The Importance of Pay Information Accessibility to Corporate Innovation: Evidence from Salary History Ban*

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Abstract

This study examines how employers' access to pay information during the recruitment process affects their innovation performance. Using the staggered implementation of state-level salary history bans (SHBs), which prohibit employers from asking job applicants about their past salaries, we find that SHBs have a significant negative effect on corporate innovation. Our evidence shows that SHBs primarily affect innovation through two channels: (1) by disincentivizing male inventors, and (2) by creating higher frictions in the inventor job market. The impact of SHBs on innovation is greater for firms with higher inventor turnover, weaker female representation, and more senior or star inventors, and in states with stricter SHB enforcement. Collectively, our results suggest that restricting employers' access to job applicants' pay history distorts the labor market and has adverse effects on corporate innovation.

JEL Classification: G14, M40, M48, O31

Keywords: Information accessibility; corporate innovation; salary history ban; gender pay gap

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Abstract

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1. Introduction

In this study, we explore the impact of salary history bans (SHBs) on corporate innovation by focusing on how limiting employers' access to pay information affects firms' innovation performance. SHBs prohibit employers from asking job seekers about their past salaries during the hiring process. Legislators argue that this policy can reduce the gender wage gap, as it prevents historical salary discrimination from perpetuating in future employment. Massachusetts became the first state to pass an SHB on August 1, 2016, and many other states and localities have followed suit. As of May 2024, 22 states and 23 localities in the U.S. have enacted SHBs.[1]

Recent studies show that SHBs effectively limit employers' access to salary information, creating a negative shock to pay information accessibility (e.g., Sinha, 2019; Hansen and McNichols, 2020; Bessen et al., 2020). This loss of information has important implications for corporate innovation, which is a key driver of firm competitiveness and long-term economic growth (Glaeser and Lang, 2024; Barth and Gee, 2024). Information plays a critical role in every stage of the innovation process, from idea generation to the marketing of final products (Coop and Edgett, 2009). Firms often benefit from information about their peers' research efforts, supply chains, product markets, and capital markets, which can help them better evaluate business opportunities and manage research and development (R&D) processes (e.g., Zhong, 2018; Brown and Martinsson, 2019; Kim and Valentine, 2021; Kong et al., 2022). Despite the recognized importance of information in innovation, little is known about the role of labor market information, such as pay history, in driving innovation outcomes.

Human capital is a fundamental driver of innovation output (e.g., Liu et al., 2017), but information asymmetry can make identifying and retaining talented inventors challenging (Contigiani et al., 2018). Information frictions can distort firms' ability to match with the best

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

candidates, potentially reducing their innovative potential (Palomeras and Melero, 2010; Agan et al., 2024).

There are two competing perspectives on how SHBs might affect corporate innovation. First, SHBs could impede innovation. By preventing firms from using candidates' past pay as a basis for job offers, SHBs may narrow the gender pay gap among new hires by increasing the salary growth of female inventors relative to male inventors (Sinha, 2019; Hansen and McNichols, 2020). Over time, this could spread across the entire employee base, reducing male inventors' work morale and productivity and increasing their propensity to switch jobs (Card et al., 2012; Cullen and Perez-Truglia, 2022). SHBs may also increase hiring frictions, as past salary can signal a candidate's work quality and reservation wage (Bessen et al., 2020). Without this information, firms face greater difficulty in assessing job candidates, leading to adverse selection problems (Sran et al., 2020; Barach and Horton, 2021) and possibly resulting in fewer or less productive hires.

Alternatively, SHBs could enhance corporate innovation by reducing the gender pay gap and improving female inventors' productivity. A more gender-inclusive work environment could attract more female inventors and increase gender diversity, which has been shown to provide informational and social benefits, fostering innovation (Østergaard et al., 2011; Parrotta et al., 2014; Xie et al., 2020; Griffin et al., 2021; Hsu et al., 2022; Gao et al., 2023). Gender diversity within a firm could enhance collaboration and improve innovation outcomes (Gao and Zhang, 2017; Jin and Zhu, 2021).

Given these opposing viewpoints, the overall impact of SHBs on corporate innovation is unclear ex ante, warranting empirical investigation. Using a large sample of U.S. listed firms from 2013–2020 and a staggered difference-in-differences (DID) design, we document that SHBs have a negative impact on corporate innovation, measured by patent counts and citation outputs. These findings are robust across firm-state-level analyses, placebo tests, subsamples, alternative DID estimators, and accounting for other pay disclosure laws. Economically, the enactment of SHBs reduces patents by 10% and citations by 11.9% relative to firms in states without SHBs. The results suggest that restricted access to job applicants' pay history leads to a decline in firms' innovation performance.

We further explore the mechanisms behind this effect. Using the household earnings data from the Current Population Survey (CPS), we show that SHBs reduce the salary growth of male inventors by 27% relative to female inventors. This reduction is followed by a decline in male inventors' productivity in terms of their patenting activities. Male inventors are also more likely to relocate from states with SHBs to those without. These findings are consistent with the male disincentive hypothesis that the SHB hurts the work morale of male workers and thus impede their productivity and mobility. Additionally, we find that the quality of new hires, measured by their past patent and citation records, declines after SHBs are adopted, consistent with the hiring friction channel that information restriction on salaries imposes greater friction in the hiring process and thus prevents firms from hiring productive talents.

In addition, we examine the heterogeneous treatment effects of SHBs on firm innovation. First, given that the SHB policy primarily affects firm outcomes through the hiring process, we show that the impact of SHBs is greater for firms with higher inventor turnover (i.e., more active hiring). Second, we find that the negative impact of SHBs on innovation is weaker for firms with higher female representation, which may be associated with a smaller ex-ante gender pay gap, and thus a weaker effect of SHBs on reducing that gap. Third, we find that the effect is stronger for firms with more senior and star inventors (i.e., inventors likely to have higher incomes). This outcome may arise because SHBs make it more difficult for high-income inventors to signal their skills when changing jobs. Finally, we show that the reduction in innovation output is greater in states that ban employers from using pay information voluntarily provided by job applicants, as voluntary pay disclosure could weaken the effectiveness of SHB laws. Additionally, we investigate several alternative innovation outcomes beyond patent and citation counts, including innovation efficiency, the economic value and impact of patents, patent originality and generality, and exploitative versus explorative innovation strategies. We obtain consistent evidence.

Our study makes several contributions to the existing research. First, we contribute to the emerging literature on information accessibility and innovation. Several prior studies suggest that better access to information enhances corporate innovation. For example, Kim and Valentine (2021) document an increase in innovation for firms whose rivals disclosed more information about their patenting activities after the American Inventor's Protection Act. Similarly, using the Google exit or blockade in China as a negative shock on corporate access to foreign information, Kong et al. (2022) and Zheng and Wang (2020) find a negative effect of the exit or blockade on innovation in firms that rely on foreign knowledge. Our research complements these studies by focusing on a relatively underexplored form of non-technology information in the labor market. In doing so, it responds to Leuz and Wysocki's (2016) call for more research exploring nontraditional forms of disclosure.

Second, our study adds to the broad accounting literature on the real effects of non-financial information disclosure (e.g., Merkley, 2014; Sutherland, 2018; Krueger et al., 2024; Mao et al., 2024; Wang, 2023). More related to our study, several prior studies examine the consequences of the mandatory corporate disclosure of CEO-worker pay ratio to capital market participants (Pan et al., 2022; Chang et al., 2023; Cheng and Zhang, 2023). As the 2023 Nobel Laureate Claudia Goldin states, "the bulk of current earnings gap is now between men and women in the same job."[2] Although the pay gap between male and female employees is a significant source of wage inequality within firms, policies regarding pay disclosure remain

[2] See: https://www.cnbc.com/2023/10/10/nobel-prize-winner-claudia-goldin-the-gender-pay-gap-will-never-close-unless-this-happens.html

underexplored. Our study extends this line of research by focusing on SHBs, which act as disclosure bans on the flow of pay information from rank-and-file employees to their employers. While SHBs have been shown to reduce the gender pay gap in the private sector (Hansen and McNichols, 2020; Sinha, 2019), we document a negative externality of these bans on corporate innovation. In this regard, our findings suggest that regulators should exercise caution when implementing disclosure bans.

Lastly, we contribute to the growing literature that examines the consequences of SHBs. Prior research on SHBs primarily focuses on their effects on the hiring process and pay dynamics. For example, Sran et al. (2020) find that online job postings are more likely to include pay information, although the level of posted pay declines after SHBs are implemented. Bessen et al. (2020) show that SHBs increase pay for job changers, particularly for women and non-whites. Sinha (2019) and Hansen and McNichols (2020) document that SHBs help reduce the gender pay gap in the private sector, while Davis et al. (2022) do not find such an effect in the public sector. However, little evidence exists regarding the impact of SHBs on broader corporate outcomes. Our study contributes to this literature by documenting an unintended consequence of SHBs on corporate innovation performance.

The rest of this paper is organized as follows. Section 2 discusses the institutional background and develops the hypothesis. Section 3 describes the research design. Section 4 reports the results of baseline analysis and robustness tests. Section 5 discusses the channel tests, and Section 6 describes tests about the heterogeneous effect of SHBs. Section 7 presents the results for other innovation outcome variables, and Section 8 concludes the paper.

2. Institutional Background and Hypothesis

2.1 Institutional Background

The gender pay gap has been a long-standing global concern. The World Bank estimates that women account for only 38% of global human capital wealth, compared to 62% for men, and reports that gender pay inequality could result in a global loss of $23,620 in wealth per person. In the U.S., women earned only 83% of what men earned across all workers in 2020, reflecting a pay gap of more than $10,435 based on median earnings (DiNapoli, 2022).

Recruiters commonly ask job seekers about their past salary histories when their productivity is not directly observable. For instance, in Hall and Krueger's (2012) survey, about half of the respondents reported being asked about their past salaries during the hiring process. Employers inquire about job candidates' salary histories not only to gauge their productivity but also to infer their outside options and determine optimal pay (Sinha, 2019). However, asking about pay history has been criticized as a contributing factor to wage inequality. If employers can access job applicants' past pay histories, which signal the applicants' reservation wages, they gain a bargaining advantage in the hiring process. Job applicants who have experienced discrimination or other disadvantages in current or past jobs lose the opportunity to escape discrimination by switching jobs, resulting in the perpetuation of inequality (Bessen et al., 2020). This pay disparity issue is particularly acute for women, who generally earn less than their male counterparts and face more significant career discontinuities over their lifetimes, mainly due to childbirth (Bertrand et al., 2010).

To address long-standing concerns about the gender pay gap, many states and localities in the U.S. have adopted SHBs to prohibit inquiries about job seekers' salary histories during the hiring process. According to the American Association of University Women, SHBs are designed to protect job seekers from receiving starting salaries based on low past salaries, which may not reflect employees' true marginal productivity due to systematic biases. SHBs primarily aim to protect women, and many of the associated bills explicitly mention the goal of addressing the gender pay gap. The underlying rationale is that if a woman starts her career with a low salary, it can constrain her pay in every subsequent job, preventing her from catching up.[3]

As of May 2024, a total of 22 states and 23 localities in the U.S. have passed SHBs. Entities operating in SHB-adopting states are prohibited from asking job applicants about their past pay at any stage of the recruiting process. The specific provisions of SHBs vary across states and localities. While most bans apply to all employers within the jurisdiction, some apply only to public sector employers. Another variation is the permissibility of using pay information voluntarily disclosed by the applicant. This practice is allowed in some states (e.g., Connecticut and Hawaii) but forbidden in others (e.g., California).

In our study, we focus only on SHBs that apply to the private sector, as our research targets public firms. We further narrow our analysis to state-wide SHBs, rather than local ones, because state restrictions override any local ambiguities when conflicts arise between state-wide and local bans.

2.2 Hypothesis Development

We hypothesize that SHBs may harm firm innovation in two possible ways. First, by preventing firms from using interviewees' past pay information to negotiate job offers, SHB policies may narrow the gender pay gap among new hires by increasing the salary growth rate for female inventors relative to male inventors (Sinha, 2019; Hansen and McNichols, 2020). Although SHBs primarily affect the recruitment process, their impact could potentially extend to the entire employee base if the firm maintains consistently high hiring activity or operates over a sufficiently long period. [4] When the relative pay growth for males decreases, their work morale may suffer, leading to a decline in productivity and a higher likelihood of job switching

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[3] See the article "Don't Ask Me About My Salary History" in the New York Times for details: https://www.nytimes.com/2019/10/22/us/dont-ask-me-about-my-salary-history.html

[4] Our data indicates that the annual newly hired inventors account for 6.6% of a firm's total inventors, meaning about 21% of inventors will be replaced in every three years. In our research design, we focus on early-adopting states to ensure that firms have sufficient time (at least three years) to turn over its employees.

(Card et al., 2012; Cullen and Perez-Truglia, 2022).[5] Given that nearly 80% of inventors involved in corporate research and development (R&D) activities are male, this "male disincentive" effect could reduce a firm's overall innovation performance.[6]

Second, SHB policies may impede firm innovation by increasing information friction in talent-recruitment procedures (Sherman et al., 2023). Employers face information challenges when hiring new staff, and past salaries can serve as a signal of a job candidate's work quality and reflect their reservation wage (Bessen et al., 2020). While a candidate's non-wage information, including her patenting record, job title, and past employment history, can provide useful insights into their productivity, they are not perfect measures for salary determination, as pay levels are influenced by various factors, including the candidate's past salary, their contributions to the research team, labor market competition, and current economic conditions. Restricting recruiters' access to salary history gives job seekers more bargaining power, adds friction to the hiring process, and exacerbates the adverse selection problem faced by firms (Sran et al., 2020; Barach and Horton, 2021). As a result, firms may end up hiring fewer or less productive inventors. Collectively, both the male-disincentive effect and the hiring friction effect suggest a negative impact of SHBs on corporate innovation.

On the other hand, SHB policies could facilitate corporate innovation. By reducing the gender pay gap, SHBs benefit female inventors and may, in turn, increase their productivity. Additionally, since the majority of inventors are male, gender diversity can be a significant issue in inventor teams. Prior research has shown that gender diversity fosters firm innovation by providing informational and social benefits and improving team collaboration (Østergaard et al., 2011; Parrotta et al., 2014; Xie et al., 2020; Griffin et al., 2021; Hsu et al., 2022; Gao et

[5] The initial pay offers serve as the benchmark for employees' future promotion and payment. Wang and Sterling (2023) document a positive relationship between the gender gap in initial salary offers and the gender gap in post-hire wages for most occupations. Their findings imply that the pay gap in new hirings likely contributes to persistent salary differences between females and males.
[6] https://patentsview.org/data-in-action/exploring-trends-gender-and-patents

al., 2023). A gender-friendly work environment can attract more female inventors, thereby increasing gender diversity within a firm (Gao and Zhang, 2017; Jin and Zhu, 2021). In this context, female inventors may prefer to work in states with SHBs. As such, SHBs could enhance firm innovation by increasing gender diversity and improving the productivity of inventor teams. Overall, the gains in female productivity and gender diversity brought by SHBs may positively impact firm innovation.

Overall, it is not clear ex ante whether SHBs impede or improve the innovation performance of firms located in relevant states. Thus, we formulate our hypothesis in a null form as follows.

Hypothesis: The passage of SHBs in U.S. states is not related to the innovation performance of firms located in these states.

3. Research Design

3.1 Data and Sample

Following prior studies (e.g., He and Tian, 2013; Huang and Yuan, 2021; Moshirian et al., 2021;), we rely on patent-based data to measure a firm's innovative performance. Comprehensive patent data from the period 1976–2024 are obtained from the PatentsView database, maintained by the U.S. Patent and Trademark Office (USPTO).[7] PatentsView provides detailed information on each patent filed at the USPTO, including its application date, grant date, technology classification, the number of citations it receives, and the assignee who owns it. To proxy for a firm's innovation performance, we construct two main measures that capture the quantity and quality of firm innovation, respectively. *Patent* is the number of patents applied (and eventually awarded) by a firm in a given year, and it indicates the quantity of a firm's innovation output. We also calculate *Citation*, defined as the number of forward citations received by all the patents filed by the firm in a given year, to measure innovation quality. To

[7] PatentsView updates the patent data on a quarterly basis. We use the latest released data up to March 2024.

account for the technology trends, we adjust firms' citations by the average number of citations received by all patents in the same technology class in the same year. The technology class is defined according to the three-digit Cooperative Patent Classification system. Lastly, we take the natural logarithm of one plus each of our dependent variables in the regressions (i.e., *Log(1+Patent)*, *Log(1+Citation)*) to account for their distributional skewness.

The PatentsView database also contains information on each inventor, including their unique ID, name, residence location, and all patents for which they ever applied. More importantly, it also provides information on the gender of each inventor, which allows us to directly examine the differential impact of SHBs on male and female inventors.

Next, we retrieve the passage dates of SHBs that apply to employees of the private sector at the state level from HR Dive.[8] We do not include states implementing the SHB in or after 2019 as the effectiveness of the SHB hinges on sufficient employee turnover which needs several years for a firm to achieve.[9] Table 1 presents the state name, state code, and the passage date of SHBs. We rely on firm headquarters locations to classify firms into the treated group (i.e., states with SHBs) and the control group (i.e., states without SHBs).

[Insert Table 1 about here]

To construct our baseline sample, we begin with all U.S. public firms in the CRSP/Compustat Merged database from 2013 to 2020. We choose 2013 as the start year of our sample because the first state adopted an SHB in 2016 (See Table 1). This choice allows us to trace the innovation performance at least three years prior to the regulation being implemented. We end the sample in 2020 because there is usually a 2-year lag between the patent application date and the grant date, creating a truncation bias in the patent data (Hall, Jaffe, and Trajtenberg, 2001).

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[8] The data can be accessed at https://www.hrdive.com/news/salary-history-ban-states-list/516662/.
[9] In Section 4.3.3, we focus only on the earliest adopting state (i.e., the Massachusetts state) and find consistent results.

To match the patent data with Compustat firms, we rely on the most updated bridge files created by Kogan et al. (2017). If a firm-year does not appear in the patent data, *Log(1+Patent)* and *Log(1+Citation)* are assigned the value of zero. We extract firms' historical headquarters locations from their 10-K filings. We remove observations with missing values on firm financial information or county economic variables. We also exclude utility firms (SIC codes 4900–4999) and financial firms (SIC codes 6000–6999) from our analysis. Our final sample consists of 16,765 firm-year observations during the 2013–2020 period.

3.2 Research Model

To examine the impact of SHBs on firm innovation, we estimate the following staggered DID regression:

$$Innovation_{i,t} = \alpha + \beta_1 Treat_{s,t} + \beta_2 Firm\ Control_{i,t-1} + \beta_3 State\ Control_{i,t-1} + Firm\ FE + Year\ FE + State\ FE + \varepsilon_{i,t} \quad (1)$$

where *i* indexes the firm, *s* denotes the state where firm *i* is headquartered, and *t* denotes the year. The dependent variable, $Innovation_{i,t}$, is our measure of firm innovation performance (i.e., *Log(Patent)* or *Log(Citation)*). The explanatory variable of interest, *Treat*, equals one if a state has passed SHBs in the past and zero otherwise.[10] If the enactment of SHBs hurts firm innovation, we expect β_1 to be negatively significant.

Following prior literature (e.g., Huang and Yuan, 2021), we control for a set of firm-level characteristics that may affect corporate innovation. These controls consist of R&D expenditure (*R&D*), firm size (*Log (Asset)*), firm profitability (*ROA*), asset tangibility (*PPE*), leverage (*Leverage*), capital expenditure (*Capex*), growth opportunity (*TobinQ*), firm age (*Log (Firm Age)*), and product market competition (*HHI*) and its squared values (*HHI^2*). Since local economic variables may correlate with a state's decision to enact SHBs and firm innovation,

[10] Our study uses the passage dates of SHBs across states in determining the treatment status. There is usually a time lag between the passage date and the effective date and the median time lag is 6 months. In an untabulated test, we use the effective dates of SHBs and obtain similar results.

we further control for the total GDP output (*Log(GDP)*), personal income per capita (*Log(Income)*), total wage (*Log(Wage)*), and unemployment rate (*Unemployment Rate*) of a state. Detailed definitions of these variables are presented in Appendix A. Data on these variables are obtained from the Bureau of Economic Analysis and other governmental authorities. All control variables are lagged by one year. We further include firm, state, and year fixed effects to control for unobservable firm- and state-specific characteristics and time trends. Standard errors are clustered at the state level to account for the potential serial correlation within a state (Bertrand, Duflo, and Mullainathan, 2004). All continuous variables are winsorized at the 1% and 99% levels.

The summary statistics of the main variables are reported in Table 2. On average, a firm has 15.3 patents and receives 15.9 adjusted citations each year. Moreover, 17.9% of our sample falls in the post-SHB period. An average firm in our sample has an R&D intensity of 0.067, natural logarithm of book assets of 6.617, ROA of 0.02, tangibility of 0.248, book leverage of 0.272, capital intensity of 0.044, Tobin's Q of 2.316, natural logarithm of firm age of 2.617 years, and Herfindahl-Hirschman Index (HHI) of 0.285.

[Insert Table 2 about here]

4. SHBs and Corporate Innovation

4.1 The Timing of SHB Adoptions

We first validate the assumption that a state's adoption of SHBs is exogenous to corporate innovation activities within that state. Following prior studies (e.g., Chen, Goyal, and Zolotoy, 2022), we estimate a Cox proportional hazard models to examine the timing of adopting SHBs. Appendix B presents the results. The "failure event" in the model is the adoption of SHBs in a given state. Our main variables of interest are the average corporate innovation of firms headquartered in a state. We also include state-level control variables used in our baseline regressions. Appendix B shows that the coefficients on both *Avg_Log(1+Patent)* and

Avg_Log(1+Citation) are not statistically significant. These results suggest that a state's adoption of SHBs is not related to local firms' innovation performance, reinforcing that SHBs adoption is plausibly exogenous in our setting.

4.2 Baseline Analysis

We then examine the impact of SHBs on corporate innovation. Our baseline regression results are reported in Table 3. In Columns (1) to (3), the dependent variable is *Log(1+Patent)*. In Column (1), we do not include any control variables, but add firm, year, and state fixed effects. The coefficient on *Treat* is significantly negative at the 1% level. In Column (2), we include a set of firm-level controls and find consistent results. In Column (3), we further add a set of state-level controls, and the results are similar. In Columns (4) to (6), we use *Log(1+Citation)* as the dependent variable and find a significantly negative coefficient on *Treat* in all three columns. Overall, the results indicate that both the quantity and the quality of firm innovation outputs decline after the enactment of SHBs. In terms of the economic magnitude, based on the regression results in Columns (3) and (6), the enactment of SHBs decreases treated firms' patents by 10% and citations by 11.9% relative to control firms. Therefore, the effect of SHBs on corporate innovation is also economically meaningful. The coefficients on the control variables are largely consistent with those in prior literature. For example, larger firms, firms with less profitability, more tangible assets, lower leverage, and higher Tobin's Q are more innovative (e.g., Chang et al., 2019; Huang and Yuan, 2021).

[Insert Table 3 about here]

4.2 Dynamic Analysis

One important condition of our DID design is the parallel trend assumption. Stated differently, obvious differences should not exist between treated and control firms in their innovation output prior to the implementation of SHBs. To empirically test this assumption, we replace *Treat* in our baseline regression model with nine alternative dummy variables, indicating the number of years relative to the enactment year of SHBs. For example, the variable $Treat_{-1}$ ($Treat_{-2}$, $Treat_{-3}$, $Treat_{-4}$) indicates one year (two, three, or four years) before the passage of SHBs. $Treat_0$ equals one if SHBs are enacted in that year. $Treat_1$ ($Treat_2$, $Treat_3$) indicates one year (two or three years) after the passage of SHBs. $Treat_{4plus}$ indicates four or more years after the passage of SHBs.

We report the results of dynamic tests in Table 4. None of the coefficients on the time indicators prior to the SHB enactment is significant at any conventional level. This outcome suggests that treated and control firms do not exhibit significant differences in terms of their innovation performance until after the implementation of SHBs. Moreover, the coefficients are negatively significant only after SHBs have been enacted for one or three years. This is consistent with that the effect of SHB is turnover-dependent and it takes time for firms to accumulate labor turnovers. Taken together, our dynamic analysis validates the parallel trend assumption behind our DID design.

[Insert Table 4 about here]

4.3 Robustness Tests

4.3.1 Firm-State-Level Analysis

In our baseline regression, we define a firm's treatment status based on its headquarters location. However, many firms have research hubs in different states. To capture this effect, we construct a firm-state-year sample based on the residence location of its inventors. For example, a firm headquartered in state A may have inventors working in states A, B, and C. We aggregate the patent outputs of inventors in each state to form our firm-state-year sample.

We present the regression results of our firm-state-year sample in Table 5 Panel A. In this test, we only keep firm-years with patent application records. In other words, if a firm does not file a patent in a year, it is not included in our sample. In Columns (1) and (3), we control for firm, year, and state fixed effects. In Columns (2) and (4), we control for firm-state and year fixed effects. We find consistent evidence that the implementation of SHBs has a negative impact on firm innovation performance.

4.3.2 Adjacent States

In our baseline regression, we include firms in states that have never passed SHB policies as the control sample. However, treated states and control states may exhibit systematic differences that confound our findings. In this section, we restrict our analysis to a subsample of firms located in adopting states and firms located in non-adopting states that are adjacent to the adopting states. It is reasonable to assume that the economic conditions between two adjacent states are similar. This restricted sample could help mitigate the concern that our findings are driven by state-level heterogeneities other than SHB policies.

The results are presented in Table 5 Panel B. The sample size becomes smaller since we only include six states that adopt SHB laws and 15 neighboring states.[11] The coefficients on *Treat* are still negative and statistically significant. Overall, the results indicate that our findings remain consistent when adjacent states constitute our control sample, which lends more confidence that our findings are not driven by differences among states.

4.3.3 Earliest-adopting State

Since SHBs mainly take effect in the hiring process, an important condition for them to affect corporate outcomes is that there is enough time to accumulate labor turnovers. In our main analysis, we end our sample in 2020 to ensure that the latest-adopting SHB has been in effect for at least three years. To ensure our findings suffer less from the timing issue, we conduct our third robustness check by focusing on Massachusetts, the first state to pass the SHB in 2016. In this case, the SHB has been in effect for five years as of 2020, whereby the employee turnover rate is expected to be high. Specifically, we only keep treated firms in Massachusetts and control firms in states that never enacted the SHB in our analysis and repeat

[11] Hawaii is removed from our analysis since it does not neighbor any other U.S. state.

our baseline regression.

The results are presented in Table 5 Panel C. We still find significantly negative coefficients on *Treat* in both columns when exclusively focusing on Massachusetts. Thus, this robustness test alleviates the concern that our findings are affected by sample imbalance between pre- and post-SHB enactments.

4.3.4 Placebo Test

Our fourth robustness check rests on a placebo test based on SHBs covering public sectors. In several states, SHBs were enacted to cover only government jobs and we expect public SHBs to have a muted effect on corporate innovation. This test helps address the concern that our measured treatment effects are driven by public attention or concerns about the gender wage gap. We report the public SHB passage dates across states in Appendix C.

Panel D of Table 5 demonstrates the regression results for the effect of the passage of public SHBs on corporate innovation. The explanatory variable of interest, *TreatPub*, equals one if a firm's headquarter state has passed SHBs that cover public employees only and zero otherwise. The panel shows that the coefficient on *TreatPub* is not statistically significant, indicating that the ban on salary histories for public sector employees has no effect on firm innovation. The placebo tests alleviate the concern that our findings are driven by public attention or concerns about the gender pay gap.

4.3.5 Innovative Firms

An important issue in our study is that many firms have no patenting record during our sample period, which may create a bias in an ordinary least squares framework (Griliches, 1990). To mitigate this potential bias, we follow Acharya and Xu (2017) and conduct an analysis using a subsample of firms with at least one patent during our sample period. The results are presented in Table 5 Panel E. Although the sample size shrinks, we still find negatively significant coefficients on *Treat* in both columns. Thus, our results are robust when we focus on innovative subsamples.

4.3.6 Decile Ranking

Cohn et al. (2022) suggest that count-like outcomes with non-negative values are highly right-skewed distributed and the linear regressions of the natural logarithm of one plus the outcome may produce biased estimates. To mitigate this concern, we use the rank of *Patent* and *Citation* as the dependent variables and repeat our baseline estimation. Specifically, we assign observations with zero patents (citations) to one group and then divide (from low to high) the remaining observations into deciles.[12] We construct the categorical variables: *Patent_Rank* (*Citation_Rank*), which equals 0 for the zero-patent (citation) group and 1 to 10, respectively, if the patent (citation) values fall in the lowest decile up to the highest decile.

We re-estimate the baseline regression using *Patent_Rank* and *Citation_Rank* as the dependent variables. The regression results are reported in Table 5 Panel F, which shows that the coefficient on *Treat* is still significantly negative. The results suggest that our findings in the baseline analysis hold when using an alternative way of correcting for the skewness in the distribution of the dependent variables.

4.3.7 Matched Sample Analysis

To further mitigate the concern that our results are driven by fundamental differences between treated firms and control firms, we repeat our baseline analysis using two matched samples: one by propensity score matching and the other by entropy balancing matching. For the propensity score matching approach, we first regress the *Treat* dummy against all firm-level controls in our baseline analysis. Then, we calculate the propensity score based on a logit regression model. Next, we perform a one-to-one nearest neighbor match with replacement; that is, for each treated firm whose headquarters state has passed SHBs (*Treat*=1), we find a matched (control) firm whose headquarters state has not passed SHBs (*Treat*=0) with the

[12] Over 50% of the observations have zero patents (citations). Therefore, we assign them to a separate group.

nearest score. To ensure that the treated and control firms are not significantly different in terms of their firm characteristics, we use the caliper matching method and match within a caliper of 1%, where the caliper refers to the difference in the predicted probabilities between the treatment and control firms. After matching, our final sample includes 5,341 observations.

We further employ the entropy balancing matching approach, which matches the treatment and control groups, by constructing a set of matching weights that forces certain balance metrics to hold (Hainmueller, 2012; McMullin and Schonberger, 2020). Specifically, via a maximum-entropy reweighting scheme (Hainmueller and Xu, 2013), we perform entropy balancing on the first three moments (i.e., mean, variance, and skewness) of all firm covariates to ensure that the distributions of all included control variables are similar for treated firms and other control firms.

Afterward, we re-run the baseline regression using the two matched samples and report the results in Panel G of Table 5. For both samples, the coefficient on *Treat* is significantly negative, consistent with the results in our baseline analysis that, following the enactment of SHB laws, firm innovation output declines.

4.3.8 Callaway and Sant'Anna-DID Estimation

Recent studies suggest that staggered DID estimation can be biased when the treatment effects vary significantly across time (Baker, Larcker, and Wang, 2022). To mitigate such a concern, we follow the suggestions from Callaway and Sant'Anna (2021) and use the Callaway and Sant'Anna-DiD estimation as a robustness check. Specifically, we group firms treated in the same year and firms never treated or treated after this year into one cohort. We estimate the individual cohort-time-specific average treatment effect on the treated (ATT), allowing for treatment effect heterogeneity. Then, we aggregate all the ATT results to obtain the overall treatment effects. The results are presented in Panel H of Table 5, which shows that the coefficient on *Treat* remains negative and significant. The results are consistent with our baseline findings, suggesting that our findings are robust when time-varying treatment effects are considered.

4.3.9 Drop States with Pay Secret Law

Gao et al. (2023) show that the staggered implementation of pay secret laws, which prohibit firms from implementing pay secrecy rules and practices, has a positive effect on firm innovation. To disentangle from the effect of pay secret laws, we repeat our baseline regression by removing firms headquartered in states that ever have pay secret laws or firms headquartered in states that passed pay secret laws after 2013 (the start year of our sample period). The regression results are presented Panel I of Table 5 and are qualitatively similar to our baseline findings.

[Insert Table 5 about here]

5. Channel Tests

Having established that the SHB laws reduce firm innovation outputs, we now discuss the potential channels through which SHBs affect firm innovation. Specifically, we propose two possible channels in developing the hypothesis, including the male disincentive channel and the hiring friction channel. In this section, we perform tests on each of the two channels.

5.1 Male Disincentive Channel

Our first channel states that SHBs benefit women more than men and thus create a disincentive for male inventors. To provide evidence on this channel, in the following subsections, we examine the effect of SHBs on male inventor wage, productivity, and mobility, respectively.

5.1.1. Inventor Wage

We start our analysis by examining the effect of SHBs on male inventor wages. To perform this test, we collect the monthly earnings data from the CPS Outgoing Rotational Groups between 2013 and 2020. The CPS samples roughly 60,000 households each month using a rotating panel design and the average response rate is about 90%. We use the earning data on each household member as reported by the call recipient. Since our study focuses on public firms, we only keep full-time workers in the private for-profit sector. We also require individuals to be at their prime working age (i.e., 22–55). We then drop the observations in states that passed an SHB on or after 2019. This filtering process leaves us with 523,902 individual-year-month observations. We further seek to identify potential inventors from our household sample based on their occupation information. Specifically, if a worker's occupation title includes the following keywords: scientist(s), science, engineer(s), engineering, technician(s), or developer, we regard that worker as an inventor (Gao et al., 2023).[13] After this additional filtering process, we have 42,329 inventor-year-month observations in the sample.

To examine the impact of SHBs on male inventor wage, we implement the following triple-differences regression model:

$$\begin{aligned} Log\ (Salary)_{p,y,m} = {} & \alpha + \beta_1 Treat_{s,y,m} \times Male_p + \beta_2 Treat_{s,y,m} + \beta_3 Male_p \\ & + Individual\ Controls + State\ Controls + FEs + \varepsilon_{p,s,y,m} \end{aligned} \tag{2}$$

where *p* indexes the surveyed inventor, *y* denotes the year, *m* denotes the month, and *s* denotes the residence state. *Log(Salary)* is the weekly salary received by inventor *p* in year *y* month *m*. *Treat* equals one if a state has passed SHBs in the past and zero otherwise. *Male* indicates whether the inventor is male. The variable of interest is the interaction term between *Treat* and *Male*. We also control for a number of personal characteristics that could be related to an individual's salary, including the natural logarithm of their age (*Log(Age)*), whether they graduate from college (*College*) or earn a postgraduate degree (*Postgrad*), their race (i.e., white, black, etc.), and their job status (i.e., full time or part-time). We further add a set of state economic variables, as we described in the previous section, such as *Log(GDP)*, *Log(Income)*,

[13] We rely on the 2010 Census Bureau occupational classification system to categorize potential inventors.

Log(Wage), and *Unemployment*. Standard errors are clustered at the state level to account for the potential serial correlation within a state (Bertrand et al., 2004).

The regression results are presented in Table 6. In Column (1), we incorporate state, year, and month-fixed effects. The coefficient on *Treat* is -0.008 and is not statistically significant, indicating that the overall wage level of inventors does not change after the passage of SHBs. In Column (2), we add the interaction term *Treat*×*Male*. The coefficient on *Male* is 0.269, suggesting on average male inventors earn about 27% more than female peers. More importantly, the coefficient on *Treat*×*Male* is -0.113 and significant at the 1% level. It suggests that SHBs reduce male inventors' earnings relative to their female peers by 11.3% and reduce the gender gap by about 42% (0.113/0.269).[14] In Column (2), we tighten our specification by adding state-year, male-state, and male-year fixed effects. These fixed effects help control state-level shocks and gender differences across states and years. We find similar results.

In Columns (3) and (4), we use the full sample of households (not just inventors) from CPS and find consistent results. Moreover, the economic magnitude is only about half that using the inventor sample, implying that the effect is stronger for inventors than the average population. Overall, the results demonstrate that the implementation of SHBs does not change the overall pay level but alters the salary mix between male and female, particularly for high-skill talents (i.e., inventors).

[Insert Table 6 about here]

5.1.2. Inventor Productivity

After showing the impact of SHBs on male inventor wage, we then test how this effect interacts with male inventor productivity. We argue that the reduced salary for male inventors should lead to a decline in their innovation productivity. We collect data on the patenting

[14] Another possible explanation is that SHBs would increase firm total wage expense that crowds out hiring resources. However, the sum of coefficients on *Treat* and *Treat*×*Male* is negative, implying that SHBs reduce salary levels, consistent with prior findings (Sran et al., 2020; Davis et al., 2022).

activities of more than 500,000 unique inventors from PatentsView and construct an inventor-year panel dataset from 2013 to 2020.

To compute each inventor's productivity, we calculate the number of patents (*Log(1+Patent)*) and technology class adjusted forward citations received (*Log(1+Citation)*) for each inventor in each application year. If the inventor did not file any patent in a particular year, we set the values of *Log(1+Patent)* and *Log(1+Citation)* to zero. We regress the two measures on *Treat*, *Male*, and their interaction term.[15] The variable of interest is the interaction term between *Treat* and *Male*, which captures the asymmetric effect of SHBs on the productivity of male and female inventors. All baseline controls from Table 3 Column (3) are included in regressions. The standard errors are clustered at the state level.

The regression results using the inventor sample are reported in Table 7. The dependent variable is *Log(1+Patent)* in Columns (1) to (3), and *Log(1+Citation)* in Columns (4) to (6). We control for the firm, state, year, inventor fixed effects, or firm-state fixed effects.[16] In Column (1), the coefficient on *Treat* is negatively significant, suggesting that on average SHBs have a negative impact on the productivity of individual inventors. In Columns (2) and (3), we incorporate the interaction term *Treat*×*Male*. The coefficients on *Treat* are no longer significant, suggesting that SHB has no impact on the productivity of female inventors. In contrast, the coefficient on *Treat*×*Male* is negatively significant, suggesting that the negative effect of SHBs mainly concentrates on male inventors. Combined with our empirical findings on inventor wage, these results imply that, although an SHB could help reduce the gender gap, it hurts male earnings and productivity, and finally destroys firms' overall innovation performance. Thus, the inventor-level results are consistent with the male disincentive channel from the perspective of innovation productivity.

[15] The standalone variable *Male* is absorbed by the inventor fixed effects; thus, it does not appear in the model.
[16] Firm-state fixed effects are included to control for unobservable time-constant factors for each firm-state pair omitted variables.

[Insert Table 7 about here]

5.1.3. Inventor Mobility

Besides the adverse impact on male inventor productivity, SHBs may also affect male inventor mobility. That is, SHB-adopting states may become less attractive to men and male inventors may be more likely to choose to work in states without SHBs. We test this hypothesis from two aspects: the static inventor composite and dynamic inventor flow.

Using the inventor sample from the PatentsView database, we first construct three static measures on a firm's inventor composition: the number of inventors, calculated as the natural logarithm of one plus the number of inventors (*Log(1+Inventor)*); female inventors, calculated as the natural logarithm of one plus the number of female inventors (*Log(1+Female Inventor)*); and male inventors, calculated as the natural logarithm of one plus the number of male inventors (*Log(1+Male Inventor)*). Meanwhile, we compute three inventor mobility measures to examine the impact of SHBs on inventor mobility. *%Move In* is the number of inventors that move in a firm scaled by the total number of inventors in a firm. *%Female Move In* (*%Male Move In*) is calculated as the number of female (male) inventors that move in a firm scaled by the total number of inventors in a firm. Since SHB policies vary across states, we restrict our analysis to relocating inventors whose joining firm and leaving firm are headquartered in different states.

To capture inventor mobility, we rely on the patent filing history of each inventor to identify their employer changes. One challenge to constructing the mobility proxies is that the patent data only records active inventors with observable patent filing activities. In other words, if an inventor did not apply for any patent in a year, they are not included in the data, making it difficult to precisely estimate the total number of a firm's inventors. To account for this issue, we follow Melero et al. (2020) and assume that an inventor does not move if the assignee of their two consecutive patent fillings does not change.[17]

We regress the three inventor composite measures on *Treat* and the same control variables and fixed effects in the regression in Equation (1). In Table 8 Panel A, we present the results on static inventor composite. The dependent variables in Columns (1) to (3) are the natural logarithms of the number of inventors, female inventors, and male inventors, respectively. We find that following the SHB implementation, the total number of inventors of a firm decreases by 15.3%. Moreover, the number of male inventors decreases by 15.0%, while the number of female inventors decreases by only 7.4%. These findings are consistent with our conjecture that SHBs have a much more negative impact on firms' total number of male inventors relative to their female peers. The decline in the number of female inventors may be explained by the following two reasons. First, when highly productive male colleges leave the state, the overall working environment gets worse, contributing to the move of female inventors. Second, it may be explained by the backlash story that when external pressure clash with gender stereotypes prevailing with in a firm, managers may push back by taking actions to undermine gender outcomes elsewhere (i.e., avoid hiring female employees) (Bian et al., 2023).

Next, we focus on dynamic inventor flow across firms and investigate whether inventors are less likely to move into firms in states with SHBs. We report the results in Table 8 Panel B. Columns (1) examine the overall inventor flow. The coefficient on *Treat* is negatively significant for *%Move In*. This finding implies that, compared with firms in states without SHB programs, firms in states with SHBs are less attractive to inventors. Columns (2) and (3) examine the inflow of male and female inventors, respectively. We find that the coefficients on *Treat* are both negatively significant for *% Female Move In* and *%Male Move In,* suggesting that both male and female inventors are less likely to move in states with SHBs, but the

[17] For example, if inventor *a* filed patents in the year 2013 and 2015 and both with firm *i* as the assignee, inventor *a* is believed to be working in firm *i* during 2013–2015. In other cases, inventors may move from one company to another as evidenced by the changes in assignees owning the patents they filed. In this situation, we define the "jump" year as the first year that the inventor starts their career with a new employer.

coefficient for male inventors is 4 times that for female inventors. The heterogeneous effects of SHB policies on women and men further confirm our male disincentive mechanism.

[Insert Table 8 about here]

5.2 Hiring Friction Channel

In addition to the disincentive effect on male inventors, SHBs may also hurt a firm's innovation by affecting its hiring process. Salary can be an important signal for a job seeker's professional skills and productivity in the workplace (Murray and Gerhart, 1998; Zhang, 2007). Disallowing asking job candidates about their past salary can increase the information asymmetry and make the adverse selection problem more severe in the job application pool (Sran et al., 2020). Consequently, the quality of corporate new hiring deteriorates and firm innovation output is eventually harmed.

To test the hiring friction channel, we construct two variables to proxy for the quality of newly hired inventors: the natural logarithm of the number of patents (*Log(1+Past Patent)*) and their technology-class adjusted citations (*Log(1+Past Citation)*) obtained by newly hired inventors during their past career (i.e., from first patent to the latest one before joining the firm). An inventor is identified as a new hire in a firm-year if they file a patent that is owned by the firm for the first time.

We regress the quality of all newly hired inventors, newly hired male inventors, and newly hired female inventors on *Treat* and the same control variables in the regression in Equation (1), respectively. The results are reported in Table 9. In Columns (1) and (4), the coefficient on *Treat* is negative and significant, indicating a decline in the overall quality of newly hired inventors. In addition, the coefficients on *Treat* are negative and significant in Columns (3), (5), and (6), suggesting that the decline in the quality of new hires holds for both male and female inventors. Moreover, the magnitude of the reduction for male inventors is about twice that for female new hires, implying that the quality of newly hired male inventors declines more than that of newly hired female inventors. The results echo our earlier findings that SHBs have more adverse effects on male than female inventors. Overall, these results on new hiring suggest that recruitment barriers can be another important channel through which SHBs affect corporate innovation.

[Insert Table 9 about here]

6. Heterogeneous Effect

In this section, we conduct four heterogeneity tests to explore factors that are likely to influence the documented relation between SHBs and corporate innovation.

6.1 Inventor Turnover

We first examine the effect of inventor turnover on the SHB–corporate innovation relation. The SHB policy affects firm outcomes mainly through the recruitment process. For employees who have already joined the company, the impact on them is expected to be smaller. Thus, the effectiveness of SHBs should be greater for firms with a greater employee turnover (i.e., higher intensity of hiring activities). To perform the test, we calculate the turnover rate of inventors within a firm. In particular, *Turnover Rate* is computed as the number of inventors that move into a firm plus the number of inventors that move out of a firm, divided by the total number of inventors in a year. Since some firms do not file any patents and we could not get the information on inventor turnover, in this test, we drop these firms. We re-estimate the regression in Equation (1) by adding *Turnover Rate* and its interaction term with *Treat* in the regression specification.

The regression results are reported in Table 10 Panel A. The panel shows that the coefficient on *Treat*×*Turnover Rate* is negatively significant in both columns, implying that the negative impact of state SHB policies on firm innovation is stronger when a firm has a higher inventor turnover rate. The findings are consistent with our expectations.

6.2 Female Representation

We further examine the role of female representation in shaping the relation between SHB and corporate innovation. As discussed earlier, one potential benefit of SHBs is to increase female inventors' productivity by reducing the gender pay gap. When female employees gain more power in a firm, the issue of a pay discrepancy between male and female workers is likely less acute (Cohen and Huffman, 2007; Carter et al., 2017), resulting a smaller effect of SHBs on gender pay gap. Accordingly, we expect that the adverse impact of SHB policies on innovation is weaker when a firm has higher female representation.

To test this hypothesis, we measure female representation using director gender information collected from the ExecuComp database. Then we construct female board representation using the number of female directors on the board scaled by the total number of directors (*%Female Director*). We interact the variable with *Treat* to the regression specification in Equation (1), respectively. The regression results are reported in Table 10 Panel B. The coefficients on *Treat×%Female Director* are significantly positive, indicating that when a firm has a higher level of female representation, the relation between SHB policies and corporate innovation becomes weaker. The findings are consistent with our priori.

6.3 Inventor Income

One interesting question related to our study is whether the effect of SHBs varies among inventors with different levels of income. Past salaries could signal a job candidate's work quality and reflect their reservation salary (Bessen et al., 2020). For employees with above-average salaries, bans on salary history can make it more difficult to signal their skills when they switch jobs. In contrast, for workers with low salaries, withholding this information benefits them when they are bargaining with their prospective employers. We therefore predict that the negative effect of SHBs on corporate innovation should be stronger for firms with more high-income inventors.

However, we cannot observe the salary for each inventor due to data constraints. To perform the test, we rely on inventor tenure and performance as two proxies for income levels. We assume that senior inventors and star inventors earn more than their peers. Inventor tenure is calculated as the number of years since the year of an inventor's first patent filing. We then use the natural logarithm of one plus the average inventor tenure for a firm in a year to define the firm-level inventor tenure (*Log(1+Inventor tenure)*). Second, we identify star inventors if their patent filing quantity ranks in the top 5% in a year and calculate the fraction of star inventors in a firm (*%Star Inventor*). We re-estimate the regression in Equation (1) by adding the two variables and their interaction terms with *Treat*, respectively.

The regression results are reported in Table 10 Panel C. The coefficients on *Treat×Log(1+Inventor tenure)* and *Treat×%Star Inventor* are significantly negative, suggesting that the effect of SHBs on innovation is stronger for firms with more senior inventors. The findings are consistent with our hypothesis that the SHB policies have a stronger impact on firms with more high-income inventors.

6.4 Ban on Voluntary Disclosure

In our final heterogeneity test, we explore a state's attitude toward the voluntary disclosure of salary information. Some SHB regulations allow employers to use salary information voluntarily provided by the job applicant, while others ban it. For example, in California, even if the employee self-selects to share their past salary information with the employer in the interview process, the employer is not allowed to use this information to determine pay. The ban on voluntary disclosure may reinforce the effectiveness of the SHB laws. We therefore conjecture that the reduction in innovation output should be greater in states with a ban on voluntary disclosure.

To perform the test, we define *Ban Voluntary,* which equals one for states that do not allow employers to use the salary information disclosed by job applicants voluntarily, and zero otherwise. We re-estimate the regression in Equation (1) by including *Ban Voluntary* and its interaction term with *Treat*. The regression results are reported in Table 10 Panel D. The panel shows that the coefficient on *Treat×Ban Voluntary* is negatively significant, suggesting that the decline in innovation is larger for states that disallow employers' use of voluntary salary information to set the pay level.

[Insert Table 10 about here]

7. Other Innovation Outcomes

To enrich our empirical analysis, we further examine various nuanced innovation measures beyond patent and citation counts as an additional test. The first measure is patent per employee (*Pat/Emp*), calculated as the number of patents scaled by the number of employees. The second measure is citation per employee (*Cit/Emp*), calculated as the number of technology class-adjusted citations scaled by the number of employees. The two measures capture a firm's innovation efficiency. The third measure is citation per patent (*Cit/Pat*), calculated as the average technology class-adjusted citations received by each patent. Further, we follow prior literature (Balsmeier et al., 2017; Manso, 2011) and calculate the natural logarithm of one plus the number of impactful patents (*Log(1+ImpactPatent)*). A patent is regarded as impactful if the number of its forward citations is above the top 10% of patents in the same technology class and application year. We also compute the natural logarithm of one plus the number of valuable patents (*Log(1+ValuablePatent)*), with valuable patents being defined by economic values that are among the top 10% of patents in the same technology class and application year.

We re-estimate the regression in Equation (1) by replacing *Log(1+Patent)* and *Log(1+Citation)* with these variables. Columns (1) to (5) of Table 11 show that the coefficient on *Treat* is negatively significant in all regressions, indicating that treated firms exhibit lower innovation efficiency, and have fewer impactful and valuable patents.[18]

In addition, we look at the generality and originality of a firm's patent portfolio (Hall et al., 2001). The generality (originality) score of a patent is defined as one minus the Herfindahl index of the technology class distribution of all patents that have cited (have been cited by) the focal patent. If a patent is cited by subsequent patents that belong to a wide range of technological fields, its generality score will be higher. If a patent cites previous patents that belong to a wide range of technological areas, it tends to have a higher originality score. We then aggregate the originality and generality score of each patent at the firm-year level and take the natural logarithm one plus the scores as *Log(1+Orig)* and *Log(1+Gen)*. We re-estimate the regression in Equation (1) using these two variables as the dependent variable, respectively. The results are reported in Columns (6) and (7) of Table 11. The coefficient on *Treat* is negative and significant, implying that patents filed by firms affected by SHB policies have lower scores for both generality and originality.

Finally, we attempt to distinguish between a firm's exploitative and explorative patenting strategies (e.g., March, 1991; Gao, Hsu, and Li, 2018). Explorative innovation extends beyond a firm's existing knowledge, while exploitative innovation digs deeper along the path of existing expertise. A firm's existing expertise is defined as the combination of its existing patents and the backward citations made by those patents over the past five years. A patent is categorized as explorative if at least 80% of its backward citations are based on new knowledge outside a firm's existing expertise (i.e., not citing the firm's existing patents or the citations made by its existing patents). A patent is classified as an exploitative one if at least 80% of its citations are based on a firm's existing expertise (i.e., citing the firm's existing patents or the citations made by its existing patents). We then compute the number of a firm's exploitative

[18] In Columns (1) and (2), 246 observations are dropped because of missing or zero employee reported in Compustat.

and explorative patents in its patent portfolio each year and take the natural logarithm of one plus the numbers as *Log (1+ExploitPatent)* and *Log(1+ExplorePatent)*. A higher fraction of exploitative patents indicates that the firm focuses more on reinforcing its existing known expertise. In contrast, a higher number of explorative patents suggests that the firm is shifting its innovation strategy from its current trajectory to new and unknown technological territories.

We re-estimate the regression in Equation (1) using the two variables as the dependent variable, respectively, and report the results in Columns (8) and (9) of Table 11. The coefficient on *Treat* is not statistically significant in Column (8), suggesting that SHBs do not have a material effect on firms' exploitative activities. However, the coefficient on *Treat* is negative and significant in Column (9), indicating that firms are less willing to venture into their new and unfamiliar knowledge areas as proxied by their explorative patents when SHBs reduce inventors' productivity. Overall, the results indicate that the detrimental effect of the SHB policy is mainly on a firm's explorative strategy.

[Insert Table 11 about here]

8. Conclusion

In the era of information, the ability to access and utilize data is critical for firms' sustainable growth. Since information is an essential input for corporate innovation, any regulation that restricts firms' access to information can pose a significant threat. The passage of SHB laws across U.S. states highlights this concern. Women are statistically more likely to earn less than men, and the gender pay gap has become a global issue, drawing significant attention from both policymakers and academics. Since 2016, several U.S. states have enacted SHB laws, which prohibit employers from asking job seekers about their past salaries during recruitment. Legislators advocating for this policy believe it could help reduce the gender pay gap.

In this paper, we examine the impact of pay information accessibility on corporate innovation. Using the staggered adoption of SHBs across U.S. states, we find that SHBs negatively affect corporate innovation activities. Our results remain robust across multiple tests. Furthermore, we demonstrate that the male disincentive effect and increased hiring barriers are two possible channels through which SHBs hinder corporate innovation. We also find that the negative effects of SHBs on firm innovation are more pronounced in firms with higher inventor turnover, lower female representation, more high-profile inventors, and in SHB states that prohibit voluntary disclosures of salary information.

We contribute to emerging research on the impact of information accessibility on corporate innovation by focusing on the role of information within the labor market. Additionally, we extend the literature on pay disclosure by examining the effects of a disclosure ban on the flow of salary information from rank-and-file employees to employers. Furthermore, we add to studies on the real consequences of SHBs by showing that they create negative externalities for corporate innovation. Our findings offer insight into the unintended outcomes resulting from the adoption of SHBs and suggest that equal pay policies, such as SHBs, can discourage male workers and diminish their contributions to overall social welfare. Since the enactment of SHBs remains a subject of debate in some non-adopting states, our findings hold important policy implications.

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Table 1. List of States with Salary History Ban Concerning the Private Sector

This table lists the dates of the passage of the salary history ban covering the private sector across states between 2016 and 2019. The information is obtained from: https://www.hrdive.com/news/salary-history-ban-states-list/516662/.

State name	State code	Passage date
Massachusetts	MA	August 1, 2016
Oregon	OR	June 1, 2017
Delaware	DE	June 14, 2017
California	CA	October 12, 2017
Vermont	VT	May 11, 2018
Connecticut	CT	May 22, 2018
Hawaii	HI	July 5, 2018

Table 2. Summary Statistics

This table reports the summary statistics of the main variables in this paper. All continuous variables are winsorized at 1% and 99% levels. Detailed variable definitions are provided in Appendix A.

	(1) N	(2) Mean	(3) S.D	(4) P25	(5) P50	(6) P75
Patent	16,765	15.275	61.848	0.000	0.000	3.000
Citation	16,765	15.862	65.960	0.000	0.000	0.875
Treat	16,765	0.179	0.384	0.000	0.000	0.000
R&D	16,765	0.067	0.129	0.000	0.007	0.076
Log (Asset)	16,765	6.617	2.037	5.218	6.698	7.986
ROA	16,765	0.020	0.263	0.008	0.094	0.146
PPE	16,765	0.248	0.241	0.066	0.155	0.357
Leverage	16,765	0.272	0.245	0.059	0.236	0.408
Capex	16,765	0.044	0.052	0.013	0.027	0.053
TobinQ	16,765	2.316	1.828	1.204	1.680	2.703
Log (Firm Age)	16,765	2.617	1.071	1.946	2.890	3.367
HHI	16,765	0.285	0.224	0.117	0.216	0.375
Log (GDP)	16,765	13.511	0.954	12.773	13.442	14.392
Log (Income)	16,765	10.866	0.161	10.750	10.850	10.976
Log (Wage)	16,765	19.663	0.943	19.074	19.584	20.491
Unemployment	16,765	6.344	2.487	4.500	5.900	7.400

Table 3. Baseline Results

This table reports the results of difference-in-differences regressions that examine the impact of the SHBs on firm innovation during 2013-2020. The dependent variable from Columns (1) to (3) is the natural logarithm of one plus the number of patents a firm applied for and were subsequently granted in a year (*Log(1+Patent)*). The dependent variable from Columns (4) to (6) is the natural logarithm of one plus the total number of citations received by a firm's patents in a year adjusted by the average number of citations received by all patents in the same technology class in the same year (*Log(1+Citation)*). The explanatory variable of interests, *Treat*, is an indicator that equals one if a state has passed a SHB and zero otherwise. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1+Patent)			*Log(1+Citation)*		
Treat	-0.125***	-0.134***	-0.100***	-0.141***	-0.148***	-0.119***
	(-4.257)	(-4.571)	(-2.889)	(-4.925)	(-5.288)	(-3.356)
R&D		0.171**	0.186**		0.032	0.049
		(2.386)	(2.583)		(0.256)	(0.381)
Log (Asset)		0.098***	0.102***		0.064***	0.068***
		(3.727)	(3.889)		(4.308)	(4.865)
ROA		-0.067**	-0.057**		-0.078***	-0.067**
		(-2.644)	(-2.370)		(-2.876)	(-2.328)
PPE		0.264**	0.256**		0.265**	0.256**
		(2.057)	(2.022)		(2.115)	(2.075)
Leverage		-0.111**	-0.110**		-0.086**	-0.085**
		(-2.138)	(-2.153)		(-2.466)	(-2.404)
Capex		-0.312*	-0.244*		-0.397**	-0.320**
		(-2.006)	(-1.761)		(-2.665)	(-2.198)
TobinQ		0.013**	0.013**		0.005	0.006
		(2.436)	(2.429)		(0.873)	(0.906)
Log (Firm Age)		-0.015	-0.012		0.020	0.022
		(-0.770)	(-0.647)		(0.810)	(0.983)
HHI		0.029	0.037		0.046	0.057
		(0.136)	(0.167)		(0.169)	(0.202)
HHI^2		-0.118	-0.118		-0.123	-0.126
		(-0.627)	(-0.622)		(-0.482)	(-0.486)
Log (GDP)			0.212			0.648
			(0.427)			(1.086)
Log (Income)			-0.890**			-0.846*
			(-2.111)			(-1.740)
Log (Wage)			-0.165			-0.488
			(-0.324)			(-0.675)
Unemployment			0.004			0.004
			(0.812)			(0.624)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,765	16,765	16,765	16,765	16,765	16,765
R-squared	0.925	0.926	0.926	0.878	0.879	0.879

Table 4. Dynamic Analysis

This table reports the regression results that examine the dynamic impact of SHBs on firm innovation. The dependent variable is *Log(1+Patent)* in Column (1) and *Log(1+Citation)* in Column (2), respectively. We construct five dummy variables, which indicate the number of years relative to the enactment year of the SHBs. For example, the variable $Treat_{-1}$ ($Treat_{-2}$, $Treat_{-3}$, $Treat_{-4}$)indicates one year (two, three, or four years) before the passage of SHBs. $Treat_0$ equals one if the SHB is enacted in the current year. $Treat_1$ ($Treat_2$, $Treat_3$, $Treat_{4plus}$) indicates one year (two, three, four or more years) after the enactment of SHBs. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. We also control for firm and year fixed effects. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
$Treat_{-4}$	0.101	0.128
	(1.585)	(1.473)
$Treat_{-3}$	0.015	0.053
	(0.359)	(0.788)
$Treat_{-2}$	-0.050	0.002
	(-1.127)	(0.028)
$Treat_{-1}$	-0.060	-0.042
	(-1.286)	(-0.517)
$Treat_0$	-0.070	-0.053
	(-1.479)	(-0.677)
$Treat_1$	-0.111**	-0.091
	(-2.040)	(-1.131)
$Treat_2$	-0.144**	-0.133
	(-2.025)	(-1.462)
$Treat_3$	-0.278***	-0.284**
	(-3.013)	(-2.358)
$Treat_{4plus}$	-0.384***	-0.378***
	(-5.288)	(-3.741)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	16,765	16,765
R-squared	0.928	0.880

Table 5. Robustness Tests

This table reports the results of several robustness tests. In Panel A, we report the firm-state level results by considering the locations of research hubs within the same firm. In Panel B, we conduct difference-in-differences tests using firms located in states that are adjacent to treated states as our control sample. In Panel C, only keep treated firms in Massachusetts and control firms in states that never enact the SHB. In Panel D, we conduct placebo tests by examining the effects of the SHBs covering employees in the public sector on firm innovation. In Panel E, we focus on a subsample of firms that filed at least one patent during our sample period. In Panel F, we use the decile rank of *Patent* and *Citation* as the dependent variables. In Panel G, we repeat our analysis using propensity score or entropy-matched sample. In Panel H, we use the Callaway and Sant'Anna (2021) difference-in-differences estimators. In Panel I, we drop firms headquartered in states that ever have pay secret laws in Columns (1) and (2) and drop firms in states that passed pay secret laws after 2013 in Columns (3) and (4). All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Firm-state level analysis

	(1)	(2)	(3)	(4)
	Log(1+Patent)		*Log(1+Citation)*	
Treat	-0.035**	-0.052***	-0.074**	-0.072**
	(-2.321)	(-4.139)	(-2.388)	(-2.248)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No
Firm-State FE	No	Yes	No	Yes
Observations	29,605	29,605	29,605	29,605
R-squared	0.290	0.868	0.294	0.776

Panel B. Adjacent states

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	-0.130**	-0.157***
	(-2.734)	(-3.265)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	9,599	9,599
R-squared	0.919	0.877

Panel C. Evidence from the Massachusetts State

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	-0.123***	-0.164***
	(-5.553)	(-8.897)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	12,196	12,196
R-squared	0.926	0.865

Panel D. Placebo test using Public SHBs

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
TreatPub	-0.002	0.018
	(-0.045)	(0.440)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	16,765	16,765
R-squared	0.926	0.879

Panel E. Innovative subsample analysis

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	-0.103**	-0.110**
	(-2.227)	(-2.251)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	8,654	8,654
R-squared	0.897	0.850

Panel F. Decile ranking

	(1)	(2)
	Patent_Rank	*Citation_Rank*
Treat	-0.231***	-0.260***
	(-2.934)	(-3.278)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	16,765	16,765
R-squared	0.906	0.857

Panel G. Matched sample analysis

	(1)	(2)	(3)	(4)
	PSM		Entropy	
	Log(1+Patent)	*Log(1+Citation)*	*Log(1+Patent)*	*Log(1+Citation)*
Treat	-0.078*	-0.135**	-0.097**	-0.134***
	(-1.798)	(-2.345)	(-2.364)	(-2.886)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	5,341	5,341	16,765	16,765
R-squared	0.935	0.894	0.923	0.877

Panel H. CSDiD estimation

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Aggregated ATT	-0.073*	-0.129**
	(-1.650)	(-2.190)
Controls	Yes	Yes
Observations	15,719	15,719

Panel I. Drop states with pay secret laws

	(1)	(2)	(3)	(4)
	Drop states that ever have pay secret laws		Drop states that passed pay secret laws after 2013	
	Log(1+Patent)	*Log(1+Citation)*	*Log(1+Patent)*	*Log(1+Citation)*
Treat	-0.093**	-0.125***	-0.099***	-0.119***
	(-2.431)	(-2.778)	(-2.818)	(-3.253)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	10,935	10,935	16,165	16,165
R-squared	0.923	0.868	0.926	0.879

Table 6. Inventor Salary

This table reports the results of difference-in-differences regressions that examine the impact of the SHBs on inventor salary during 2013-2020. The sample is constructed at the individual-month level from CPS Outgoing Rotational Groups. The dependent variable, *Log(Salary)* is the natural logarithm of weekly income earned by each household member. The explanatory variable of interests is the interaction term between *Treat* and *Male*. *Treat* is an indicator that equals one if a state has passed a SHB and zero otherwise. *Male* is an indicator that equals one if the person is male and zero otherwise. The analyses in Columns (1) to (3) are conducted using the inventor sample, while the analyses in Columns (4) to (6) are conducted using the full household sample. Inventors are identified based on their occupation title which includes the following keywords: scientist(s), science, engineer(s), engineering, technician(s), or developer. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Inventor			All	
			Log (Salary)			
Treat	-0.008	0.079***	0.019	-0.000	0.033***	-0.020
	(-0.637)	(6.962)	(0.636)	(-0.061)	(4.392)	(-1.239)
Male		0.269***			0.259***	
		(31.754)			(36.148)	
Treat×Male		-0.113***	-0.053**		-0.058***	-0.014**
		(-6.829)	(-2.187)		(-5.541)	(-2.167)
Log (Age)	0.540***	0.517***	0.516***	0.500***	0.498***	0.498***
	(45.123)	(45.745)	(46.217)	(66.099)	(66.997)	(67.839)
College	0.346***	0.357***	0.358***	0.372***	0.394***	0.394***
	(19.291)	(20.683)	(20.715)	(20.051)	(22.086)	(22.099)
Postgrad	0.210***	0.207***	0.206***	0.325***	0.321***	0.321***
	(18.963)	(20.089)	(19.742)	(58.080)	(66.999)	(65.022)
Log (GDP)	-0.181	-0.210		-0.189	-0.154	
	(-0.685)	(-0.858)		(-1.675)	(-1.424)	
Log (Income)	-0.141	-0.124		-0.094	-0.094	
	(-0.511)	(-0.476)		(-0.759)	(-0.762)	
Log (Wage)	0.324	0.373		0.414***	0.380***	
	(1.165)	(1.454)		(3.435)	(3.236)	
Unemployment	-0.012	-0.009		0.004	0.004	
	(-0.781)	(-0.645)		(1.311)	(1.239)	
Race Fe	Yes	Yes	Yes	Yes	Yes	Yes
Job Status Fe	Yes	Yes	Yes	Yes	Yes	Yes
State Fe	Yes	Yes	No	Yes	Yes	No
Year Fe	Yes	Yes	No	Yes	Yes	No
Month Fe	Yes	Yes	Yes	Yes	Yes	Yes
State-year Fe	No	No	Yes	No	No	Yes
Male-state Fe	No	No	Yes	No	No	Yes
Male-Year Fe	No	No	Yes	No	No	Yes
Observations	42,329	42,329	42,329	523,902	523,902	523,902
R-squared	0.204	0.235	0.244	0.219	0.254	0.257

Table 7. Inventor Productivity

This table reports the regression results that examine the impact of SHBs on the productivity of individual inventors. The sample is constructed at the inventor level. The dependent variable in Columns (1) to (3) is the natural logarithm of one plus the number of patents an inventor applied for and were subsequently granted in a year (*Log(1+Patent)*). The dependent variable in Columns (3) to (6) is the natural logarithm of one plus the total number of citations received by an inventor's patents in a year adjusted by the average number of citations received by all patents in the same technology class (*Log(1+Citation)*). *Male* indicates whether the inventor is male or not. *Treat* is an indicator that equals one if a state has passed a SHB and zero otherwise. In Columns (1), (2), (4), and (5), we control for firm, state, year, and inventor fixed effects. In Columns (3) and (6), we control for firm-state, year, and inventor fixed effects. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Log(1+Patent)			*Log(1+Citation)*	
Treat	-0.012*	-0.005	-0.005	-0.035***	-0.011	-0.011
	(-1.845)	(-0.730)	(-0.731)	(-2.841)	(-0.970)	(-0.971)
Treat×Male		-0.004***	-0.004***		-0.023***	-0.023***
		(-2.758)	(-2.762)		(-6.118)	(-6.126)
R&D	-0.036	-0.012	-0.012	-0.428***	-0.414***	-0.414***
	(-1.014)	(-0.355)	(-0.355)	(-3.399)	(-3.309)	(-3.314)
Log (Asset)	-0.001	-0.000	-0.000	-0.083***	-0.080***	-0.080***
	(-0.170)	(-0.001)	(-0.001)	(-8.158)	(-7.657)	(-7.666)
ROA	0.042**	0.051**	0.051**	0.035	0.044	0.044
	(2.098)	(2.380)	(2.383)	(0.801)	(0.987)	(0.989)
PPE	0.101	0.116	0.116	-0.057	-0.050	-0.050
	(1.276)	(1.411)	(1.413)	(-0.409)	(-0.351)	(-0.352)
Leverage	-0.094***	-0.093***	-0.093***	-0.170***	-0.165***	-0.165***
	(-3.965)	(-3.876)	(-3.881)	(-4.364)	(-4.250)	(-4.255)
Capex	0.004	0.003	0.003	0.535**	0.544**	0.544**
	(0.029)	(0.022)	(0.022)	(2.692)	(2.555)	(2.558)
TobinQ	0.004***	0.005***	0.005***	0.000	0.001	0.001
	(5.171)	(5.191)	(5.197)	(0.063)	(0.248)	(0.248)
Log (Firm Age)	-0.020**	-0.022**	-0.022**	-0.124***	-0.122***	-0.122***
	(-2.527)	(-2.523)	(-2.526)	(-5.621)	(-5.750)	(-5.757)
HHI	0.117	0.129	0.129	0.047	0.054	0.054
	(1.112)	(1.149)	(1.150)	(0.252)	(0.290)	(0.291)
HHI^2	-0.105	-0.115	-0.115	-0.068	-0.073	-0.073
	(-1.248)	(-1.278)	(-1.280)	(-0.472)	(-0.499)	(-0.500)
Log (GDP)	0.033	0.022	0.022	-0.368	0.008	0.008
	(0.321)	(0.237)	(0.237)	(-1.594)	(0.035)	(0.035)
Log (Income)	-0.090	-0.095	-0.095	-0.209	-0.241*	-0.241*
	(-1.506)	(-0.838)	(-0.839)	(-1.643)	(-1.801)	(-1.804)
Log (Wage)	-0.097	-0.048	-0.048	0.092	-0.301*	-0.301*
	(-0.967)	(-0.305)	(-0.306)	(0.528)	(-1.750)	(-1.752)
Unemployment	0.001	0.001	0.001	-0.000	0.000	0.000
	(0.716)	(1.292)	(1.294)	(-0.146)	(0.157)	(0.157)
Firm FE	Yes	Yes	No	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	Yes	Yes	No
Inventor FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-state FE	No	No	Yes	No	No	Yes
Observations	610,906	610,906	610,906	610,906	610,906	610,906
R-squared	0.563	0.583	0.583	0.545	0.545	0.545

Table 8. Inventor Mobility

The table reports the regression results that examine the effect of the SHBs on inventor mobility. In Panel A, we examine the impact of the SHBs on the total number of inventors (*Log(1+ Inventor)*), the number of female inventors (*Log(1+Female Inventor)*), and male inventors (*Log(1+Male Inventor)*) in each firm. In Panel B, we examine the percentage of inventors that move in (*%Move In*), female inventors that move in (*%Female Move In*), or male inventors that move in (*%Male Move In*) a firm in a year. The explanatory variable of interests, *Treat*, is an indicator that equals one if a state has passed a SHB and zero otherwise. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Inventor Composition

	(1) *Log(1+ Inventor)*	(2) *Log(1+Female Inventor)*	(3) *Log(1+Male Inventor)*
Treat	-0.153**	-0.074**	-0.150**
	(-2.399)	(-2.448)	(-2.394)
R&D	0.058	0.036	0.032
	(0.343)	(0.471)	(0.161)
Log (Asset)	0.041**	0.021*	0.042**
	(2.270)	(1.725)	(2.368)
ROA	-0.110***	-0.036	-0.116***
	(-2.762)	(-1.318)	(-3.259)
PPE	0.528***	0.257**	0.517***
	(2.781)	(2.481)	(2.728)
Leverage	-0.090	-0.036	-0.084
	(-1.625)	(-1.024)	(-1.548)
Capex	-0.938***	-0.410***	-0.842***
	(-4.517)	(-5.246)	(-3.830)
TobinQ	-0.027***	-0.011***	-0.028***
	(-5.005)	(-4.021)	(-5.145)
Log (Firm Age)	0.111***	0.083***	0.095***
	(3.880)	(6.780)	(3.692)
HHI	0.596**	0.296**	0.599**
	(2.318)	(2.180)	(2.391)
HHI^2	-0.475**	-0.249*	-0.472**
	(-2.056)	(-1.895)	(-2.095)
Log (GDP)	-0.404	-0.455	-0.281
	(-0.271)	(-0.655)	(-0.186)
Log (Income)	-3.822**	-1.983**	-3.527**
	(-2.517)	(-2.552)	(-2.387)
Log (Wage)	1.682	1.118	1.446
	(0.965)	(1.437)	(0.812)
Unemployment	-0.015*	-0.008*	-0.014*
	(-1.830)	(-1.873)	(-1.921)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	16,765	16,765	16,765
R-squared	0.838	0.830	0.827

Panel B. Inventor Flow

	(1) *%Move In*	(2) *%Female Move In*	(3) *%Male Move In*
Treat	-0.005***	-0.001**	-0.004***
	(-3.061)	(-2.369)	(-2.754)
R&D	0.011	0.000	0.010
	(1.083)	(0.120)	(0.860)
Log (Asset)	0.003**	0.000	0.003**
	(2.277)	(0.009)	(2.683)
ROA	-0.000	-0.002	0.001
	(-0.010)	(-1.433)	(0.271)
PPE	0.007	0.001	0.006
	(0.796)	(0.561)	(0.787)
Leverage	-0.002	-0.001	-0.001
	(-0.580)	(-0.895)	(-0.354)
Capex	-0.018	-0.003	-0.015
	(-0.988)	(-1.396)	(-0.845)
TobinQ	0.000	-0.000	0.000
	(1.085)	(-0.221)	(1.489)
Log (Firm Age)	-0.000	-0.001	0.000
	(-0.182)	(-1.045)	(0.211)
HHI	0.005	0.001	0.004
	(0.252)	(0.221)	(0.190)
HHI^2	-0.002	-0.002	-0.000
	(-0.086)	(-0.477)	(-0.016)
Log (GDP)	-0.017	0.002	-0.018
	(-0.431)	(0.497)	(-0.474)
Log (Income)	-0.034	-0.005	-0.029
	(-1.002)	(-0.899)	(-0.895)
Log (Wage)	-0.004	-0.005	0.001
	(-0.091)	(-1.321)	(0.011)
Unemployment	-0.000	-0.000	-0.000
	(-0.727)	(-1.280)	(-0.506)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	16,765	16,765	16,765
R-squared	0.232	0.171	0.230

Table 9. Newly Hired Inventors

This table reports the regression results that examine the effect of the SHBs on the quality of newly hired inventors. An inventor is identified as a newly hired inventor in a firm-year if he/she is in the first year of joining the firm. We use the natural logarithm of the number of patents (*Log(1+Past Patent)*) and their citations (*Log(1+Past Citation*) obtained by newly hired inventors in the past (i.e., from the first patent to joining the firm) to measure their quality. The explanatory variable of interest is the *Treat*. *Treat* is an indicator that equals one if a state has passed a SHB and zero otherwise. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log (1+Past Patent)			*Log (1+Past Citation)*		
	All	Female	Male	All	Female	Male
Treat	-0.041**	-0.022	-0.041**	-0.058**	-0.027*	-0.061***
	(-2.107)	(-1.494)	(-2.155)	(-2.605)	(-1.891)	(-2.885)
R&D	-0.001	-0.011	-0.003	-0.150	0.009	-0.142
	(-0.003)	(-0.167)	(-0.009)	(-0.497)	(0.110)	(-0.496)
Log (Asset)	0.119***	0.020	0.114***	0.109***	0.023	0.104***
	(3.988)	(0.958)	(4.357)	(3.502)	(1.103)	(3.642)
ROA	-0.137	-0.026	-0.135	-0.148	-0.033	-0.136
	(-1.243)	(-1.299)	(-1.186)	(-1.295)	(-1.286)	(-1.143)
PPE	0.139	0.078	0.129	0.143	0.085	0.125
	(1.137)	(1.180)	(1.123)	(1.475)	(1.369)	(1.404)
Leverage	-0.105*	-0.064**	-0.094	-0.082	-0.070**	-0.066
	(-1.826)	(-2.470)	(-1.512)	(-1.463)	(-2.384)	(-1.120)
Capex	-0.151	-0.083	-0.067	-0.159	-0.053	-0.075
	(-1.495)	(-1.369)	(-0.619)	(-1.344)	(-0.841)	(-0.621)
TobinQ	-0.012*	-0.010**	-0.010	-0.013*	-0.008**	-0.012
	(-1.980)	(-2.593)	(-1.580)	(-1.980)	(-2.056)	(-1.557)
Log (Firm Age)	-0.101***	-0.033**	-0.094***	-0.100***	-0.035**	-0.087***
	(-4.180)	(-2.681)	(-3.846)	(-3.619)	(-2.537)	(-3.120)
HHI	0.116	0.005	0.101	0.142	0.027	0.133
	(0.402)	(0.052)	(0.348)	(0.450)	(0.233)	(0.413)
HHI^2	-0.139	-0.005	-0.124	-0.217	-0.032	-0.203
	(-0.504)	(-0.064)	(-0.442)	(-0.704)	(-0.361)	(-0.647)
Log (GDP)	-0.115	-0.119	-0.133	-0.575	-0.220	-0.585
	(-0.167)	(-0.451)	(-0.200)	(-0.709)	(-0.791)	(-0.740)
Log (Income)	-0.331	0.262	-0.395	-0.440	0.250	-0.522
	(-0.692)	(1.009)	(-0.952)	(-0.771)	(1.193)	(-1.034)
Log (Wage)	-0.113	-0.210	-0.040	0.320	-0.125	0.396
	(-0.194)	(-0.691)	(-0.073)	(0.469)	(-0.448)	(0.609)
Unemployment	0.004	-0.002	0.005	0.005	-0.002	0.006
	(0.879)	(-0.571)	(0.954)	(0.988)	(-0.786)	(1.341)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,765	16,765	16,765	16,765	16,765	16,765
R-squared	0.797	0.618	0.796	0.777	0.595	0.775

Table 10. Heterogenous Treatment Effect

This table reports the regression results that examine the heterogeneous effect of the SHBs on firm innovation. The dependent variable is *Log(1+Patent)* or *Log(1+Citation)*. In Panel A, we examine the impact of the inventor turnover rate. The explanatory variable of interest is the interaction term between *Treat* and *Turnover rate*. *Turnover rate* equals the total number of inventors that move in and out of the firm in a year divided by the total number of inventors. *Treat* is an indicator that equals one if a state has passed a SHB and zero otherwise. In Panel B, we examine the impact of female representation. The explanatory variable of interest is the interaction term between *Treat* and *%Female Director*. *%Female Director* is the fraction of female directors on the board in a firm each year. In Panel C, we examine the impact of inventor income. *Log(1+Inventor Tenure)* is the natural logarithm of one plus the average inventor tenure for a firm in a year. Inventor tenure is defined as the number of years since the year of an inventor's first patent filing. *Star Inventor* is the percentage of inventors that are classified as star inventors for a firm in a year. Star inventor is defined as the top 95% of inventors according to patent filings and citations for a year. In Panel D, we examine a state's attitude toward voluntary salary disclosure. *Ban Voluntary* equals one for states that do not allow employers to use the salary information disclosed by job applicants voluntarily, and zero otherwise. From Panels A-C, we only include firm-years with at least one patent application in our analysis. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Inventor turnover rate

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	0.003	-0.036
	(0.082)	(-0.980)
Turnover Rate	-0.069***	-0.064*
	(-2.784)	(-1.799)
Treat×Turnover Rate	-0.185***	-0.090*
	(-2.992)	(-1.690)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	8,615	8,615
R-squared	0.937	0.899

Panel B. Female representation

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	-0.085***	-0.110**
	(-3.369)	(-2.968)
%Female Director	-0.136*	-0.127
	(-1.902)	(-1.078)
Treat×%Female Director	0.282**	0.369*
	(2.463)	(1.991)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	8,698	8,698
R-squared	0.940	0.897

Panel C. Inventor seniority

	(1)	(2)	(3)	(4)
	Log(1+Patent)	*Log(1+Citation)*	*Log(1+Patent)*	*Log(1+Citation)*
Treat	0.023	0.060	-0.040	-0.055
	(0.575)	(1.232)	(-1.255)	(-1.533)
Log(1+Inventor Tenure)	-0.018	-0.021		
	(-1.465)	(-1.180)		
Treat × Log(1+Inventor Tenure)	-0.038***	-0.070***		
	(-2.991)	(-5.161)		
%Star Inventor			0.438***	1.354***
			(4.765)	(7.498)
Treat×%Star Inventor			-0.262***	-1.244***
			(-2.924)	(-7.362)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	8,615	8,615	8,615	8,615
R-squared	0.939	0.901	0.939	0.901

Panel D. Ban on voluntary disclosure

	(1)	(2)
	Log(1+Patent)	*Log(1+Citation)*
Treat	-0.043	-0.067
	(-1.235)	(-1.707)
Treat × Ban Voluntary	-0.069**	-0.059*
	(-2.561)	(-2.071)
Controls	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Observations	16,765	16,765
R-squared	0.927	0.880

Table 11. Alternative Innovation Outcomes

This table reports the results of the impact of SHBs on firm innovation beyond patent and citation. *Pat/Emp* is the number of patents scaled by the number of employees. *Cit/Emp* is the number of citations scaled by the number of employees in a firm. *Cit/Pat* is the average citations received by each patent. *Log(1+ImpactPatent)* is the natural logarithm of one plus the total number of a firm's impactful patents. *Log(1+ValuablePatent)* is the natural logarithm of one plus the total number of a firm's valuable patents. *Log(1+Orig)* is the natural logarithm of one plus the sum of originality scores of patents applied by a firm. *Log(1+Gen)* is the natural logarithm of one plus the sum of the generality scores of patents applied by a firm. *Log(1+ExploitPatent)* is the natural logarithm of one plus the number of exploitative patents. *Log(1+ExplorePatent)* is the natural logarithm of one plus the number of explorative patents. All baseline controls from Table 3 are included in regressions, whose coefficients are not reported for brevity. Detailed variable definitions are provided in Appendix A. All continuous variables are winsorized at 1% and 99% levels. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pat/Emp	*Cit/Emp*	*Cit/Pat*	*Log (1+ImpactPatent)*	*Log (1+ValuablePatent)*	*Log (1+Orig)*	*Log (1+Gen)*	*Log (1+ExploitPatent)*	*Log (1+ExplorePatent)*
Treat	-3.904***	-5.170***	-0.082***	-0.047**	-0.031**	-0.205***	-0.140***	-0.006	-0.017**
	(-4.257)	(-4.500)	(-3.458)	(-2.337)	(-2.100)	(-3.770)	(-4.480)	(-1.112)	(-2.134)
R&D	-1.999	-13.125**	-0.153	0.048	-0.063**	0.012	-0.076	0.085	-0.031
	(-0.846)	(-2.297)	(-1.269)	(0.687)	(-2.147)	(0.154)	(-0.862)	(1.229)	(-0.722)
Log (Asset)	-2.358**	-2.496**	0.009	0.010	-0.008	-0.004	-0.024**	0.021**	0.005
	(-2.565)	(-2.184)	(0.607)	(1.385)	(-0.992)	(-0.302)	(-2.141)	(2.270)	(1.064)
ROA	0.416	-0.918	-0.048*	0.001	0.003	0.020	0.004	-0.006	-0.005
	(0.342)	(-0.710)	(-1.894)	(0.036)	(0.193)	(0.684)	(0.337)	(-0.576)	(-0.477)
PPE	0.944	1.542	0.067	0.124**	0.066	0.281**	0.103*	0.026	0.009
	(0.360)	(0.666)	(0.573)	(2.184)	(1.356)	(2.105)	(1.968)	(1.324)	(0.484)
Leverage	-2.355*	-1.449	-0.054	-0.032	0.023	-0.006	-0.007	-0.020	-0.003
	(-1.894)	(-1.275)	(-1.199)	(-1.529)	(0.930)	(-0.211)	(-0.267)	(-1.402)	(-0.203)
Capex	-11.142*	-10.968*	0.015	-0.170***	-0.123**	-0.415**	-0.199**	-0.031	0.039
	(-2.017)	(-1.803)	(0.126)	(-3.127)	(-2.062)	(-2.684)	(-2.019)	(-0.581)	(0.720)
TobinQ	0.184*	-0.047	-0.004	-0.003*	-0.001	-0.021***	-0.014**	0.003*	0.004***
	(1.769)	(-0.324)	(-1.008)	(-1.736)	(-0.703)	(-4.755)	(-2.615)	(1.897)	(2.999)
Log (Firm Age)	-4.527***	-4.681***	-0.045	0.029***	0.087***	0.229***	0.111***	-0.012***	-0.016*
	(-3.582)	(-3.957)	(-1.426)	(2.800)	(6.243)	(7.266)	(6.025)	(-2.963)	(-1.909)
HHI	2.656	3.565	-0.042	-0.041	0.099	0.104	-0.075	-0.041	0.047
	(0.542)	(0.558)	(-0.174)	(-0.291)	(0.827)	(0.404)	(-0.653)	(-0.797)	(0.759)
HHI^2	-4.006	-4.346	0.017	0.027	-0.068	0.013	0.140	0.016	-0.023
	(-0.988)	(-0.679)	(0.067)	(0.189)	(-0.652)	(0.053)	(1.136)	(0.336)	(-0.423)
Log (GDP)	-7.470	-16.059	0.540	-0.205	0.113	0.663	0.394	-0.027	0.013

	(-0.746)	(-1.118)	(0.945)	(-0.564)	(0.356)	(0.856)	(0.790)	(-0.187)	(0.085)
Log (Income)	-26.058**	-33.729**	-0.426	-0.762*	-0.458	-1.849*	-1.072**	-0.109	-0.019
	(-2.464)	(-2.066)	(-1.020)	(-1.839)	(-1.155)	(-2.018)	(-2.295)	(-0.906)	(-0.133)
Log (Wage)	-4.230	5.190	-0.569	0.303	0.171	-0.020	-0.219	0.009	-0.091
	(-0.317)	(0.272)	(-0.846)	(0.794)	(0.419)	(-0.025)	(-0.490)	(0.041)	(-0.508)
Unemployment	-0.059	-0.061	0.003	0.002	0.000	0.000	0.003	0.001	0.001
	(-0.599)	(-0.796)	(0.682)	(1.153)	(0.234)	(0.009)	(1.110)	(0.789)	(1.141)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,429	16,429	16,765	16,765	16,765	16,765	16,765	16,765	16,765
R-squared	0.781	0.709	0.600	0.857	0.863	0.747	0.598	0.667	0.572

Appendix A. Variable Definition

Variable	Definition
Innovation measures	
Log(1+Patent)	The natural logarithm of one plus the number of patents that a firm applied for and were subsequently granted in a year.
Log(1+Citation)	The natural logarithm of one plus the number of citations received by all patents applied by a firm in a year adjusted by the average number of citations received by all patents in the same technology class in the same year. The technology class is defined according to the 3-digit Cooperative Patent Classification system.
Pat/Emp	The number of patents scaled by the number of employees in a firm.
Cit/Emp	The number of technology class adjusted citations scaled by the number of employees in a firm.
Cit/Pat	The average technology class adjusted citations received by each patent.
Log(1+ImpactPatent)	The natural logarithm of one plus the total number of a firm's impactful patents. Impactful patents are defined as the top 10% most cited patents within a given technology class and application year. The technology class is defined according to the 3-digit Cooperative Patent Classification system.
Log(1+ValuablePatent)	The natural logarithm of one plus the total number of a firm's valuable patents. Valuable patents are defined as the top 10% patents in terms of their economic values within a given technology class and application year. The technology class is defined according to the 3-digit Cooperative Patent Classification system. The economic value of each patent is from Kogan et al., (2017)
Log(1+Orig)	The natural logarithm of one plus the sum of originality scores of patents applied by a firm in a year. The originality score is defined as one minus the Herfindahl index of the technology class distribution of all patents that have been cited by the focal patent.
Log(1+Gen)	The natural logarithm of one plus the sum of generality scores of patents applied by a firm in a year. The generality score is defined as one minus the Herfindahl index of the technology class distribution of all patents that have cited the focal patent.
Log(1+ExploitPatent)	The natural logarithm of one plus the number of exploitative patents. A patent is defined as an exploitative one if more than 80% of its backward citations are based on a firm's existing knowledge (i.e., citing the firm's existing patents or the citations made by its existing patents).
Log(1+ExplorePatent)	The natural logarithm of one plus the number of explorative patents. A patent is defined as an explorative one if more than 80% of its backward citations are based on new knowledge outside of a firm's existing expertise (i.e., not citing the firm's existing patents or the citations made by its existing patents).
Log(1+Past Patent)	The natural logarithm of one plus the total number of patents that a newly hired inventor has filed before he joined the firm.
Log(1+Past Citation)	The natural logarithm of one plus the total number of citations that a newly hired inventor received before he joined the firm.
Firm Characteristics	
Treat	An indicator that equals one if a firm's headquarter state has passed the Salary History Ban that covers all private employees.
TreatPub	An indicator that equals one if a firm's headquarter state has passed the Salary History Ban that covers public employees.
R&D	Research and development expense scaled by total assets. If the item of R&D expense is missing, we set its value to zero.
Log (Asset)	The natural logarithm of total assets (in millions of dollars).
ROA	Net income scaled by total assets.
PPE	Plant, property, and equipment scaled by total assets.
Leverage	Long-term debt scaled by total assets.
Capex	Capital expenditure scaled by total assets.
TobinQ	Market value of assets scaled by total asset.
Log (Firm Age)	The natural logarithm of one plus the number of years since a firm is covered by the CRSP database.
HHI	Herfindahl-Hirschman Index of firm sales in an industry (three-digit SIC).
HHI^2	The square of HHI.

Log(1+Inventor)	The natural logarithm of one plus the number of inventors in a firm in a year.
Log(1+Female Inventor)	The natural logarithm of one plus the number of female inventors in a firm in a year.
Log(1+Male Inventor)	The natural logarithm of one plus the number of male inventors in a firm in a year.
%Move In	The number of inventors that move in a firm scaled by the total number of inventors in a firm.
%Female Move In	The number of female inventors that move in a firm scaled by the total number of inventors in a firm.
%Male Move In	The number of male inventors that move in a firm scaled by the total number of inventors in the firm.
Turnover Rate	The number of inventors that move in a firm plus the number of inventors that move out of a firm, divided by the total number of inventors in a year.
%Female Director	The number of female directors on the board scaled by the total number of directors.
Log (1+Inventor Tenure)	The natural logarithm of one plus the average inventor tenure for a firm in a year. Inventor tenure is defined as the number of years since the year of an inventor's first patent filing.
%Star Inventor	The percentage of inventors that are classified as star inventors for a firm in a year. Star inventor is defined as the top 5% inventors according to patent filings and citations for a year.
State characteristics	
Ban Voluntary	An indicator that equals one for states that do not allow employers to use the salary information disclosed by job applicants voluntarily, and zero otherwise.
Log (GDP)	The natural logarithm of the GDP in a state in a year.
Log (Income)	The natural logarithm of per capita income in a state in a year.
Log (Wage)	The natural logarithm of the average wage in a state in a year.
Unemployment	The unemployment rate in a state in a year.
Individual characteristics	
Log (Salary)	The natural logarithm of weekly salary earned by each household member.
Male	An indicator that equals one if the person is male, and zero otherwise.
Log (Age)	The natural logarithm of the age of the household member.
College	An indicator that equals one if the person obtained a bachelor's degree, and zero otherwise.
Postgrad	An indicator that equals one if the person obtained a postgraduate degree, and zero otherwise.

Appendix B. Timing of State Adoption of SHBs

This table estimates a Cox proportional hazard model where the "failure event" is the adoption of SHBs in a given state. *Avg_Log(1+Patent)* is the average *Log(1+Patent)* across all firms headquartered in a state. *Avg_Log(1+Citation)* is the average *Log(1+Citation)* across all firms headquartered in a state. Robust t-statistics, adjusted for state-level clustering, are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Cox proportional hazard model Failure event	
Avg_Log(1+Patent)	0.527	
	(0.474)	
Avg_Log(1+Citation)		0.815
		(0.922)
Log (GDP)	3.584	3.510
	(0.930)	(1.006)
Log (Income)	6.286***	6.359***
	(3.594)	(3.793)
Log (Wage)	-3.122	-3.059
	(-0.816)	(-0.874)
Unemployment	-0.305	-0.306
	(-1.129)	(-1.173)
Observations	276	276
Wald Chi-squared	18.36	20.21

Appendix C. List of States with Salary History Ban Only Concerning the Public Sector

This table lists the dates of the passage of the salary history ban covering only the public sector across states between 2016 and 2018. The information is obtained from: https://www.hrdive.com/news/salary-history-ban-states-list/516662/.

State name	State code	Passage date
District of Columbia	DC	November 17, 2017
Pennsylvania	PA	June 6, 2018
Illinois	IL	January 15, 2019
North Carolina	NC	April 2, 2019
Virginia	VA	June 20, 2019