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Perpetuating wage inequality: evidence from salary history bans

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Abstract
Pay gaps for women and minorities have persisted after accounting for observable differences. Recently, a dozen US states have banned employer access to salary histories. We analyze the effects of these salary history bans (SHBs) on private employer wage posting and pay. We develop a theoretical model of firms' choices between posting wages and bargaining, drawing out the implications of SHBs on wages for different groups of jobs. We then implement a comprehensive analysis in a difference-in-differences design, using Burning Glass job posting data in the US and the Current Population Survey. The results show that following SHBs, private employers posted wages more often and increased pay for job changers, particularly for women (6.2%) and non-whites (5.8%). The results imply that when employers can access applicants' salary histories while bargaining over wages, they can take advantage of past inequities, perpetuating inequality. There is also no evidence of adverse selection of workers overall or adverse employer reactions in the short run. Bargaining behavior and the use of salary histories appear to account for much of the difference in pay between disadvantaged job changers and others.

Keywords Gender wage gap · Salary history ban · Labor discrimination · Wage inequality · Wage bargaining

JEL classification J16 · J31 · J71 · J78

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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 pay with job applicants—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 in the U.S. 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 1). The solid line in Fig. 1 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 in the U. S. We estimate these effects using a differences-in-differences design for private sector jobs, 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 are related to productivity and differ by group. We find that wage gaps for disadvantaged workers sharply diminish after an SHB, suggesting that much of the wage gap is not based on worker characteristics related to productivity.

We begin with a simple theoretical 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 2023), or discrimination.[2]

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

[2] Some papers address discrimination and wage gap in general, empirically or theoretically QueryWood et al. (1993), Black (1995), Weinberger (1998), Blau and Kahn (2000), Lang, Manove and Dikens (2005). Some focus on a specific way of discrimination as it can rise in a variety of ways. Taste 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, under some assumptions: 1) more firms to post wages, 2) increased pay for job changers who bargain over wages, 3) modest increases in posted wages, and 4) greater wage increases for those workers with the lowest reservation wages. To the extent that certain groups have historically been disadvantaged, these groups will see greater wage gains for job changers.

We find empirical support for each of the main predictions of our model. First, looking at online help-wanted ads, we find a significant rise in the probability that the ads list salary information after SHBs go into effect. Second, we estimate that after an SHB, job-changing workers in the private sector earn 4.0% 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 (6.2%) and non-whites (5.8%). 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 worker characteristics that might affect productivity.

Finally, we find that salary history bans have little effect on the composition of private sector job changers except for 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.

A few contemporaneous 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 et al. 2020; Davis et al. 2022).[3] We believe our paper makes a unique contribution to the literature by correctly identifying the right treatment groups and controls necessary to analyze the impacts of SHBs on private sector hiring.

First, our model suggests that although the intent of the legislation in most states might have been to improve gender pay equity, the SHB should also have effects on 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. In contrast, Sinha (2019) and Hansen and McNichols (2020) study the impact of SHBs on the gender wage gap, missing the impact on other groups.

Second, the model implies that wage effects should apply mainly to job changers, not to incumbent workers. Sinha, Hansen and McNichols, and Davis et al. (in their CPS analysis) pool incumbent workers and job changers in measuring treatment effects, although Hansen and McNichols note that most of their measured effects arise from job changers. Nevertheless, including incumbent workers dilutes estimates of the treatment effect. We separately measure treatment effects for incumbent workers. Furthermore, their approach obscures

Footnote 2 (continued)

of employers, co-workers etc. are illustrated in Becker's (1971) model, Bowlus and Eckstein (2002), and Flabbi (2010). Arrow (1971; 1974), Phelps (1972), Aigner and Cain (1977), Oettinger (1996) present evidence for statistical discrimination. Some of the recent papers also talk about discriminations in a different country setting (Deshpande et al. 2018; Ahmed and McGillivray 2015, etc.). But overall, historical discrimination persisted and has been studied as one of the important reasons behind racial and gender wage gaps.

[3] The first version of this paper was circulated around in June 2020.

Table 1 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"

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

identification by including possibly confounding behavior of incumbent workers. There is good reason to think that incumbent female workers might see their pay increase following an SHB aside from any causal effect of the SHB. Indeed, 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 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 possibly conflates the treatment effect of an SHB with rising general concern about gender pay inequity.[4] 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

[4] Incumbent wages might also rise as an actual result of an SHB if incumbent workers, seeing the higher pay offered to new hires, renegotiate their wages. However, we cannot identify whether the rise in incumbents' pay derives causally from the SHB or from general concern about pay inequity. To be conservative, we treat increases in incumbent pay as a possible confounder.

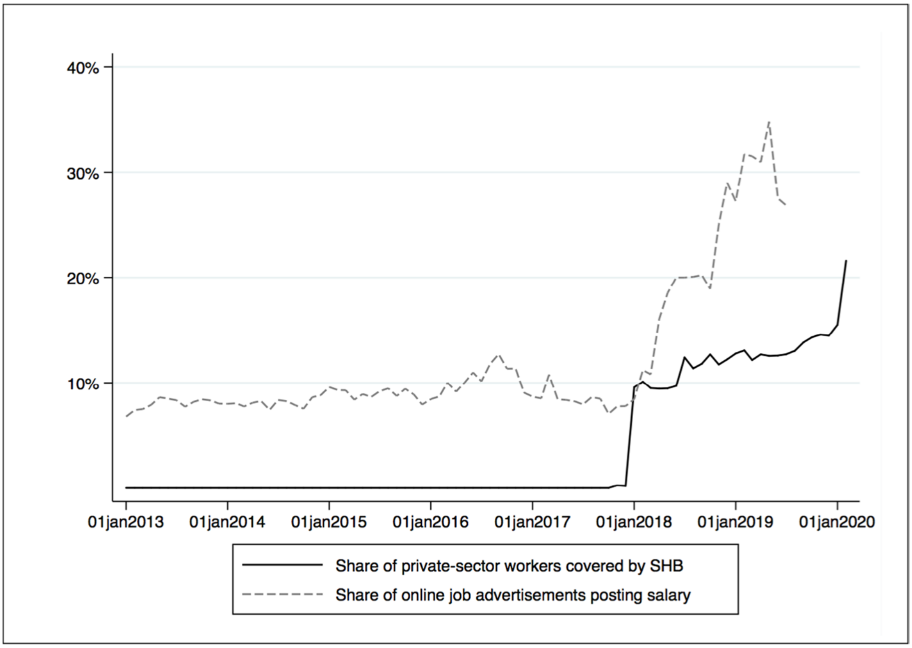

Fig. 1 Coverage of Salary History Bans and Online Salary Posting. Source: Current Population Survey; Burning Glass. Note: This figure shows share of private-sector workers covered by a salary history ban policy in the United States and the share of online job advertisements that posted a salary or salary range. Shortly after the first salary history bans went into effect the share of job ads that posted wages nearly tripled

workers. We do, in fact, find a small but significant increase in the pay of female incumbent workers. We conservatively interpret this as a confounding effect, and we subtract it from the pay increase for female job changers to obtain the treatment effect. Second, we select a control group among adjacent county-pairs across state boarders, which likely have similar general concerns and responses regarding pay inequities. We also test alternative control groups.

Also, none of the other papers look at how salary history bans affect the composition of new hires. A variety of mechanisms, including adverse selection and better information flows, might affect which workers choose to change jobs. If the composition of job switchers changes, then estimates of the treatment effect on job switchers may be biased. We explore the effect of SHBs on job switching behavior and find little concern for our treatment effect estimates. In addition, adverse selection might indicate a negative consequence of SHBs. While some of the other papers discuss adverse selection, they do not estimate whether highly paid workers actually change jobs less often.[5]

Finally, our paper focuses on the effect that SHBs covering private sector employers have on private sector employees. Many SHBs were passed that cover only public sector employees and two papers also consider these (Sran et al. 2020; Davis et al. 2022). There are several issues with these analyses. First, including private sector employees in the

[5] Davis et al. look at departures of public sector employees.

sample (Sran et al. and Davis et al.'s CPS analysis) contaminates estimates of the treatment effect because it is not clear why private sector wages would change with SHBs covering public workers. Second, public sector hiring is very different from private sector hiring. In particular, most public sector job ads display salary information (61% in 2019), implying that the role of wage bargaining is limited. More generally, civil service regulations sometimes impose strict pay grades that constrain bargaining. In our model, this means that the effect on wages is ambiguous. Davis et al. find a negative effect of SHBs on the pay of public sector employees, but that result is not necessarily valid for the much larger number of private sector employees. Third, that result is also likely biased because of selection effects. If SHBs open opportunities for higher paying jobs at private sector employers, more high-skilled workers might prefer private sector employment relative to public sector employment. We find, in fact, that with an SHB, there is a relative decrease in public sector hiring of highly educated workers and workers with high Mincer residuals (see Appendix Table A15). Hence public sector pay might decline with an SHB because fewer highly skilled workers take public sector jobs, biasing the Davis et al. estimates.

The paper proceeds as follows. In Section 2, we describe the institutional background and theoretical considerations affecting the impact of salary history bans. In Section 3 we describe our data and empirical estimation, while the results are presented in Section 4. And in Section 5 we discuss the results conclude in Section 6.

2 How might SHBs change job advertising?

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.

This logic might be correct, but it is not sufficient for empirical inference. In the Appendix we show a formal model of wage advertising that identifies the assumptions underlying the presumed effects, where those assumptions are not likely to hold, and possible confounders. The intuition behind the model is that firms will bargain over wages for jobs where the labor supply is elastic, and they will post wages for jobs with inelastic labor supply. This result comes from the basic tradeoff between posting and bargaining: employers obtain lower wages when they post but only when applicants accept the job; without posting, all applicants apply, but the firm pays more. The gap between the bargained wage and the posted wage is smaller for jobs with more elastic supply—the posted wage, which is a monopsony wage, has a smaller markdown for jobs that have elastic supply. Therefore, firms will prefer to post wages for jobs with inelastic labor supply curves, all else equal, and bargain for jobs where the labor supply elasticity exceeds a threshold.

There is some evidence to support this relationship between labor supply elasticity and wage posting. 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.[6] Brenzel et al. (2014) find higher unemployment is associated with

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

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 et al. 2018; Azar et al. 2020a, b).[7] 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 A10), implying that salary posting rates are greater for lower wage elasticity jobs.

This link between wage posting and labor supply elasticity means that a decrease in firm bargaining power will raise the threshold above which firms choose bargaining over posting; more firms will choose to post wages instead. If we accept the contention that SHBs decrease firm bargaining power and if we assume that the distribution of jobs remains unchanged, then the model tells us that after an SHB:

1. More jobs will be posted with salary information.
2. Average pay of job changers will rise for those jobs that bargain over wages if bargaining is unconstrained. However, this might not be true if bargaining is constrained as it might be for union or public sector jobs.
3. Assuming that the supply elasticity of jobs is uncorrelated with their productivity, the average posted wage will also increase.
4. Bargained wages will rise the most for those workers with the lowest reservation wages. 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. The mechanism explained by reservation wage gap is corroborated by Roussille (2022), which shows that reservation wage gaps lead to wage ask gap in gender inequality and it also explains the bid gap offered by employers.

Below we test each of these predictions. However, these predictions are based on some assumptions that also need to be validated. First, the key result on wage changes (#2) applies only to job changers, not to incumbent workers. While incumbent workers might renegotiate wages after an SHB, their pay might rise for other reasons, so, to be conservative, we treat incumbent pay as a baseline, and we distinguish between these two groups in our analysis. Second, this result might not apply to union jobs or public sector jobs. Below, we test the impact of SHBs on these two types of jobs as placebo tests.

Third, the model predictions only apply if the distribution of jobs does not change. If the composition of jobs switchers changes or if the supply elasticities change, then the model might not apply. This is important because adverse selection or statistical discrimination might accompany an SHB. Meli and Spindler (2019) contend that without salary histories, employers will base wages on average productivity (see also Greenwald 1986; Sran et al. 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.

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

However, there are some reasons to question this line of thinking. 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. In any case, we test for adverse selection below. Adverse selection implies that highly paid workers should change jobs less often and the pay of job changers should decline.

The distribution of jobs might also change 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 average pay of job changers. Below we test whether SHBs change the composition of job changers, for highly skilled disadvantaged workers specifically and for other demographic groups more generally. We also conduct tests 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.

Finally, the model assumes that employers learn each worker's productivity in the hiring process. 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.

A related literature looks at other ways information affects wage determination in different settings, regarding pay transparency (Mas 2017; Baker et al. 2023; Bennedsen et al. 2022; Cullen and Pakzad-Hurson 2023; Cullen 2023), statistical discrimination (A. Agan and Starr 2018; Bartik and Nelson 2019; Doleac and Hansen 2020), and worker's outside options (Caldwell and Danieli 2022).[8] Our paper also relates to a literature on wage posting and bargaining discussed in the next section.[9]

[8] One major distinction is that pay transparency works on multiple levels and could mean that peer salary information becomes available anytime during or before individual employment. Salary History Bans, however, mainly works through the negotiation process before individuals are hired. For example, Cullen (2023) shows that peer pay transparency policies seem to lower worker's bargaining power and reduce worker's pay, while SHBs increase the bargaining power and worker's pay. Bennedsen et al. (2022) studies the Danish gender disaggregated wage statistics disclosure by firms, and Baker et al. (2023) investigates Canadian public sector salary disclosure laws. Both policies reduce the gender wage gaps. However, the mechanisms might be different since the salary information would be available to the job applicants or employees anytime.

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

3 Empirical analysis

3.1 Data

Our two main data sources are job advertisements collected by Burning Glass Technologies (BG, now EMSI), 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.[10] 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 et al. 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.[11] 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 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.[12]

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.[13]

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.[14] In addition to a range of worker characteristics,[15] wage, weekly earnings,[16] and hours worked are reported in the outgoing

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[10] Following a merger, Burning Glass International has been renamed Lightcast. For a detailed discussion of the representativeness of job posting data, see the appendix of Hershbein and Kahn (2018)

[11] See https://www.burning-glass.com/about/faq/ for more details.

[12] 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.

[13] 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.

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

[15] 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 Classification System (NAICS) equivalents, respectively.

[16] 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.

rotation groups, months 4 and 8. Our sample contains 1.1 million observations with wage or earnings data. However, when we limit the sample to control and treatment groups and look at demographic subgroups, the effective sample is much smaller. We provide tests below to demonstrate that these analyses have sufficient statistical power.

Importantly, the CPS asks if respondents are working for a new employer in months 2–4 and 6–8 in the survey. We use this information to determine whether workers in the outgoing rotation groups (months 4 and 8) have changed jobs during the last three months. Approximately 52,000 of our wage observations are for workers who changed employers during the three-month window.[17]

3.2 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.[18] 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 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 et al. 2010, Card and Krueger 1994).

Some differences may remain between adjacent counties over state borders. We control for county fixed effects and 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.[19] This is a conservative

Footnote 16 (continued)

When normalized to annual earnings, 0.67% of observations are top coded. Excluding top-coded observations does not significantly alter our results.

[17] 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.

[18] 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).

[19] 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.

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.

Finally, although our preferred sample uses paired counties, we also run estimates with the full sample including never-treated states and other alternative control groups. In Appendix Table A8, we explore control groups where we include non-reporting counties in adjacent states, all observations in adjacent states, and all non-treated states (see also Table A5, col. 2). These the full sample and alternative control group choices generate similar point estimates across choices of control group.

3.3 Heterogenous treatment effects

We seek to estimate average treatment effects around SHBs using difference-in-differences regressions. A recent literature highlights estimation problems that arise in two-way fixed effects regressions when treatment effects trend over time (de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2020; Goodman-Bacon 2021). To avoid these problems, we follow Cengiz et al. (2019, Appendix D) and construct panels for each SHB date cohort, excluding observations from states that have been previously treated—that is, we use "clean controls." Let p designate the SHB date for each panel and P be the set of units that are treated at date p. Let t be the date of observations in the treated cohort for panel p. Our DID specification for unit i, county s, year t, outcome variable Y is

$$Y_{ipt} = \delta \bullet 1(t \geq p) \bullet 1(i \in P) + \mu_{sp} + \gamma_{tp} + \beta X_{it} + \epsilon_{ipt} \tag{1}$$

where δ is the average treatment effect, μ_{sp} is the county x panel fixed effect, γ_{tp} is the year x panel fixed effect, and X_{it} is a vector of control variables.

4 Results

4.1 Salary posting

To study the effect of SHBs on salary posting, we use specification (1) on our dataset of job advertisements with a dependent variable that equals 1 if the advertisement lists salary information and zero otherwise. The controls include labor market tightness, experience required (and squared experience), education required, county, firm, and occupation. The first column in Table 2 estimates a treatment effect using our treatment and comparison groups. Errors are clustered by panel x state, the primary unit for the assignment of treatment.[20] The estimate is about 3 percentage points (on a baseline of 9.3%) and is highly significant, which is the conditional increase in firms' salary-posting behavior.

[20] 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.

Figure 2 shows event-study coefficients for a comparable regression plotted against the quarter relative to the ban with a 95% confidence interval.[21] 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 2 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 before they came into *effect*.[22] 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 in fact negative and statistically significant, consistent with the anticipation effect seen in the event study. These studies, along with geographical evidence of no pre-trends, address concerns about policy endogeneity and spillovers from public sector SHBs.[23]

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 Fig. 1. 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 et al. 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 2 explore whether the SHB changed the salaries advertised conditional on being posted. Consistent with the model, the SHB is associated with a small, weakly significant increase in the average log salary posted (column 4) and no change in the size of the range of salaries posted (column 5).[24] While employers may change their behavior in terms of posting wages, it does not appear that they adjust the characteristics of posted wages.

[21] Let $\tau, \underline{\tau} \leq \tau \leq \bar{\tau}$ be the time relative to the event for each panel. The coefficients δ_τ are obtained from regressing a dummy variable for posting, $Y_{ipt} = \sum_{\tau=\underline{\tau},\neq -1}^{\bar{\tau}} \delta_\tau \cdot 1(t = p + \tau) \cdot 1(i \in P) + \mu_{sp} + \gamma_{tp} + \beta X_{it} + \epsilon_{ipt}$. Following Borusyak et al. (2021), we omit two time dummies (the earliest and -1).

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

[23] 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.

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

Table 2 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	0.028***	0.001	0.027***	0.013***	0.002
	(0.007)	(0.005)	(0.007)	(0.005)	(0.004)
Enacted Date			-0.009***		
			(0.001)		
Observations	17,530,375	5,490,708	17,530,375	1,486,575	1,486,575
R-squared	0.477	0.501	0.478	0.682	0.484

This table shows the stacked Diff-in-Diff (DD) results of the effect of SHBs on firms' salary posting and salary offered, using pooled panels for each SHB date excluding previously treated firms as controls. 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 in parentheses are clustered by panel x state. 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

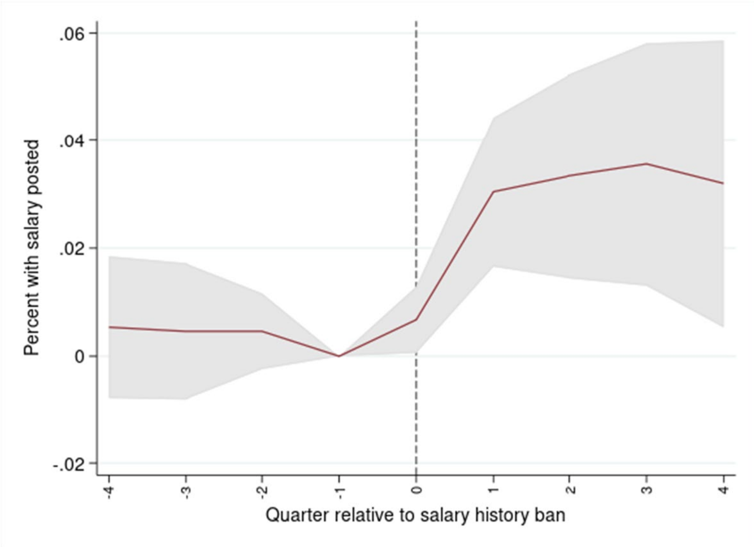

Fig. 2 Event study of online salary posting**.** Note: This figure shows an event study of the probability of posting a salary in an online job advertisement. Compared to Fig. 1 these shares may seem low, but the specification controls for county, education, experience, experience squared, occupation, 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

4.2 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. 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 for their main job during the last three months and we can also observe their hourly wages a year earlier. 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 percentage points, is large and statistically significant.[25]

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-difference-in-differences design (DDD). Since we assume that the SHB affects the pay of job changers but not of incumbent workers,

$$\begin{aligned} Y_{ipt} &= \delta_{jc} \cdot 1(t \geq p) \cdot 1(i \in P) \cdot 1(job\ changer) + \delta_{inc} \cdot 1(t \geq p) \cdot 1(i \in P) \cdot 1(incumbent) \\ &\quad + \rho_{jc} \cdot 1(job\ changer) + \mu_{sp} + \gamma_{tp} + \beta X_{it} + \epsilon_{ipt} \end{aligned} \tag{2}$$

where Y_{ipt} is a pay measure for individual I in panel p at time t. Here, the treatment effect is estimated separately for job changers and incumbents and we include baseline terms for job changers. The net treatment effect is measured as $\delta_{jc} - \delta_{inc}$, assuming that incumbent pay changes capture confounding influences. This may understate the treatment effect if, in fact, incumbents renegotiate their wages in response to an SHB. 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 (1). The net treatment effect is 3.9% and highly significant, close to the simple unconditional estimates. This is equivalent to a \$1500 increase at the sample mean. Column (2) repeats the estimation on county pairs only, which provide an additional level of control. This is our baseline specification we use in later tables. Here there is a small effect for incumbent workers, but the net effect, 4.0%, is very similar to that of the full sample. Figure 3 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.[26] Column (3) repeats the regression of column (2), 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. To correct for possible state-specific trends, Appendix Table A3, column (1) shows an estimation with state-by-year fixed effects. Table A5 reports estimates of Table 3 using Coarsened Exact Matching to balance the characteristics of the treatment and control groups. Table A6 reports treatment effects when each event panel is estimated separately. Although there is some

[25] The probability value of a t-test is .035. Given that the mean log hourly wage (shown in Appendix Table A1) is 2.989, this 4pp difference equates approximately \$.79 per hour.

[26] The sum of the dummies for job changers from $\tau - 5$ to $\tau - 2$ is not significantly different from zero with a null hypothesisi probability value of 0.303.

variance in the estimates (some state samples are quite small), there does not appear to be any trend in treatment effects. Overall, we see little significant bias arising from heterogeneous treatment effects and estimates reported using pooled stacked DID are very close to estimates using traditional OLS DID. Table A7, for example, reports the traditional DID results corresponding to the stacked estimation in Table 3, Column 2. Also, if we exclude California (regression not shown), the largest SHB state, the estimated treatment effect is similar, 0.043 (0.014), with a higher standard error.

We also test our findings on an alternative dataset, the Quarterly Workforce Indicators (QWI) from the Census (see also Sran et al. (2020)[27]). 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 A11 in the Appendix, we find an SHB treatment effect of 3.0% for job changers, quite similar to our estimates from CPS data.

According to our model, SHBs should increase pay specifically for workers who bargain over pay. This means that we can conduct a placebo test for certain jobs where pay bargaining is constrained, such as union jobs (union contracts) or public sector jobs (civil service regulations).[28] Appendix Table A14, column (1) repeats the analysis of Table 3, Column (3), but interacts union membership with the treatment dummy. The net treatment effect for union workers is distinctly smaller and not statistically significant. Column 2 runs the comparable regression using only public sector employees only treated by all SHBs that cover public employers. The net treatment effect is negative but not statistically significant.[29] As discussed above, SHBs tend to reduce public sector hiring of highly paid workers, so this estimate is biased downward. Thus, consistent with our model, we do not see a significant positive wage effect for jobs where bargaining is likely constrained.

Our estimates, while economically and statistically significant, are not large compared to other studies. Barach and Horton (2021) conduct a field experiment and find an even greater difference, 9%, when salary history is suppressed. Baker et al. (2023) 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.

4.3 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 4 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, where we further interact terms for job-changing and incumbent workers with dummy variables for different groups (female, nonwhite).

In the first column of Panel A, we see that job-changing women earn 7.8% more under an SHB. SHBs also have a weakly significant effect on the pay of incumbent women; this could be the result of renegotiation or because of a general concern about gender equity affecting incumbents; there is not a significant effect on the pay of

[27] Sran et al. (2020) ‘s analysis on job changers is only based on the QWI.

[28] We thank Ellora Derenoncourt for suggesting this. Note that most public sector job ads post salary information.

[29] Davis et al. also find negative wage effects for public sector workers.

Table 3 Salary History Bans and Pay by Job Changers and Incumbents

	(1)	(2)	(3)
Dependent variable (log)	Annual Earnings	Annual Earnings	Hourly Wage
Sample	Full Sample	County Pairs	County Pairs
Treatment effects			
Incumbent x Post-SHB	-0.006	0.004	0.003
	(0.008)	(0.007)	(0.007)
Job Changer x Post-SHB	0.033***	0.044***	0.045***
	(0.009)	(0.009)	(0.008)
Baseline			
Job Changer	-0.033***	-0.032***	-0.030***
	(0.001)	(0.002)	(0.001)
Observations	1,041,923	366,945	358,963
R-squared	0.547	0.556	0.509
Net effect for job changers	0.039***	0.040***	0.042***
	(0.005)	(0.005)	(0.005)

This table shows the DDD estimation results of the effect of SHBs on wages with a stacked panel. The main specification of these regressions can be seen in Eq. (2). 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 panel x 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. The sample in column 1 includes never treated counties. Reported observations are sample sizes before stacking

incumbent men. As above, the difference between the coefficient on female job changers and the coefficient on female incumbents demonstrates a 6.2% 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 $\frac{6.2}{14.3} = 43\%$. 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 7.8% increase in wages and a net increase, relative to incumbent non-whites, of 5.8% that is statistically significant. Column (4) repeats the exercise, but only

Fig. 3 Event Study of Job Changer Salaries. Note: This event study shows the log annual earnings of job changers from the Current Population Survey. The figure repeats the regression of Table 3, Column 2, but instead of a single time dummy for the post-treatment period, a time dummy for job changers (and incumbents) is estimated for each quarter. The modified version of Eq. (2) is $Y_{ipt} = \sum_{\tau=\underline{\tau},\neq -1}^{\overline{\tau}} \delta_\tau \cdot 1(t=p+\tau)\cdot 1(i \in P)\cdot 1(job\ changer) + \sum_{\tau=\underline{\tau},\neq -1}^{\overline{\tau}} \delta_\tau \cdot 1(t=p+\tau)\cdot 1(i \in P)\cdot 1(incumbent) + \mu_{sp} + \gamma_{tp} + \beta X_{it} + \epsilon_{ipt}$. where $\tau, \underline{\tau} \leq \tau \leq \overline{\tau}$ is the time relative to the event for each panel. Standard errors are clustered by panel x 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

for male workers. Non-white male job changers experience a 6.3% increase in wages relative to white male job changers with a 4.3% net effect that is statistically significant. Altogether, these findings suggest that these groups might, indeed, be disadvantaged, perhaps because of discrimination.

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 subsamples analyzed in Table 4.[30] 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.

4.4 Changes in the composition of job changers

Table 5 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

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

Table 4 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	-0.007	-0.001
	(0.006)	(0.006)
Male **x** Job Changer **x** Post-SHB	0.014	0.037
	(0.007)	(0.022)
Female **x** Incumbent **x** Post-SHB	0.016*	-0.009
	(0.008)	(0.006)
Female **x** Job Changer **x** Post-SHB	0.078***	0.016
	(0.019)	(0.022)
Baseline effect (gap)		
Female **x** Job Changer	-0.143***	-0.153***
	(0.002)	(0.004)
Observations	366,945	186,846
R-squared	0.558	0.565
Net Effect for Female Job Changers	0.062***	0.025
	(0.013)	(0.022)
Net Effect / Gap for Job Changers	43%	16%
Panel B: White / Non-White		
	(3)	(4)
Sample:	All	Males Only
Treatment effects		
White **x** Incumbent **x** Post-SHB	-0.000	-0.007
	(0.006)	(0.005)
White **x** Job Changer **x** Post-SHB	0.032*	0.004
	(0.010)	(0.013)
Non-White **x** Incumbent **x** Post-SHB	0.020**	0.020
	(0.013)	(0.016)
Non-White **x** Job Changer **x** Post SHB	0.078***	0.063**
	(0.025)	(0.025)
Baseline effect (gap)		
Non-White x Job Changer	-0.087***	-0.104***
	(0.003)	(0.006)
Observations	366,945	185,652
R-squared	0.557	0.552
Net Effect for Non-White Job Changers	0.058***	0.043***
	(0.015)	(0.017)
Net Effect / Gap for Job Changers	67%	40%

This table shows the stacked DDD results of SHBs on annual earnings by group. Estimates are on pooled panels, one for each SHB date, excluding previously treated control firms. Errors are clustered by panel x 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

Table 4 (continued) 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. Observations reported are prior to constructing stacked panels

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).[31] 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 stacked 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. As above, however, public sector jobs do exhibit selection.

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.

[31] 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.

Table 5 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	-0.001	0.004***	-0.000	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	337,700	337,700	337,700	337,700	337,700	95,766
R-squared	0.006	0.006	0.006	0.006	0.006	0.010
C. Treatment effect, probability worker changed jobs x above/below median Mincer residual						
Treated, low residual	-0.002	-0.001	0.003	-0.003	-0.003	
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	
Treated, high residual	-0.001	-0.001	0.012***	0.002	0.003	
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	
Observations	105,686	105,686	105,686	105,686	105,686	
R-squared	0.010	0.010	0.010	0.010	0.010	
D. Treatment effect, probability worker entered labor force						
Treated	0.0003	0.0034	0.0017	-0.0029	0.0037	
	(0.0021)	(0.0033)	(0.0031)	(0.0035)	(0.0033)	
Observations	97,868	97,868	97,868	97,868	97,868	
R-squared	0.1006	0.1006	0.1006	0.1007	0.1007	

Standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.10$. The 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. Panels B and C show stacked DDD results of SHBs on the composition of job-changing workers. Estimates are on pooled panels, one for each SHB date, excluding previously treated control firms. Regression errors are clustered by panel x state. 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. Panel B shows the probability of job changing. 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

5 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 workers. Nor do these groups experience lower pay; rather, the opposite seems to be the case. There appears to be selection out of the public sector for these workers, however (Table A15).

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 5 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 (2), 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.

6 Conclusion

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

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 for these workers cannot be attributed to observable differences in worker characteristics that affect 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 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. Our data also don't 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, future research needs to be done. Nevertheless, we have identified a major mechanism that appears to perpetuate inequality and our analysis implies that the persistent pay gaps remaining for women and non-whites are not mainly about unmeasured productivity differences. Our results make clear that informational concerns may be key to designing more equitable policies.

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Data availability The Burning Glass Dataset (now EMSI) used in the current study is not publicly available as it contains proprietary information that the authors acquired through a license. Information on how to obtain it and reproduce the analysis is available from the corresponding author on request. The Current Population Survey and Quarterly Workforce Indicators are publicly available at IPUMS: https://cps.ipums.org/cps/index.shtml and the US Census: https://lehd.ces.census.gov/data/#qwi.

Declarations

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