with the exception highly paid nonwhite workers. We also find that salary history bans are not associated with a greater probability of job-switching and turnover, 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

[2] Also, they include salary history bans that cover public employees; identification may be further obscured because public employers likely do not behave the same as private ones. They also only use the aggregated data of the Quarterly Workforce Indicators examining the job changers.

control group, drawing conclusions on the effects on possibly disadvantaged groups by gender and race. Third, we also explore possible non-wage impacts of SHBs including the composition of the workforce, wage posting behavior, firm job advertising rates, and worker rates of changing jobs. We can rule out adverse selection of workers and we find no evidence of adverse employer reactions in the short run. It is possible, of course, that workers and firms may behave differently in the future when they have more experience with SHBs.

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 discuss the results conclude in Section 7.

2. Background

We seek to explore how SHBs might affect employer wage advertising and wage offers. One way that SHBs might affect these outcomes is through changes in wage bargaining. The advocates for SHBs contend that salary history information gives firms a bargaining advantage. Remove that advantage, and bargaining will result in higher pay for some workers. Also, if bargaining becomes relatively less profitable for employers, some will shift to posting wages instead. In the next section, we develop a formal model that encompasses these effects.

However, there are several other mechanisms that might affect posting and wages. First, salary history information collected before an interview might serve to screen job applicants. That is, salary histories might help employers to avoid interviewing applicants whose desired salary is too high. Without that information, some employers might switch to advertising wages rather than wasting time interviewing applicants who won't accept the job. Hence, an SHB might raise wage posting rates, which will also serve as a screen.

A second mechanism is adverse selection or statistical discrimination. Meli and Spindler (2019) contend that without salary histories, employers will base wages on average productivity (see also Greenwald 1986; Sran, Vetter, and Walsh 2020). For example, wage offers for female workers will be based on the average productivity of women. They argue that under an SHB, highly paid, highly productive women will be less likely to change jobs because they earn more than the average wage, job changers will earn lower pay, and job matches will be of lower quality.

A third effect arises if salary advertisements motivate disadvantaged workers to switch jobs. Disadvantaged workers might lack access to networks that communicate job openings. To the extent that networks might be important for highly skilled jobs, salary posting might encourage highly paid disadvantaged workers to change jobs, thus raising the pay of job changers.

Note that the institutional details of salary history bans pose some hurdles for the first two stories. While salary histories obtained in advance of an interview might help screen out applicants whose desired salary is too high, employers have a more direct way of ascertaining applicant's desired salary: by asking for it. Employers regularly do so and none of the bans on salary histories prevent employers from continuing to do so. Agan et al. (2020) surveyed 504 employed workers and found that most (66%) who had been asked to provide their salary information were also asked their desired salary.[3] Only 4.8% of workers were asked their current salary before the interview and were not asked about their desired salary. This makes it seem unlikely that SHBs would reduce the ability of many employers to screen applicants before the interview.

Second, none of the SHB laws prevent workers from *volunteering* their salary histories. There is no reason that highly paid workers under an SHB should be at any informational disadvantage that discourages them from seeking work. Nor is volunteered salary information any less credible or verifiable than information obtained at employer request—workers can voluntarily produce W-2 forms and voluntarily permit income verification agencies or employers to confirm their information. Agan et al. (2021) conduct an experiment with recruiters and find that highly paid applicants are not at a disadvantage when they voluntarily disclose their salaries.

[3] Our analysis of their graciously provided data.

In any case, these various stories each imply different outcomes that we test below. A decrease in bargaining advantage should increase wage posting rates and wages; a loss of screening should increase wage posting rates but implies no change in wages; adverse selection implies that highly paid workers should change jobs less often and the pay of job changers should decline; and greater pay information might encourage disadvantaged workers to change jobs more often.

A related literature looks at other ways information affects wage determination in different settings, regarding pay transparency (Mas 2014; Baker et al. 2019; Bennedsen et al. 2019; Cullen and Pakzad-Hurson 2019) and statistical discrimination (A. Agan and Starr 2018; Bartik and Nelson 2019; Doleac and Hansen 2020). 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 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 salary posting behavior across all major occupational categories. We abstract away from occupational differences by constructing a model for a single occupation.

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. Their key assumption is that workers have heterogeneous reservation wages that are a priori unknown to employers. When employers post wages, this gives rise to

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

monopsony wages.[5] 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 ith worker's reservation wage, z_i, is drawn from a distribution as

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

We assume that job seekers encounter employers randomly and one at a time. This makes ψ_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. In keeping with the short-run focus of this paper, we assume that these distributions are fixed; in a long-term model, we might want to treat these distributions dynamically. We further assume that employers do not know each worker's productivity, p_i, which they learn in the interview. Workers' expected productivity, $\bar{p}$, is common knowledge. Also, to keep things simple, worker productivity is not correlated with reservation wages. In the Appendix, we show that the basic results of the model regarding firm behavior hold when worker productivity is correlated with the reservation wage.

The employer first decides whether to post a wage or to advertise without listing a wage, leading to bargaining. First, consider wage posting, by which we mean advertising the wage.[6]

[5] Unobserved worker heterogeneity is a common element of what Alan Manning (2021) calls "New Classical Monopsony" models such as Card el al. (2018).

[6] We treat advertised wages as if they were take-it-or-leave-it offers. Obviously, some bargaining might take place when wages are advertised, especially if a range of salaries is indicated. However, the stylized treatment here reflects that bargaining after a salary has been advertised is more constrained than in the case where no salary information is advertised.

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, conditional on being hired, is then

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

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

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

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, $z_i \leq p_i$, they bargain. The negotiation can be modeled as a sequential bargaining process where the parties split the surplus, $p_i - z_i$, with $\gamma(p_i - z_i)$ going to the employer and $(1 - \gamma)(p_i - z_i)$ 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). We assume that the salary history reveals the reservation wage, albeit with some possible noise.[7] 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, γ, is less than it would be under

[7] In a more complicated model with a distribution of reservation wages conditional on the salary history, risk-neutral employers would take the worker's bargaining threat as the expected reservation wage with, perhaps, an upward adjustment. In our stylized treatment, we treat the salary history as fully revealing the reservation wage to avoid this complication. The general result—that salary history information provides a bargaining advantage—remains.

complete information, $0 < \gamma_{SHB} < \gamma_{noSHB} \leq 1$.[8] The ith worker's wage, conditional on a bargain being concluded, is then

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

The applicant and employer will only reach a bargain if $z_i \leq p_i$, so the ex-ante expected reservation wage conditional on a deal is

$$\bar{z}_b \equiv E[z|z_i \leq p_i] = \frac{\psi}{\psi+1}\bar{p} = w_p$$

The average wage, conditional on employment, is then

$$\bar{w}_b = \bar{z}_b + (1-\gamma_k)(\bar{p} - \bar{z}_b) = w_p + (1-\gamma_k)(\bar{p} - w_p). \tag{6}$$

Note that this wage is generally higher than the posted wage. The ex-ante expected profit is

$$\pi_b = \gamma_k R(\bar{p})(\bar{p} - \bar{z}_b) = \gamma_k \frac{w_p^{\psi+1}(\psi+1)^{\psi}}{\psi^{\psi+1}} \tag{7}$$

Comparing (4) and (7), 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 ψ_j, the firm will choose bargaining when

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

otherwise, the firm will post the wage. A solution will exist for $\gamma > \frac{1}{e}$. Thus, as long as γ 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.[9] The intuition for this result comes from the basic tradeoff between posting and bargaining: employers obtain lower wages when they post but only

[8] For simplicity, and without loss of significant generality, we let γ_{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.

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

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.[10] 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 A8). 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 2020; Azar et al. 2020).[11] 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 A8), 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.

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

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

b) Bargain before, post after SHB. In this range, $\psi^*(\gamma_{SHB}) > \psi_j \geq \psi^*(\gamma_{noSHB})$, equation (6) implies that the average wage falls by $(1 - \gamma_{noSHB})(\bar{p} - w_p)$. 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 $(\gamma_{noSHB} - \gamma_{SHB})(\bar{p} - w_p)$.

From this setup, we can draw several implications for our empirical analysis about what happens when an SHB decreases γ. 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 ψ^*, equation (8) means that a decrease in γ implies an increase in ψ^*. 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 γ_{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 $\Delta w_{bi} = -(p_i - z_i) \cdot \Delta\gamma$. 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. 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 overall and only a small shift for nonwhite workers.

Also, the model assumes that employers learn each worker's 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. Further research will be needed to assess long-term outcomes.

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.[12] 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). More recent analysis by Burning Glass shows that their coverage of all job openings has improved, with roughly 85% of all openings posted online.[13] Because BG skews towards educated and white-collar occupations and jobs, we use occupational weights derived from CPS to make our sample more representative. BG data includes the

[12] For a detailed discussion of the representativeness of job posting data, see the appendix of Hershbein and Kahn (2018)
[13] See https://www.burning-glass.com/about/faq/ for more details.

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.[14]

Our BG sample spans from January 2010 to July 2019. 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. Table A2 displays summary statistics for these discarded advertisements. In total, about 41 million postings meet these criteria. [15]

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 February 2020.[16] In addition to a range of worker characteristics,[17] wage, weekly earnings[18], 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

[14] 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.

[15] 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.

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

[17] 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.

[18] 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.

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.[19]

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 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.[20] A county is more similar to its cross-state counterpart in the same commuting zone than to 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

[19] 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.

[20] 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).

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).

Some differences may remain between adjacent counties over state borders. We control for county fixed effects and also for time-varying differences in the minimum wage. We also use a triple differences design, comparing job changers to incumbent workers in both treatment and control groups. This eliminates time-varying state differences that affect all workers. In the Appendix, we find our results robust to other control group definitions, including using synthetic controls with algorithmically defined weights.[21]

This is a conservative approach that might understate the measured treatment effects because labor market competition might cause control group firms to post wages or raise offers to women more often, diminishing the difference between the treatment and control groups. To the extent there are such spillover effects, our results will be biased downward.

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 A8, 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 control group choices generate similar point estimates across choices of control group.

5. Results

5.1. Salary posting

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

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

[21] We use California, one of the earliest and largest states that implemented SHB as the treated state for synthetic control. The alternative control groups also address contamination concerns of adjacent counties within the same labor market affecting each other.

where P_{ist} is 1 if ad i lists salary in state s at time t, and 0 otherwise. α and β are state and time fixed effects, X_{ist} is a vector of controls,[22] and ϵ_{ist} is the error term. γ 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, τ_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.[23] 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.[24] The rate of posting increases sharply the quarter after the ban goes into effect. There are no significant pre-event trends, although perhaps a slight negative anticipation effect can be seen the quarter before the ban. 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 salary 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 salary posting after the salary history bans were *enacted* but

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

[23] 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.

[24] The coefficients $\gamma_{t-\tau}$are obtained from regressing a dummy variable for posting, P, $P_{ist} = \alpha_s + \beta_t + \sum_{\substack{t-\tau \\ t-\tau\neq -1}} \gamma_{t-\tau} \cdot \mathbf{1}(t \geq \tau_s) \cdot \mathbf{1}(s \in T) + \delta X_{ist} + \epsilon_{ist}$ where τ_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.

before they came into *effect*.[25] 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, along with graphical evidence of no pre-trends, address concerns about policy endogeneity and spillovers from public sector SHBs.[26]

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 stem from our attempt to measure the direct effect of the SHB on salary 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 posting behavior in response to competitors across state boundaries who are subject to it. Firms have been shown to adjust their online advertising after other firms' decisions to change their minimum wage (Derenoncourt, Noelke, and Weil 2021). 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 salary 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).[27] While employers may change their behavior in terms of posting wages, it does not appear that they adjust the characteristics of posted wages.

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

[26] 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.

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

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 a regression on log salary for our sample of matched counties, Column (1) of Table III finds evidence of this. The difference in the coefficient of the state posting rate between treated job changers and treated incumbent workers is a highly significant .240.[28]

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.[29]

It is possible that SHB states tended to have some other factor that affected earnings. While we control for minimum wage changes, we can control for other possible confounding 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

[28] All regressions include controls for experience, experience squared, education, union coverage, minimum wage, marital status x gender, child in household, industry, county, occupation, month and year.
[29] The probability value of a t-test is .035.

and treated workers.[30] Identification of these estimates assumes that the composition of job changers is similar before and after the SHB to rule out selection bias. Below we test for compositional changes on a range of observables including residual wages. We find only minor differences suggesting that substantial selection bias does not affect our salary treatment effect estimates.

This regression is run on the full outgoing rotation group sample and is shown in Column 2. The estimated treatment effect is similar (3.2%) and there does seem to be a significant effect among incumbent workers.[31] If we subtract the coefficient on incumbents from the coefficient on job changer, we obtain a *net* effect of 3.9% 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. Here there is a small effect for incumbent workers, 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.

The above estimates stand up to a variety of robustness checks (see Appendix). To correct for possible state-specific trends, Table 3, 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 A5 reports estimates of Table III using Coarsened Exact Matching to balance the characteristics of the treatment and control groups.

These changing effects over time raises questions about heterogeneity in treatment effects. Along with a staggered rollout of treatment, we follow recent developments in the

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[30] 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.

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

difference-in-differences literature and estimate each event independently and present a stacked estimation, following Cengiz et al (2019) and Chaisemartin and D'Haultfœuille (2020). These results are reported in tables A6 and A7.

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 A9 in the Appendix, we find an SHB treatment effect of 3.0% for job changers, quite similar to our estimates from CPS data.

Our estimates, while economically and statistically significant, are not large compared to other studies. Barach and Horton (2020) conduct a field experiment and find an even greater difference, 9%, when salary history is suppressed. Baker et al. (2019) find that the gender pay gap decreased by 30% when salaries were disclosed. Although this is not directly comparable, it shows that information about pay can have large effects on disparities.

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 8.3% more under an SHB. SHBs also have a weakly 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 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 10.8% increase in wages. But there is a significant 3.2% 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 7.6% increase for non-white workers. Column (4) repeats the exercise, but only for male workers. Non-white male job changers experience an 9.4% increase in wages relative to white male job changers with a 6.2% 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.[32]

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 A13), we conduct power tests finding sufficient statistical power for the sub-samples

[32] 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.

analyzed in Table IV.[33] In addition, Table A9 shows results using the QWI which generate similar effect sizes for women and Black workers (compared to non-white workers). These results might also reflect changes in the composition of job changers, which we explore next.

5.4. Changes in the Composition of Job Changers

Table V shows changes on the extensive margin. These are important for two reasons. First, our estimates may reflect changes in the composition of the workforce rather than changes in what employers pay for an employee with given characteristics. Second, these changes help evaluate the role of screening, adverse selection, the quality of job matches, and effects of changes in information about job openings.

The top panel compares the composition of treated and untreated workers who changed jobs during the previous three months, showing the means, their difference (and standard error). The significance of the differences is measured with t-tests. The Mincer residual is calculated for the year-earlier salary (fourth month in survey for observations in the eighth month).[34] With the exception of an increase in the share of job changers who are nonwhite, the compositions of the treated and control groups are not significantly different.

The second panel performs a difference-in-differences estimation to measure the treatment effect of SHBs on the probability that a worker will be a job changer. This analysis is similar, but controls for a wide range of observables. The dependent variable in these regressions is 1 if the worker changed jobs, 0 otherwise, and they include controls for experience, experience squared, union membership, part time status, marital status x gender, motherhood, county, occupation, industry, education, month and year. The treatment dummy is interacted with a dummy for each group. SHBs do not appear to raise the likelihood of job-changing except for a small increase for nonwhite workers.

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

[34] 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 third panel interacts the treatment effect with a dummy variable indicating whether the worker's year-earlier Mincer residual was above- or below-median. It seems that highly paid nonwhite workers are more likely to change jobs, although the effect is not large.

Finally, the bottom panel performs a difference-in-differences estimation where the dependent variable is 1 if the worker was not in the labor force a year earlier (and not disabled or retired), and 0 otherwise. It appears that more experienced workers were more likely to be drawn into the labor force under an SHB, but no significant changes for other groups.

These results suggest that our estimates of wage changes are largely robust to concerns about changes in the composition of job changers; they reflect changes in pay rather than changes in who is switching jobs. Our estimate of the SHB treatment effect for nonwhite workers is likely biased upwards, however, the bias would appear to be small.

6. Discussion

Our results are consistent with our model of bargaining advantage: with an SHB, firms advertise salaries more, they pay higher wages job changers for bargained jobs, and the increase is greater for disadvantaged groups. These results do not necessarily rule out the role of screening or recruitment. An SHB might prompt some firms to advertise salaries to weed out applicants whose salary expectations are too high. But this behavior doesn't explain the substantial increase in pay we find for job changers, suggesting most of the increase in advertising under SHBs might arise from a loss of bargaining advantage.

The increase in advertised salaries might provide information that prompts disadvantaged workers to switch jobs. There is some evidence that advertised salaries might encourage highly paid nonwhite workers to switch jobs or for experienced workers to re-enter the labor force. However, the effects are not large, and they are not at odds with the bargaining advantage model.

But our evidence is hard to square with accounts of adverse selection or statistical discrimination. Consistent with institutional details noted above that make adverse selection seem irrelevant in this setting, women and nonwhites are not less likely to change jobs under an

SHB, including specifically highly paid women and nonwhite. Nor do these groups experience lower pay; rather, the opposite seems to be the case.

Even without adverse selection, SHBs might generate poor job matches if salary histories provide employers with information about worker skills and productivity. Of course, employers gain information about worker productivity in other ways. To the extent that SHBs reduce information from salary histories, employers might seek additional information under an SHB. We found some evidence of this. We tested whether SHBs are associated with higher skill requirements listed in the ads (see Appendix Table A9). 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.

If SHBs produced significantly lower quality job matches despite these adjustments, then we would expect higher turnover of workers and possibly lower productivity under SHBs. Neither effect seems to occur. The first column of Table V shows that under an SHB workers are slightly less likely to switch jobs, although the effect is not statistically significant. As a robustness check we also tested employee turnover using the QWI (see Appendix Table A12), 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 A3, 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. To the extent that SHBs reduce information about worker productivity, these findings suggest that employers are able to substitute other information so that there is no significant increase in turnover or decrease in productivity.

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 or reduce their demand for labor generally. Table A4 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. Our results are most consistent with an explanation based on bargaining advantage. It is possible, perhaps even likely, that more significant changes on the extensive margins might emerge over time—our time window post-SHB is short. Nevertheless, our results suggest that SHBs significantly affect bargaining differences.

7. 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. 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.

This breadth of employer use of salary history information helps explain why salary history bans—a seemingly modest restriction on firm practices—nevertheless appear to have a significant impact on pay. The pay of job-changing workers subject to SHBs rose 3.8% on average compared to equivalent job changers in other states. The effects are even larger for groups subject to historical discrimination. Following SHBs, the pay of job-changing women rose about 6.4% and the pay of job-changing non-whites rose about 7.6% 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 (Blau and Kahn 2017). Our analysis suggests that around half of the residual wage gap for job-changing women disappears under an SHB, implying that 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. Even if employers do not individually discriminate, the use of salary histories appears to perpetuate the effects of past discrimination or other group inequities.

As a policy directed to address pay inequities, salary history bans appear to have had a temporary 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 might 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.

8. References

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9. 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. *** p<0.01, ** p<0.05, * p<0.10. 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	0.065			
	(0.125)			
Job Changer x State Posting Rate	0.305**			
	(0.136)			
Incumbent x Post-SHB		-0.007	0.004	0.004
		(0.008)	(0.009)	(0.009)
Job Changer x Post-SHB		0.032***	0.045***	0.047***
		(0.009)	(0.012)	(0.011)
Baseline				
Job Changer	-0.056***	-0.033***	-0.033***	-0.031***
	(0.008)	(0.003)	(0.006)	(0.005)
Observations	340,936	1,041,923	366,945	358,963
R-squared	0.554	0.547	0.553	0.508
Net effect for job changers	0.240***	0.039***	0.041***	0.042***
	(0.049)	(0.006)	(0.008)	(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. *** p<0.01, ** p<0.05, * p<0.10. This sample includes private sector employed workers in control and treatment groups. All regressions include controls for experience, experience squared, education, union coverage, minimum wage, 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	-0.006	-0.005
	(0.009)	(0.007)
Male **x** Job Changer **x** Post-SHB	0.015	0.033
	(0.013)	(0.024)
Female **x** Incumbent **x** Post-SHB	0.017*	-0.012
	(0.009)	(0.007)
Female **x** Job Changer **x** Post-SHB	0.079***	0.014
	(0.014)	(0.024)
Baseline effect (gap)		
Female **x** Job Changer	-0.146***	-0.154***
	(0.009)	(0.012)
Observations	366,945	186,846
R-squared	0.555	0.563
Net Effect for Female Job Changers	0.0624***	0.0257
	(0.011)	(0.024)
Net Effect / Gap for Job Changers	43%	17%
Panel B: White / Non-White		
	(3)	(4)
Sample:	All	Males Only
Treatment effects		
White **x** Incumbent **x** Post-SHB	0.001	-0.006
	(0.010)	(0.009)
White **x** Job Changer **x** Post-SHB	0.033*	0.005
	(0.017)	(0.019)
Non-White **x** Incumbent **x** Post-SHB	0.021**	0.020*
	(0.009)	(0.011)
Non-White **x** Job Changer **x** Post SHB	0.079***	0.063*
	(0.025)	(0.035)
Baseline effect (gap)		
Non-White x Job Changer	-0.087***	-0.104***
	(0.012)	(0.023)
Observations	366,945	185,652
R-squared	0.554	0.548
Net Effect for Non-White Job Changers	0.059***	0.043
	(0.022)	(0.031)
Net Effect / Gap for Job Changers	68%	41%

Note: This table shows the DDD results of SHBs on annual earnings by group. Errors are clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10. 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, minimum wage, 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 the Composition of Job Changers

	(1)	(2)	(3)	(4)	(5)	(6)
				Above median		
	All	Female	Nonwhite	Education	Experience	Mincer residual
A. Composition of job changers						
Untreated		0.487	0.187	0.473	0.494	0.490
Treated		0.482	0.233	0.482	0.486	0.512
Difference		-0.005	0.046***	0.009	-0.008	0.021
		(0.012)	(0.009)	(0.012)	(0.012)	(0.020)
B. Treatment effect, probability worker changed jobs						
Treated	-0.002	-0.000	0.005**	0.000	-0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Observations	337,700	337,700	337,700	337,700	337,700	95,766
R-squared	0.005	0.005	0.005	0.005	0.005	0.009
C. Treatment effect, probability worker changed jobs x above/below median Mincer residual						
Treated, low residual	-0.002	-0.001	0.004	-0.003	-0.003	
	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	
Treated, high residual	-0.001	-0.001	0.012***	0.002	0.003	
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	
Observations	105,686	105,686	105,686	105,686	105,686	
R-squared	0.009	0.009	0.009	0.009	0.009	
D. Treatment effect, probability worker entered labor force						
Treated	0.0009	0.0024	0.0011	-0.0033	0.0067**	
	(0.0018)	(0.0033)	(0.0032)	(0.0032)	(0.0025)	
Observations	97,868	97,868	97,868	97,868	97,868	
R-squared	0.1006	0.1006	0.1006	0.1007	0.1007	

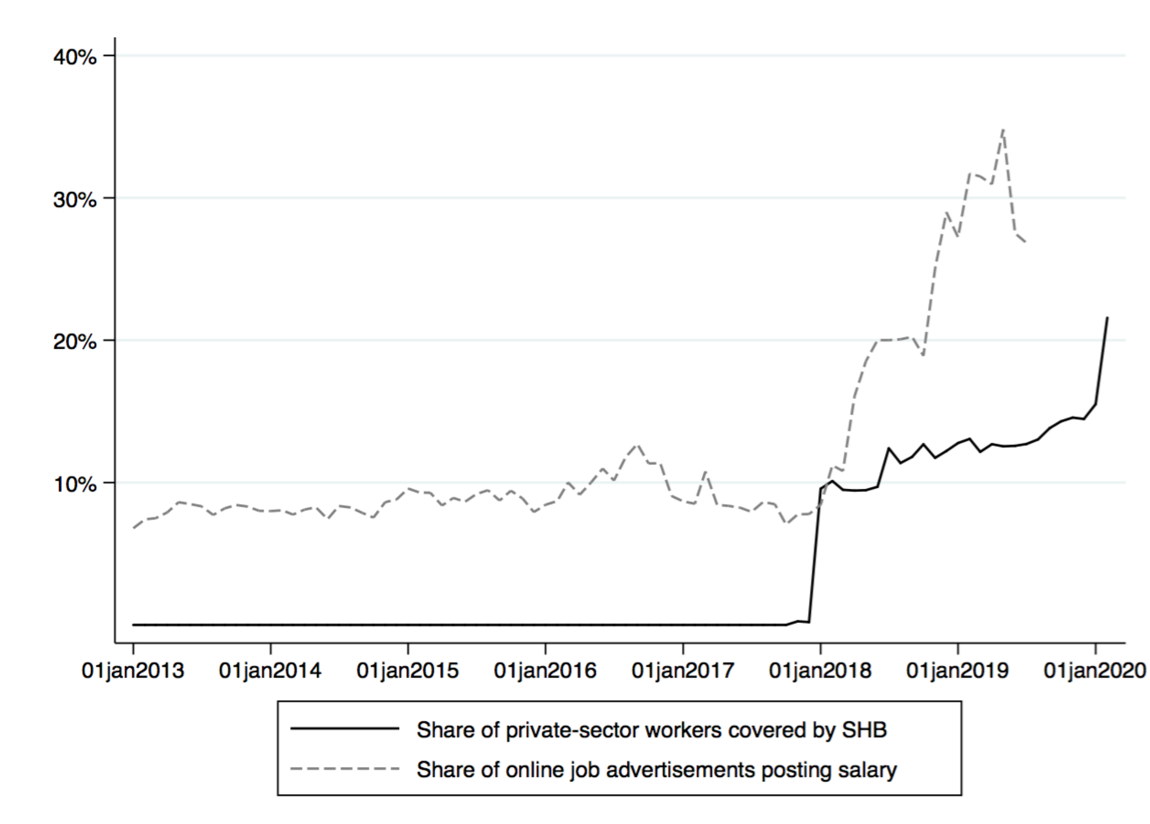
Note: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10. Regression errors are clustered by state. Top panel shows means of binary variables for treated and non-treated observations, and the difference in means. Significance is measured with t-test. Non-white is defined as any respondent who does not identify as white in the CPS. The Mincer residual is from a regression for year-earlier log salary on experience, experience squared, union membership, part-time status, married x female dummies, county, occupation, industry, education, month, and year. The second panel shows the probability that a treated worker is a job changer. Job changers are determined by those in outgoing rotation groups who report that they changed employers in the previous 3 months. The dependent variable is 1 if the worker is at a new employer and 0 otherwise and regressions include controls for experience, experience squared, education, union coverage, marital status x gender, child in household, industry, county, occupation, month and year. The treatment dummy is multiplied by the group dummy variable. Panel C repeats the regression but interacts the treatment dummy with both the group dummy and a dummy for whether the Mincer residual is above median or not. Panel D repeats Panel B with a dependent variable that is 1 if the worker was not in the labor force and not disabled of retired on year earlier. Data drawn from the Current Population Survey. Sample includes private-sector employed workers in control and treatment groups.

10. Figures

Figure I

Coverage of Salary History Bans and Online Salary Posting

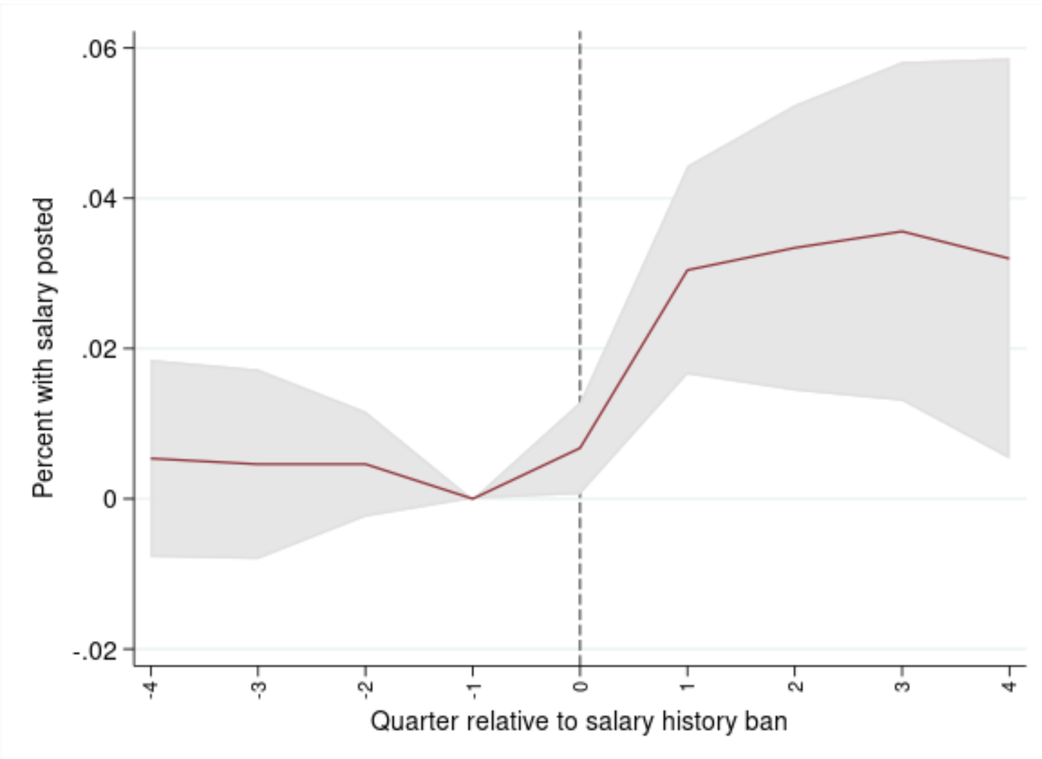
Source: Current Population Survey; Burning Glass



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. Standard errors are clustered by state. 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. Standard errors are clustered by state. 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.

11. Appendix: Robustness checks and discussion

11.1. State variation

One concern is that different states may have experienced heterogeneous effects from SHBs and our overall results might unduly reflect a few states. California accounts for a large share of treated observations. Table A3 explores the possibility of state-specific time trends (column 1), effects of different cohorts (column 2), and the effect of SHBs on state output per worker (column 3). These are discussed further in the text.

11.2. Control Group

Another concern is that our results might be sensitive to the choice of control group. In this section we explore alternative control groups. The main text uses a difference-in-differences design with a control group defined by adjacent counties within common labor market areas. This control group is a conservative choice because there might be common labor market effects. For instance, if treated firms offer higher wages to women, then competing firms in the same labor market might also be driven to raise their wage offers. The estimated treatment effect will then be smaller. Similarly, wage posting behavior might "spill over" to other firms within the labor market or as human resource professionals voluntarily avoid using salary histories. To explore the significance of alternative control groups, we use the method of synthetic controls where weights are algorithmically assigned to control units (Abadie, Diamond, and Hainmueller 2010). We perform an analysis for wage posting rates for California, the first large state to implement an SHB. We aggregate posting rates to the state and quarter and use the pre-event posting rates for the donor states as controls, with weights assigned algorithmically. Figure A3 shows two examples, the first using all non-treated states as donors, the second using the five states with the lowest population density as donors (Alaska, Montana, North Dakota, South Dakota, and Wyoming). While the DID analysis estimated a treatment effect of 3%, the first synthetic control has an effect of about 7%, and the second of about 18%. These alternative controls suggest that SHBs may have a large indirect effect.

We further examine the sensitivity of our results by balancing the pre-treatment covariates between the treatment and control group using the Coarsened Exact Matching (CEM) design.[35] With the weights calculated from CEM, we implement the same differencing methods for outcomes in Table III. Table A5 indicates that the original results hold true after the CEM with very similar coefficients.[36]

With the CPS data, another problem arises because the BLS suppresses county codes for some portion of the sample. This means that we cannot positively identify all observations that should be in the control group. In the paper we use only those observations that are positively identified as such. We do not have a similar problem with treatment group observations because we can assign them based on the state rather than the county for the most part. Table A8 explores the sensitivity of our estimated treatment effect to alternative ways of assigning control group observations. Column 1 shows the strict assignment used in the paper. Column 2 adds other observations in the adjacent state that do not report county codes. Column 3 adds all observations in adjacent states while column 4 adds all non-treated observations. As the control group expands, the estimated treatment effect grows progressively larger, suggesting that our preferred approach is conservative.

A final concern would be the possible worker flows of individuals residing in a treated or control unit but traveling to an adjacent counterpart for work. This would induce contamination between the treatment and control group constructed. We test for it using the ACS data, where both place of work and place of residence information are available. We construct the ratio of individuals working in the adjacent FIPS unit that belongs to the opposite group to the number of individuals in their original place of residence. At both county and state level, the majority of the ratios are less than 1%. The top three states with the highest (and over 1%) worker mobility rates are Maryland (27.1%), Arizona (7.2%) and Massachusetts (3.7%). After dropping the states and

[35] In summary statistics, the balances between the treatment and control in race and disabled are not ideal.

[36] With CEM, we included as much covariates as possible. After matching on experience, education, union coverage, part-time, marital status, gender, non-white, labor market tightness, and having a child, the imbalance measure improved from 0.71 to 0.08.

their counterpart states, the results on both employer hiring and actual pay remain essentially unchanged, with differences only at the third or fourth decimal of the coefficients.

11.3. Additional Dataset — Quarterly Workforce Indicators

To compensate for the sample size concern using CPS, we also performed pay and worker turnover analysis with the Quarterly Workforce Indicators (QWI). QWI provides information on average monthly earnings of the stable job changers, the rate of turnover by quarter, number of stable job changers up until the 3rd Quarter of 2019. It allows selection based on firm characteristics such as industries, and worker characteristics such as gender and race. Therefore, with limited controls,[37] we are able to replicate the analysis at the industry-county-year-quarter level. Table A9 shows that for monthly earnings, SHBs increased the earnings of job changers by 3%. Female and Black job changers had a 6.9% and 5.4% gain separately. And Hispanic job changers saw a 6% decrease. Results on the general effect and by gender with QWI is comparable with that from CPS. We see smaller effects with Black and Hispanic groups combined, which could be caused by the difference of racial group categorization in QWI and CPS, or the suppression of QWI.[38] For worker turnover, there is no significant effect or close to zero effect in general and by group, which is similar to the results we have with CPS.

11.4. Power Test

Sample sizes raise a concern about estimating treatment effects for different demographic groups. We address some of these concerns using the QWI data which has a much larger sample than the CPS. We also conducted power tests to determine the statistical power of tests of sample

[37] Information regarding worker's experience, part-time working status, union, marital status are not available in QWI.

[38] QWI provided 6 racial groups: White Alone, Black or African American Alone, American Indian or Alaska Native Alone, Asian Alone, Native Hawaiian or Other Pacific Islander Alone, and Two or More Race Groups. The other difference is that earnings measured in QWI is representative of both full and part-time work. Thus, we cannot separate out part-time effect. The third point to notice is that data in QWI could be severely suppressed due to confidentiality protection by the Census publication standards. For example, in our overall sample, above 10% of earning_info is suppressed. About 20% of turnover info is suppressed. In our by-gender and by age group (to filter out the above 65 age group) subsample, 35%-40% of the earning_info is suppressed, and around half of the turnover info is suppressed. The suppression rate may be higher with finer selection criteria in race, ethnicity etc., given that we are using the county-industry-level variation. So, we may expect selection bias to some extent with geographic area by industry cells that have small population size underrepresented. If the suppressed groups are composed of more disadvantaged individuals with lower wage, then QWI will provide a lower bound estimation. And vice versa.

means for different groups. Using our CPS sample of county pairs, we compared the mean log annual earnings for job changers treated by an SHB and those not treated. The null hypothesis is that the two samples have the same mean. Table A13 below shows the probability values of t-tests of these mean differences as well as the statistical power of a .05 significance test and the total number of treated workers. The power is the probability that the comparison of means will correctly reject the null hypothesis (1 minus the rate of false negatives).

These tests show good statistical power for tests of female, male, white, and non-white job changers, but low power for tests involving Black workers and Hispanics. Sample size appears sufficient for the analyses shown in Table IV.

11.5. Voluntarily Disclosure and Adverse Selection

Salary history bans are inquiry bans, not sharing bans. It is only on the employer side that discussion of a salary history is banned and, depending on the state, they can ask once an initial offer has been made. An additional loophole is that there is no ban in asking about desired salary, which is likely to be a decent proxy of past salary. As noted above, Agan et al. (2020) lay out a theoretical framework for instances in which workers comply with the policy. As such, any results in terms of evaluating changes in worker salary can be thought of as an intent to treat effect. However, this any non-compliance phenomenon is not easily traced to changes in employer behavior, at least not in the short term.

One of the key reasons salary history bans cannot be evaluated in the longer term is the danger for adverse selection and unraveling. As noted, these laws all allow job applicants to reveal their salary history if they wish. Those with advantageous salary histories, especially those who belong to a demographic with poor salary histories on average, will reveal their salary history to improve their bargaining position. This could lead to an unraveling in which more and more people reveal their salary to avoid being assumed to have a "bad" salary history, potentially undercutting the policy in the long run. For more on the adverse selection in the context of salary history, see Agan, Cowgill, and Gee (2020).

11.6. Reservation wage correlated with productivity

Suppose each worker's productivity is correlated with their reservation wage:

$$p_i = \underline{p} + \rho z_i + \epsilon_i, \quad \rho < 1$$

so that

$$E[p \mid z] = \underline{p} + \rho z_i$$

$$E[p \mid z < w] = \underline{p} + \rho E[z \mid z < w] = \underline{p} + \rho \frac{\psi w}{\psi + 1}.$$

Expected profits from salary posting are then

$$E[\pi_{post}] = (E[p \mid z < w] - w) R(w) = \left(\underline{p} - \left(1 - \rho \frac{\psi}{\psi + 1}\right) w\right) R(w)$$

Solving the profit maximizing condition:

$$\widehat{w}_p = \frac{\psi \underline{p}}{1 + (1 - \rho)\psi}$$

$$\hat{\pi} = \frac{\underline{p}^{\psi+1} \psi^{\psi}}{(\psi + 1)\,(\psi(1 - \rho) + 1)^{\psi}} = \frac{w_p^{\psi+1}}{\psi} \frac{(\psi(1 - \rho) + 1)}{\psi + 1}$$

Comparing these expressions to (3) and (4), the wage posted is higher and expected profits lower when $\rho > 0$. Employers who do not advertise salaries learn both the reservation wage and productivity during bargaining, so equation (7) still holds. This means that the value of $\psi^*(\gamma)$ changes, but the nature of the solution zones and changes that occur with SHBs are of the same form.

12. Appendix Tables

Table A1. Summary Statistics[39]

Variable	Mean (Standard Deviation)		
Panel A: Burning Glass Data, Year: 2010-2018			
	Overall	**Treated**	**Control**
Salary Posting (=1)	0.093	0.094	0.093
Log Annual Salary	10.845	10.953	10.802
	(0.558)	(0.561)	(0.551)
Salary-range Posting (=1)	0.062	0.065	0.061
Labor market tightness	0.739	0.665	0.787
	(0.377)	(0.343)	(0.391)
Education	*Share*		
=16	0.505	0.555	0.473
=12	0.304	0.259	0.333
=14	0.088	0.072	0.098
=0	0.048	0.049	0.047
=18	0.043	0.049	0.039
=21	0.012	0.015	0.010
Experience	3.611	3.813	3.477
	(2.826)	(2.867)	(2.790)
Top Industries	*Share*		
Health Care	0.202	0.179	0.211
Finance and Insurance	0.133	0.140	0.131
Professional, Scientific and Technical Services	0.122	0.124	0.122
Educational Services	0.073	0.082	0.069
Manufacturing	0.072	0.078	0.069
Retail Trade	0.068	0.059	0.071
Top Occupations	*Share*		
Management	0.191	0.212	0.177
Computer and Math	0.148	0.161	0.139
Sales	0.124	0.117	0.128
Healthcare Practice	0.106	0.100	0.116
Office and Admin	0.097	.092	0.099
Business and Finance	0.094	0.090	0.090
Top States	*Share*		
California	0.130		
Texas	0.085	n/a	n/a
New York	0.058		
Florida	0.052		
Illinois	0.045		
N	40,891,481	16,208,829	24,682,652
Panel B: Current Population Survey, Year: 2013-2020			

[39] Our treatment group includes 14 states: Alabama, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Maine, Massachusetts, New Jersey, New York, Vermont, Oregon, and Washington.

	Overall	Treated	Control
Log Annual Earnings	10.551	10.551	10.549
	(0.852)	(0.848)	(0.875)
Log Hourly Wage	2.989	2.989	2.984
	(0.614)	(0.614)	(0.611)
Employed with New Firm (=1)	*Share*		
	0.046	0.045	0.051
Part time status (=1)	0.183	0.182	0.186
Education	*Share*		
Bachelor	0.250	0.248	0.265
High School or Equivalent	0.245	0.246	0.246
Some College	0.172	0.173	0.163
Master’s Degree	0.112	0.112	0.114
Associate Degree	0.060	0.059	0.061
Experience	20.494	20.534	20.214
	(13041)	(13.030)	(13.117)
Union (=1)	0.148	0.154	0.104
Female (=1)	0.493	0.492	0.494
Black (=1)	0.088	0.090	0.076
Non-white (=1)	0.202	0.210	0.140
Hispanic (=1)	0.187	0.198	0.114
Married (=1)	0.533	0.532	0.536
Disabled (=1)	0.028	0.027	0.033
Top Industries	*Share*		
Health Care	0.153	0.153	0.151
Educational Services	0.108	0.108	0.104
Accommodation and Food Services	0.080	0.075	0.082
Retail Trade	0.080	0.080	0.080
Professional, Scientific and Technical Services	0.070	0.081	0.086
Construction	0.061	0.057	0.064
Top Occupations	*Share*		
Office and Admin Support	0.123	0.123	0.121
Management	0.108	0.107	0.122
Sales and Related	0.101	0.101	0.103
Education, Training and Library	0.070	0.071	0.067
Transportation	0.061	0.062	0.053
Healthcare Practitioner	0.063	0.063	0.065
Food Prep and Servicing	0.060	0.060	0.061
Production	0.054	0.055	0.050
Construction	0.045	0.044	0.050
Top States	*Share*		
California	0.259		
New York	0.126	n/a	n/a
Illinois	0.094		
New Jersey	0.065		
Massachusetts	0.064		
N	337,700	295,301	42,399

Note: This table shows the summary statistics of the key dependent variables and control variables. Panel A presents the Burning Glass job board summary statistics for years 2013 to 2018. Panel B presents the CPS summary statistics for years 2013 to 2020. Sample includes private-sector employed workers in control and treatment groups and excludes observations with missing key variables.

Table A2. Dropped Advertisements

		Means/(sd)			
Filtering Variable	**Dropped (missing or == 1)**	**Edu**	**Exp**	**Ln(sal)**	**Salary Post**
Firm ID	62,786,608	12.921	3.5370	10.748	0.251
		(5.411)	(2.699)	(0.630)	(0.434)
Public Sector	4,779,863	13.400	2.767	10.903	0.521
		(5.414)	(2.463)	(0.517)	(0.500)
Internship	2,016,617	12.408	2.014	10.470	0.117
		(6.161)	(2.013)	(0.498)	(0.322)
Part Time	919,732	9.231	1.787	10.387	0.257
		(6.396)	(1.812)	(0.496)	(0.437)
All Filters	69,317,816	12.862	3.442	10.762	0.265
		(5.513)	(2.683)	(0.620)	(0.441)

Note: This table shows summary statistics for the dropped advertisements from the Burning Glass data that do not meet our criteria.

Table A3. State Variation

	(1)	(2)
Model	State x year FEs	Labor productivity
Dependent variable	Log annual earnings	Annual State GDP/worker
Post-SHB		-0.001
		(0.002)
Incumbent x Post-SHB	0.003	
	(0.008)	
Job changer x Post-SHB	0.043***	
	(0.011)	
Job changer	-0.033***	
	(0.007)	
Observations	337,699	459
R-squared	0.553	0.983

Note: This table shows the DDD results of the SHBs on log annual earnings and the DD result on the labor productivity. Errors are clustered by state in parentheses. Data are from the Current Population Survey. Sample includes private-sector employed workers in control and treatment groups. *** p<0.01, ** p<0.05, * p<0.10. Column 1 includes controls for experience, experience squared, education, union coverage, marital status, child in household, state minimum wage, industry, county, occupation, month and year. Column 2 includes year and state fixed effects. Column (1) uses the full outgoing rotation groups, allowing for separate year dummies for each state. Column (2) uses a sample of all states (and DC) from 2010 through 2019.

Table A4: Switching State in Salary Posting

Dependent variable: Log number of online help-wanted ads for each state-month

	(1) Multi-state firms	(2) All firms
SHB treatment	0.002	0.012
	(0.080)	(0.135)
Observations	5,865	11,729
R-squared	0.977	0.379

Note: This table shows the DD results of SHBs on overall number of help-wanted ads placed. Standard errors are clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.10. Data are from the Burning Glass online salary posting microdata. All regressions have state and month fixed effects. Sample includes state-month level observations summing help-wanted ads for private firms, excluding ads for interns, part-time jobs, and jobs without firm names.

Table A5. Coarsened Exact Matching

VARIABLES	(1) Annual Earnings (County Pairs)	(2) Annual Earnings (Full Sample)	(3) Annual Earnings (County Pairs)	(4) Hourly Wage (County Pairs)
Incumbent x State Posting Rate	0.239*			
	(0.135)			
Job Changer x State Posting Rate	0.487***			
	(0.135)			
Job Changer	-0.057***	-0.032***	-0.033***	-0.029***
	(0.008)	(0.004)	(0.007)	(0.006)
Incumbent x Post-SHB		0.044***	0.025**	0.028**
		(0.009)	(0.012)	(0.012)
Job Changer x Post-SHB		0.080***	0.064***	0.063***
		(0.010)	(0.013)	(0.016)
Observations	309,870	1,041,523	333,603	326,293
R-squared	0.553	0.545	0.553	0.508
Net effect	0.248	0.037	0.039	0.034
Standard Error	0.048	0.006	0.007	0.005
P-Value	0.000	0.000	0.000	0.000

Note: This table repeats the analysis of Table III using Coarsened Exact Matching. 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 job changers' and incumbents' wage in the treated states. The dataset is from the Current Population Survey. *** $p<0.01$, ** $p<0.05$, * $p<0.10$ Errors are clustered by state in parentheses. 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. Column (1) measures the correlation of annual earnings and the state monthly share of job ads that list pay; column (2) is the basic regression for job changers; column (3) includes incumbent workers and also dummy variables for job changers and incumbents; column 4 uses log hourly wages as the dependent variable; column (5) uses the change in wages from month 4 to month 8 in the survey; column (6) repeats the regression of column (2) but adds a placebo dummy variable that is 1 if the observation occurs after the SHB was enacted but before its effective date.

Table A6: Separate Events for Event Study on Job Changers' Salary

Event	Treatment Effect	Standard Error
1 OR	0.001	(0.007)
2 NY, NYC	0.054	(0.008)
3 DE	0.052	(0.006)
4 CA	0.042	(0.006)
5 MA	0.057	(0.007)
6 VT	0.021	(0.006)
7 NY, Westchester	0.053	(0.007)
8 CT	-0.026	(0.007)
9 HI	-0.066	(0.006)
10 NY, Suffolk	-0.026	(0.008)
11 WA	0.073	(0.007)
12 AL	0.138	(0.007)
13 ME	-0.092	(0.009)
14 IL	0.035	(0.006)
15 NJ	0.02	(0.006)
16 NY, other	0.104	(0.009)

	Mean	Std. Err.	95% C.I.
Estimates	0.0271541	0.0136151	0.0018659

Note: This table estimates coefficients on log salary for each "wave" of SHB laws, rather than a single coefficient. While some waves show minimal or even negative treatment effects, others are quite large.

Table A7: Stacked Estimation

	(1) Log Annual Salary
Incumbent x Post-SHB	0.004
	(0.009)
Job Changer x Post-=SHB	0.044***
	(0.011)
Baseline	
Job Changer	-0.032***
	(0.007)
Observations	5,014,995
R-squared	0.556
Net effect	0.0401***
	(0.008)

Note: This table shows a “stacked” difference-in-difference estimation, following (Cengiz et al. 2019). For each wave of SHB passage, we create subsets of treated units from the wave and control units that are never-treated. These subsets of data are appended together and the same specification from Table III column (3) is estimated, with the addition of dataset indicator variables. This process allows for the comparison group to always be never-treated units.

Table A8. Alternative Control Groups

Dependent variable: Log annual earnings

Control group	(1) Reporting counties	(2) Imputed counties	(3) States	(4) All Non-treated
Job Changer	-0.033***	-0.038***	-0.035***	-0.033***
	(0.006)	(0.006)	(0.005)	(0.003)
Post-SHB	0.004	-0.003	0.000	-0.007
	(0.009)	(0.008)	(0.008)	(0.008)
Job Changer x Post-SHB	0.041***	0.045***	0.042***	0.039***
	(0.008)	(0.008)	(0.007)	(0.006)
Observations	366,945	453,572	505,074	1,041,923
R-squared	0.553	0.552	0.555	0.547

Note: This table reports the DDD estimations of SHBs on annual earnings with alternative control groups. *** p<0.01, ** p<0.05, * p<0.10 Errors are clustered by state in parentheses. 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, state minimum wages, industry, county, occupation, month and year. The control group in column 1 includes counties reporting FIPS information that are in commuting zones that also include treated counties. This is the control group used in the paper. Column 2 adds counties in adjacent states that do not report FIPS codes. Column 3 includes all counties in adjacent states. Column 4 includes all states.

Table A9. Education, Experience, Skill Posting

Dependent Variable	Experience Posting		Education Posting		Skill Posting	
	(1)	(2)	(3)	(4)	(5)	(6)
post-SHB	0.005*	0.005*	0.028***	0.027***	0.001***	0.001***
	(0.003)	(0.003)	(0.007)	(0.007)	(0.000)	(0.000)
Salary-posting Control	No	Yes	No	Yes	No	Yes
Observations	27,743,479	27,743,479	43,944,772	43,944,772	27,743,479	27,743,479
R-squared	0.285	0.285	0.352	0.394	0.145	0.147

Note: This table shows the DD regression estimates of the probability of experience/education/skill required before and after the implementation of the SHBs. Data are from Burning Glass job board data for years 2013 to 2018. Errors are clustered by state in parentheses. Sample includes private-sector employed workers in control and treatment groups. Column (2), (4), and (6) additionally controlled for a dummy variable indicating salary posting in a job ad. All regressions include controls for labor market tightness, industry, county, quarter and year.

Table A10. Share of Advertisements with Salary Information
Dependent variable: Share of jobs with advertised salaries

	(1)	(2)	(3)
Sample	Czone x occupation	Czone x occupation	Czone x year x month
Herfindahl-Hirschman Index of firms ranked by share of job postings	0.037***	0.016***	
	(0.009)	(0.005)	
Labor Market Tightness			-0.007***
			(0.001)
Observations	16,716	16,716	85,185
R-squared	0.003	0.176	0.669
Occupation FE	No	Yes	
Geo. FE			Yes
Time FE			Yes

Note: This table shows the association between wage posting and employer concentration or labor market tightness. *** p<0.01, ** p<0.05, * p<0.10Robust standard errors in parentheses. The first two columns regress the posting rate against the Herfindahl-Hirschman Index for each commuting zone by detailed (SOC6) occupation cell. The sample is listed to 26 large occupations as identified in (Azar, Marinescu, and Steinbaum 2020). Column 2 adds a fixed effect for SOC6 occupation. The third column regresses the posting rate against labor market tightness for each commuting zone for each month and year. The regression includes fixed effects for commuting zone, year, and month and is weighted by the number of job postings. Posting rates appear to increase with labor market concentration and decrease with labor market tightness, consistent with the model's prediction that posting rates decline with the elasticity of labor supply.

Table A11. Average Monthly Earnings of Stable Job Changers
(QWI, 2013Q1 – 2019Q2[40])

Dependent Variable: Log Annual Earnings

	(1) All	(2) Female	(3) Black	(4) Hispanic
Treatment effects				
Post-SHB	0.030***	-0.003	0.024***	0.059***
	(0.005)	(0.009)	(0.004)	(0.008)
Group		-0.355***	-0.109***	-0.130***
		(0.006)	(0.008)	(0.006)
Post-SHB x Group		0.069***	0.054***	-0.060***
		(0.014)	(0.011)	(0.011)
Observations	1,268,463	2,278,475	3,447,565	2,029,123
R-squared	0.411	0.391	0.392	0.429
Effect / gap, job changers		19%	50%	46%

Note: This table supplement the main analysis using CPS and it shows the DD estimations of SHBs on the earnings of job changers. Data are from the Quarterly Workforce Indicators. *** p<0.01, ** p<0.05, * p<0.10 Errors are clustered by state in parentheses. Note that in QWI, earnings of job changers (stable new hires) or all employees are available. However, it's not feasible to accurately identify earnings of incumbents through subtraction, as the all-employee group includes any type of employee who works with the same firm throughout the quarter. Sample includes private-sector employed workers in control and treatment groups. All regressions include controls for industry, county, quarter and year. At industry-county-year-quarter level, 20,737 out of 1,521,665 new-hire observations are under SHB.

[40] The most updated data in QWI is 2019 Q3. None of the states have observations with earnings of the job changers in 2019 Q3 and none have turnover data in 2019 Q3 and Q2.

Table A12. Worker Turnover Rate (QWI, 2013Q1 – 2019Q1)

	(1) All	(2) Female	(3) Black	(4) Hispanic
Treatment effects				
Post-SHB	0.000	-0.002	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
group		-0.007***	0.031***	0.012***
		(0.001)	(0.001)	(0.002)
Post-SHB x group		0.004***	-0.003	-0.001
		(0.001)	(0.002)	(0.004)
Observations	1,160,077	2,151,041	3,447,565	1,602,786
R-squared	0.298	0.305	0.392	0.297

Note: This table supplements the main analysis estimating the effect of SHBs on worker turnover rate with QWI. *** p<0.01, ** p<0.05, * p<0.10 Errors are clustered by state in parentheses. Sample includes private-sector employed workers in control and treatment groups. All regressions include controls for industry, county, quarter and year.

Table A13. Power tests

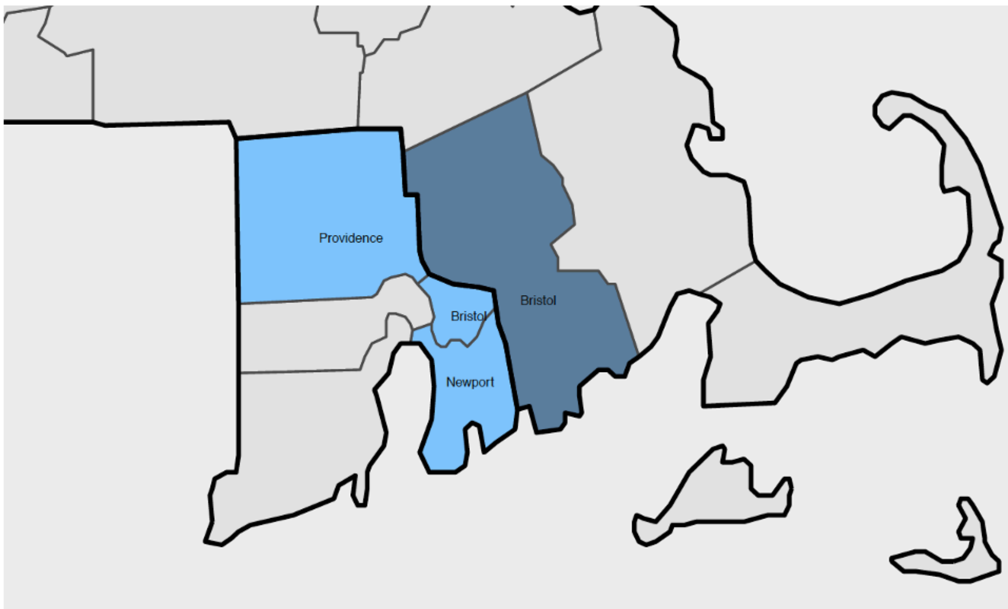
Sample	**T-test**	**Power**	**No. treated**
All job changers	0.000	1.000	2104
Female job changers	0.000	1.000	1014
Non-white job changers	0.000	1.000	490
Black job changers	0.008	0.860	166
Hispanic job changers	0.006	0.843	533
Non-white, male job changers	0.001	0.978	248
Black male job changers	0.051	0.620	88

Note: This table shows the power tests for the subgroups analyzed in Table IV.

13. Appendix Figures

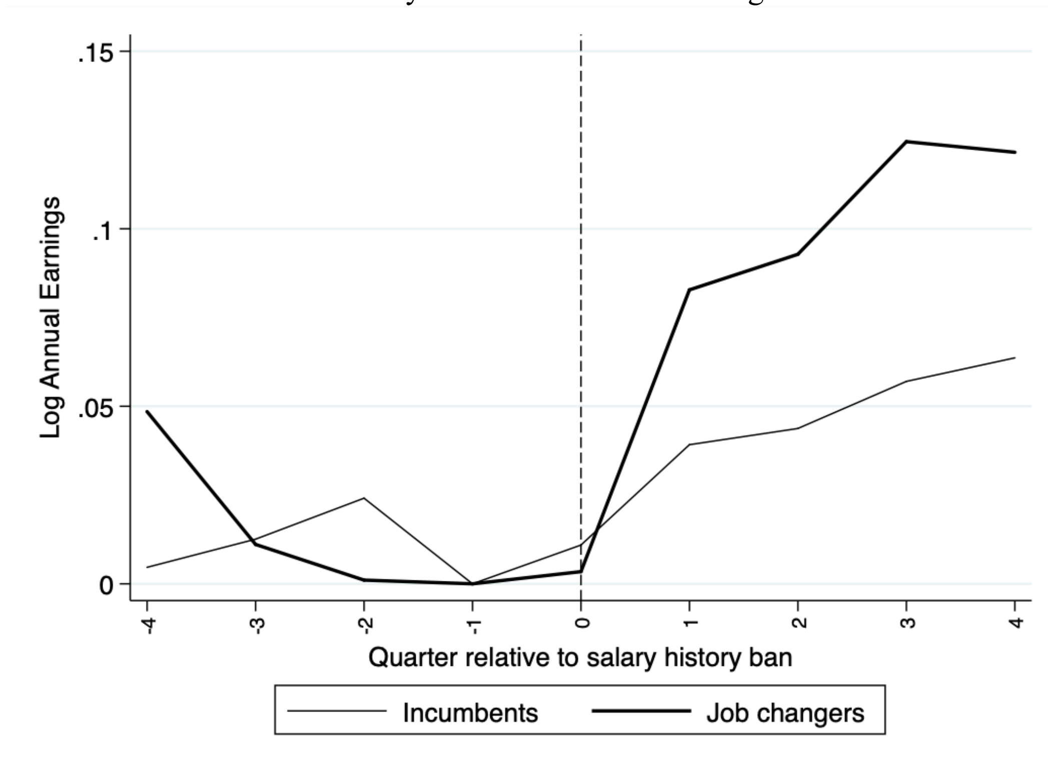
Figure A1

Example Labor Market Area (Commuting Zone 20401)



This figure shows an example of one cross-border Labor Market Area in Massachusetts and Rhode Island that contains treated and control counties. Bristol County Massachusetts, including the towns of Fall River and New Bedford, is under an SHB. On the contrary, Providence, Bristol, and Newport counties in Rhode Island and in the same Labor Market Area do not have an SHB.

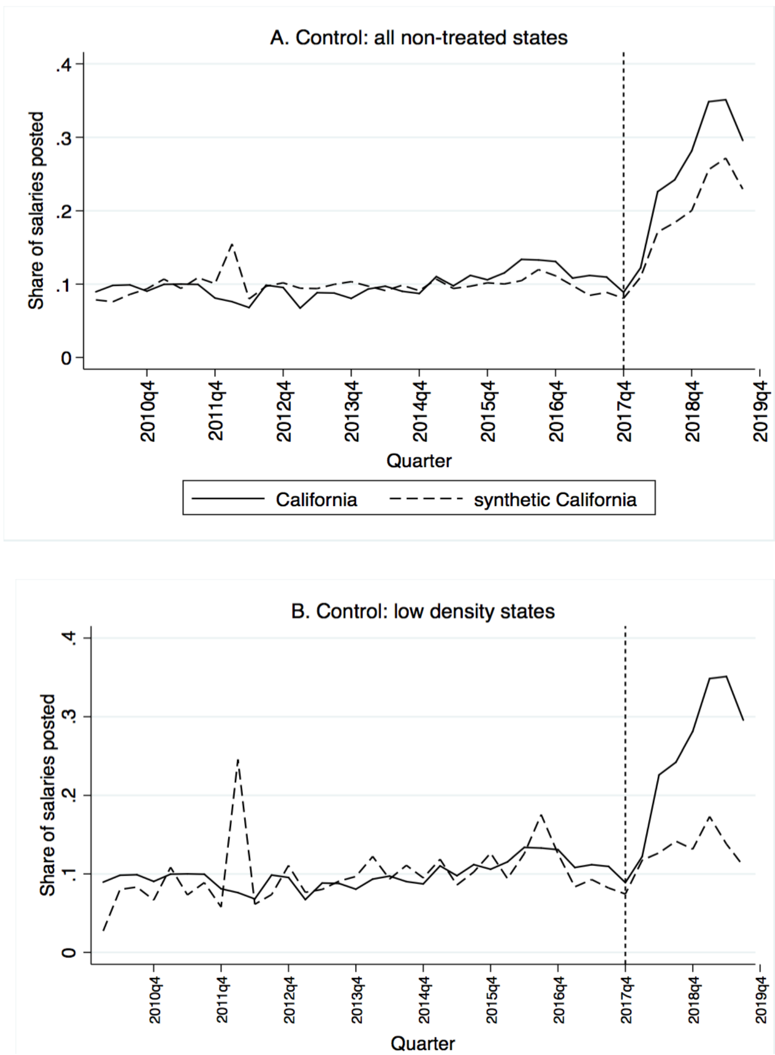
Figure A2
Event Study: Incumbents v. Job Changers



Note: This event study shows treatment effect differences between incumbents and job changers. Standard errors are clustered by state. While our model focuses on job changers, it appears that incumbents also see a positive, albeit smaller increase in earnings after a salary history ban is enacted.

Figure A3

Synthetic Control Estimates for California's Wage Posting Rates



Note: This figure shows the Synthetic Control results of salary history bans on wage posting rate, with California being the treated state. Standard errors are clustered by state. Synthetic control weights are available from the authors upon request. Figure A uses all non-treated states as donors and Figure B uses the five states with the lowest population density as donors.