Employee Peer Pay Inequality and Innovation: Evidence from Salary History Bans

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Abstract

We distinguish horizontal (peer-level) employee pay dispersion from vertical pay dispersion across hierarchical levels and examine its impact on innovation. Using the staggered adoption of state-level salary history bans, we show that the laws significantly reduce horizontal pay dispersion but not vertical dispersion. Treated firms experience an 11–16% increase in patent value. The effects are stronger in firms where innovation is more sensitive to horizontal inequality, and policy exposure and hiring are greater. Post-treatment, firms experience greater employee retention, inventor productivity, and team collaboration and performance. Our findings suggest that mitigating pay inequality among employee peers improves innovation.

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1. Introduction

Economic theory offers conflicting predictions on the effects of pay inequality. On the one hand, tournament models argue that larger pay gaps strengthen incentives by rewarding relative performance (Lazear and Rosen, 1981). Assignment and superstar models likewise argue that wider pay differences facilitate the efficient allocation and retentional of talent when returns to talent are highly nonlinear (Rosen, 1981; Gabaix and Landier, 2008). Consistent with this view, studies document a positive relation between within-firm pay inequality and firm outcomes (e.g., Kale, Reis, and Venkateswaran, 2009; Mueller, Ouimet, and Simintzi, 2017; Wallskog et al., 2026).

In contrast, equity and fair-wage theories emphasize the costs of pay inequality. Workers compare themselves to similar peers, and perceived unfairness can reduce effort or increase exit (Adams, 1963; Akerlof and Yellen, 1990). When output depends on collaboration and non-contractible effort, rewarding individual performance too strongly may dissuade cooperation (Holmström and Milgrom, 1991). Empirical evidence supports these concerns. Field experiments show that workers respond negatively to pay differences relative to coworkers with similar jobs (e.g., Breza, Kaur, and Shamdasani, 2018). Firms reporting a high CEO-to-median employee pay ratio experience a negative market reaction (Pan et al., 2022) and lower employee job satisfaction (Boone, Starkweather and White, 2023).

A potential reconciliation of these conflicting theories and empirical findings is a distinction between pay inequality among peers (horizontal pay dispersion), which is more likely to affect fairness perceptions and collaboration, and pay inequality across hierarchy levels (vertical pay dispersion), which strengthens tournament incentives and rewards for effort and promotion. A few recent studies document the importance of this distinction. Cullen and Perez-Truglia (2022)

through a field experiment show that employees tend to reduce effort when informed that co-workers earn more and increase effort when they learn that managers, that is supervisors, earn more than expected. Kacperczyk and Balachandran (2018) use Swedish data to show that vertical pay dispersion reduces cross-firm employee mobility while horizontal pay dispersion increases employee mobility. Comparable evidence for U.S. firms is limited due to the lack of comprehensive employee-level compensation data, especially for measuring horizontal pay dispersion within firms. Moreover, because pay dispersion is endogenously determined, existing evidence primarily documents associations.

In this paper, we distinguish between vertical and horizontal pay dispersion and examine their effects on innovation in U.S. firms. Innovation provides a particularly suitable setting for studying this distinction because successful innovation depends on both sustained individual effort and collaboration among employee peers. To identify causal effects, we exploit the staggered adoption of state-level salary history bans between 2017 and 2021, which should reduce horizontal pay dispersion while leaving vertical pay dispersion largely unchanged. Using employee-level compensation data, we first confirm these predicted effects of the bans on pay dispersion. We then use firms' exposure to the bans to provide evidence on the effect of reducing peer pay inequality on innovation outcomes.

Salary history bans prohibit employers from requesting or relying on applicants' prior salary information during hiring and compensation negotiations. By preventing firms from anchoring salary to the applicant's prior salary, the laws increase reliance on job level pay scales and harmonize wages among workers in the same rank, reducing horizontal pay dispersion. As salary history bans do not alter job hierarchies or promotion structures, they leave vertical pay dispersion largely unchanged. Adoption of salary history bans by states therefore serve as

exogenous shocks to horizontal pay dispersion. Using a stacked difference-in-differences (DiD) framework with firm-by-cohort and year-by-cohort fixed effects, we compare firms with meaningful workforce located in treated jurisdictions to control firms.

To assess the effects of salary history bans on pay dispersion, we first construct measures of horizontal and vertical pay dispersion from employee-position-level compensation data provided by Revelio Labs for U.S. public firms from 2005 to 2023. The dataset includes more than 57 million position-level observations for approximately 29 million unique individuals across nearly 8,139 publicly listed U.S. firms. Using the Gini coefficient, a standard measure of income dispersion widely used in both economics and finance (e.g., Yitzhaki and Lerman, 1991; Mueller, Ouimet, and Simintzi, 2017), we separately construct within-seniority-level and between-seniority-level measures of horizontal and vertical pay dispersion, respectively.

Revelio Labs estimates compensation at the individual position level by combining observable firm- and position-specific salary information from job postings, H-1B visa applications, and other public available sources with employee-specific characteristics such as tenure, experience, and location. Although the estimated salaries may not exactly match actual wages, the model is intended to capture differences in pay across firms, hierarchical levels, positions, and employees in similar positions. To assess whether the Revelio compensation data preserve the distributional characteristics necessary for measuring within-firm pay dispersion, we conduct a series of validation exercises. Specifically, we show that the distribution and relative ranking of compensation in Revelio are highly consistent with those documented using U.S Social Security data (Song et al., 2019), ExecuComp, and median employee salary reported in EDGAR proxy filings.

Using the stacked DiD design, we find that exposure to salary history bans significantly reduces horizontal pay dispersion while having no significant effect on vertical pay dispersion. Within-level Gini coefficients decline significantly following policy adoption, whereas between-level dispersion does not change significantly. These results hold across different pay dispersion measures and are robust to alternate strategies for identifying treated firms.

Turning to innovation outcomes, firms exposed to salary history bans experience significantly improved innovation performance. Specifically, patent market value increases by 11 to 16 percent within two years of policy adoption. Patent counts also increase, though the effect is stronger for patent value, suggesting larger improvements in innovation quality. We also observe broader improvements in firm outcomes. Knowledge capital, defined as the capitalized value of accumulated R&D expenditures following Peters and Taylor (2017), increases by roughly 5 percent. Market-based measures such as Tobin's Q also rise meaningfully. These findings suggest that the innovation gains are economically significant and translate into higher intangible capital and firm valuation.

We next connect the reduction in horizontal pay dispersion to the observed innovation gains by examining heterogenous treatment effects. We first examine pre-event firm characteristics that should make innovation more sensitive to peer pay inequality. The innovation gains are stronger among firms with higher pre-treatment horizontal pay dispersion, smaller workforces, higher R&D intensity, and greater importance of teamwork. These patterns suggest that the effects of peer-pay compression are largest in environments where employee effort and collaboration are more important for innovative production. We then examine post-event treatment intensity measures that capture the extent to which salary history bans affect wage-setting within firms. The innovation gains are stronger among firms with greater workforce exposure to adopting

states, dual exposure to pay transparency laws as well as salary history bans, and more newly filled positions following policy adoption. The effects are also stronger when newly filled positions contain a larger share of minority employees and, to a lesser extent, female employees. Together, these patterns reinforce the interpretation that the increase in innovation is closely tied to wage-setting in new roles and the resulting horizontal pay compression, rather than to unrelated confounding factors.

We then examine employee- and team-level outcomes to provide more direct evidence on the mechanisms through which reductions in horizontal pay dispersion improve innovation. Because peer-level inequality operates directly on employees, its effects should be visible in workforce stability, individual effort, and collaborative activity. Following policy adoption, employee turnover declines, including among high-skill workers who are central to innovation. We also find that patent applications per inventor increase, suggesting higher individual effort and productivity. At the same time, inventor teams become larger, the number of inventors increases, patent output per team rises, and employee sentiment toward internal communication improves. These patterns suggest not only improved employee retention and effort but also stronger collaboration and joint effort within teams.

Finally, we conduct a series of robustness analyses to further support a causal interpretation of our findings. Parallel trend analysis shows no evidence of differential pre-trends in pay dispersion, innovation, or other outcomes. Results are robust to alternative difference-in-differences estimators (Callaway and Sant'Anna, 2021), alternative treatment definitions, and instrumental variables specifications that use exposure intensity to instrument for horizontal dispersion. We also conduct a placebo test by randomly assigning treated status while preserving the firm-by-cohort treatment structure. Across 2,000 simulations, the placebo estimates are

centered around zero and fail to generate significant changes in either pay dispersion or patent outcomes. Together, these tests alleviate concerns that our findings are driven by confounding state-level trends, biases arising from staggered policy adoption, or spurious correlations in the stacked DiD sample.

Our paper contributes to the literature on within-firm pay inequality by examining an exogenous shock to peer pay inequality and its effect on innovation and firm value. Much of the existing finance literature focuses on vertical pay gaps, particularly dispersion between executives and lower-level employees. We complement this line of research by shifting the focus to peer pay inequality among employees and showing that it has fundamentally different implications. While vertical pay dispersion may strengthen incentives through promotion and tournament effects, peer pay inequality can impede innovation by weakening employee retention, effort, and collaboration.

Our findings also add to the literature on labor market regulation. A growing body of research examines how labor laws and employment regulations shape firm behavior, productivity, and innovation. For example, Autor, Kerr, and Kugler (2007) and Acharya, Baghai, and Subramanian (2014) show that labor market institutions affect firm performance through changes in hiring practices, risk-taking, and innovation incentives. More recently, Gao, Hsu, and Zhang (2025) demonstrate that state-level pay secrecy laws increase inventor productivity by enhancing pay transparency within firms. We complement this line of research by examining salary history bans, which operate through the distinct channel of reducing employee peer pay inequality. Our findings highlight the internal structure of pay dispersion as a novel mechanism through which labor market regulation can influence firm outcomes.

Lastly, we contribute to the literature on innovation and incentive design. Prior work shows that compensation structures, including pay-for-performance sensitivity and promotion-based

incentives, affect innovation (Lerner and Wulf, 2007; Ederer and Manso, 2011; Manso, 2011). We show that pay differences among employee peers are another important but underexplored dimension of compensation design. Our findings broaden the literature by showing that innovation depends on horizontal pay structures within firms.

The rest of the paper proceeds as follows. Section 2 details the institutional background and literature review. Section 3 discusses the data and sample construction, Section 4 discusses the empirical results, Section 5 discusses the robustness tests, and Section 6 concludes.

2. Institutional Background, Literature, and Hypotheses

2.1. Institutional Background

Salary history bans prohibit employers from asking job applicants about their past salaries and from relying on this information when setting compensation. If minorities, women or others with lower bargaining and negotiating skills earn lower salaries, these laws hope to prevent the perpetuation of prior pay discrimination into their new jobs and positions. Massachusetts adopted the first statewide ban in 2016, which took effect in 2018.[2] Several other states followed in subsequent years. Table 1 lists the states in our sample that enacted salary history bans and the year each law took effect. The adoption years range from 2017 to 2021 and vary across states. This staggered timing provides the variation we exploit in our empirical analysis. As seen in Figure 1 the adoption is not concentrated in a single region, which mitigates concerns that the results are due to regional economic trends.

[2] For example, the Massachusetts statute states that "An employer shall not seek the wage or salary history of a prospective employee from the prospective employee or a current or former employer…" and further that an employer "shall not rely on the wage or salary history of a prospective employee in determining the wages offered" (Mass. Gen. Laws ch. 149, § 105A).

The salary history bans when adopted by a state are applicable to all employment in the state. Many firms, especially large firms operating in multiple states, adopt a single hiring policy across all locations once they are exposed to the law in one state.[3] Maintaining different interview procedures across states can be costly and creates compliance risk. We use sufficiently high employment in a state that adopts the salary history ban as an exogenous shock to pay inequality. We detail the treatment effect later in the paper and also examine an alternate treatment effect based on the state of the firm's headquarters.

The economic mechanism that we hypothesize operates through wage-setting. Before salary history bans, employers often used prior salary as a reference point when setting compensation for new hires or internal promotions. Past salary thus served as a bargaining anchor. If an applicant previously earned a lower wage, the employer could use that information to justify a lower starting pay than the firm's existing pay scales for the same position and rank. Over time, this process can propagate wage differences among employees hired into comparable positions. When salary history bans remove access to prior salary information, firms must rely more heavily on internal job-level pay scales and external market benchmarks. Compensation therefore becomes less dependent on individual wage histories, and more aligned with job levels resulting in lower peer pay differences.

This change has different implications for horizontal and vertical pay dispersion. Horizontal dispersion refers to wage differences among employees at the same seniority level or in comparable roles. Such pay differences often reflect negotiation outcomes and prior salary

[3] A Korn Ferry survey finds that "Nearly half of the executives polled (46 percent) said choosing to comply with the most stringent legislation is the likely mode of adapting to the new legislation, as opposed to complying to each local legislation." (Employers Unsure of Salary-History Bans - HR Executive; (More Employers Avoid Legal Minefield By Not Asking About Pay History). For example, following California's enactment in 2018, major employers such as Amazon, Facebook, and Google adopted salary history bans across all U.S. locations rather than limiting compliance to regulated jurisdictions.

anchoring. By limiting the use of past pay in wage-setting, salary history bans should reduce this within-level variation. Vertical dispersion, in contrast, refers to wage differences across hierarchical levels that are tied to different job responsibilities, rank, and promotion structures. As salary history bans do not alter organizational hierarchies or promotion ladders and do not impact how firms differentiate compensation across ranks their effect on vertical pay gaps should be limited.

2.2. Related Literature

Economic theory offers varying predictions of wage inequality on employee effort and productivity. On the one hand, tournament models predict that larger pay gaps strengthen incentives and increase effort (Lazear and Rosen, 1981). Large wage differentials are also consistent with the presence of superstars (Rosen, 1981) and to managerial talent in competitive assignment models (Gabaix and Landier, 2008 and Terviö, 2008). Under these frameworks, pay dispersion reflects efficient incentive design and talent sorting. Empirically, Kale et al. (2009) use pay differences between CEO and the median top five executives to document that larger pay gap is associated with stronger firm performance. Mueller et al. (2017) use proprietary data from a sample of UK firms to capture pay differences across nine hierarchy levels and document that higher pay dispersion is associated with stronger operating performance. More recently, Wallskog et al. (2026) examine Swedish data on wages to document that within-firm pay inequality is associated with higher productivity. These papers provide empirical support that pay inequality captures tournament incentives and sorting of managerial talent and is associated with increased firm performance.

On the other hand, fairness-based and team production theories suggest that pay inequality may entail real efficiency costs. Workers may form preferences over relative pay, and when

compensation falls short of what they perceive as fair, they may reduce effort (Adams, 1963; Akerlof and Yellen, 1990). A related mechanism operates through incentive design. When production involves multiple tasks or requires collaboration among workers, high-powered individual incentives may lead agents to substitute effort toward easily measured, contractible tasks and away from cooperative or non-contractible activities—a distortion emphasized in the multitasking model of Holmström and Milgrom (1991). Empirical support for fairness theories is provided by several studies through field experiments. Card, Mas, Moretti and Saez (2012) examine the effect of disclosing peer salaries to a subset of employees. They find that workers with salaries below the median reported lower job dissatisfaction and had increased likelihood of departure. Breza et al. (2018) randomly assign workers to groups with low and high pay dispersion. They find that workers in high pay dispersion groups doing similar jobs had lower attendance and productivity. Breza et al. (2018) document lower effort when pay differences are visible and perceived to be unfair. Evidence for a negative effect of pay dispersion also comes from the 2018 SEC mandate that required firms to disclose the ratio of CEO pay to that of the median worker. Firms reporting a high ratio experience negative market reaction (Pan et al. 2022) and lower employee job satisfaction (Boone, Starkweather and White, 2023).

A potential reconciliation of these conflicting theories and empirical findings is a distinction between pay inequality that arises within peers (horizontal pay dispersion) and is more likely to generate concerns of fairness and collaboration and pay inequality that arises between hierarchy levels (vertical dispersion) that generates tournament incentives and reward for effort and promotion. A few recent studies document the importance of this distinction. Cullen and Perez-Truglia (2022) through a field experiment show that employees tend to reduce effort when informed that co-workers earn more and increase effort when informed that managers/ supervisors

earn more than expected. Kacperczyk and Balachandran (2018) use Swedish data to show that vertical pay dispersion reduces cross-firm employee mobility while horizontal pay dispersion increases cross-firm employee mobility.

However, data limitations prevent measurement of pay dispersion within firms and especially those of horizontal pay dispersion. The paper contributes by developing measures of both vertical and horizontal pay dispersion in a large sample of firms. We argue and validate that exogenous adoption of salary history bans by states lead to a reduction of horizontal pay dispersion in firms leaving vertical pay dispersion unchanged. Using the exposure to salary history bans as an exogenous shock, we examine the effect of reducing peer pay inequality on innovation that requires significant collaboration and teamwork.

2.3. Hypotheses and Tests

The institutional setting and the competing theoretical perspectives yield clear testable predictions. First, if prior salary anchors wage setting and results in persistent differences among employees in similar roles, removing this anchor should compress peer-level wage variation and have limited effect on vertical pay dispersion. We first examine the impact of salary history bans on horizontal and vertical pay dispersion of exposed firms.

Innovation activities often depend on sustained individual effort as well as collaboration among employees. When employees at the same level experience large wage differences, social comparisons and fairness concerns may discourage effort, increase turnover, and lower individual productivity. In team-based environments, these effects can also weaken collaboration and joint problem-solving. If horizontal pay dispersion undermines both individual effort and team coordination, then reducing peer-level inequality should improve innovation outcomes. We examine if exposure to salary history bans lead to improvement in innovation outcomes.

To link the changes in innovation outcomes to changes in pay dispersion, we present two sets of analyses. First, we examine heterogenous treatment effects to examine whether the changes in innovation outcomes are in line with our proposed mechanism. We examine if firms with greater exposure intensity to salary history bans and firms more likely to be impacted by pay compression have stronger innovation outcomes. These difference-in-difference-in-differences (DiDiD) tests help establish that the observed innovation gains are closely tied to changes in horizontal pay dispersion rather than to unrelated state-level shocks. Secondly, we directly test the proposed mechanism by constructing variables of employee turnover and team-level output to examine if they respond to changes in peer pay compression.

3. Sample and Variable Construction

3.1. Sample

We obtain employee-level compensation data from Revello Labs from 2005 to 2023. Revelio Labs compiles large-scale position-level employment records using publicly available professional profile data (including LinkedIn), job postings, and other digital sources. We merge the Revelio data with Compustat using firm identifiers and restrict the sample to U.S employees of U.S public firms.[4] To ensure reliable measurement of within-firm pay dispersion, we restrict the sample to firms that have at least one year during the sample period with more than 100 employees in the Revelio dataset. This mitigates small-sample noise in the pay dispersion measures. The resulting baseline sample consists of 40,927 firm-year observations. Restricting the sample to a balanced event window spanning two years before and two years after policy adoption - the specification for most of our results - further reduces the sample to 25,509 firm-year observations.

[4] We further exclude observations with missing value of total assets, salary, and seniority. After applying these filters, we construct a sample of approximately 57 million position-level observations corresponding to nearly 29 million unique individuals across 8,139 unique U.S. public firms, covering more than 76,257 firm-year observations.

For every user position, Revelio Labs reports a beginning and end of position salary. The salary data is obtained from Revelio Lab's salary model that predicts the salary for each position using position-specific information such as job title, seniority, company, location as well as user characteristics such as years worked. [5] As employer identity, position, user characteristics, location and year enter the model directly, predicted salaries vary systematically both across and within firms, even for similar job titles and seniority levels. The model is trained on salaries found in publicly available visa applications data, self-reported data, and job postings.[6] To construct annual compensation measures, we linearly interpolate salaries across the duration of each position using the reported beginning and ending salary values. [7]

3.2 Validation Analysis of Revelio Salary Data

Although the Revelio salary data are used to measure changes in pay dispersion, they serve a supporting role in our empirical design. Specifically, they are used to verify that salary history bans reduce horizontal pay dispersion while leaving vertical pay dispersion largely unchanged, whereas identification of the innovation effects comes from firms' exposure to the adoption of salary history bans.

Because the Revelio salary data are estimated from public sources, we assess whether they preserve the distributional characteristics of compensation needed to measure within-firm pay dispersion. Although the Revelio salary data may not capture all components of total compensation, salary-based pay dispersion is meaningful in its own right. Compared with equity compensation,

[5] We use Revelio's salary estimates rather than its total compensation estimates because they are more highly correlated with compensation reported in ExecuComp (see Appendix A and Table A.1) and rely on fewer assumptions regarding the valuation and realization of equity and other incentive compensation.
[6] Further detail on the Revelio salary model can be obtained from https://www.data-dictionary.reveliolabs.com/methodology.html#salary
[7] We also estimate dispersion measures using only the reported starting and ending salaries for the corresponding position years, without interpolation. As this eliminates employee years in the middle, it reduces the number of observations. The results with this method remain qualitatively unchanged.

salary is less affected by vesting schedules, equity exercise or trading decisions, and individual liquidity needs, making it a cleaner measure of firms' ongoing pay policies. Moreover, because salary is typically less dispersed than total compensation, our analyses are based on a narrower and more conservative measure of within-firm pay inequality. The validation analyses summarized below compare the Revelio salary data with several external compensation datasets, with additional details provided in Appendix A.

First, we decompose the overall variance of salary data from Revelio Labs into that arising from within-firm differences and that from between-firm differences in line with Song et al. (2019). Specifically, consistent with the findings of Song et al. (2019), we find that the increase in overall variance of wages is primarily coming from an increase in between-firm wage differences. This suggests that wage decomposition using Revelio salary data is similar to that obtained using the near universe of wage data from U.S Social Security administration that is the basis of Song et al. (2019) findings (See Appendix A for details).

Secondly, we obtain the salary of the top executives from the Summary Compensation Tables (SCT) that firms are required to report. We match these executives by firm identifiers and executive names to Revelio Labs and obtain their salary and total compensation from both datasets. We find that the rank correlation of salary from SCT tables and that from Revelio Labs is reasonable at 0.67. Though absolute pay captured by Revelio Labs differs from that of SCT, the ranking of compensation within firms is mostly preserved. Lastly, we also obtain median employee pay from firm's required disclosures and find that the rank correlation of median pay across firms in a year obtained from firm disclosure and that from Revelio Labs is also reasonable (See Appendix A for further details).[8]

[8] As the correlation between firm's disclosed data and Revelio Labs is higher for salary, rather than total compensation, we use salary to calculate pay dispersion

Overall, these analyses show that the Revelio salary data largely maintain the distributional characteristics of compensation both within and across firms. This makes pay dispersion based on the Revelio salary data a valid measure of wage inequality. Further, note that pay dispersion from the Revelio salary data is used to validate that salary history bans reduce horizontal pay dispersion in firms while leaving vertical pay dispersion unchanged. Our identification of innovation effects comes from firms' exposure to the exogenous salary history bans rather than from the estimated pay dispersions measured using the Revelio data.

3.3. Within-Firm Pay Dispersion Measures

We measure overall pay dispersion using the Gini coefficient and also compute separate pay dispersion measures for within and between seniority levels. Revelio Labs provides a seniority variable that classifies each employee-position into one of seven ordinal levels, ranging from Entry Level to Senior Executive Level.[9] These levels are constructed using a proprietary model that incorporates information on job title, occupation taxonomy, industry, company size, career history, and age to infer relative hierarchical position within the firm. Because these levels correspond closely to observable organizational ranks, they provide a natural basis for separating pay dispersion within seniority level (horizontal) from dispersion across ranks (vertical).

Let x_j denote employee j's salary in a given firm-year and let n denote the number of employees in the firm-year. Let $x_{(1)} \leq \cdots \leq x_{(n)}$ denote salaries sorted in ascending order within a firm-year. The total Gini coefficient is defined as

[9] The numerical levels that range from 1 to 7 correspond to Entry Level, Junior Level, Associate Level, Manager Level, Director Level, Executive Level and Senior Executive level respectively.

$$G = \frac{2\sum_{j=1}^{n} j x_{(j)}}{n\sum_{j=1}^{n} x_{(j)}} - \frac{n+1}{n}.$$

The higher the Gini coefficient the greater the salary inequality within the firm. To estimate horizontal and vertical pay dispersions, we follow the subgroup estimation approach of Yitzhaki and Lerman (1991). Let g index seniority levels, and define n_g as the number of employees in seniority level g, $y_g = \sum_{j \in g} x_j$ as total salary in seniority level g, and $s_g = y_g / \sum_{j=1}^{n} x_j$ as the seniority level's income share. The within-seniority Gini (horizontal dispersion) is defined as

$$G_{\text{within}} = \sum_{g} s_g \, G_g,$$

where G_g is the Gini coefficient computed using only salaries within seniority level g. This component captures wage inequality among employees at the same hierarchical level.

The between-seniority Gini (vertical dispersion) captures stratification, that is the degree to which seniority levels overlap with each other in the hierarchical measure. If compensation of a seniority level does not overlap with that of other groups, the firm has distinct stratification across seniority levels and a high degree of pay dispersion (See Yitzhaki and Lerman (1991)). Specifically, it is computed using the covariance formulation:

$$G_{\text{between}} = \frac{2}{\mu} \, \text{Cov}_{\pi}(\mu_g, \bar{r}_g),$$

where μ is the firm-year mean salary, μ_g is the mean salary in seniority level g, $\bar{r}_g$ is the average fractional rank of seniority level g in the overall salary distribution, and $\text{Cov}_{\pi}(\cdot)$ denotes the weighted covariance across seniority levels using population shares $\pi_g = n_g/n$ as weights. A higher degree of correlation implies seniority level strongly predicts overall rank resulting in a higher value of G_{between} and greater inequality between seniority levels.

Table 2 reports summary statistics for the pay dispersion measures. The mean *Total_Gini* is 29.39 with a standard deviation of 4.19.[10] This suggests economically meaningful dispersion in compensation within firms and is comparable in magnitude to within-firm inequality documented in prior research. The average *Within_Gini* is 17.16 and the average *Between_Gini* is 17.53, with similar median values. These magnitudes suggest that both horizontal and vertical dispersions contribute meaningfully to overall within-firm pay dispersion.

As a robustness check, we also use the entropy-based Theil-T index, which provides a complementary way to distinguish horizontal and vertical dispersion. Compared to Gini, the Theil-T index permits an exact additive decomposition into within- and between-group components. It is also relatively more sensitive to inequality at the top of the pay distribution, whereas the Gini coefficient places greater weight on inequality around the middle of the distribution. All our results are robust to the Theil-T based measures.

3.4 Defining Treatment Status

As discussed above, the salary history ban is applicable to all employment in the state of adoption. However, as many firms adopt uniform hiring policies across all locations after becoming subject to regulation in one state, the effects of the law may extend beyond the directly regulated location. We use sufficiently high employment in a state that adopts the salary history ban as an exogenous shock to pay inequality to define treatment.[11]

Specifically, let t_s denote the year in which state s implements the job history ban, and let $A = \{(s, t_s)\}$ be the set of state–year adoption events. For firm f and year t, let $E_{f,s,t}$_denote the

[10] For convenience we have multiplied the Gini Coefficient with a 100.

[11] If there are firms that do not adopt firm-wide policies then their inclusion in our treatment group would attenuate the measured treatment effect, making it harder to detect statistically significant effects.

number of employees located in state s, and let $E_{f,t}$ denote the firm's total employment. A firm is classified as treated if in the year a given state adopts the job history ban, the firm's employment share in that state exceeds a threshold τ with τ being 0.10. The firm's first treatment year is the year when a state's adoption of the law exposes at least 10% of its employees to the law. Note, that treatment cannot be triggered by accumulating employment shares across multiple adopting states over time. Only the employment share in the newly adopted stated, evaluated in the adoption year, can activate treatment. Once treated, the firm continues to be treated in the subsequent years.

For example, Delaware enacts the law in 2017 and California in 2018. If a firm share of employment in Delaware in 2017 is greater than 10% then 2017 is classified as the first year of treatment. If the firm share of employment in Californio is also greater than 10% then the earlier of the two years is considered the first year of treatment, in this case 2017. If the firm does not have 10% employment in Delaware in 2017 or 10% employment in California in 2018, it remains in the control group even if its combined share of employment in Delaware and California exceeds 10%. Figure 2 displays the geographic distribution of treated firms.

For robustness we also use firm headquarters to define treatment status. Specifically, a firm is considered as treated if its headquarters is located in a state that adopts the policy during the sample period and untreated otherwise. The results are qualitatively similar with this treatment definition and are discussed later in the robustness section.

3.5 Innovation Outcome Measures and Other Variables

Patent data are obtained from the KPSS patent database and PatentsView and matched to Compustat firms using established linking procedures (e.g., Baghai et al., 2024). Following prior literature, we use patent grant year to assign innovation output to firm-years. Missing values for patent counts and citation measures are replaced with zero.

Our first measure is *#Patents*, defined as the number of patents granted in a year, which captures innovation quantity and has been used by a large literature to capture innovation outcomes. Kogan et al. (2017) point out that patents can be highly heterogenous in their economic value making the number of patents a noisy measure of the value of innovation and propose an alternate measure based on the market value of patents. The second innovation measure, referred to as *PatentValue*, follows Kogan et al. (2017) and is estimated as the aggregate nominal market value of patents granted to the firm in a given year.[12]

In addition to patent-based outcomes that capture output from R&D effort we also capture firm's investment in its innovation activities by examining the impact on firm *KnowledgeCapital*. Knowledge capital represents the capitalized stock of accumulated R&D expenditures and serves as a proxy for the firm's intangible innovation assets. We use estimates of *KnowledgeCapital* from Peters and Taylor (2017).[13] We also examine measures of firm value. Specifically, we use Peter and Taylor(2017)'s measure of total Tobin's Q that includes the value of the firm's intangible capital. We also examine long-run value-to-book (*VB*), constructed following Rhodes-Kropf, Robinson, and Viswanathan (2005). These measures allow us to assess whether changes in pay dispersion's effects on patenting activity are reflected in broader innovation assets and firm value.

Table 2 reports summary statistics for the innovation outcome variables. The average number of patents per firm-year is 10.40 and the average patent value is 411.49 million dollars, with both medians equal to zero, suggesting skewness in patenting activity. On average, sample firms have 300 million dollars of *KnowledgeCapital* with the median being zero. These patterns

[12] Kogan et al. (2017) use market adjusted stock returns on the day of the announcement of the patent grant multiplied by the market value of the firm to capture the value of patent. Value of individual patents is aggregated across all patents granted in a year to get the value of patents at the firm year level.
[13] Peter and Taylor (2017) measure is available from WRDS and is obtained by capitalizing firm R&D expenditures and using industry specific depreciation rates.

suggest that a sizeable fraction of firm-years report no patent grants or R&D activity. To mitigate skewness and reduce the influence of extreme observations, we apply a log transformation to these variables. For robustness, we verify that our main results hold when restricting the sample to firms that have at least one patent granted during the sample period or positive R&D expenditures. The mean (median) value of *Log(VB)* is 0.83 (0.92) and that of *Tobin's Q* is 1.63 (0.83).

4. Empirical Results

4.1. Pay Dispersion and Salary History Bans

We begin by examining whether salary history bans affect within-firm pay dispersion. We estimate the following staggered difference-in-differences specification:

$$Y_{i,t} = \beta\, \text{Treated}_i \times \text{Post}_{i,t} + \gamma X_{i,t-1} + \alpha_{c,i} + \delta_{c,t} + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$is a measure of pay dispersion for firm i in year t. Treated × Post equals one if the firm is treated, that is exposed to salary history ban as described above and the observation occurs in or after the firm's first treatment year. $\alpha_{c,i}$ denotes cohort-by-firm fixed effects, and $\delta_{c,t}$ denotes cohort-by-year fixed effects. Control variables include firm size, leverage, capital expenditures, firm age, ROA, and R&D intensity, all lagged by one year. Standard errors are clustered at the firm level.

Figure 3 presents event-study estimates of the interaction between treatment status and event time for pay dispersion measures, spanning four years before and four years after policy adoption. The pre-treatment coefficients are statistically insignificant across all three measures, supporting the parallel trends assumption. Following policy adoption, total and within-level dispersions decline persistently, while between-level dispersion shows no consistent post-treatment pattern and is statistically insignificant in most post-treatment years.

Table 3 reports results from the estimation of Equation (1). The coefficient on Treated × Post is negative and significant for *Total_Gini* as well as for *Within_Gini*. There is no significant impact on *Between_Gini* after the exposure to salary history bans. These results indicate that exposure to a salary history ban significantly reduces horizontal pay inequality while leaving vertical pay inequality unchanged in line with our hypothesis.

4.2. Innovation Outcomes and Salary History Bans

We next examine whether the exposure to salary history bans has an impact on the firm's innovation outcomes. As seen in Figure 4, the pre-treatment coefficients are statistically indistinguishable from zero indicating that there is no evidence of differential trends in the number of patents or patent value before exposure to salary history bans. However, following treatment there is significant evidence of persistent increase in patent value.

Table 4 reports regression results for a balanced five-year event window spanning two years before and two years after exposure, including the event year. The dependent variable is *Log(#Patents)* and *Log(PatentValue)* as specified in the column heading. We first estimate results in a sample of firms with at least one year with 100 or more employees, the base sample which was used for Table 3. We find significant positive coefficients for both, that is in the years after exposure treatment firms have an increase in the number of patents and also in the value of these patents.

As patent outcome data does not rely on Revelio Labs, we also examine the effect of exposure to salary history bans on patent outcomes for all firms in Compustat with valid financial data within the two-year event window of the stacked DiD design. As seen in columns 3 and 4, the results continue to hold with the increase in sample size leading to less noisy estimates with higher statistical significance. As the patent data is skewed, for robustness we also estimate the model in

a sample of firms that have at least one patent granted during the sample period. This restriction also mitigates the concern that findings are potentially due to difference between patenting and non-patenting firms. As seen in columns 5 and 6 the results continue to hold and are somewhat stronger.

The effects are economically significant as well. Depending on the specification, the implied increase in patent quantity ranges from 3.1% to 5.3%, while patent value increases by 10.9% to 16.4% following exposure to salary history bans. The stronger response in patent value suggests that the policy increases patent quality more than patent quantity.

The results from the impact on *KnowledgeCapital* and firm value are reported in Table 5. Panel C of Figure 4 shows no pre-trends in *KnowledgeCapital* and an increase after treatment. In line with Table 4, we report results for estimation in our base sample as well as in a sample of all Compustat firms. We find significant positive coefficients in both specifications indicating an increase in *KnowledgeCapital* after exposure to a salary history ban. The results with *Tobins Q*, and *Log (VB)* are similar. There is little evidence of pre-trends and a significant increase in firm value after exposure to salary history bans.

4.3. Linking Innovation Outcomes to Pay Compression

Our results so far indicate that salary history bans result in a decrease in horizontal pay dispersion and also increase innovation outcomes like quantity and value of patents. Our proposed underlying mechanism for increased innovation outcomes is a reduction in horizon pay dispersion and its effect on improved individual effort and team collaboration. In this section, we provide evidence of this link by examining heterogenous treatment effects. Specifically, we test whether the innovation effects are stronger in settings where the policy should have a larger impact on

wage-setting and peer pay inequality. We divide the analyses into two sets based on (i) pre-treatment firm characteristics and (ii) post-treatment intensity measures.

4.3.1 Pre-treatment firm characteristics

We first examine whether pre-treatment firm characteristics are associated with the strength of the innovation response to salary history bans. If the policy improves innovation by compressing horizontal pay dispersion, then the effects should be larger in firms where peer-pay inequality is more pronounced or where innovation depends more heavily on employee effort and collaboration. Figure 5 presents the results of these difference-in-difference-in-differences (DiDiD) analyses using *Patent Value* as the outcome variable.

We begin by splitting firms based on pre-treatment horizontal pay dispersion, defined as the average *Within_Gini* over the four years preceding exposure. Firms are classified as high dispersion if their pre-treatment *Within_Gini* exceeds the median among treated firms. Figure 5(a) shows no evidence of pre-trends and a higher post-treatment increase in patent value for the high-dispersion group. This pattern is consistent with the pay-compression channel: firms with greater baseline horizontal inequality have more scope for the salary history bans to reduce pay dispersion and, in turn, improve innovation outcomes.

Figures 5(b) and 5(c) examine heterogeneity based on pre-treatment workforce size and R&D intensity. Patent value increases are stronger among firms with smaller workforces and higher R&D intensity. In smaller organizations, innovation is often more dependent on employee coordination and individual contributions, as each worker represents a larger share of the firm's human capital and innovative capacity. Similarly, R&D-intensive firms rely more heavily on knowledge sharing and human capital, whose productivity is more directly affected by employee

effort and collaboration. Together, these patterns suggest that the innovation effects of peer pay compression are stronger in environments where individual effort and collaborative production are particularly important.

To further shed light on the role of collaboration, Figure 5(d) split the sample based on the pre-treatment teamwork culture index developed by Li et al. (2021). The index is constructed using machine-learning analysis of earnings call transcripts and captures the extent to which firms emphasize teamwork and collaboration in corporate communications. Firms with stronger pre-treatment importance of teamwork experience significantly larger patent value gains following policy adoption. This finding supports the importance of collaboration in understanding the post-event increase in innovation value.

4.3.2 Treatment-intensity measures

In the second set of heterogeneity analyses, we focus on treatment-intensity measures and examine whether increases in innovation value are stronger in firms that are likely to experience larger changes in horizontal pay dispersion following the policy change. Figure 6 displays the results of these difference-in-difference-in-differences (DiDiD) analyses. We begin with policy exposure intensity, measured as the share of a firm's employees located in states that adopt salary history bans in the year of adoption. Treated firms are classified into high- and low-exposure groups based on the median exposure level among treated firms. As greater exposure implies a larger share of employees subject to the law and a stronger push to change wage setting, it should result in higher pay compression and better innovation outcomes. As seen in Figure 6(a), there is no difference between the high- and low-exposure groups prior to the treatment, satisfying the parallel trends assumptions. After treatment, however, firms with higher exposure intensity experience larger post-treatment increases in patent value.

We recognize that salary history bans are one of several recent labor market regulations aimed at promoting pay equity. Other policies, including pay transparency laws, equal pay laws, and anti-pay discrimination legislation, may lead firms to reassess compensation more broadly, affecting both horizontal and vertical pay differences among employees. In contrast, salary history bans provide a cleaner setting for identifying changes in horizontal pay dispersion by restricting the use of applicants' prior salaries when determining compensation for new positions, making them particularly likely to compress pay among employees entering similar positions.

Moreover, although some states adopted multiple pay equity policies during our sample period, this partial overlap is unlikely to drive our results because our staggered difference-in-differences design exploits the timing of salary history ban adoption. Nevertheless, overlapping policies provide an opportunity to examine whether complementary pay equity regulations further strengthen the effects of salary history bans. Among these policies, pay transparency laws are particularly relevant because, like salary history bans, they directly affect the wage-setting process for new hires, albeit through a different mechanism. We therefore examine whether the effects of salary history bans are amplified when accompanied by pay transparency laws.

Pay transparency laws require employers to disclose salary ranges for job postings, increasing the information available to job applicants. Gao, Hsu, and Zhang (2025) show that the adoption of pay transparency laws increases inventor productivity. To capture the combined effect of these policies, we identify states that adopted pay transparency laws within the three years prior to the adoption of salary history bans. Firms in these states are exposed to both policies during the post treatment years. If the two policies operate through complementary channels, firms subject to both should experience larger reductions in horizontal pay compression, and consequently, stronger innovation gains.

Six states satisfied this criteria and firms with treatment effect originating in these states were identified as having *Dual* exposure while other treated firms were classified as having *Single* exposure that is only to the salary history ban.[14] Figure 6(b) displays the results and shows that as hypothesized the effects are stronger for firms exposed to both laws. However, even firms that were treated to only the salary history ban show significant increase in patent value in the post treatment years. The stronger impact on innovation outcomes in dual-policy states is consistent with complementarity between transparency of pay and preventing anchoring of salary to prior pay. When pay differences are more visible, reducing horizontal inequality may have a larger impact on collaboration and effort.

Because salary history bans primarily affect compensation-setting when employees entre new roles, firms with greater hiring activity should exhibit stronger innovation responses if the mechanism operates through wage setting. To examine this, we split firms by hiring intensity, measured by the number of new positions filled during the four years following policy exposure. Firms with above-median hiring intensity are classified into the high group. As seen in Figure 6(c), we find no pre-trends and a stronger post-treatment increase in patent value when firms have more new positions. In contrast, there appears to be significant negative effect for firms with below-median hiring intensity. We further split firms based on the share of minority and female employees among new positions filled after the shock. Figure 6(d) shows that firms with higher minority representation in new positions experience consistently larger increases in patent value across all four post-treatment years. The results are somewhat weaker for female representation (Figure 6(e)).

[14] Massachusetts and Maryland adopted both laws in 2018, Oregon, Hawaii and Washington (New York) adopted the pay transparency laws one (three) year prior to the salary history ban. There are no states that adopt pay transparency laws after adoption of salary history ban. Note that states classified as *Single* exposure may also have adopted pay transparency law but they did it several years prior so that their effect is captured both in the years before and after treatment to salary history ban.

For two of the four post-treatment years, firms with a higher share of female employees in newly filled positions experience larger innovation gains than firms with lower female representation. Together, these findings suggest that the effects of salary history bans are stronger in firms where the policy more directly affects minority workers and, to a lesser extent, female workers entering new positions.

Taken together, Figures 5 and 6 show that the innovation gains are stronger both among firms whose pre-treatment characteristics make peer pay inequality more consequential for innovation, and among firms where salary history bans are likely to induce larger reductions in horizontal pay dispersion. These patterns reinforce the interpretation that the observed innovation effects are related to peer pay compression.

4.4. Mechanism Analysis

The central premise of our paper is that horizontal pay dispersion affects innovation through employee behavior. Unlike vertical pay dispersion, which primarily shapes promotion incentives and executive effort, horizontal pay dispersion operates at the employee peer level. Employees working alongside similarly ranked colleagues are more likely to compare and respond to peer-level pay differences. If salary history bans reduce horizontal pay dispersion, the effects should be reflected in employee behavior and team outcomes. We therefore examine whether the observed innovation gains are accompanied by greater workforce stability, employee effort, and team collaboration.

We begin by examining employee turnover. When pay differences among peers are large, fairness concerns and social comparisons may reduce morale and increase employee exit. In team-based environments, dissatisfaction among similarly ranked employees can weaken coordination and reduce collective effort. If salary history bans compress peer-level inequality, we should

observe reduced employee turnover, particularly among skilled employees who are central to innovation. We measure *Employee_Turnover* as the number of employees who leave the firm during the year divided by total firm employment at the end of that year. The average *Employee_Turnover* in our sample is about 19% (See Table 2). As some of the departures might be forced or firings, we also try to capture voluntary departures which are more likely to be due to employee concerns with pay inequality. This measure, referred to as *Likely_Voluntary* is defined as the fraction of departing employees who secure their next employment within six months of leaving. Employees leaving voluntarily are likely to have their next job lined up when they leave and six months capture potential garden leaves or desired time off between jobs (See also Cheng and Kedia (2025)). On average about 16% of employee departures are classified as voluntary by this definition.

As seen in Columns 1 and 2 of Table 6, the coefficient of DiD is negative and significant for both *Employee_Turnover* and *Likely_Voluntary* indicating that employee turnover and voluntary departures decline in treated firms after exposure to the salary history ban. As innovation outcomes are more impacted by skilled workers we also capture turnover and voluntary departures specifically for skilled employees. In line with prior literature, we classify employees as skilled if their O*NET Job Zones are categorized as 4 and 5 in Revelio labs.[15] We continue to find significant reduction in skilled employee departures after exposure (see Columns 3 and 4).

To shed light on employee effort, we next examine inventor productivity. If peer-level pay inequality discourages employee motivation, compressing pay differences among similarly ranked employees could increase effort and productivity. Compared with patent grants, patent applications provide a more immediate measure of inventor effort because they are filed before patents are

[15] O*NET Job Zones range from 1 (little or no preparation needed) to 5 (extensive preparation required), with Zones 4 and 5 representing high-skill occupations requiring considerable education and training.

granted. We therefore construct *#Applications_Per_Inventor*, defined as the number of patent applications filed per inventor in a given year among inventors affiliated with the firm during our sample period. Unlike granted patents, however, assignee (i.e., firm) information is unavailable for most patent applications, making it difficult to link patent applications directly to firms. We therefore first identify each firm's inventor pool using the granted patent data over our sample period and then retrieve the corresponding patent applications for those inventors from the PatentsView application database. As a result, the inventor pool underlying this measure may not perfectly align with the firm's actual inventor pool in a given year, making *#Applications_Per_Inventor* a noisier measure of innovative effort.

As reported in Column 5 of Table 6, the coefficient of the DiD estimator is positive and statistically significant, indicating that patent applications per inventor increase following exposure to salary history bans. The estimated effect is also economically meaningful, corresponding to nearly a 10% increase in inventor productivity.

Innovation depends not only on employee retention and effort but also on collaboration. We next examine team-level dynamics to shed light on their contribution to improved innovation outcomes. We first construct *#Patent_Per_Team*, which measures the number of patents per inventor team. Increase in the number of patents associated with a team captures increased productivity of the team. We also examine *Log(#Invetors)* which is the natural logarithm value of the total number of inventors. An increase in the number of inventors captures more successful hiring. Next, we construct *Avg_Team_Size* which is the average number of inventors per patent. Larger team sizes indicate greater collaboration and joint production among employees. Finally, to capture changes in employees' sentiment toward workforce collaboration, we also include

Revelio Lab's sentiment score for internal communication, *Internal_Communication*.[16] Together, these measures capture team productivity, workforce expansion, and the intensity and sentiment of collaboration within firms.

Table 7 displays the results, and we find that treated firms experience significant increases in all four measures after exposure. These results indicate that following exposure to a salary history ban, firms expand their inventor base, form larger teams, generate greater output per team, and improve internal communication. These patterns are consistent with improved coordination and are consistent with a reduction in peer-level wage differences resulting in greater employee willingness to share information, cooperate, and engage in joint problem-solving, increasing team productivity.

5. Robustness Checks

In this section, we conduct several robustness analyses to assess the validity of our identification strategy and the stability of our results. First, we implement an instrumental variables approach to isolate exogenous variation in horizontal pay dispersion. Second, we redefine treatment status based on headquarters location to ensure that our findings are not sensitive to the workforce-exposure threshold. Third, we apply an alternative staggered difference-in-differences estimator to address potential concerns about heterogeneous treatment timing. Fourth, we conduct a placebo test by randomly assigning treated firms and find no significant effects on either pay dispersion or innovation outcomes. Together, these analyses provide additional support for the robustness of our findings and the validity of our identification strategy.

[16] Internal communication is a sentiment-based measure constructed by Revelio Labs using natural language processing (NLP) of employee review text. The measure captures employees' perceptions of communication and collaboration within the firm, with higher values indicating more positive sentiment toward internal workplace interactions.

5.1. IV/2SLS analysis

In addition to the staggered DiD analysis, we implement a two-stage least squares (2SLS) framework to provide complementary evidence on the relation between horizontal pay dispersion and innovation. We instrument horizontal dispersion using firm-level exposure to salary history bans, thereby isolating plausibly exogenous variation driven by regulatory changes. This approach also allows us to more directly link the observed innovation gains to changes in pay dispersion.

In the first stage, we instrument *Within_Gini* using the interaction between post-shock timing and firm-level exposure intensity (*Post* × *Exposure*). Exposure intensity is the share of a firm's employees located in adopting states in the shock year. Because salary history bans directly restrict wage-setting in those jurisdictions, this instrument captures exogenous variation in horizontal dispersion driven by regulatory exposure. In the second stage, we estimate the effect of the predicted value of *Within_Gini* on one-year-ahead patent values.

Table 8 reports the IV/2SLS results. The first-stage estimates show that the instrument, *Post* × *Exposure*, significantly reduces *Within_Gini*, confirming that exposure intensity predicts horizontal pay compression. The Kleibergen–Paap Wald F-statistic exceeds conventional thresholds, indicating a strong instrument. In the second stage, *Within_Gini* is negatively related to future patent value. Results hold in both the full panel data from 2006 to 2023 (See Columns 1 and 2) as well as in the 2013-2023 sample that matches the DiD regressions (See columns 3 and 4). In summary, the IV estimates reinforce the DiD findings. The negative second-stage coefficients are consistent with the view that horizontal pay dispersion reduces the value of patents.

5.2. Using the Headquarters State to Define Treatment

Our baseline treatment definition relies on minimum threshold of employees in the adopting state. However, one might argue that firms respond primarily based on the location of

their headquarters. To address this concern, we reclassify treatment status using the state of the firm's headquarters. A firm is considered treated if its headquarters is located in a state that adopts a salary history ban.

Table 9 reports the results. Panel A shows that *Total_Gini* and *Within_Gini* significantly decline after the adoption of salary history bans in the headquarters state, while *Between_Gini* remains statistically insignificant. Panel B shows that patent value decreases significantly across all three samples, our base sample, all Compustat firm sample and those with at least one patent. The economic magnitudes are either comparable or slightly larger than those reported in Tables 3 and 4. This consistency in results suggests that our findings are not driven by the specific workforce-exposure threshold and are robust to alternative treatment definitions.

5.3. Alternative Estimation Method

As a robustness check, we re-estimate our main specifications using the Callaway and Sant'Anna (2021) (CSDID) estimator, which accommodates staggered treatment timing and heterogeneous effects.

Figure 6 presents the CSDID event-study estimates for pay dispersion and innovation outcomes. There is no evidence of pre-trends across all pay dispersion measures and patent value, which further supports the parallel trends assumption. Panel A shows that *Total_Gini* decreases significantly following treatment. Panels B and C show that *Within_Gini* declines significantly after policy adoption, while *Between_Gini* exhibits no systematic post-treatment response. Panel D shows that patent value increases significantly following treatment. Overall, the CSDID estimates closely mirror the baseline DiD results.

Taken together, the IV analysis, alternative treatment definition, and alternative estimation method all yield consistent results. Salary history bans reduce horizontal pay dispersion, and this

reduction corresponds to significant increases in patent value. These robustness checks strengthen the interpretation that horizontal pay compression, rather than coincident state-level confounding factors or estimation artifacts, drives the observed results.

5.4. Placebo test

We further conduct a placebo test following Chen, Ke, and Kuang (2026). Using the same stacked difference-in-differences sample, we randomly assign placebo treatment within each cohort while preserving the actual number of treated firms in that cohort and re-estimate the baseline specification 2,000 times. This test examines whether the observed treatment effects could emerge in the absence of the actual policy treatment.

Figure 8 plots the distributions of the placebo estimates for *Within_Gini*, *Log(#Patents)*, and *Log(PatentValue)* in Panels (a)−(c), respectively. Across all three outcomes, the placebo coefficients are centered around zero, whereas the actual treatment effects (shown by the black vertical line) lie well outside the placebo distributions. These results indicate that our findings are unlikely to be driven by spurious correlations or artifacts of the stacked DiD design.

6. Conclusion

This paper examines whether pay dispersion within firms affects innovation. Using employee-level compensation data and the staggered adoption of salary history bans as an exogenous shock, we isolate changes in horizontal pay dispersion. We show that salary history bans compress peer-pay inequality while leaving vertical pay dispersion largely unchanged. Firms exposed to these laws experience economically meaningful increases in innovation outcomes, accompanied by lower employee turnover, greater inventor productivity, and improved team output and collaboration.

These findings suggest that horizontal pay dispersion can impose real costs in innovation-intensive settings. While vertical pay gaps primarily strengthen promotion incentives, pay differences among similarly ranked employees could discourage effort, weaken collaboration, and reduce workforce stability. By compressing horizontal dispersion, salary history bans increase employee retention and team cohesion and enhance the economic value of innovation.

Our results have implications for investors, policymakers, and researchers. For investors, the internal structure of pay dispersion may provide information about a firm's capacity for innovation beyond the overall level of pay dispersion. For policymakers, the findings suggest that regulations affecting wage-setting mechanisms can influence firm outcomes through internal compensation structures. Public debate often centers on executive–worker pay gaps, but our evidence highlights the importance of inequality among employees at the same hierarchical level. Policies that affect peer-level wage formation may have distinct productivity consequences compared to policies targeting vertical pay gaps.

For researchers, decomposing pay inequality into horizontal and vertical components can be important for understanding its economic effects. Several avenues for future research remain. For example, future work could examine whether horizontal pay compression affects not only the level and value but also the type and direction of innovation, such as risk-taking, breakthrough patents, or commercialization outcomes that rely more on knowledge sharing. Researchers could also explore how peer-level inequality interacts with performance-based pay and promotion incentives, shedding light on the joint design of compensation systems. More broadly, our findings suggest that compensation design could shape the collaborative environment in which innovation occurs. Understanding how internal pay structures influence teamwork and productivity remains an important direction for future research.

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Appendix A. Revelio Lab Data Validation

This appendix reports validation exercises for the employee-level compensation data from Revelio Labs. Because these data are constructed using a predictive model rather than firm payroll records, an important question is whether the estimated salaries reflect meaningful firm-specific pay structures and dispersion patterns.

Revelio's compensation estimates are constructed at the employee–position level using a model that incorporates employer identifiers, job title, seniority level, location, tenure, and other career-history characteristics. Predictions are made at the individual-position level rather than at firm or occupation averages, allowing compensation to vary across employees within the same firm and job level. Because both employer identity and position enter the model directly, predicted salaries vary systematically both across and within firms, even for similar job titles and seniority levels. Importantly, the model is trained on multiple sources of observed true compensation data, including visa application data, job postings, and self-reported salary information. By combining firm-specific and individual-level features, this approach generates granular compensation estimates that preserve within-firm ranking and dispersion patterns, which are essential for measuring horizontal and vertical pay dispersions.

We conduct four validation exercises to assess whether the data preserve within-firm ranking and relative dispersion across employees. First, we examine whether the industry distribution of firms in Revelio aligns with that of Compustat, ensuring broad representativeness. Second, we replicate the variance decomposition of pay following Song et al. (2019) to benchmark within- and between-firm dispersion patterns. Third, we evaluate the alignment between within-firm executive pay rankings derived from Revelio and those reported in official SEC disclosures. Fourth, we assess whether cross-firm median employee pay rankings from Revelio correspond to those disclosed in CEO Pay Ratio filings.

A.1 Industry Distribution Comparison

Appendix Figure A.1 compares the industry distribution of firms in the Revelio-based sample to that of the full Compustat universe to assess sample representativeness. The solid line represents the proportion of firms in each of the ten industry sectors in Compustat, while the dashed line shows the corresponding proportion for firms with employee salary information available in Revelio.

The figure shows that the industry composition of the Revelio sample closely aligns with that of Compustat across major sectors. There is no evidence of systematic overrepresentation or underrepresentation in specific industries. This similarity reduces concerns that our results are driven by industry-specific sampling biases and supports the generalizability of the employee-level data across publicly listed firms.

A.2 Pay Variance Decomposition

Appendix Figure A.2 implements a variance decomposition of employee pay following Song et al. (2019). Song et al. (2019) use wage data from U.S. Social Security records to decompose overall variance of log earnings into within and between firm components. They report that of the 19 log point rise in overall variance of log earnings during the period 1978 to 2013, about 13 log points was due to the rise in between firm inequality and 6 log points was from within-firm component.

That is about 69% of the rise in the earning inequality was due to dispersion of average earnings between firms.

We follow Song et al. (2019) methodology and decompose the total variance of log annual salaries into within-firm and between-firm components using the full sample of publicly listed firms covered by Revelio. Consistent with Song et al. (2019), the decomposition indicates that the majority of the change in total variance in salaries is attributable to change in between-firm salary variance. The within-firm variance is relatively stable over the time period. Importantly, the relative magnitudes of the within- and between-firm components closely match those reported in Song et al. (2019).

This suggests that the distributional properties of compensation in Revelio resemble those documented using confidential data compiled and maintained by the U.S. Social Security Administration that is the basis of Song et. al. (2019) paper. As the distributional properties of the Revelio salary data match that of a universe of wage data, it validates its use to capture distribution-based measures like pay dispersion. In particular, the ability to replicate established variance shares supports the reliability of the constructed compensation measures for pay dispersion analysis.

A.3 Within-Firm Executive Pay Ranking Consistency

As firms are required to disclose the compensation of the top five executives in the firm we get this data from the Summary Compensation Table (SCT) parsed from proxy filings via directEDGAR. We use firm identifiers and the name of the executive to identify them in Revelio and obtain their salary and total compensation data from Revelio. We next compare Revelio compensation data to that of the SCT to examine if Revelio accurately captures the distribution of compensation. Specifically, for each firm-year, we construct rank orderings of executives based on total compensation and salary from SCT and compare them to rank ordering of the salary and total compensation measures constructed by Revelio.

Table A.1 reports rank correlations between these measures. The correlations are high and statistically significant across specifications. The correlation between SCT salary and Revelio salary is 0.67. As Revelio's salary model is likely to be better at capturing base salary than total compensation, it is not surprising that the correlation of salary is higher than that of total compensation which is about 0.57. Because our main analysis focuses on dispersion rather than absolute pay levels, accurate rank ordering is particularly important. The high within-firm rank correlation between Revelio and the SCT suggest that Revelio data preserves internal compensation hierarchies in a manner consistent with official pay records.

A.4 Cross-Firm Median Employee Pay Ranking Consistency

We next examine if Revelio salary data preserves cross-firm differences in employee levels. To examine this, we obtain officially disclosed median employee salaries from CEO Pay Ratio disclosures and compare them to firm-year median salaries computed from Revelio. For each year, we assign percentile ranks to firms based on their median-pay value and compute rank correlations between the two series. The rank correlation between the disclosed median employee pay and Revelio median pay is approximately 0.56 and statistically significant as seen in Appendix Table A.2. This high correlation indicates that Revelio preserves firms' relative positions in the cross-sectional distribution of median pay.

A.5. Summary

Across industry composition, variance decomposition, within-firm executive ranking, and cross-firm median pay ranking, the validation exercises show that Revelio compensation data replicate key distributional properties observed in official compensation sources. The executive-ranking and median-pay comparisons indicate that Revelio-based measures reliably capture both within-firm pay structure and cross-firm pay variation. Although no dataset is without limitations, these results provide strong support for the use of Revelio data in measuring within-firm pay dispersion.

Appendix B

Variable Definitions

Variable Name	Variable Definition	Data source
	Pay Ratio Variables	
Total_Gini	Overall within–firm-year pay inequality computed from cleaned, strictly positive salaries using the standard Gini formula. Higher values indicate greater dispersion in employee pay within the firm-year.	Revelio Labs
Within_Gini	Captures within-seniority pay dispersion; decomposition follows the subgroup approach of Yitzhaki and Lerman (1991)	Revelio Labs
Between_Gini	Captures between-seniority pay dispersion; decomposition follows the subgroup approach of Yitzhaki and Lerman (1991)	Revelio Labs
	Innovation and Valuation Variables	
#Patents	The number of distinct patents issued by the firm in a given year.	KPSS dataset
PatentValue	The sum of the nominal innovation values (in millions of USD) of patents issued by the firm in a given year.	KPSS dataset
#Patents_Per _Team	Firm-year average patent output per inventor team, where a team is defined as a unique combination of inventors jointly listed on a patent.	PatentsView
Log(#Inventors)	The natural logarithm value of the total number of unique inventors across all patents granted to the firm in that year.	PatentsView
Avg_Team_Size	Average number of inventors listed per granted patent in the firm-year.	PatentsView
	Firm Performance and Employee Outcome Variables	
Log(VB)	The logarithm of the ratio of long-run firm value to book value, following the method of Rhodes-Kropf et al. (2005).	Compustat, CRSP
KnowledgeCapital	The knowledge capital component of total intangible capital, reflecting R&D-based capital accumulation.	WRDS (Peters and Taylor Total Q)
Tobins Q	Total q, a proxy for Tobin's q, which is the ratio of capital's market value to its replacement cost.	WRDS (Peters and Taylor Total Q)
Internal _Communication	The median value of individual employee review scores on internal communication within a firm-year. The variable is constructed only for firm-year observations with at least five employee reviews in that year.	Revelio Labs
Employee_Turnover	The number of employees who left the company during the year / The total number of employees during the same year	Revelio Labs
Likely_Voluntary	Share of employees who find a new job within six months after leaving, used as a proxy for voluntary turnover.	Revelio Labs
Skill_Turnover	Turnover rate among high-skill employees (job positions classified as O*NET Job Zone 4 or 5), defined as high-skill leavers during the year divided by total high-skill employees in the same year.	Revelio Labs + O*NET
Skill_Likely	Share of high-skill leavers (O*NET Job Zone 4 or 5) who find a new job within six months after leaving.	Revelio Labs + O*NET

Appendix: Variable Definitions (Continued)

Variable Name	Variable Definition	Data source
#Applications_Per_Inventor	Firm-year patent applications per inventor, defined as the total number of patent applications filed in a given year by inventors associated with the firm, divided by the number of unique inventors associated with the firm in that year. Firm-inventor associations are identified based on granted patent records during the 2013-2023 sample period.	PatentsView
	Control Variables	
Firm Size	Log (Total assets)	Compustat
Leverage	(Debt in current liabilities + Long-term debt total) / Total assets	Compustat
CapEx to TA	Capital expenditures / Total asset	Compustat
Log(Firm_Age)	Logarithm of firm age, measured as the number of years from the firm's earliest available CRSP security trading record to the current year.	CRSP
ROA	Operating Income Before Depreciation and Amortization / book value of total assets	Compustat
R&D Intensity	R&D expenditures / Total asset (If R&D expenditures are missing, the variable is assigned a value of 0)	Compustat
	Mediating Variables	
High_Within_Gini	Pre-shock within-firm pay inequality, measured as the average Within_Gini over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median.	Revelio Labs
Number of New Positions	Total number of new positions created by treated firms during the four years after the shock. Defined as a dummy variable equal to one if the post-shock four-year value is above the treated-firm median.	Revelio Labs
Female Share of New Positions	The share of new positions filled by female employees during the four years after the shock. Defined as a dummy variable equal to one if the post-shock four-year share is above the treated-firm median.	Revelio Labs
Minority Share of New Positions	The share of new positions filled by minority employees during the four years after the shock. Defined as a dummy variable equal to one if the post-shock four-year share is above the treated-firm median.	Revelio Labs
Fraction of Treated Employees	Policy exposure intensity, measured as the share of a treated firm's employees located in policy-effective states in the shock year. Defined as a dummy variable equal to one if the exposure measure is above the treated-firm median.	Revelio Labs
High_Minority	Pre-shock minority share, measured as the average fraction of non-white employees over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median.	Revelio Labs

Appendix: Variable Definitions (Continued)

Variable Name	Variable Definition	Data source
High_Female	Pre-shock female share, measured as the average fraction of female employees over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median.	Revelio Labs
Teamwork_Culture _Index	Pre-shock teamwork culture, measured as the average teamwork culture index over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median. The index is obtained from Li, K. et al. (2021).	WRDS
High_R&D _Intensity	Pre-shock R&D intensity, measured as the average ratio of R&D expenses to total assets over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median.	Compustat
Workforce_Size	Pre-shock workforce size, measured as the average number of employees over the four years before the shock. Defined as a dummy variable equal to one if the pre-shock average is above the treated-firm median.	Compustat
Policy Bundle	Indicator for treated firms located in the six states where job history bans and pay transparency laws take effect in close succession, used to test additive effects under overlapping policy timing. Equals 1 for treated firms in these six states and 0 for treated firms in other job-history-ban states.	State policy data

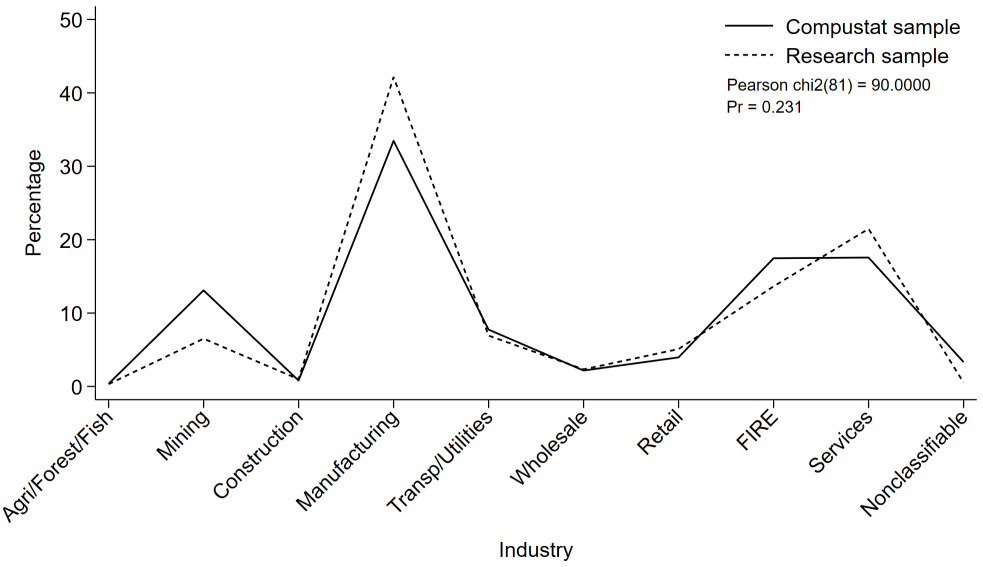


Figure A.1: Industry Distribution of Companies in the Sample

Figure A.1 plots the proportion of companies in different industries within the sample. The solid line represents the industry distribution of all companies in Compustat over the sample period, while the short-dashed line represents the industry distribution of firms for which employee salary information is available in Revelio Labs.

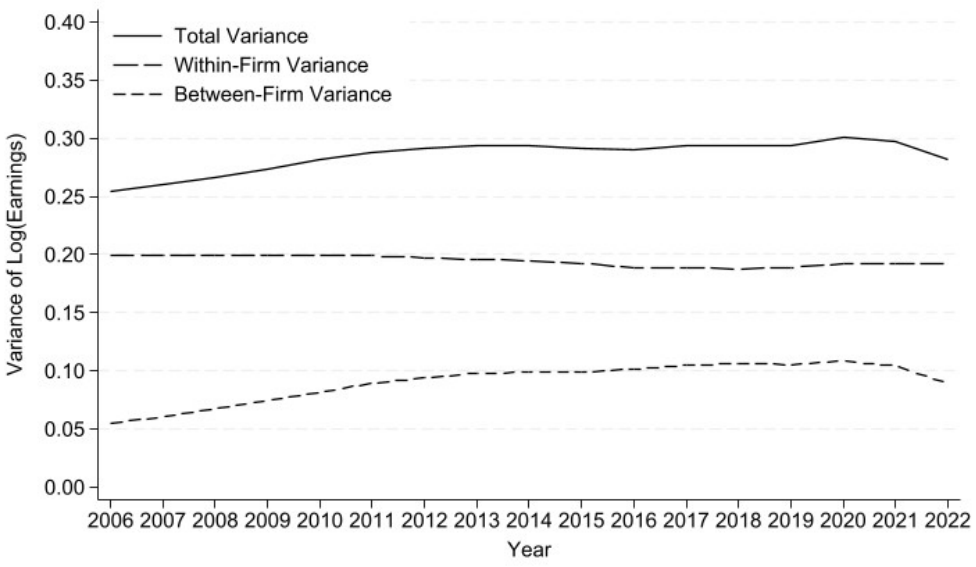


Figure A.2: Pay Variance Decomposition

Figure A.2 implements the employee-pay decomposition following the methodology in Song et al. (2019), using the full sample of publicly listed firms covered by Revelio Labs. Specifically, we decompose the total variance of employee pay into between-firm and within-firm components as described in the paper. Only firms and individuals in firms with at least 20 employees are included. Firm variance is calculated using mean log earnings and is weighted by the number of employees. Within firm variance is calculated based on the difference between individual log earnings and the firm's mean log earnings.

Table A.1: Within-Firm Executive Pay Ranking Correlation

Table A.1 evaluates whether the internal pay hierarchy among top executives within the same firm-year is consistently reflected across datasets. Using firm identifiers and executive last names, we match executives disclosed in the Summary Compensation Table (SCT) in proxy filings parsed by DirectEdgar (official, publicly disclosed compensation) to individuals in Revelio Labs. For each firm-year, we compute rank orderings of executives based on multiple compensation measures (e.g., total compensation and salary from the SCT, and salary and total compensation from Revelio). We also retain only firm-year observations in which the firm has at least two matched executives in that year. We then compute correlations between these rank variables across the two sources. High correlations indicate that Revelio Labs captures the relative compensation ordering among executives within the firm-year. We report standard errors in parentheses below each estimate. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	SCT_total	SCT_salary	salary	compensation
SCT_total	1.000			
	(0.000)			
SCT_salary	0.873***	1.000		
	(0.000)	(0.000)		
salary	0.667***	0.674***	1.000	
	(0.000)	(0.000)	(0.000)	
compensation	0.571***	0.554***	0.741***	1.000
	(0.000)	(0.000)	(0.000)	(0.000)

Table A.2: Across-Firm Median Employee Pay Ranking Correlation

Table A.2 focuses on cross-firm comparisons of employee pay. We obtain the officially reported median employee salary from CEO Pay Ratio filings and compare it to the firm-year median salary computed from Revelio Labs for employees in the same firm-year. We also retain observations only for firms that have at least one year during the sample period with employee counts exceeding 100. For each year, we assign percentile ranks to firms based on each median-pay measure and compute correlations between the rank variables from the two data sources. High correlations imply that Revelio Labs not only approximates the level of median employee pay but also preserves firms' relative positions in the cross-sectional distribution of employee pay. We report standard errors in parentheses below each estimate. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	rank_mediansalary	rank_median_Revelio
rank_mediansalary	1.000	
rank_median_Revelio	0.557***	1.000
	(0.000)	

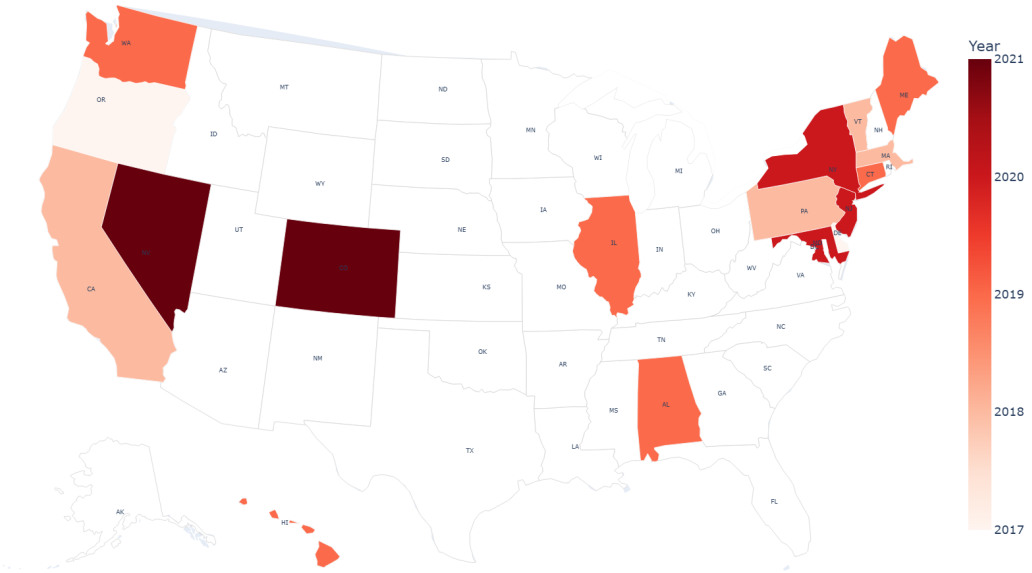


Figure 1: Geographic Distribution of Job History Ban Adoption Across U.S. States

Figure 1 illustrates the geographic distribution of U.S. states that implemented job history ban laws. States shown in red indicate policy adoption, with lighter to darker shades representing earlier to later implementation years. Specifically, the lightest shade denotes 2017 and the darkest shade denotes 2021. As the figure indicates, adopting states are spread across the country rather than clustered in a specific geographic area.

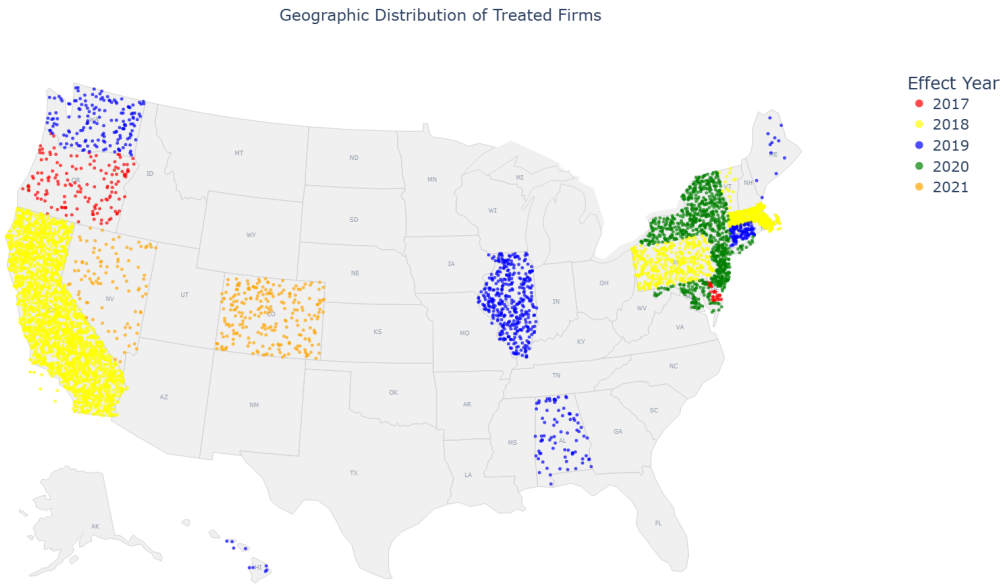
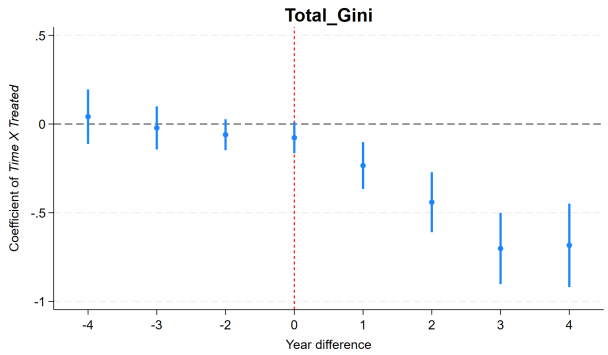
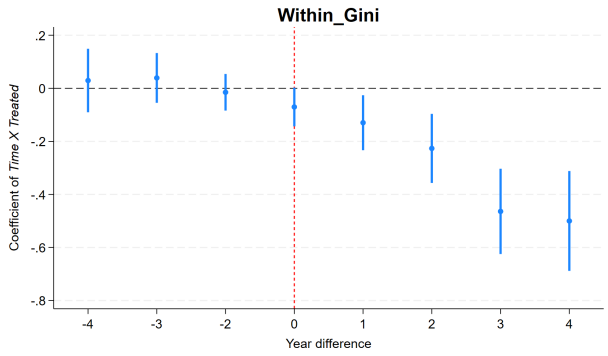


Figure 2: Geographic Distribution of Job History Ban Adoption Across U.S. States

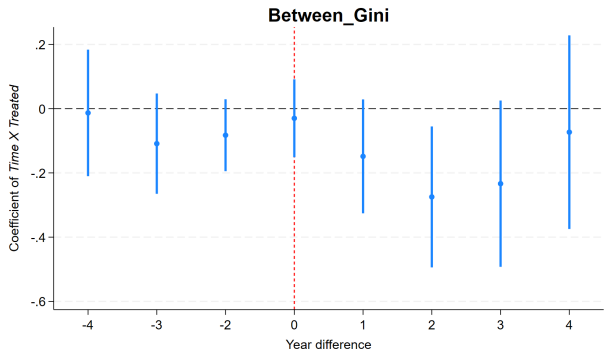
Figure 2 shows the nationwide distribution of firms affected by job history ban laws. A firm is classified as treated if, in the year the law becomes effective, employees located in the treated state account for more than 10% of the firm's total workforce. Each firm is assigned to the state whose policy affects the firm and is plotted accordingly. Marker colors indicate the treatment year, with red, yellow, blue, green, and orange representing 2017, 2018, 2019, 2020, and 2021, respectively.



(a) Total_Gini



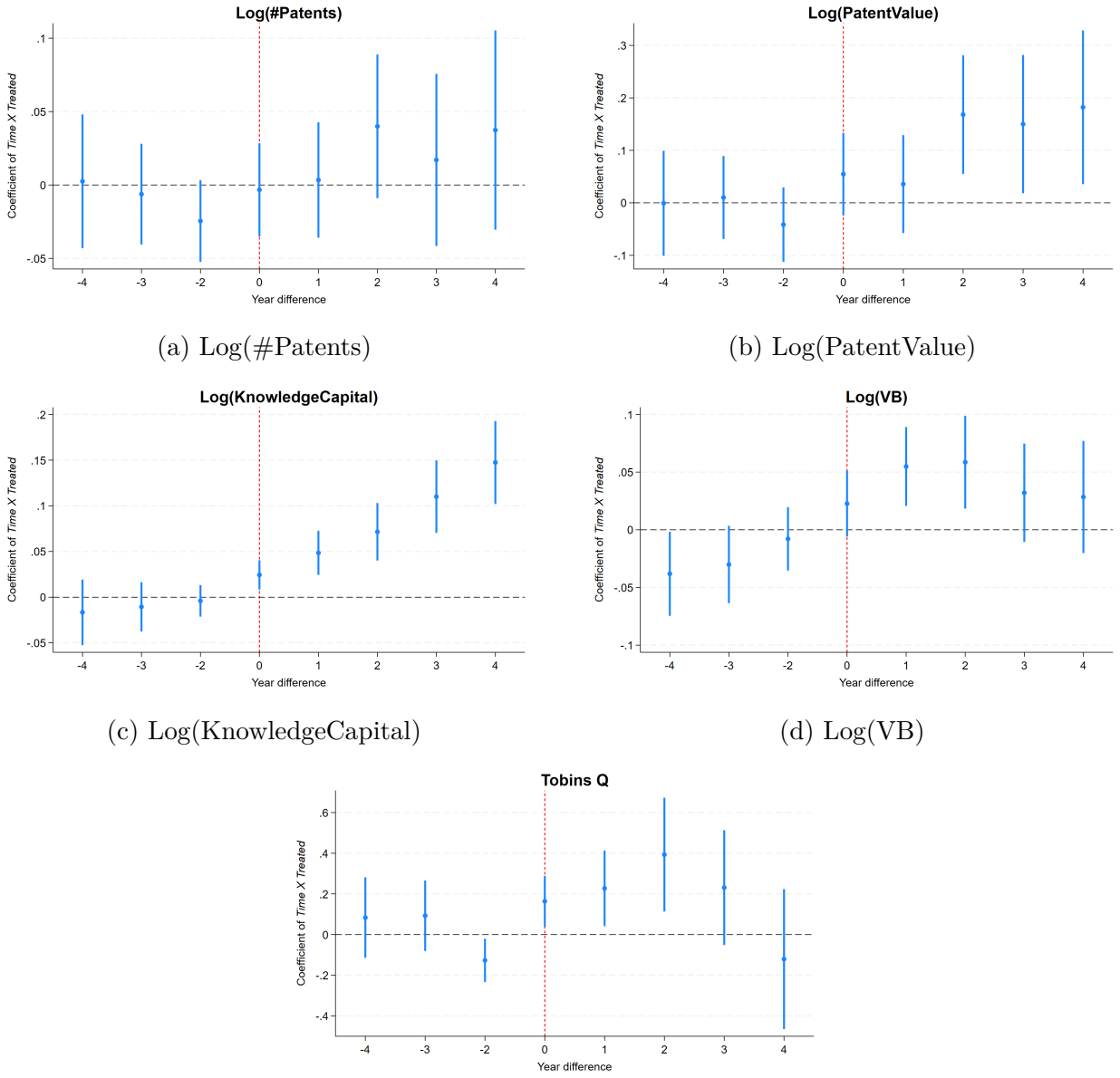
(b) Within_Gini



(c) Between_Gini

Figure 3: Parallel Trend Analysis for Pay Dispersion Variables

Figure 3 plots the coefficient estimates from regressions based on Equation (1). The dependent variables are: Panel (a) Total_Gini, Panel (b) Within_Gini, and Panel (c) Between_Gini. The coefficient at event time t represents the difference in the outcome variable between treated and control firms t years relative to the policy effective date. The event window spans from $t = -4$ to $t = +4$. The vertical line indicates the policy effective date. All coefficients are normalized relative to the year immediately preceding the policy effective date ($t = -1$). Dots indicate point estimates and vertical bars indicate 95% confidence intervals.



(a) Log(#Patents)

(b) Log(PatentValue)

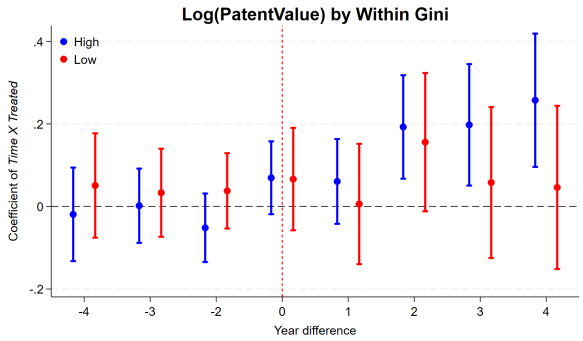
(c) Log(KnowledgeCapital)

(d) Log(VB)

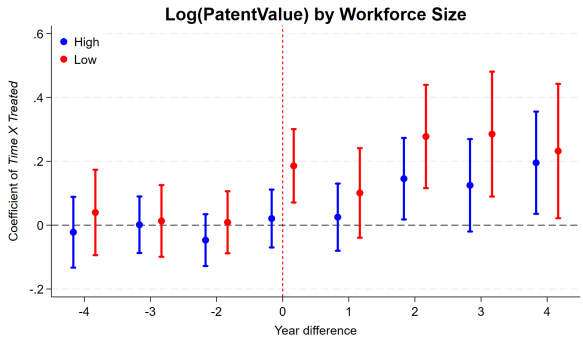
(e) Tobins Q

Figure 4: Parallel Trend Analysis for Outcome Variables

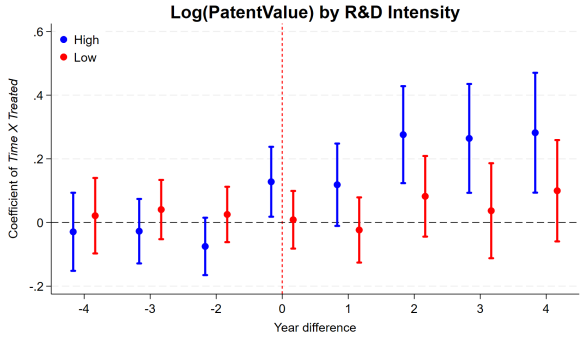
Figure 4 plots the coefficient estimates from regressions based on Equation (1). The dependent variables are: Panel (a) Log(#Patents), Panel (b) Log(PatentValue), Panel (c) Log(KnowledgeCapital), Panel (d) Log(VB) and Panel (e) Tobins Q. The coefficient at event time t represents the difference in the outcome variable between treated and control firms t years relative to the policy effective date. The event window spans from $t = -4$ to $t = +4$. The vertical line indicates the policy effective date. All coefficients are normalized relative to the year immediately preceding the policy effective date ($t = -1$). Dots indicate point estimates and vertical bars indicate 95% confidence intervals.



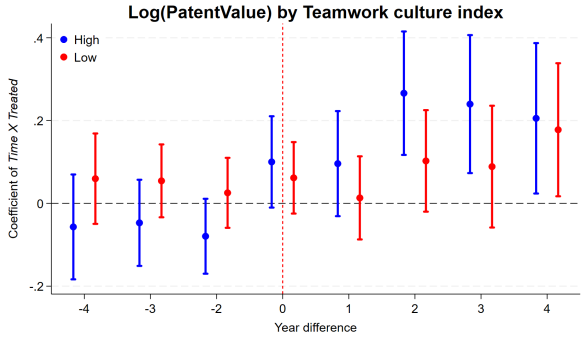
(a) Pre-shock Within-Firm Pay Inequality



(b) Pre-shock Workforce Size



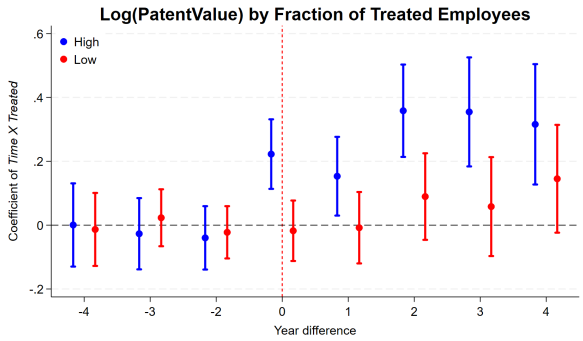
(c) Pre-shock R&D Intensity



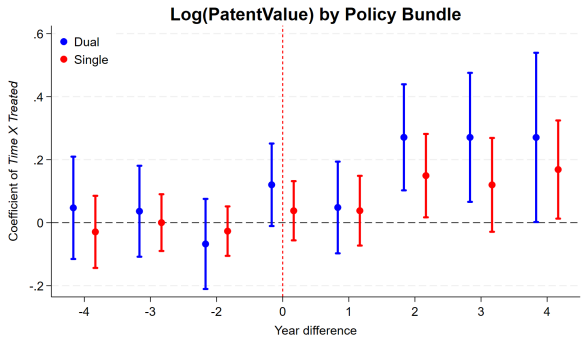
(d) Pre-shock Teamwork Culture Index

Figure 5: Heterogeneous treatment effect based on pre-event factors

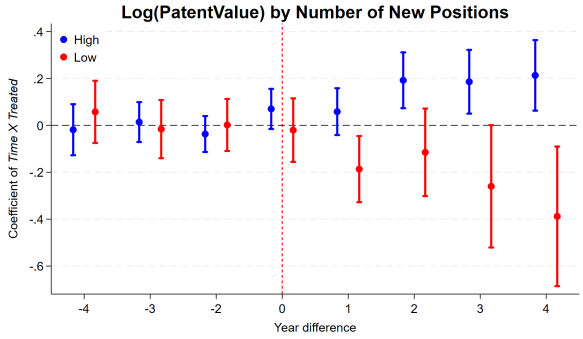
Figure 5 plots the dynamic effects on Log(PatentValue)—the log nominal market value of firm patents—over the four-year window before and after the shock. The "High" and "Low" lines correspond to splitting treated firms by different grouping variables and estimating the regression separately for each subgroup, while using the same control firms for comparison. Specifically, in all panels, each High indicator equals one if the corresponding grouping variable is above the treated-firm median (and zero otherwise). The vertical line indicates the policy effective date. All coefficients are normalized relative to the year immediately preceding the policy effective date ($t = -1$). Dots indicate point estimates and vertical bars indicate 95% confidence intervals.



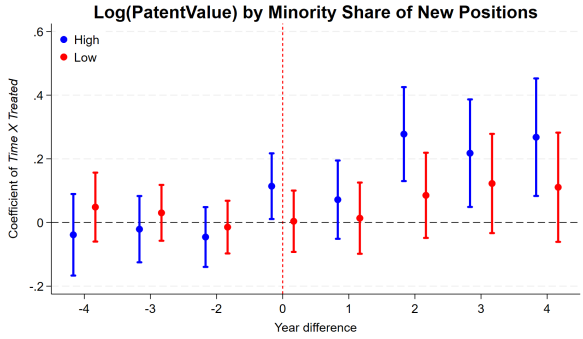
(a) Policy Exposure Intensity



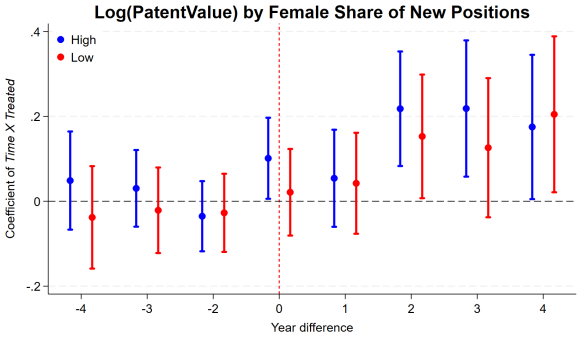
(b) Policy Bundling



(c) Post-shock New Position Creation



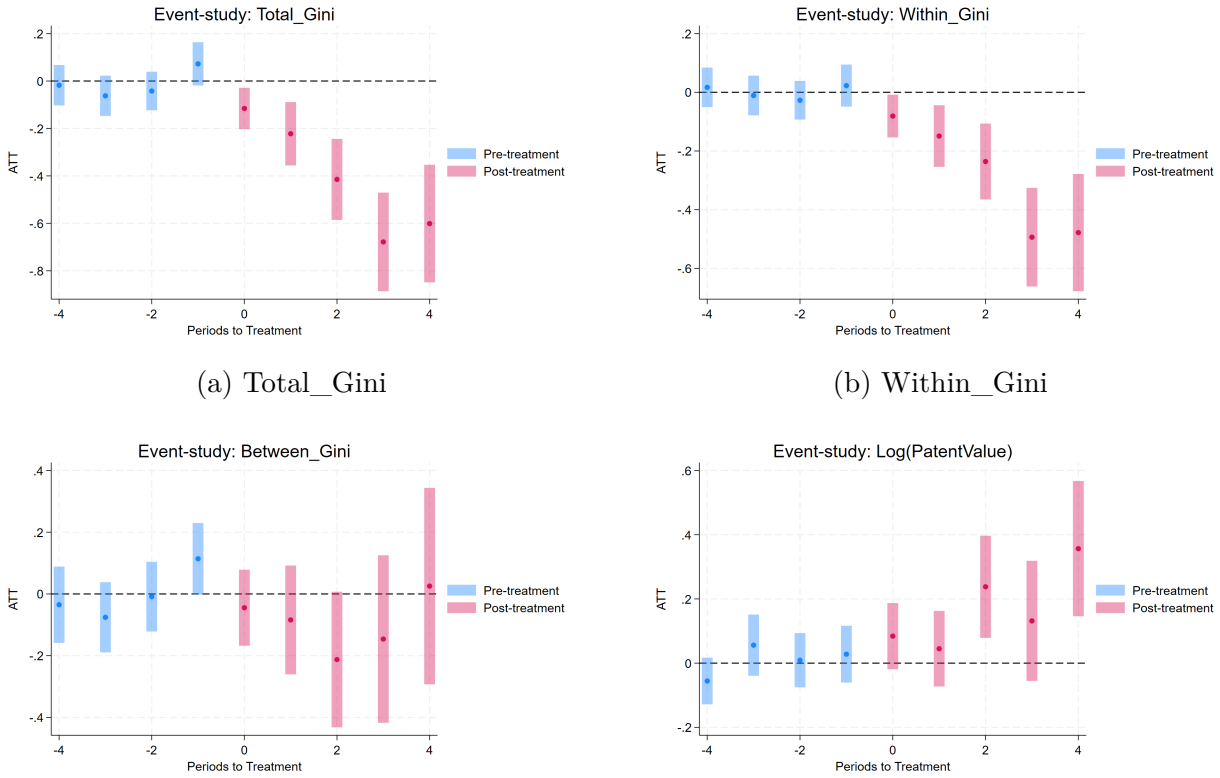
(d) Minority Share of New Positions



(e) Female Share of New Positions

Figure 6: Heterogeneous treatment effects based on post-event treatment intensity

Figure 6 plots the dynamic effects on Log(PatentValue)—the log nominal market value of firm patents—over the four-year window before and after the shock. The "High" and "Low" lines correspond to splitting treated firms by different grouping variables and estimating the regression separately for each subgroup, while using the same control firms for comparison. Specifically, in all panels except Panel (b), each High indicator equals one if the corresponding grouping variable is above the treated-firm median (and zero otherwise). Panel (b) splits treated firms into Dual and Single: we classify treated firms in MA, MD, OR, HI, WA, and NY as Dual (job history ban and pay transparency in close succession) and treated firms in all other job-history-ban states as Single. The vertical line indicates the policy effective date. All coefficients are normalized relative to the year immediately preceding the policy effective date ($t = -1$). Dots indicate point estimates and vertical bars indicate 95% confidence intervals.



(a) Total_Gini

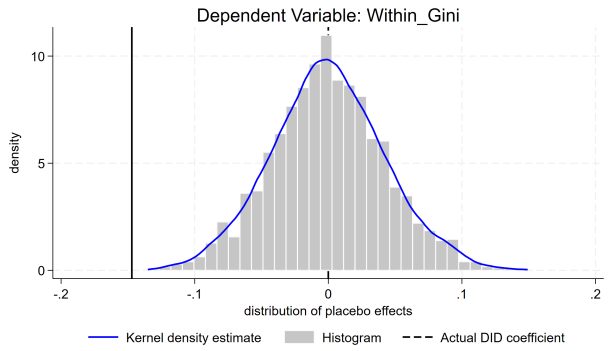
(b) Within_Gini

(c) Between_Gini

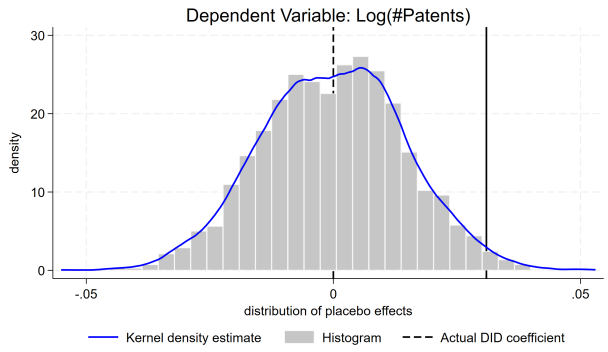
(d) Log(PatentValue)

Figure 7: Robustness Check: CSDID

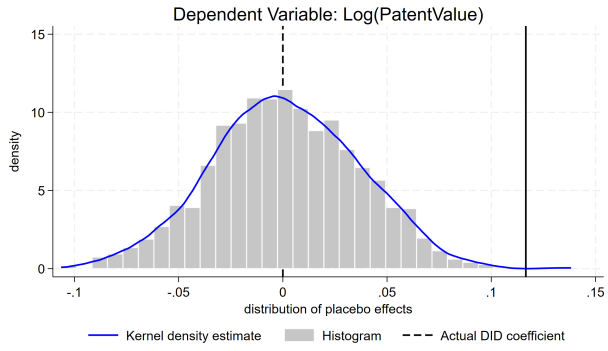
Figure 7 presents dynamic treatment effects estimated using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator (CSDID). Panel (a) reports results for Total_Gini, Panel (b) for Within_Gini, Panel (c) for Between_Gini, and Panel (d) for Log(PatentValue). The x-axis shows event time relative to the treatment year (0), with leads (-4 to -1) and lags (0 to $+4$). Each dot is the estimated ATT at event time t; shaded regions are 95% confidence intervals. The sample is restricted to a balanced event-time window so that effects at each lead/lag are identified from comparable cohorts. Estimates in the pre-treatment periods are close to zero and statistically insignificant, providing no evidence of differential pre-trends.



(a) Within_Gini



(b) Log(#Patents)



(c) Log(PatentValue)

Figure 8: Placebo Treatment Distribution for Pay Dispersion and Patent Outcomes

Figure 8 plots the distributions of placebo treatment effects from 2,000 random simulations. The dependent variables are: Panel (a) Within_Gini, Panel (b) Log(#Patents), and Panel (c) Log(PatentValue). In each simulation, placebo treatment status is randomly assigned within each cohort at the firm-by-cohort level, while preserving the actual number of treated firms in that cohort. The blue curve shows the kernel density estimate of the placebo coefficients, and the gray bars show the histogram. The vertical solid line indicates the actual estimated treatment effect from the baseline stacked DID regression.

Table 1: Implementation Years of Job History Ban Laws by State

This table lists the states in our sample that enacted salary history bans and the year in which each law became effective. The timing of adoption varies across states, with effective years ranging from 2017 to 2021. State abbreviations are reported in parentheses using the standard two-letter uppercase postal codes.

Year	State
2017	Delaware (DE), Oregon (OR), Puerto Rico (PR)
2018	California (CA), Massachusetts (MA), Pennsylvania (PA), Vermont (VT)
2019	Alabama (AL), Connecticut (CT), Hawaii (HI), Illinois (IL), Maine (ME), Washington (WA)
2020	Maryland (MD), New Jersey (NJ), New York (NY)
2021	Colorado (CO), Nevada (NV)

Table 2: Summary Statistics

This table presents summary statistics for all variables used in the main stacked difference-in-differences regressions. The sample is restricted to firms that have at least one year with more than 100 Revelio Labs employees and includes only observations within a balanced event-time window from $t = -2$ to $t = +2$.

	Obs.	Mean	S.D.	Min	p25	Median	p75	Max
Total_Gini	25,491	29.39	4.19	12.15	26.53	29.13	31.89	42.71
Within_Gini	25,491	17.16	2.18	0.93	15.91	17.19	18.49	23.64
Between_Gini	25,491	17.53	5.30	3.86	13.82	17.14	20.80	37.79
#Patents	25,509	10.40	38.64	0.00	0.00	0.00	1.00	241.00
PatentValue	25,509	411.49	1,554.85	0.00	0.00	0.00	13.72	8,628.02
Log(#Patents)	25,509	0.72	1.36	0.00	0.00	0.00	0.69	5.49
Log(PatentValue)	25,509	1.55	2.68	0.00	0.00	0.00	2.69	9.06
#Patents_Per_Team	25,509	0.39	0.66	0.00	0.00	0.00	1.00	2.40
Log(#Inventors)	25,509	0.91	1.61	0.00	0.00	0.00	1.61	6.04
Avg_Team_Size	25,509	1.05	1.75	0.00	0.00	0.00	2.50	6.00
Internal Commucation	20,292	-0.50	0.00	-0.51	-0.50	-0.49	-0.49	-0.49
KnowledgeCapital	25,457	300.28	1,059.30	0.00	0.00	0.00	120.59	7,531.67
Log(KnowledgeCapital)	25,457	2.24	2.76	0.00	0.00	0.00	4.80	8.93
Log(VB)	19,735	0.83	0.55	-0.57	0.48	0.92	1.20	2.06
Tobins Q	23,955	1.63	3.98	-5.39	0.41	0.83	1.60	77.81
Employee_Turnover	25,472	0.19	0.08	0.00	0.14	0.18	0.23	0.89
Likely_Voluntary	25,472	0.16	0.07	0.00	0.11	0.15	0.19	0.75
Skill_Turnover	25,470	0.18	0.08	0.00	0.13	0.17	0.22	0.93
Skill_Likely	25,470	0.15	0.07	0.00	0.11	0.14	0.18	0.80
#Applications_Per_Inventor	25,509	0.57	0.95	0.00	0.00	0.00	1.00	4.33
Firm Size	25,509	7.58	2.03	-2.40	6.18	7.55	8.93	12.32
Leverage	25,509	0.30	0.29	0.00	0.10	0.27	0.43	8.25
CapEx to TA	25,509	0.04	0.05	0.00	0.01	0.03	0.05	0.44
Log(Firm_Age)	25,509	2.81	1.03	0.00	2.20	3.09	3.56	4.03
ROA	25,509	0.06	0.25	-12.35	0.03	0.10	0.15	0.40
R&D Intensity	25,509	0.04	0.10	0.00	0.00	0.00	0.02	1.18

Table 3: Pay Dispersion and Job History Ban

Dependent variables: Total_Gini measures a firm's overall pay dispersion; Within_Gini measures within-seniority (horizontal) pay dispersion; Between_Gini measures between-seniority (vertical) pay dispersion. Key regressor: DiD is the interaction Treated × Post, where Treated = 1 if a firm's share of employees located in the treated state in the policy's implementation year exceeds 10% (and 0 otherwise), and Post = 1 for years greater than or equal to the policy implementation year (and 0 otherwise). This regression restricts the sample to firms that have at least one year in which the number of Revelio Labs employees exceeds 100, and includes only observations within a balanced event-time window of $t = -2$ to $t = +2$. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	(1) Total_Gini	(2) Within_Gini	(3) Between_Gini
DID	-0.212***	-0.147***	-0.092
	(0.061)	(0.048)	(0.081)
Firm Size	-0.271***	0.260***	-0.345***
	(0.072)	(0.077)	(0.113)
Leverage	0.091	-0.221	0.445**
	(0.162)	(0.147)	(0.174)
CapEx to TA	-1.334*	-0.107	-1.023
	(0.788)	(0.584)	(1.048)
log(Firm Age)	-0.198*	-0.052	-0.051
	(0.108)	(0.080)	(0.145)
ROA	-0.160	-0.253*	0.110
	(0.163)	(0.149)	(0.246)
R&D to TA	-1.098*	0.543	-1.395*
	(0.638)	(0.482)	(0.776)
Adjusted R^2	0.919	0.816	0.911
Observations	25,025	25,025	25,025
Year#Cohort FE	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes

Table 4: Innovation Outcomes and Job History Ban

Dependent variables: Log(#Patents) $\equiv$ (#Patents + 1); Log(PatentValue) $\equiv$ (PatentValue + 1). Key regressor: DiD is the interaction Treated $\times$ Post, where Treated = 1 if a firm's share of employees located in the treated state in the policy's implementation year exceeds 10% (and 0 otherwise), and Post = 1 for years greater than or equal to the policy implementation year (and 0 otherwise). Columns (1)–(2) restrict the sample to firms with at least one year in which the number of Revelio lab employees exceeds 100. Columns (3)–(4) report regressions using the full sample. Columns (5)–(6) restrict the sample to firms with patent records in at least one year. All samples are restricted to a balanced event-time window from $t = -2$ to $t = +2$. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	(1) Log (#Patents)	(2) Log (PatentValue)	(3) Log (#Patents)	(4) Log (PatentValue)	(5) Log (#Patents)	(6) Log (PatentValue)
DiD	0.031*	0.117***	0.025***	0.109***	0.053**	0.164***
	(0.018)	(0.042)	(0.009)	(0.020)	(0.024)	(0.051)
Firm Size	0.094***	0.266***	0.020***	0.053***	0.168***	0.420***
	(0.018)	(0.041)	(0.003)	(0.006)	(0.021)	(0.044)
Leverage	-0.018	-0.071	0.003***	0.006***	0.015	0.013
	(0.024)	(0.056)	(0.001)	(0.002)	(0.012)	(0.023)
CapEx to TA	-0.057	-0.104	-0.009	-0.030	-0.045	-0.463
	(0.107)	(0.276)	(0.012)	(0.026)	(0.198)	(0.432)
Log(Firm_Age)	-0.016	-0.010	0.011	-0.001	0.006	-0.033
	(0.033)	(0.065)	(0.011)	(0.022)	(0.043)	(0.078)
ROA	-0.043	-0.117*	-0.003***	-0.011***	-0.010	-0.069***
	(0.029)	(0.062)	(0.001)	(0.002)	(0.013)	(0.022)
R&D Intensity	0.088	-0.054	0.043**	0.012	0.312***	0.470***
	(0.133)	(0.243)	(0.021)	(0.033)	(0.073)	(0.115)
Adjusted R^2	0.934	0.889	0.939	0.913	0.913	0.873
Observations	25,042	25,042	107,438	107,438	27,876	27,876
Year#Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5: Alternative Firm Outcomes and Job History Ban

Dependent variable: Tobins Q , a proxy for Tobin's q, which is the ratio of capital's market value to its replacement cost and obtained from the data constructed by Taylor & Peters (2017); Log(VB) is Long-run Value/Book following the Rhodes-Kropf et al. (2005) decomposition; Log(KnowledgeCapital) = log(KnowledgeCapital + 1). KnowledgeCapital is the replacement cost of the firm's knowledge capital, obtained from the data constructed by Taylor & Peters (2017). Key regressor: DiD is the interaction Treated × Post, where Treated = 1 if a firm's share of employees located in the treated state in the policy's implementation year exceeds 10% (and 0 otherwise), and Post = 1 for years greater than or equal to the policy implementation year (and 0 otherwise). Columns (1)–(3) restrict the sample to firms with at least one year in which the number of Revelio lab employees exceeds 100. Columns (4)–(6) report regressions using the full sample. All samples are restricted to a balanced event-time window from $t = -2$ to $t = +2$. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	(1) Log (KnowledgeCapital)	(2) Tobins Q	(3) Log (VB)	(4) Log (KnowledgeCapital)	(5) Tobins Q	(6) Log (VB)
DID	0.054***	0.255***	0.047***	0.071***	0.493***	0.019*
	(0.013)	(0.088)	(0.014)	(0.007)	(0.117)	(0.011)
Firm Size	0.161***	-0.369*	-0.057***	0.064***	-0.778***	-0.055***
	(0.017)	(0.213)	(0.017)	(0.005)	(0.151)	(0.011)
Leverage	-0.032	0.094	0.334***	0.004**	0.160	0.169***
	(0.028)	(0.345)	(0.069)	(0.002)	(0.125)	(0.038)
CapEx to TA	0.199***	-0.218	-0.024	0.010	-0.826	0.011
	(0.073)	(0.790)	(0.198)	(0.017)	(0.833)	(0.101)
log(Firm Age)	0.232***	-0.284	0.004	0.221***	-2.668***	0.014
	(0.025)	(0.189)	(0.025)	(0.013)	(0.312)	(0.015)
ROA	-0.028	1.005**	0.107*	-0.010***	0.247***	0.009
	(0.031)	(0.485)	(0.058)	(0.001)	(0.082)	(0.015)
R&D to TA	0.550***	0.115	0.117	0.275***	-1.415**	0.024
	(0.077)	(0.701)	(0.117)	(0.021)	(0.560)	(0.045)
Adjusted R^2	0.995	0.736	0.655	0.994	0.818	0.674
Observations	24,993	23,499	19,258	106,843	85,294	43,536
Year#Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Mechanism Analysis: Employee Outcomes

Dependent variables: Employee_Turnover is the firm-year employee turnover rate, defined as the number of employees who left the company during the year divided by the total number of employees in the same year. Likely_Voluntary is the share of leavers who find a new job within six months after leaving, used as a proxy for voluntary turnover. Skill_Turnover is the turnover rate among high-skill employees (job positions classified as ONET Job Zone 4 or 5), defined as the number of high-skill leavers during the year divided by the total number of high-skill employees in the same year. Skill_Likely is the share of high-skill leavers who find a new job within six months after leaving. #Applications_Per_Inventor is firm-year patent applications per inventor, defined as the total number of patent applications filed in a given year by inventors associated with the firm, divided by the number of unique inventors associated with the firm in that year. Firm-inventor associations are identified based on granted patent records during the 2013-2023 sample period. Key regressor: DiD is the interaction Treated × Post, where Treated = 1 if a firm's share of employees located in the treated state in the policy's implementation year exceeds 10% (and 0 otherwise), and Post = 1 for years greater than or equal to the policy implementation year (and 0 otherwise). This regression restricts the sample to firms that have at least one year in which the number of Revelio Labs employees exceeds 100, and includes only observations within a balanced event-time window of $t = -2$ to $t = +2$. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	(1)	(2)	(3)	(4)	(5)
	Employee_Turnover	Likely_Voluntary	Skill_Turnover	Skill_Likely	#Applications_ Per_Inventor
DID	-0.006***	-0.005**	-0.007***	-0.005**	0.052***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.017)
Firm Size	0.020***	0.015***	0.020***	0.014***	-0.008
	(0.003)	(0.003)	(0.003)	(0.003)	(0.015)
Leverage	0.031***	0.026***	0.031***	0.028***	0.021
	(0.009)	(0.008)	(0.009)	(0.008)	(0.026)
CapEx to TA	0.117***	0.100***	0.118***	0.100***	0.213*
	(0.030)	(0.024)	(0.031)	(0.027)	(0.126)
log(Firm Age)	0.016***	0.014***	0.016***	0.015***	-0.025
	(0.003)	(0.003)	(0.004)	(0.003)	(0.026)
ROA	-0.020**	-0.018*	-0.020**	-0.019**	0.016
	(0.010)	(0.009)	(0.010)	(0.009)	(0.044)
R&D to TA	0.054	0.045	0.049	0.037	-0.129
	(0.043)	(0.039)	(0.042)	(0.038)	(0.110)
Adjusted R^2	0.543	0.549	0.456	0.477	0.821
Observations	25,006	25,006	25,004	25,004	25,042
Year#Cohort FE	Yes	Yes	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes	Yes	Yes

Table 7: Mechanism Analysis: Team Outcomes

Dependent variables: #Patents_Per_Team denotes the firm-year average patent output per inventor team, where an inventor team is defined as a distinct combination of inventors listed jointly on a patent. Log(#Inventors) represents the logarithm of the total number of unique inventors across all patents granted to the firm in that year; Avg_Team_Size represents the average number of inventors listed per granted patent in that firm-year. Internal_Communication represents the median value of individual employee review scores on internal communication within a firm-year. This variable is constructed only for firm-year observations with at least five employee reviews in that year. Key regressor: DiD is the interaction Treated × Post, where Treated = 1 if a firm's share of employees located in the treated state in the policy's implementation year exceeds 10% (and 0 otherwise), and Post = 1 for years greater than or equal to the policy implementation year (and 0 otherwise). The sample is restricted to firms that have at least one year with more than 100 employees in Revelio Labs. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	(1)	(2)	(3)	(4)
	#Patents_Per_Team	Log(#Inventors)	Avg_Team_Size	Internal_ Communication
DID	0.025**	0.043**	0.075**	0.017*
	(0.013)	(0.022)	(0.033)	(0.010)
Firm Size	0.054***	0.112***	0.128***	-0.035***
	(0.012)	(0.023)	(0.034)	(0.011)
Leverage	0.000	-0.037	-0.070	-0.055*
	(0.022)	(0.033)	(0.062)	(0.032)
CapEx to TA	0.024	-0.050	-0.057	0.042
	(0.101)	(0.154)	(0.331)	(0.149)
log(Firm Age)	-0.001	-0.019	-0.028	-0.044**
	(0.021)	(0.040)	(0.059)	(0.018)
ROA	-0.034	-0.029	-0.005	0.095**
	(0.031)	(0.049)	(0.112)	(0.042)
R&D to TA	0.036	0.150	0.183	-0.026
	(0.136)	(0.206)	(0.423)	(0.265)
Adjusted R^2	0.767	0.916	0.752	0.449
Observations	25,042	25,042	25,042	19,650
Year#Cohort FE	Yes	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes	Yes

Table 8: Robustness Check: IV/2SLS Approach

Estimation: This table reports two-stage least squares (2SLS) estimates. First stage: we regress Within_Gini on the excluded instrument Post×Exposure (and controls). The variable Exposure measures a firm's exposure intensity to the shock, defined as the share of the firm's employees located in the treated state in the shock year relative to the firm's total employment. Exposure is observed only from the treatment (shock) year onward for treated firms, and thus takes non-missing values in the treatment year and subsequent years; it is missing otherwise. Accordingly, Post × Exposure varies with both the post-shock timing and the firm's exposure intensity. Second stage: we use the fitted value of Within_Gini from the first stage to estimate its effect on firm outcomes. The dependent variable is F1_Log(PatentValue), which denotes the one-year-ahead value of Log(PatentValue). Log(PatentValue) $\equiv \log(\text{PatentValue}+1)$. Sample: Columns (1)–(2) use the full sample covering 2006–2023. Columns (3)–(4) restrict the sample to 2013–2023, matching the sample window used in the baseline stacked DiD specifications. The sample is restricted to firms that have at least one year with more than 100 employees in Revelio Labs. All control variables are lagged by one year. Standard errors are clustered at the firm level, and standard errors are reported in parentheses. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

	Within_Gini	F1_Log(PatentValue)	Within_Gini	F1_Log(PatentValue)
	(1)	(2)	(4)	(5)
Post×Exposure	-0.741***		-0.621***	
	(0.140)		(0.118)	
Within_Gini		-0.522***		-0.369**
		(0.151)		(0.144)
Firm Size	0.331***	0.538***	0.362***	0.441***
	(0.048)	(0.060)	(0.054)	(0.067)
Leverage	0.091	-0.007	-0.024	-0.044
	(0.160)	(0.090)	(0.165)	(0.074)
CapEx to TA	-0.460	-0.327	-1.339**	-0.579
	(0.514)	(0.368)	(0.637)	(0.441)
Log(Firm_Age)	-0.099*	-0.002	-0.176***	0.008
	(0.055)	(0.050)	(0.064)	(0.064)
ROA	0.139	0.006	0.101	-0.053
	(0.147)	(0.072)	(0.229)	(0.072)
R&D Intensity	0.492	0.758***	1.323**	1.011***
	(0.422)	(0.284)	(0.570)	(0.355)
Observations	36249	36249	20975	20975
R-squared		-0.349		-0.138
KP rk LM statistic	26.618		26.562	
KP rk LM statistic p-value	0.000		0.000	
KP rk Wald F	28.173		27.891	
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 9: Robustness Check: Using Headquarters State to Define Treatment

Dependent variables: In Panel (a), Total_Gini measures a firm's overall pay dispersion, Within_Gini measures within-seniority (horizontal) pay dispersion, and Between_Gini measures between-seniority (vertical) pay dispersion. In Panel (b), Log(PatentValue) $\equiv$ log(PatentValue + 1). Key regressor: DiD is the interaction Treated $\times$ Post, where Treated = 1 if a firm's headquarters is located in the state that implements the job history ban in the policy's effective year (and 0 otherwise), and Post = 1 for years greater than or equal to the policy effective year (and 0 otherwise). Samples: For Panel (a), the sample is restricted to firms that have at least one year with more than 100 employees in Revelio Labs. For Panel (b), Column (1) uses the same restriction (firms with at least one year with more than 100 employees in Revelio Labs), Column (2) reports regressions using the full sample, and Column (3) restricts the sample to firms with patent records in at least one year. All samples are restricted to a balanced event-time window from $t = -2$ to $t = +2$. All control variables are lagged by one year. Standard errors are clustered at the firm level. We report standard errors in parentheses below each estimate. Variable definitions are provided in Appendix B. ***, **, and * indicate significance at the 1, 5, and 10 percent levels.

(a) Pay Dispersion

	(1) Total_Gini	(2) Within_Gini	(3) Between_Gini
DiD	-0.169***	-0.109**	-0.046
	(0.062)	(0.052)	(0.081)
Adjusted R^2	0.906	0.803	0.901
Observations	34,152	34,152	34,152
Year#Cohort FE	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes

(b) R&D Performance

	(1) Log(PatentValue)	(2) Log(PatentValue)	(3) Log(PatentValue)
DiD	0.175***	0.114***	0.158***
	(0.048)	(0.023)	(0.054)
Firm Size	0.307***	0.064***	0.455***
	(0.049)	(0.008)	(0.049)
Leverage	-0.065	0.008***	0.002
	(0.056)	(0.002)	(0.028)
CapEx to TA	-0.092	-0.046*	-0.466
	(0.261)	(0.028)	(0.443)
Log(Firm_Age)	0.047	0.036*	0.102
	(0.056)	(0.022)	(0.077)
ROA	-0.090	-0.013***	-0.076***
	(0.072)	(0.002)	(0.024)
R&D Intensity	-0.070	-0.003	0.483***
	(0.240)	(0.034)	(0.115)
Adjusted R^2	0.886	0.906	0.864
Observations	34,222	115,873	31,309
Year#Cohort FE	Yes	Yes	Yes
Firm#Cohort FE	Yes	Yes	Yes