Silencing Salary Talk: How Salary History Bans Affect Corporate Investment and Performance

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Matthew Serfling
University of Tennessee-Knoxville and ECGI

Wenxuan Sun
University of Tennessee-Knoxville

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Abstract

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Keywords: Salary History Bans, Labor, Investment, Firm Performance

JEL Classifications: G30, G31, G32, J71, K31

Matthew Serfling*
layton Homes Chair of Excellence in Finance, Professor
University of Tennessee-Knoxville
Stokely Management Center, Room 433
Knoxville, TN 37996-0540, USA
e-mail: mserflin@utk.edu

Wenxuan Sun
Researcher
University of Tennessee-Knoxville
453 Haslam Business Building
Knoxville, TN 37996-4140, USA
e-mail: wsun22@vols.utk.edu

*Corresponding Author

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Matthew Serfling and Wenxuan Sun*

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ABSTRACT

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*Matthew Serfling (mserflin@utk.edu) and Wenxuan Sun (wsun22@vols.utk.edu) are at the Haslam College of Business at the University of Tennessee. We are grateful for the helpful comments and suggestions from Daniel Greene, Scott Guernsey, Tingting Liu, Ruixiang Wang (discussant), and Cheng Zhang (discussant); and conference and seminar participants at the 2025 Financial Management Association (FMA) Annual Meeting, 2025 Southern Finance Association (SFA) Annual Meeting, and University of Tennessee.

1 Introduction

Labor market regulations aimed at promoting equity and fairness have become an increasingly prominent and controversial feature of the U.S. policy landscape. While proponents argue that such policies correct historical inequities and improve labor market outcomes, critics contend that they introduce frictions that distort firm behavior, raise costs, and hinder growth. From a corporate finance perspective, the net effect of these interventions is far from obvious. Policies that reshape how firms recruit, compensate, and retain workers may reverberate well beyond wages, influencing firms' investment decisions, capital allocation, and long-run performance. Yet, despite intense debate surrounding these policies, there is limited evidence on how firms actually adjust along these margins.

In this paper, we examine how salary history bans (SHBs) affect firms' investment strategies and organizational performance. SHBs are a plausibly exogenous shock to the information available at the hiring stage and provide a particularly salient setting to study how frictions in the hiring process impact firms. Because employee performance is central to organizational success, firms face substantial uncertainty when evaluating prospective hires, particularly when attempting to assess worker productivity prior to employment. To mitigate this uncertainty, employers commonly rely on market-based signals, most notably an applicant's prior salary, as proxies for marginal productivity and reservation wages. Although imperfect, salary histories convey valuable information that helps reduce informational asymmetries in labor contracting and wage bargaining (e.g., Fossati, Wilson, and Bonoli, 2020; Barach and Horton, 2021; Davis, Ouimet, and Wang, 2022). SHBs explicitly prohibit employers from inquiring about candidates' prior compensation, with the stated objective of reducing persistent gender and racial wage gaps by limiting the extent to which historical pay disparities anchor future wages (e.g., Blau and Kahn, 2017; Bessen, Denk, and Meng, 2024).

By restricting access to a commonly used hiring signal, SHBs fundamentally alter the informational environment surrounding recruitment and compensation decisions, with potentially far-reaching implications for firms' investment behavior and organizational performance.

SHBs constitute a shock to the labor market's informational environment, generating ambiguous predictions for firm behavior. By eliminating a commonly used signal of worker productivity and reservation wages, SHBs limit firms' ability to assess and price a key production input (labor). This loss of information affects wage-setting and hiring decisions. In wage negotiations, weaker outside information shifts bargaining power toward job applicants and raises expected labor costs (e.g., Barach and Horton, 2021; Bessen et al., 2024). In hiring, SHBs amplify informational frictions, particularly when labor is costly to substitute and in skill-intensive settings where productivity is difficult to observe (e.g., Gibbons and Waldman, 2004; Acemoglu and Autor, 2011). Together, these forces increase uncertainty surrounding labor expenses and workforce quality, raising the cost of committing to long-term and irreversible investments such as capital expenditures and R&D (e.g., Bernanke, 1983; Bloom, Bond, and Van Reenen, 2007; Gulen and Ion, 2016).

At the same time, the removal of salary history may induce offsetting organizational responses. Because salary history can be noisy, biased, or shaped by firm-specific wage-setting practices, its elimination may push firms toward more structured and merit-based hiring processes that rely on observable human capital characteristics, such as education, experience, and performance-based assessments (e.g., Spence, 1973; Fossati et al., 2020). To the extent that these practices improve match quality, SHBs may enhance productivity, reduce turnover, and strengthen workplace morale (e.g., Barach and Horton, 2021; Bloom, Liang, Roberts, and Ying, 2015). Moreover, higher hiring frictions and labor costs may discipline managerial decision-making, prompting firms to scale back marginal projects and adopt more selective capital allocation strategies, thereby reducing overinvestment and improving investment efficiency (e.g., Biddle, Hilary, and Verdi, 2009; Chen, Hope, Li, and Wang, 2011; Gormley, Matsa, and Milbourn, 2013).

To identify the effect of SHBs on firms, we exploit the passage of U.S. state laws that ban private-sector employers from inquiring about prospective employees' past salaries. Since 2016, 19 states have enacted SHBs. Employers are legally required to comply with these bans, and failure to do so can lead to fines and lawsuits. Penalties for non-compliance vary by jurisdiction but can be substantial. For example, in New York City, violations can result in civil penalties up to \$125,000 for an unintentional violation, and up to \$250,000 for an intentional violation. The intended purpose of these laws is to reduce wage disparities, which is unrelated to firms' investment decisions and overall firm performance. Consistent with this notion, in a hazard model examining the timing of SHB adoptions, state-level investment rates and measures of performance do not consistently predict the adoption of SHBs. In addition, based on a large set of state-level variables that capture a state's economic condition, political leaning, tax environment, demographics, and other social policies, the only consistent predictor of SHB adoption is the generosity of a state's unemployment benefits.

We examine the effect of SHBs in a difference-in-differences (DiD) research design. We account for concerns about biases arising in two-way fixed effects (TWFE) staggered DiD settings due to treatment effect heterogeneity by using an imputation-based methodology (Baker, Larcker, and Wang, 2022; Borusyak, Jaravel, and Spiess, 2024). This approach uses observations of firms in states that never and have not yet adopted SHBs (control group) to impute the counterfactuals for the observations of firms in states that have (treatment group). The average treatment effect on the treated (ATT) equals the weighted sum of the differences between the treated observations and their imputed counterfactuals, where weights depend on treatment assignment and timing. Our tests include firm and year fixed effects. When we compare firm- and state-level characteristics across treated and control groups, we find some differences. To address concerns that these differences bias the ATTs, we weight the regressions by the inverse probability of a firm being treated, which is estimated using several firm- and state-level controls. We further estimate models with and without this extensive set of controls and show the results hold in both specifications.

Our results reveal a nuanced but coherent pattern in how firms adjust to SHBs that mirrors the mechanisms outlined above. Relative to firms in non-adopting states, firms reduce capital and R&D expenditures following SHB adoption, consistent with heightened uncertainty around labor costs and worker productivity raising the effective shadow cost of labor and discouraging irreversible investment. At the same time, investment efficiency improves, driven by a reduction in overinvestment. Consistent with the organizational adjustments induced by SHBs, profitability, total factor productivity (TFP), labor productivity, and firm value increase on average after SHBs are implemented. Together, these findings suggest that by removing a noisy and potentially biased compensation signal, SHBs push firms toward more selective and disciplined hiring and investment practices, improving worker-firm match quality and curbing marginal projects, even as overall investment declines.[1]

When we explore the dynamic effects of SHBs on these outcomes, two findings emerge. First, we find no differential trends in investment and performance between treated and control firms in the years leading up to SHB adoptions, consistent with our research design satisfying the parallel trends assumption of the DiD methodology. Second, the interplay between SHBs, investment, and firm performance becomes more complex but revealing. While reductions in investment and improvements in investment efficiency after SHBs are adopted persist for at least five years, this is not the case for all the other measures of firm performance. In particular, usually by the fourth year after SHBs are adopted, the improvements in profitability, labor productivity, and firm value are statistically indistinguishable from

[1] We formalize these effects in a simple theoretical model in Appendix A in the Online Appendix.

levels observed before the bans were put in place. These results suggest that while reducing investment initially leads to improved resource allocation and better performance, such gains are not sustainable in the long term.

To shed light on the underlying mechanisms, we first examine whether the effects of SHBs are concentrated in settings where labor-side information frictions and bargaining constraints are most consequential. We study heterogeneity in the substitutability of labor with automated capital (SLAC), worker skill requirements, union membership, and governance strength. When labor can be easily substituted with capital, hiring frictions should be less binding, implying a smaller response to SHBs. By contrast, in skill-intensive industries where worker productivity is harder to assess ex ante (e.g., Meli and Spindler, 2019) and in firms with low union coverage where wages are negotiated more flexibly and salary history plays a larger role in screening and bargaining, SHBs should have a larger impact. Similarly, if firms are optimally adjusting to increased uncertainty, the effects of SHBs should be strongest in firms with stronger governance and greater product market competition. Consistent with these predictions, we find that among firms with lower SLAC, higher worker skill, lower union membership, stronger external governance, or greater product market competition, SHBs are associated with larger declines in capital and R&D investment alongside more pronounced improvements in investment efficiency, productivity, and financial performance.

We further directly examine how SHBs affect worker outcomes and employee perceptions, which inform both the cost and quality of labor inputs faced by firms. By restricting access to salary history, SHBs weaken firms' ability to infer job candidates' reservation wages and outside options, shifting bargaining power toward workers and increasing firms' marginal hiring costs. Using worker-level data from the Current Population Survey (CPS), we find that wages rise following SHB adoption, with gains larger among women and non-white employees, groups the laws aim to protect. Wage increases are also larger among newly hired workers compared to incumbent workers, which is consistent with SHBs operating through the hiring margin where firms lack worker-specific information and previously relied more heavily on prior pay as a bargaining benchmark. These higher and more uncertain hiring costs provide a direct channel through which SHBs can discourage investment.

At the same time, SHBs may induce offsetting organizational adjustments that improve the effectiveness with which labor is deployed. By removing a noisy and potentially biased compensation signal, SHBs can push firms toward more structured and merit-based hiring practices, improving worker-firm matching and workplace quality. Using employee reviews from Glassdoor, we find that following SHB adoption, employees report improvements in perceived compensation fairness, career opportunities, and workplace culture. These patterns suggest gains in morale, match quality, and organizational cohesion, which are consistent with the productivity and performance improvements documented in our firm-level analysis.

In our final set of analyses, we explore the sensitivity of our results to possible omitted variables and further show that our results survive a number of robustness checks. First, test statistics proposed by Oster (2019) that indicate how susceptible our results are to omitted variable biases suggest that this concern is unlikely to materially affect our conclusions. Second, our results hold after further including industry-year fixed effects to control for time-varying industry heterogeneity. Third, our findings are robust to excluding years associated with the COVID-19 pandemic. Fourth, our results hold when using alternative estimators to address biases in staggered TWFE DiD analyses, including those proposed by Wooldridge (2021) and Baker et al. (2022). Finally, our results hold when restricting the sample to firms with more concentrated geographic operations.

In sum, SHBs appear to significantly influence firm behavior. Our findings reveal that while SHBs lead to a reduction in capital and R&D expenditures, they simultaneously improve investment efficiency and key performance metrics. One mechanism is that, faced with heightened hiring uncertainty, firms become more selective and shift investment toward their highest quality opportunities, thereby improving resource allocation. In parallel, SHBs appear to enhance employer-employee matching, workplace quality, and employee morale, which can further raise productivity and contribute to more efficient and effective deployment of capital. However, these performance gains are not sustained in the long run, potentially reflecting the long-term consequences of reduced investment in growth-driving activities. Our study underscores the complex interplay between labor market regulation and corporate decision-making, highlighting the importance of aligning corporate resource allocation with evolving information frictions in the hiring process.

2 Related Literature and Institutional Background

2.1 Related literature

Our paper makes several contributions to the finance literature, particularly in the areas of labor market frictions, investment under uncertainty, and firm-level responses to non-financial regulation. First, we add to a growing literature on labor market institutions and firm outcomes, especially to work examining the consequences of SHBs. Prior studies largely focus on SHBs' impacts on wage disparities, hiring dynamics, and employment. For instance, some research finds that SHBs successfully reduce wage disparities among historically disadvantaged groups (Meli and Spindler, 2019; Hansen and McNichols, 2020; Sran, Vetter, and Walsh, 2020; Mask, 2023; Bessen et al., 2024), while others report mixed or negligible effects on wage outcomes, often raising concerns about adverse selection (Agan, Cowgill, and Gee, 2020; Hansen and McNichols, 2020; Davis et al., 2022; Sherman, Brands, and Ku, 2023).[2]

[2]While findings are mixed, a growing body of evidence supports the view that SHBs work in terms of raising wages of historically disadvantaged groups (Bessen, Denk, Meng, and Meurer, 2025). We note that the findings in Davis et al. (2022) are not necessarily contradictory. They focus on public-sector employees,

Additionally, SHBs have been associated with reduced employment in local labor markets and declines in inventor hiring (Atanassov and Deng, 2024; Zhai, 2024). A few contemporaneous working papers have begun to explore firm-level responses beyond employment, including SHBs' impact on innovation (Atanassov and Deng, 2024; Chen, Yang, and Yuan, 2025) and effects on product differentiation (Loncan, 2024). Our paper expands this literature by documenting SHBs' influence on critical firm-level outcomes such as investment, capital allocation, profitability, and productivity. These findings highlight important spillover effects of wage-related regulations beyond direct labor market outcomes, connecting our work to the corporate finance literature on workplace culture, equity policies, and firm value (Edmans, 2011; Guiso, Sapienza, and Zingales, 2015; Graham, Grennan, Harvey, and Rajgopal, 2023).

Second, we contribute a new conceptual framework and empirical evidence on the mechanisms through which labor regulation influences firm behavior. We argue that SHBs alter firms' operating environment through two key channels. By removing a benchmark commonly used in wage negotiations, SHBs shift bargaining power toward job candidates, thereby increasing expected labor costs (Barach and Horton, 2021; Bessen et al., 2024) and amplifying informational frictions in hiring (Spence, 1973; Gibbons and Waldman, 2004; Acemoglu and Autor, 2011). We show that these twin forces have meaningful effects on corporate investment decisions. Firms respond by reducing capital expenditures and R&D, but also by becoming more selective in their investments and hiring practices, leading to improved capital allocation efficiency and firm performance.

Third, our study introduces labor-side information loss as a novel form of uncertainty in corporate decision-making. Existing research on investment under uncertainty has traditionally emphasized macroeconomic, regulatory, or geopolitical risks (Bernanke, 1983; Bloom

where rigid pay scales, collective bargaining, and budget constraints limit wage adjustment, whereas private-sector labor markets feature greater wage flexibility, allowing the removal of salary history to increase hiring uncertainty, shift bargaining power toward job seekers, and affect wages and firm behavior differently.

et al., 2007; Julio and Yook, 2012; Gulen and Ion, 2016). We broaden this framework by demonstrating that restrictions on labor market information due to removing salary history as a signal of worker productivity creates a distinct form of input quality uncertainty. Unlike labor supply shocks that affect the availability or quantity of workers (Xu, 2025), SHBs do not alter workforce size but instead limit firms' ability to infer the productivity and reservation wages of prospective hires. This degradation in information increases the risk of mismatched hiring and raises the shadow cost of labor, prompting firms to respond with more cautious capital allocation strategies. Thus, we extend recent work on hiring frictions and real options theory (Den Haan, Freund, and Rendahl, 2021), and contribute to the emerging literature linking labor-related uncertainty to corporate investment (Xu, 2025).

Finally, our findings complement studies documenting how other forms of labor market frictions affect firm-level investment and capital allocation (e.g. Dessaint, Golubov, and Volpin, 2017; Bai, Fairhurst, and Serfling, 2020; Gustafson and Kotter, 2023; Jeffers, 2024, among others). Further, we highlight a dynamic tradeoff: while SHBs improve perceived fairness, morale, and short-run productivity, they also reduce long-term investment, potentially constraining growth. By tracing how regulatory changes in labor markets shape firm-level decisions, our paper contributes to a broader understanding of the real effects of non-financial regulation on corporate behavior.

Together, these contributions highlight how wage-related regulation, which is often viewed primarily as a labor market policy, can significantly influence firm-level investment decisions, resource allocation efficiency, and financial performance. By linking labor market policies to core corporate finance decisions, we provide new insights into the broader economic consequences of equity-driven regulatory interventions, offering valuable implications for researchers, policymakers, and market participants.

2.2 Institutional background on salary history bans

Wage inequality, particularly gender-based pay disparities, remains a pervasive issue globally. Recent U.S. Census data indicate that, in 2023, women working full-time, year-round earned approximately 83 cents for every dollar earned by men in comparable positions.[3] Despite the Equal Pay Act of 1963, which sought to eliminate gender-based pay discrimination by mandating equal pay for equal work, substantial wage gaps persist (Blau and Kahn, 2017). The persistence of these disparities underscores the complexity of the issue and the limitations of legislative efforts aimed at achieving true pay equity.

Since the enactment of the Equal Pay Act, additional federal and state policies have been introduced to promote fairness in compensation not only based on gender but also on race, ethnicity, and other personal characteristics. These policies aim to ensure that compensation is determined by skills, experience, and performance rather than by individual demographic factors. Despite these efforts, the challenges in closing the wage gap are multifaceted, involving not only legal enforcement (O'Reilly, Smith, Deakin, and Burchell, 2015; Polachek, 2019) but also cultural (Black, Haviland, Sanders, and Taylor, 2008) and organizational factors that contribute to persistent inequalities. Research indicates that unconscious biases and systemic barriers continue to influence hiring and compensation decisions (Neumark, Bank, and Van Nort, 1996; Alan, 2011; Kline, Rose, and Walters, 2022; Bailey, Helgerman, and Stuart, 2024), necessitating a combination of regulatory action, public awareness, and corporate accountability to foster meaningful change.

One such policy initiative is the adoption of pay transparency measures, which require employers to disclose pay rates or pay scales to employees or the public. The goal is to reduce the risk of discriminatory pay practices by fostering greater transparency. California became

[3] U.S. Census Bureau,"Income in the United States: 2023," available at: https://www.census.gov/library/publications/2024/demo/p60-282.html, Table A-7.

the first state to implement a pay transparency law in 2018, and as of 2024, nine other states have enacted similar legislation. Pay transparency aims to empower employees by providing them with the information needed to negotiate fair compensation and to hold employers accountable for inequitable pay practices (Cullen and Pakzad-Hurson, 2023; Cullen, 2024). Studies have analyzed the effects of such laws on wage dynamics, revealing both intended and unintended consequences (Gomez and Wald, 2010; Mas, 2017; Cullen and Perez-Truglia, 2022; Baker, Halberstam, Kroft, Mas, and Messacar, 2023; Cullen and Pakzad-Hurson, 2023).

Another major policy aimed at addressing wage inequality is the adoption of SHBs. These laws prevent employers from asking prospective employees about their prior salaries during the hiring process, with the goal of preventing past wage discrimination from affecting future compensation, particularly for women and minorities, who have historically been underpaid. The rationale behind SHBs is that relying on past salary information can perpetuate existing disparities, as individuals who have been underpaid in previous roles are likely to receive lower offers in subsequent positions. By eliminating the use of salary history, SHBs aim to ensure that compensation offers are based on the value of the role and the candidate's qualifications, rather than historical earnings (Davis et al., 2022; Bessen et al., 2024).

SHBs have gained momentum as a key tool in the fight against wage inequality. Massachusetts became the first state to implement an SHB in 2016, prohibiting salary history inquiries until after a job offer is made. Since then, another 23 states and some local jurisdictions have adopted similar bans, and the trend continues to grow. For example, New York City implemented an SHB for city agencies in 2017 and extended it to all employers in 2019. In addition to state-level actions, some local governments have introduced SHBs to address wage disparities in their jurisdictions, reflecting a broader recognition of the importance of these measures in promoting fair pay.

As of 2024, 19 states have SHBs applicable to all employers, as shown in Table 1, while an additional four states apply bans to only state agencies.[4] The growth in SHBs across the country highlights the increasing emphasis on addressing structural inequities in the labor market and ensuring that pay practices are fair and equitable for all candidates, regardless of their background or previous earnings.

The implementation and enforcement of SHBs vary across states. Some states have enacted strict enforcement mechanisms to ensure compliance. For instance, in New York City, the Commission on Human Rights can impose fines of up to $125,000 for unintentional violations and $250,000 for intentional or malicious infractions. This reflects a commitment to ensuring that employers adhere to the regulations and that violations are met with significant consequences. In other states, such as Minnesota, SHBs prohibit not only direct inquiries about salary history but also attempts to gather salary data from previous employers or public sources. This stricter approach is intended to prevent employers from circumventing the intent of the law and to close potential loopholes that could undermine its effectiveness.

Voluntary reporting of salary history remains a concern, as it may undermine the effectiveness of SHBs. Agan et al. (2020) found in a survey that over half of respondents who would typically disclose their salary chose not to do so following the implementation of an SHB. This finding underscores the importance of SHBs in changing social norms around salary disclosure, as individuals become more cautious about sharing their past earnings. Similarly, Kahn and Lange (2014) used an experimental approach to show that many individuals opted not to disclose past wages when given the option. In addition, some prominent firms, like Amazon, will not use salary history information even if the candidate volunteers it (O'Donovan, 2018). Overall, these findings suggest that even when voluntary disclosure

[4] We note that all of our main results are robust to excluding states that adopted pay transparency laws within two years of the SHBs in our sample. No state adopted pay transparency laws before adopting SHBs. Colorado, Maryland, Nevada, and Rhode Island adopted pay transparency laws the same year they adopted SHBs, and Connecticut adopted a pay transparency law two years after it adopted an SHB.

is permitted, SHBs can effectively reduce reliance on salary history, thereby mitigating the continuation of wage gaps. The reduction in salary disclosures also indicates a shift in the power dynamics of the hiring process, empowering candidates to negotiate based on the value of their skills rather than their past compensation.[5]

The potential benefits of SHBs are significant, particularly for historically marginalized groups who have faced systemic wage discrimination. By removing prior salary information from the hiring process, SHBs create an opportunity for candidates to negotiate salaries based on their skills, experience, and the requirements of the position, rather than being anchored to previous, potentially inequitable earnings. This can be particularly impactful for women and minorities, who are more likely to have been underpaid in past roles (Sinha, 2019; Hansen and McNichols, 2020; Davis et al., 2022; Bessen et al., 2024). Moreover, SHBs can contribute to a cultural shift in how compensation is determined, encouraging employers to adopt more objective and standardized pay-setting practices (Meli and Spindler, 2019; Agan, Cowgill, and Gee, 2025).

Despite these potential benefits, SHBs are not without their challenges. Critics argue that banning salary history inquiries may backfire, leading employers to offer lower initial salaries due to uncertainty about candidates' salary expectations (Sran et al., 2020; Davis et al., 2022). This can be particularly disadvantageous for high-performing candidates who may have been able to command higher offers reflective of their true productivity (Meli and Spindler, 2019). Further, there is concern that some employers may attempt to find alterna-

[5]Some states that adopt SHBs prohibit employers from requesting prior pay but explicitly allow workers to voluntarily disclose it. While voluntary disclosure might be expected to weaken SHBs by partially restoring information to employers, we find the opposite: SHB effects on investment and performance are generally stronger in states that allow voluntary disclosure. A possible explanation follows from classic insights in the adverse-selection and disclosure literature (Akerlof, 1970; Grossman and Stiglitz, 1976; Dye, 1985). When disclosure is either mandatory or prohibited, employers face a relatively uniform informational environment. By contrast, when disclosure is optional, workers selectively reveal prior pay only when it is favorable, making non-disclosure a negative but imprecise signal. This partial-disclosure regime increases uncertainty about worker quality and reservation wages relative to both full and no disclosure, amplifying firms' precautionary investment and reallocation responses following SHB adoption.

tive ways to estimate a candidate's past compensation, such as through indirect questioning or by making assumptions based on the candidate's work history and industry norms (Barach and Horton, 2021; Agan et al., 2025).

However, research suggests that SHBs have a meaningful impact on reducing wage disparities. For instance, studies have shown that SHBs contribute to narrowing the gender pay gap by limiting the influence of past discrimination on future earnings (Sinha, 2019; Hansen and McNichols, 2020; Davis et al., 2022; Bessen et al., 2024). By focusing on the value of the job and the qualifications of the candidate, SHBs help create a more equitable starting point for salary negotiations. Additionally, SHBs can lead to broader changes in hiring practices, encouraging employers to develop more comprehensive strategies for evaluating candidates and determining appropriate compensation levels without relying on potentially biased historical data (Barach and Horton, 2021).

3 Data and Empirical Methodology

3.1 Sample selection

We construct our base sample using the Compustat database from 2012 to 2023. We select 2012 as the starting year to capture the five years before the first SHBs became effective in 2017, and we end the sample in 2023 because it is the last year with full Compustat coverage. We exclude utility (SIC 4900-4999), financial (SIC 6000-6999), and quasi-public firms (SIC greater than 9900). We also exclude small firms with both sales and net PP&E below $5 million, firms that report not having employees, and observations with missing values for our control variables. We obtain financial statement data from Compustat and data on firm-level TFP following the steps outlined in Olley and Pakes (1996). We identify each firm's historical state of headquarters using the file COMPHIST from the CRSP/Compustat Merged databases. We obtain data on state-level corporate tax rates and political leaning from the Book of the States, gross domestic product (GDP) from the U.S. Bureau of Economic Analysis, unemployment rates from the Bureau of Labor Statistics, unemployment benefits from the Department of Labor, demographic information from the Current Population Survey, and dates for medical marijuana legalization from Guernsey, Serfling, and Yan (2025).

The final sample includes about 26,211 firm-year observations, but the sample size can vary depending on the availability of a specific outcome variable. Further, the number of observations can vary in the analyses due to using the DiD imputation method and the model specification. The appendix provides variable definitions. We winsorize firm-level continuous variables at their 1st and 99th percentiles and express dollar values in 2022 dollars. Table 2 reports summary statistics for the full sample.

3.2 Identification strategy

Our identification strategy exploits the staggered adoption of SHBs in a DiD research design with TWFEs. The traditional TWFE regression with staggered adoption of treatment takes the form of:

$$y_{it} = \alpha_i + \beta_t + \tau D_{it} + \gamma X_{it-1} + \varepsilon_{it}, \tag{1}$$

where y_{it} is a measure of corporate investment or performance at firm i in year t. α_i are firm fixed effects, and β_t are year fixed effects. D_{it} is an indicator variable that equals one if a firm is headquartered in a state that has adopted an SHB by year t, and zero otherwise. X_{it-1} are lagged firm- and state-level control variables (defined in the next section and the appendix). We cluster standard errors by state to account for serial correlation in the standard errors within a firm over time and across firms within the same state.

Recent studies have raised concerns about the traditional TWFE model in staggered DiD settings (e.g., Goodman-Bacon, 2021; Baker et al., 2022; Borusyak et al., 2024). Goodman-Bacon (2021) demonstrates that the ATT derived from TWFE DiD regressions is a variance-weighted average of multiple "2×2" DiD estimators, which compare treated and control groups within a pre- and post-treatment window. In a staggered DiD framework, an issue arises when some of these "2×2" estimators compare newly treated units to those already treated. These "forbidden comparisons" are problematic because they can lead to biased estimates when treatment effects vary over time, potentially even reversing the sign of the true ATT. In our sample, approximately 46% of the observations are from states that never adopt SHBs. Using the decomposition method proposed by Goodman-Bacon (2021), we find that around 10% of the treatment effects for SHBs in our sample are influenced by forbidden comparisons. To obtain unbiased DiD estimates in the SHB context, it is necessary to use an alternative estimation approach that addresses these concerns.

To estimate the effect of SHBs on investment and performance while accounting for issues associated with traditional staggered DiD regressions, we apply the imputation-based estimation method proposed by Borusyak et al. (2024). While alternative estimators have been developed to address these concerns (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Wooldridge, 2021), the imputation-based approach offers advantages such as transparency, efficiency, and the ability to calculate asymptotically conservative standard errors.

The imputation estimator is constructed in three steps. First, Eq. (1) is estimated on the subsample of firm-year observations from states without SHBs (i.e., when $D_{it} = 0$) to obtain estimates of $\hat{\alpha}_i$, $\hat{\beta}_t$, and $\hat{\gamma}$. Second, the treatment effect for each firm-year observation in a state that has adopted an SHB is calculated as $\hat{\tau}_{it} = Y_{it} - \hat{\alpha}_i - \hat{\beta}_t - \hat{\gamma}X_{it-1}$. Finally, the

ATT is computed as the weighted sum of these individual treatment effects, with weights determined by treatment timing and assignment.

3.3 Threats to identification

As with any DiD analysis, the primary identification assumption is that firms in states without SHBs provide valid counterfactuals for those in states with SHBs. By incorporating firm fixed effects, the DiD imputation method accounts for static differences between firms in adopting and non-adopting states, ensuring that these differences do not influence the results. Further, year fixed effects control for overall trends in SHBs and firm outcomes.

Table 3 evaluates the differences between firms in states with and without SHBs across various dimensions. Ideally, SHBs would be randomly assigned, making the characteristics of firms in adopting states indistinguishable from those in non-adopting states. To assess this, we compare the covariate balance between the two groups during the year before SHB adoption (i.e., t-1). The characteristics analyzed include firm-level metrics such as book value of assets, cash flow, market-to-book ratio, cash holdings, and book leverage, as well as state-level attributes like per capita GDP, per capita GDP growth, corporate tax rates, unemployment rates, political affiliation of governors, and unemployment benefit policies. Additional variables include whether a state has legalized medical marijuana, the average age of the population, and the proportion of the population that is female.

The analysis reveals differences between firms in adopting and non-adopting states across several characteristics. Among 15 characteristics examined, 10 differ significantly at the 10% level or higher. These imbalances raise concerns that our ATT estimates for SHBs might be biased if certain types of firms or their outcomes evolve differently over time in ways correlated with SHB adoption. We address these concerns in two ways. First, we estimate the effects of SHBs using the DiD imputation method with inverse probability weighting

(Austin, 2011). This method weights observations based on the likelihood of a firm being in a state that adopts an SHB, creating a pseudo-population where treatment assignment is independent of observed covariates. The weights are calculated using a logit regression model with the full set of controls from Table 3. As shown in the final columns of Table 3, covariate balance improves substantially after weighting, with no statistically significant differences remaining between treatment and control groups. Second, we further mitigate potential biases by conducting analyses with and without these 15 control variables, demonstrating that our results are consistent across specifications.

Our identification strategy also relies on the assumption that the timing of SHB adoption is not systematically related to firms' investment and performance decisions or to state-level economic and political factors that might directly influence changes in our firm outcomes of interest. To evaluate this assumption, we estimate Cox proportional hazard models, defining a "failure event" as the year a state adopts an SHB. The analysis spans the period from 2012 to 2023, excluding states after the SHB enactment, and lags all independent variables by one year. Key predictors include *State CapEx*, *State R&D*, *State InvEff*, *State OROA*, *State TFP*, *State Ln(Sales/Emp)*, and *State Ln(MVE)*, which capture state-level averages of capital expenditures, R&D expenditures, investment efficiency, profitability, TFP, sales-per-employee, and market value of equity, respectively. Additionally, we include all of the state-level variables from Table 3.

Table 4 reports the findings. Columns 1-7 include each state average firm characteristic individually, revealing that the only significant predictor of SHB adoption is *State InvEff*, which is negative and significant at the 5% level. When all state average firm characteristics are included together in column 8, *State InvEff* is no longer significant. Among state-level characteristics, only unemployment benefit policies consistently predict SHB adoption, with states offering higher benefits more likely to implement SHBs. Overall, these findings support the assumption that SHB adoption is largely exogenous to local firms' investment decisions and performance. Additionally, we include these economic and political characteristics as controls in our fully specified regression models to ensure robustness.

4 Results

We begin by examining the impact of SHBs on corporate investment and performance. We then explore cross-sectional heterogeneity in these effects, assess how SHBs influence worker wages, and evaluate their implications for employee perceptions using workplace ratings. Finally, we discuss several robustness checks to assess the validity of our findings.

4.1 SHBs and investment

Table 5 reports the effects of SHBs on capital expenditures, R&D expenditures, and total investment. The dependent variables in columns 1-6 are: capital expenditures minus cash proceeds from the sale of PP&E all scaled by the average of beginning and end-of-year net PP&E (*CapEx*); R&D expenditures scaled by the average of beginning and end-of-year net PP&E (*R&D*);[6] and the sum of capital expenditures (net of cash proceeds from the sale of PP&E) and R&D expenditures all scaled by the average of beginning and end-of-year net PP&E (*TotalInvest*). For each dependent variable, the analysis uses the DiD imputation method, incorporating firm and year fixed effects. We estimate the regressions both without time-varying controls and with an extensive set of firm- and state-level controls.

The findings indicate negative ATTs on investment, suggesting that firms headquartered in states adopting SHBs invest less compared to firms in states that do not adopt these bans. Column 1, which controls only for firm and year fixed effects, shows that SHBs are

[6] Our R&D results are similar if we instead scale R&D expenditures by sales or average beginning and end-of-year book assets.

associated with a reduction in capital expenditures of 3.2 percentage points (t-stat = -5.47). Adding time-varying firm- and state-level controls in column 2 slightly attenuates this effect, with capital expenditures decreasing by 3.1 percentage points (t-stat = -4.59). Relative to the standard deviation of *CapEx*, these results suggest that SHBs lead to economically significant reductions in capital expenditures, equivalent to 18.8% to 19.4% of a standard deviation. As a benchmark, Gulen and Ion (2016) find that a doubling in policy uncertainty is associated with a reduction in capital expenditures of 16.8% of a standard deviation.

Columns 3 and 4 evaluate the relationship between SHBs and R&D intensity. Without time-varying controls in column 3, SHBs are associated with a 19.1 percentage-point decline in R&D intensity (t-stat = -5.71). When time-varying firm- and state-level controls are added in column 4, the decline moderates to 18.3 percentage points (t-stat = -7.44). Relative to the standard deviation of R&D intensity, these findings indicate that SHBs decrease R&D expenditures by approximately 14.4% to 15.1% of a standard deviation.

Columns 5 and 6 focus on the impact of SHBs on total investment. Column 5, which includes only firm and year fixed effects, shows a decline in total investment of 31.9 percentage points (t-stat = -7.44). Adding time-varying controls in column 6 reduces the effect slightly, with total investment falling by 29.3 percentage points (t-stat = -5.14). Compared to the standard deviation of *TotalInvest*, these results suggest that SHBs reduce total investment by 15.1% to 16.4% of a standard deviation.

In sum, the findings in Table 5 are consistent with the interpretation that SHBs increase uncertainty in the hiring process, thereby discouraging firm investment. By restricting employers from accessing prior salary information, SHBs may limit firms' ability to accurately assess the productivity and reservation wages of potential hires, thereby introducing greater informational asymmetry. This increase in uncertainty complicates firms' ability to forecast and plan capital allocation, prompting more cautious behavior in committing resources to long-term investments. The observed reductions in investment align with this explanation. This behavior is consistent with theoretical models of investment under uncertainty, such as real options theory, which posits that firms delay or scale back irreversible investments when uncertainty rises (e.g., Bernanke, 1983; Jurado, Ludvigson, and Ng, 2015).

4.2 SHBs and investment efficiency

We next investigate the impact of SHBs on corporate investment efficiency. Investment efficiency measures how well firms allocate their resources to maximize returns, avoiding both overinvesting in unprofitable projects and underinvesting in valuable ones. Following the methodology of prior studies (e.g., Biddle et al., 2009), we define investment efficiency as the deviation from the expected optimal level of investment. This is represented by the absolute value of the residuals from the following investment model:

$$INV_{it} = \beta_0 + \beta_1 \Delta Sales_{it-1} + \varepsilon_{it}, \tag{2}$$

where INV_{it} represents total investment for firm i in year t, calculated as the sum of capital expenditures (net of cash receipts from PP&E sales) and R&D expenditures all scaled by the average of beginning and end-of-year net PP&E. $\Delta Sales_{it-1}$ is the lagged growth rate in a firm's sales. We estimate Eq. (2) for each Fama-French 49 industry and year, requiring at least 20 observations per group for inclusion in the analysis.[7] Investment efficiency (*InvEff*) equals the negative of the absolute residuals, with higher values indicating more efficient investment. Overinvestment (*OverInv*) is calculated as positive residuals, with zero assigned to non-positive values, so that lower values of *OverInv* indicate less overinvestment. Similarly, underinvestment (*UnderInv*) is defined as negative residuals, with zero assigned to non-negative values, such that higher values of *UnderInv* indicate less underinvestment.

[7] Table A1 shows that our investment efficiency results hold when estimating Eq. (2) after further controlling for lagged market-to-book ratios, the logarithm of assets, cash flow scaled by assets, and book leverage.

Table 6 shows that SHBs significantly improve investment efficiency. In column 1, which includes only firm and year fixed effects, the estimated ATT suggests that SHBs increase investment efficiency by 12.7 percentage points (t-stat = 4.45). When adding time-varying firm- and state-level controls in column 2, the effect slightly decreases to 11.2 percentage points (t-stat = 4.34). Relative to the standard deviation of *InvEff*, these results suggest that SHBs improve investment efficiency by 13.7% to 15.5% of a standard deviation.

Columns 3–4 and 5–6 further decompose the effects into overinvestment and underinvestment, respectively. In column 3, without time-varying controls, SHBs reduce overinvestment by 14.8 percentage points (t-stat = -5.69). In column 4 with controls, the reduction in overinvestment is slightly smaller at 11.8 percentage points (t-stat = -5.36). Compared to the standard deviation of *OverInv*, these findings suggest a decrease in overinvestment of 15.5% to 19.5% of a standard deviation.[8] Columns 5 and 6 reveal no statistically significant effect of SHBs on underinvestment, suggesting that the improvements in investment efficiency are driven primarily by reductions in overinvestment rather than changes in underinvestment.

In sum, the findings in Table 6 imply that SHBs lead to improvements in investment efficiency, mainly through reducing overinvestment. Given that we previously found that SHBs reduce total investment, the findings suggest that while SHBs lead firms to invest less overall, they also encourage firms to invest more judiciously, prioritizing projects with higher expected returns and reducing wasteful expenditures. This pattern can be explained by considering how SHBs affect managerial decision-making and resource allocation under uncertainty. By restricting access to salary history, SHBs may increase uncertainty in labor cost forecasting, prompting firms to adopt a more cautious approach to investment. Rather

[8] Prior to the adoption of SHBs, managers may overinvest due to agency-driven incentives such as empire building. By increasing hiring-related uncertainty and labor costs, SHBs constrain managerial discretion over marginal investments, making it more costly to pursue projects that primarily expand firm scale rather than create value. The reduction in overinvestment is consistent with SHBs curbing agency-driven behaviors, rather than reflecting shifts in underlying investment opportunities.

than broadly reducing investment across the board, firms appear to reallocate their constrained resources to projects with clearer, higher-value prospects. This reallocation aligns with improved investment efficiency, as firms scale back on projects that are more likely to drive overinvestment. SHBs may also indirectly improve the quality of hiring and workforce productivity by shifting firms' focus toward more merit-based hiring practices. Better-quality employee-firm matches can lead to more precise assessments of project risks and returns, enhancing the ability of firms to allocate capital effectively. While total investment declines due to the increased caution stemming from labor cost uncertainty, the remaining investments are better targeted and more productive, reflecting improved efficiency.

4.3 SHBs and firm performance

Table 7 evaluates the impact of SHBs on four key measures of firm performance. Columns 1 and 2 analyze the effect of SHBs on firm profitability, measured by operating income before depreciation and amortization scaled by the average of beginning and end-of-year book value of assets (*OROA*). In column 1, which includes only firm and year fixed effects, the estimated ATT indicates that SHBs increase profitability by 1.6 percentage points (t-stat = 3.23). Adding time-varying firm- and state-level controls in column 2 slightly strengthens the effect, with SHBs increasing profitability by 1.7 percentage points (t-stat = 3.81). Relative to the standard deviation of *OROA*, these estimates imply that SHBs enhance profitability by approximately 10.4% to 11.0% of a standard deviation.

Columns 3 and 4 explore the relationship between SHBs and total factor productivity (*TFP*), which measures the portion of a firm's output not accounted for by inputs such as capital, labor, and materials. *TFP* is estimated using a Cobb-Douglas production function following the methodology outlined in Olley and Pakes (1996). In column 3, without time-varying controls, SHBs are associated with a 6.6% increase in *TFP* (t-stat = 3.46). When time-varying controls are included in column 4, the effect grows slightly, with SHBs increasing *TFP* by 7.6% (t-stat = 4.77).

We examine labor productivity, measured as the natural logarithm of firm sales scaled by the average number of employees at the beginning and end of the year, in columns 5 and 6. Column 5, which includes only firm and year fixed effects, shows that SHBs increase labor productivity by 5.8% (t-stat = 5.07). Including time-varying controls in column 6 slightly reduces the effect, with SHBs increasing labor productivity by 5.6% (t-stat = 4.78).

Finally, columns 7 and 8 analyze the impact of SHBs on firm value, measured as the natural logarithm of a firm's market value of equity. In column 7, without time-varying controls, SHBs are associated with a 16.7% increase in firm value (t-stat = 2.77). When time-varying controls are added in column 8, the effect diminishes, with SHBs being linked to an 8.3% increase in market value of equity (t-stat = 2.47).

In sum, the results in Table 7 indicate that SHBs are associated with significant improvements in multiple dimensions of firm performance, despite reducing overall investment. These improvements suggest that SHBs encourage firms to optimize the use of resources and workforce, likely due to better employee-job matches and more efficient capital allocation. These findings align with the earlier evidence of SHBs improving investment efficiency, as firms appear to reduce overinvestment and focus on higher-return projects, thereby enhancing profitability and market valuation. Thus, while SHBs decrease total investment levels, they simultaneously improve how resources are allocated, resulting in enhanced operational performance and firm value. These results support the theory that SHBs drive firms to adopt more merit-based hiring practices and efficient investment strategies, reducing wasteful expenditures while strengthening the alignment between resource deployment and firm objectives. The net effect is a notable improvement in firm performance, underscoring the broader economic implications of labor market policies like SHBs.

4.4 Dynamic effects of SHBs

This section investigates the timing of changes in firm-level outcomes relative to the enactment of SHBs. To interpret the observed reductions in corporate investment and improvements in investment efficiency and firm performance as causal, the parallel trends assumption must hold. This assumption requires that trends in firm-level outcomes for states adopting SHBs and those that do not are similar in the years preceding the enactment. We test this by plotting DiD coefficients for up to five years before and after SHB enactment, using models both with and without firm- and state-level controls.

Figures 1-3 reveal no evidence of pre-trend differences in corporate investment, investment efficiency, or firm performance between states that adopt SHBs and those that do not, regardless of whether firm- and state-level controls are included. The figures indicate that firms begin to respond to SHBs only in the post-enactment period. These results support the validity of the parallel trends assumption, reinforcing the causal interpretation of the findings from our DiD analysis.

Beyond confirming the parallel trends assumption, these figures provide insight into the longer-term effects of SHBs on investment and performance. Notably, the impact of SHBs on investment is persistent. For at least five years following SHB enactment, capital expenditures, R&D spending, and total investment remain significantly lower. Similarly, investment efficiency remains elevated, while overinvestment is reduced during this period. However, the positive effects of SHBs on profitability, labor productivity, and firm value appear to be more transitory. Within a few years of enactment, these improvements dissipate and become statistically indistinguishable from pre-SHB levels.

These findings suggest that the initial reductions in investment following SHBs may have long-term implications for firm performance. While SHBs seem to promote operational efficiency and optimize resource allocation in the short term, lower investment in critical areas such as R&D and capital assets may hinder a firm's ability to innovate, grow, and adapt to changing market dynamics. Over time, this lack of reinvestment can erode competitive advantages, leading to the reversal of initial gains in productivity and firm value. However, because many SHBs were adopted later in our sample period, we are unable to observe full five-year post-treatment effects for all treated states. Thus, it is possible that the reversals in performance are caused by noisy estimates in the later years after SHBs are adopted. Nevertheless, we do not observe the same reversal patterns in investment, making it unlikely that the reversal in performance is entirely due to measurement issues.

4.5 Mechanism analyses

To examine the mechanisms through which SHBs affect firm behavior, we conduct a series of tests that link labor-market frictions, bargaining power, and organizational adjustment to firms' investment and performance responses. First, we study cross-sectional heterogeneity to assess whether SHB effects are strongest in settings where labor-side information frictions and bargaining constraints are most salient. Second, we directly examine the effect of SHBs on worker-level wages to identify labor-cost channels. Finally, we examine organizational adjustment and labor quality channels using employee review data to test whether SHBs are associated with improvements in perceived compensation fairness, career opportunities, and workplace culture, which would indicate better worker-firm matching and help reconcile reduced investment with improved productivity and firm performance.

4.5.1 The effect of automation and labor skill

Table 8 evaluates the impact of SHBs on investment and performance measures, focusing on differences based on the ability of firms to substitute labor with automated capital and worker skill levels. We apply the DiD imputation method to estimate these cross-sectional regressions in a way equivalent to interacting a cross-sectional variable with an SHB indicator. For these analyses, we tabulate results from models that include firm- and state-level controls for brevity, but note that the results are generally stronger in models without these controls.

Panel A splits firms into those with below- and above-median values of the substitutability of labor with automated capital (SLAC) measure, as defined in Bates, Du, and Wang (2025). Given that we hypothesize that the effect of SHBs on corporate investment and performance arises from introducing labor market frictions, if firms could easily replace labor with automated capital, SHBs should have a smaller impact on firms, as labor market frictions would be less of a burden. Thus, we predict that the effect of SHBs on firm outcomes should be greater when SLAC is lower. Following Bates et al. (2025), we measure SLAC based on estimates of the likelihood of occupational computerization, which assess the potential automation of specific jobs given technological advancements in engineering fields like machine learning and mobile robotics. *SLAC* is then defined as the proportion of an industry's existing employees that is susceptible to replacement by automation in a given year (see the Appendix for calculation).[9] Overall, consistent with the prediction, in each specification, we find statistically significant larger decreases in capital and R&D expenditures as well as larger increases in investment efficiency and firm performance after SHBs are implemented for firms that are less able to substitute labor with automated capital.

Panel B splits firms into those with below- and above-median values of worker skill, as defined in Belo, Li, Lin, and Zhao (2017). In high-skilled sectors, workers' productivity is closely tied to intangible assets, such as innovation and expertise, making it difficult to evaluate ex-ante. In contrast, low-skilled workers' productivity is more directly linked to measur-

[9] We also examined the effect of SHBs on employment. Using more granular employment data from Data Axle, we find evidence that employment growth slows following SHB adoption. However, due to data quality concerns, we do not tabulate these results in the paper.

able tasks, allowing for relatively straightforward performance assessment. A worker's salary history serves as an important signal of unobservable productivity, particularly for highly compensated professionals (Meli and Spindler, 2019). By removing this signal, SHBs force firms to rely on alternative screening methods, increasing hiring costs and amplifying uncertainty in the recruitment process. This challenge is especially pronounced in skill-intensive industries, where mismatches in hiring impose greater costs due to a higher contribution of skilled workers to firm value creation. Thus, we predict that the effect of SHBs on firm outcomes should be especially pronounced for firms that rely more on skilled workers.

We measure worker skill levels by the fraction of workers in the firm's industry employed in occupations requiring high levels of training and preparation, as defined by Belo et al. (2017). This metric is constructed using employment data from the Bureau of Labor Statistics' Occupational Employment Statistics program and the Specific Vocational Preparation (SVP) index from the Dictionary of Occupational Titles provided by the Department of Labor. The SVP index quantifies the time required for a typical worker to acquire the skills and knowledge necessary for average performance in a specific role, with values ranging from one to nine. An SVP value of one corresponds to minimal preparation, while a value of nine represents the highest level of preparation required. High-skilled occupations are defined as those with an SVP value of seven or above (indicating at least two years of preparation). Overall, consistent with our prediction, across all specifications, we find significantly larger declines in capital and R&D expenditures, along with greater increases in investment efficiency and firm performance, after SHBs are implemented for firms with more skilled labor.

4.5.2 The effect of union membership

In unionized firms, wages are largely determined by collective bargaining agreements, job ladders, and seniority rules, which elevate workers' baseline bargaining power and limit managerial discretion in wage-setting. Because salary history plays a relatively minor role in anchoring negotiations in these settings, the incremental informational loss and bargaining-power shift induced by SHBs is muted. By contrast, in firms with low union coverage, where wages are negotiated more flexibly and workers typically have weaker bargaining positions, salary history serves as a more important screening and bargaining device. Removing this signal represents a sharper disruption to the hiring process, increasing uncertainty over labor costs and intensifying wage negotiations. As a result, we predict that firms in low-union environments face larger adjustments in hiring, investment, and operational decisions following SHB adoption, leading to more pronounced effects on firm outcomes.

To test this prediction, we use CPS data and calculate the fraction of workers in each state and 3-digit SIC industry each year ($Union$) that are members of a labor union as a proxy for a firm's unionization rate. Table 9 shows the effect of SHBs on firm outcomes for firms with above and below median unionization rates. The results indicate that firms in low-union-coverage firms exhibit larger reductions in investment following the adoption of SHBs, together with greater improvements in performance. The differences between the two groups is statistically significant across all but two outcomes.

4.5.3 The effect of corporate governance

Governance mechanisms shape how managers respond to shocks that increase labor market uncertainty and costs. When governance is strong, managers are more constrained to allocate resources in ways that enhance firm value, whereas weaker governance allows greater discretion and slack. In the context of SHBs, this distinction is particularly important: the bans heighten uncertainty about labor costs and worker productivity, and strong governance ensures that managers respond by making disciplined, value-enhancing adjustments rather than reacting with inefficient caution or wasteful persistence. Thus, governance strengthens firms' ability to make optimal decisions in the face of labor uncertainty introduced by SHBs.

Capital market monitoring and product market competition serve as complementary governance mechanisms. Analyst coverage provides external monitoring by reducing information asymmetry and limiting managerial discretion, thereby constraining wasteful investment and fostering more efficient allocation of capital (Yu, 2008; Chen, Harford, and Lin, 2015; Irani and Oesch, 2016). Product market competition can similarly discipline managers by reducing slack and pressuring them to operate efficiently (Giroud and Mueller, 2010, 2011). We therefore use analyst coverage and product market concentration, as measured by the Herfindahl-Hirschman Index using sales and 3-digit SIC industry codes, as two complementary proxies for governance. Firms with greater analyst coverage or operating in more competitive industries should be better positioned to translate the heightened frictions introduced by SHBs into more disciplined capital allocation and, ultimately, improved firm performance.

Table 10 reports the results. Panel A splits the sample based on analyst coverage. Consistent with the prediction, we find statistically significant larger decreases in investment as well as larger increases in investment efficiency and firm performance after SHBs are implemented for firms with more analyst coverage. Similarly, Panel B shows that for firms operating in more competitive industries, SHBs lead to large reductions in investment, along with greater improvements in investment efficiency, profitability, labor productivity, and firm value. Overall, these results suggest that stronger governance allows firms to respond to the labor uncertainty introduced by SHBs not with wasteful caution, but by reallocating resources more optimally, thereby improving investment efficiency and firm performance.

4.5.4 SHBs and worker bargaining power

An individual's past salary serves as an indicator of their reservation wage, enabling employers to make competitive yet cost-efficient offers (Bessen et al., 2024). By limiting access to this information, SHBs shift bargaining power from firms to job seekers, reducing firms' ability to negotiate salaries at the lowest acceptable level. This change creates uncertainty around labor costs and can increase the cost of hiring and retaining workers. Thus, this shift in bargaining dynamics may lead to higher labor costs, which could discourage investment.

To test whether SHBs create hiring frictions through this labor cost channel, we directly examine how workers' income changes after SHBs are implemented. We obtain worker-level data from the Integrated Public Use Microdata Series (IPUMS)-CPS database. We utilize the Annual Social and Economic Supplement of the CPS, which is based on a nationally representative survey of more than 75,000 households. We focus on household heads in the civilian population that are between the ages of 16 and 64. In our DiD-imputation estimation, we always include fixed effects for state, year, occupation, industry, race, education level, age, sex, and marital status. In other specifications, we control for the same local economic characteristics as our previous analyses.

In column 1 of Table 11, which only controls for fixed effects, the estimated ATT indicates that SHBs increase wages by about 2.9% (t-stat = 6.89). Adding time-varying state-level controls in column 2 slightly weakens the effect, with SHBs increasing incomes by 2.1% (t-stat = 4.85). The goal of SHBs is to increase the wages of those who have historically faced wage discrimination, such as women and minorities. Therefore, in columns 3-4 and 5-6, we examine the effect of SHBs on incomes for males versus females and white versus non-white individuals, respectively. Consistent with SHBs supporting historically disadvantaged workers, the results show that the positive effect of SHBs on workers' income is statistically larger for females with ATTs ranging from 3.9% to 4.7%. While the economic magnitudes of the effects of SHBs on the incomes of non-white workers is larger than that of white workers, the differences are not statistically significant at conventional levels (p-values of 0.16 and 0.18). In columns 7-8, we stratify the sample along gender and race dimensions, resulting in four mutually exclusive groups. The positive income effects of SHBs are again concentrated among female workers and especially among female non-white workers.

Finally, columns 9-10 examine the effect of SHBs on the incomes of new hires versus incumbent employees. SHBs are likely to have a larger effect on the wages of newly hired workers because salary history is primarily used at the point of hire to anchor wage negotiations and infer reservation wages. In contrast, incumbent workers' compensation is typically governed by internal pay structures, existing contracts, and performance-based adjustments, making it less directly responsive to changes in hiring-related information. To identify new hires, we exploit the panel structure in the CPS database and utilize the variable EMPSAME, which captures the question of whether the individual was employed by the same employer that they reported in the previous month's survey. We classify an employee as a new hire, if she switched employers within the last three months. The results show that, while the wages of incumbent workers increase by 1.9% to 2.7% after SHBs are adopted, the wages of new hires rise by 6.0% to 6.9%, and these differences are statistically significant at the 5% level.[10] Overall, the magnitudes we document are consistent with those in prior studies (e.g., Sinha, 2019; Hansen and McNichols, 2020; Atanassov and Deng, 2024; Bessen et al., 2024).

[10]We note that, even if SHBs primarily operate through the wage-setting of new hires, these changes can spill over to incumbent workers through internal equity considerations and pay compression concerns. As firms adjust entry wages upward, they may also increase incumbent compensation to maintain morale, retention, and consistency within the wage structure.

4.5.5 SHBs and employee ratings

We next test whether the performance gains following SHBs in part stem from improved employer-employee matching, workplace quality, and morale. To do so, we examine employee-level Glassdoor ratings of compensation, career opportunities, and culture, which reflect employee perceptions of pay fairness, growth potential, and organizational values. By eliminating access to prior salary, SHBs may push firms to rely more on qualifications and potential rather than historical compensation when making hiring decisions (Spence, 1973; Fossati et al., 2020). This shift can broaden applicant pools, improve match quality, and increase employee satisfaction. SHBs may also promote pay equity by reducing wage discrimination (Sinha, 2019; Bessen et al., 2024), which can boost morale and retention. If this mechanism is at play, we expect higher Glassdoor ratings in SHB states, consistent with improved workforce-firm alignment.

Glassdoor ratings are on a scale from one to five. The compensation rating reflects employees' perceptions of fair and competitive pay, benefits, and transparency regarding wages. The career rating captures the degree to which employees believe they have opportunities for skill development, advancement, and meaningful contributions to the firm. The culture rating measures how employees perceive interpersonal relationships, leadership quality, and shared organizational values.

Table 12 shows that all three dimensions of employee ratings improve following the adoption of SHBs. Columns 1 and 2 indicate that SHBs are followed by increases in employee compensation ratings of 0.103 (t-stats of 6.85 and 2.22). Similarly, career ratings in columns 3 and 4 increase by 0.091 and 0.129 (t-stats of 1.86 and 2.01) after SHBs are enacted. Columns 5 and 6 show that SHBs are also associated with increases in culture ratings of 0.077 and 0.102 (t-stats of 1.83 and 2.14). Given that the standard deviations of compensation, career, and culture ratings are 1.28, 1.38, and 1.45, respectively, these estimates imply that SHBs improve attitudes towards these dimensions by 8.1%, 5.8%–8.9%, and 5.6%–7.4% of their respective standard deviations.

Overall, these results suggest that SHBs lead to meaningful improvements in how employees perceive their compensation, career opportunities, and workplace culture. The observed increases in Glassdoor ratings indicate that SHBs may enhance employer-employee matching and foster more equitable, engaging work environments. By shifting firms away from relying on prior pay and toward evaluating candidates based on potential and qualifications, SHBs appear to promote greater fairness, satisfaction, and alignment between workers and firms.

4.6 Robustness tests

We assess the robustness of our finding that SHBs reduce corporate investment while improving investment efficiency and performance along four dimensions. For these analyses, we only tabulate results from regressions with the full set of firm- and state-level controls for brevity, but note that the results are generally stronger without these controls.

First, we evaluate the potential influence of omitted variable using the approach proposed by Oster (2019). This method calculates a "delta" statistic to measure how much the model's explanatory power and coefficients change when adding controls. The delta statistic quantifies how much stronger unobserved factors would need to be, relative to observed factors, to overturn the ATT. An absolute value of delta greater than one is generally considered evidence of robustness, with a value of one indicating equal importance between unobserved and observed variables. Using our baseline regressions with and without controls, we calculate delta values ranging from 0.27 to 130, with only delta statistics for labor productivity and firm value falling below one. These results suggest that omitted variable bias is unlikely to affect our conclusions.

Second, Table A2 demonstrates the robustness of our results to the inclusion of two-digit SIC industry fixed effects interacted with year fixed effects. This accounts for potential unobserved time-varying heterogeneity at the industry level.

Third, Table A3 shows that our results, besides the result that $Ln(MVE)$ increases after SHBs are adopted, continue to hold after excluding the COVID-19 pandemic period (2020).

Fourth, while our primary analysis relies on the DiD imputation approach from Borusyak et al. (2024) to address concerns about heterogeneous treatment effects in TWFE DiD models, we also validate our results using alternative DiD estimators from Wooldridge (2021) and Baker et al. (2022). Specifically, Panel A of Table A4 shows that all the results hold using the Wooldridge (2021) staggered DiD estimator. In Panel B, we employ a stacked DiD estimator in which, for each SHB adoption event date, we obtain all treatment and control firm observations around this date, pool all the cohort-specific samples together, and estimate OLS regressions while controlling for the interaction of firm and year fixed effects with cohort indicator variables. For this analysis, we focus on the ±3-year window around an adoption year and restrict the sample to firms that do not change their headquarters state. The results show that the effect of SHBs on investment and performance continues to hold using this specification. The one exception is that while the effect of SHBs on firm value is positive, it is not statistically significant at conventional levels (p-value $= 0.251$).

Fifth, matching state-level SHBs to a firm's headquarters assumes that the headquarters location reflects a significant share of the firm's employment. However, this assumption may introduce measurement error, as many U.S. public firms operate across multiple states.[11] To address this concern, Table A5 reports results excluding firms more likely to have geographically dispersed operations. In Panel A, we exclude firms with significant operations across

[11] This assumption aligns with prior research, which shows that a firm's main facilities, such as plants, operations, and R&D centers, are often located near its headquarters (e.g., Howells, 1990; Henderson and Ono, 2008; Karlsson, 2008).

multiple states based on their industry affiliation (retail, wholesale, and transportation). In Panel B, we obtain establishment-level employment counts from Data Axle (formerly Infogroup). After manually matching establishment names to CRSP and Compustat, we exclude firms that do not have at least 75% of their employees in their headquarters state. Overall, using either approach, the results continue to show that SHBs are associated with decreases in investment and improvements in investment efficiency and performance. These results suggest that our findings are not driven by firms with dispersed operations.

5 Conclusion

This study investigates the effect of SHBs on corporate investment and firm performance, providing a nuanced view of the interplay between labor market regulations and corporate decision-making. Using a staggered DiD framework, we uncover that SHBs significantly reshape corporate strategies by restricting firms' access to prior salary information, a key signal historically used to assess potential hires' productivity.

Our findings indicate that SHBs introduce labor information frictions in the hiring process, prompting firms to adopt more cautious investment strategies as reflected in declines in both capital and R&D expenditures after SHBs are enacted. These reductions align with theories positing that increased uncertainty and potentially higher labor costs discourage firms from making investments. Yet, despite these reductions in investment, SHBs drive improvements in investment efficiency by curtailing overinvestment. This demonstrates that firms recalibrate their resource allocation strategies under regulatory-induced constraints. In the short term, these adjustments yield improvements in firm performance metrics, including profitability, TFP, labor productivity, and firm value. Firms appear to benefit from more merit-based hiring practices that prioritize skills and potential over historical pay benchmarks, leading to better talent matching and more effective use of resources. However, these performance gains tend to diminish over time, potentially reflecting the long-term consequences of reduced investment in growth-driving activities.

As further evidence that SHBs affect corporate investment decisions and firm performance by impacting labor market frictions, we find that the effect of SHBs is more pronounced in settings where labor-side information frictions and bargaining constraints are most consequential. We also find that SHBs lead to sharper reductions in investment and larger efficiency and performance gains among firms with greater analyst coverage and those operating in more competitive industries, consistent with firms optimally reallocating resources under hiring uncertainty. Further, SHBs lead to higher worker incomes, reflecting increased labor costs that, in turn, discourage investment and shape broader corporate behaviors. In addition, using Glassdoor ratings, we show that SHBs lead to meaningful improvements in how employees perceive their compensation, career opportunities, and workplace culture. These findings suggest that SHBs foster more equitable and supportive workplaces, potentially further enhancing productivity and performance.

A natural question raised by our findings is why firms did not voluntarily stop using salary history if doing so improves performance and investment efficiency. One reason is that prior pay serves as a convenient, low-cost proxy for worker productivity and reservation wages. Even if imperfect, it reduces perceived hiring risk, making firms reluctant to abandon it unilaterally, especially if doing so raises wages without guaranteed productivity gains. SHBs address this coordination problem by requiring all firms to shift hiring practices simultaneously, leveling the playing field. They also disrupt organizational inertia, pushing firms to reassess entrenched HR practices and compensation norms. In doing so, SHBs act not just as regulatory constraints but as catalysts for more merit-based, forward-looking hiring strategies that uncover underutilized talent and improve resource allocation.

Overall, this study contributes to the growing literature on the economic consequences of labor market regulations. We document how labor-side information loss can serve as a novel source of uncertainty with material consequences for corporate resource allocation. While SHBs aim to address systemic pay disparities, our findings reveal that these policies also produce broader consequences for firm investment behavior and performance. For policy-makers, these results underscore the importance of designing labor regulations that balance social objectives with potential economic repercussions. For firms, the findings suggest a need to adapt hiring and investment strategies to maintain long-term competitiveness in an evolving regulatory environment.

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Appendix: Variable Definitions

This table provides variable definitions. Variables not included here are defined in the corresponding table captions. Compustat variables are listed in italics when appropriate.

Variable	Definition
%ΔGDP	One-year percent change in per capita state-level gross domestic product.
%Female	Fraction of women in a state's population.
Age	Average age of a state's population.
Analysts	Average number of analysts covering a firm during a year.
Assets	Book value of a firm's total assets (*at*) in 2022 dollars.
BLEV	Book value of debt scaled by book value of assets [(*dltt*+*dlc*)/*at*].
CapEx	Capital expenditures minus cash proceeds from the sale of PP&E all scaled by the average of beginning and end-of-year net PP&E [(*capex*-*sppe*)/(avg *ppent*)].
CashFlow	Income before extraordinary items plus depreciation and amortization scaled by book value of assets [(*ib*+*dp*)/*at*].
CashHolding	Ratio of cash and short-term investments to total assets (*che*/*at*).
DemGov	An indicator variable that equals one if a state governor is from the Democratic party, and zero otherwise.
Educ	Fraction of a state's population aged 25 or older with a Bachelor's degree.
GDP	Per capita state-level gross domestic product.
HHI	Herfindahl–Hirschman Index based on sales and 3-digit SIC industries.
InvEff	The negative absolute value of the residuals from a regression of total investment on the change in sales, following Biddle et al. (2009).
Marijuana	An indicator variable that equals one if a firm is headquartered in a state that has legalized medical marijuana by year t, and zero otherwise.
MB	Market value of assets divided by book value of assets [(*prcc_f*×*csho*+*at*-*ceq*)/*at*].
MVE	Market value of equity (*prcc_f*×*csho*) in 2022 dollars.
OROA	Operating income before depreciation scaled by average total assets from the previous and current year [*oibdp*/(avg *at*)].
OverInv	Positive residuals from the regression of total investment on the change in sales, indicating that the firm is investing more than expected given its sales growth, following Biddle et al. (2009).
R&D	Research and development expenses scaled by the average of beginning and end-of-year net PP&E [*xrd*/(avg *ppent*)]. *xrd* is set to zero when missing.
Sales/Emp	Sales (in 2022 dollars) divided by the average number of employees from the previous and current year [*sale*/(avg *emp*)].
Skill	Industry-level labor skill. Data are from Belo et al. (2017).

SLAC	Substitutability of labor with automated capital, following Bates et al. (2025), calculated as the weighted average probability of computerization across all occupations that constitute industry j in year t. Formally, $\text{SLAC}_{j,t} = \sum_o (Prob_o) \times \frac{Emp_{j,o,t} \times Wage_{j,o,t}}{\sum_o (Emp_{j,o,t} \times Wage_{j,o,t})}$, where $Prob_o$ is the probability of computerization for occupation o taken from Frey and Osborne (2017), and $Emp_{j,o,t}$ and $Wage_{j,o,t}$ are the number of employees and average annual wages for occupation o in industry j at time t. Data are from the Occupational Employment and Wage Statistics program.
TaxRate	State-level highest marginal corporate tax rate.
TFP	Total factor productivity, following Olley and Pakes (1996), calculated as the residual from a semiparametric production function estimating firm output based on capital and labor inputs.
TotalInv	Sum of capital expenditures (net of cash proceeds from the sale of PP&E) and R&D expenditures all scaled by the average of beginning and end-of-year net PP&E [(*capex*-*sppe*+*xrd*)/(avg *ppent*)]. *xrd* is set to zero when missing.
UnderInv	Negative residuals from the regression of total investment on the change in sales, indicating that the firm is investing less than expected given its sales growth, following Biddle et al. (2009).
UnempBen	State-level total unemployment benefits, calculated as the maximum benefits multiplied by the maximum number of weeks.
UnempRate	State-level unemployment rate, measured as the fraction of workers who are unemployed.
Union	The fraction of workers in a 3-digit SIC and state that are members of a labor union.

Figure 1: DiD Dynamics for Investment

These figures present the results of the effect of SHBs on firm investment over the period 2012 to 2023, estimated using the DiD imputation method. The dependent variables include *CapEx*, *R&D*, and *TotalInv*. The red line represents estimates without control variables, while the blue line represents estimates with lagged firm- and state-level control variables. Controls include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. 95% confidence intervals based on standard errors clustered by state are reported.

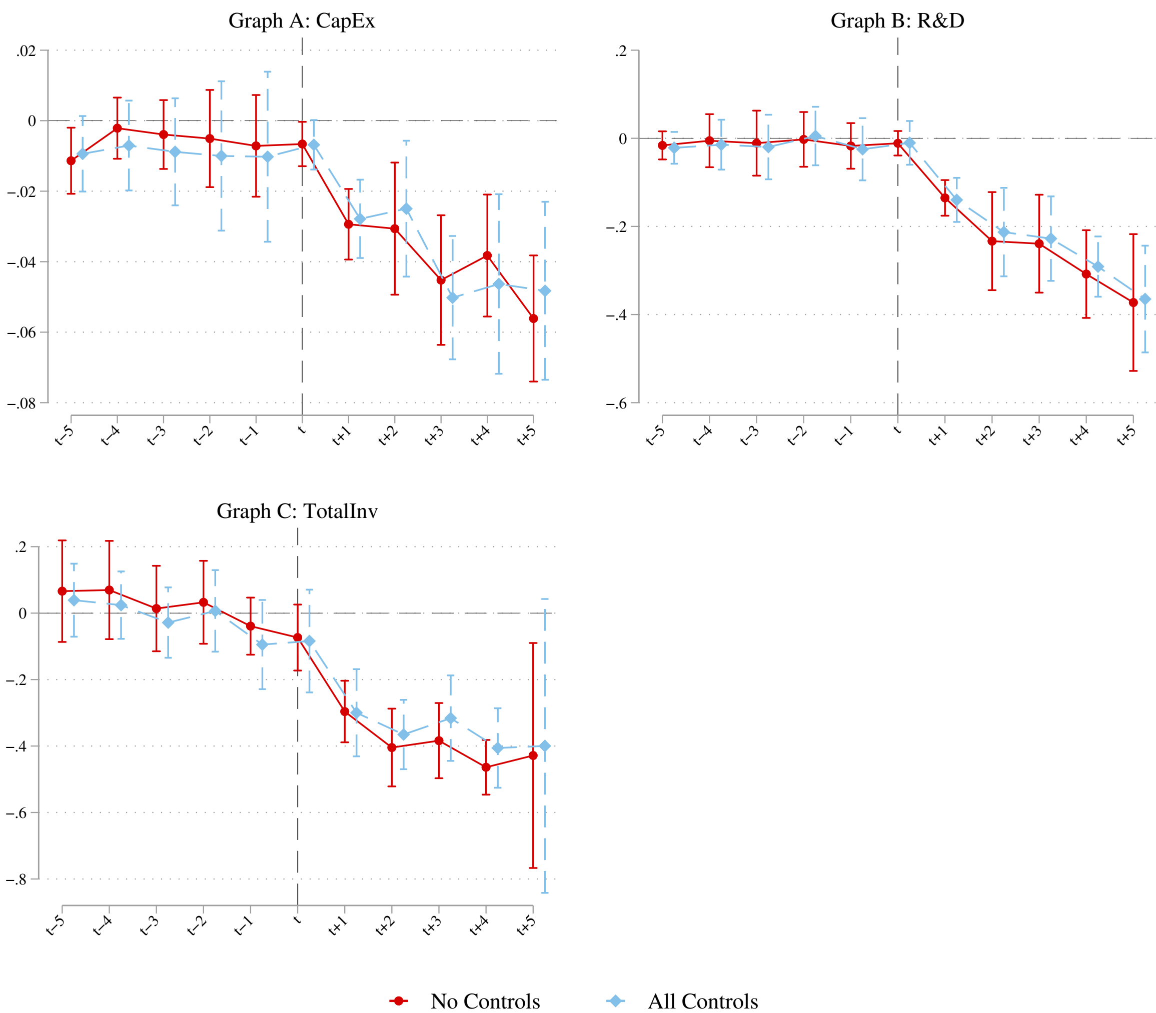

Figure 2: DiD Dynamics for Investment Efficiency

These figures present the results of the effect of SHBs on firm investment efficiency, overinvestment, and underinvestment over the period 2012 to 2023, estimated using the DiD imputation method. The dependent variables include measures of investment efficiency (*InvEff*), overinvestment (*OverInv*), and underinvestment (*UnderInv*). The red line represents estimates without control variables, while the blue line represents estimates with lagged firm- and state-level control variables. Controls include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%*Δ*GDP*, *TaxRate*, *UnempRate*, *%DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. 95% confidence intervals based on standard errors clustered by state are reported.

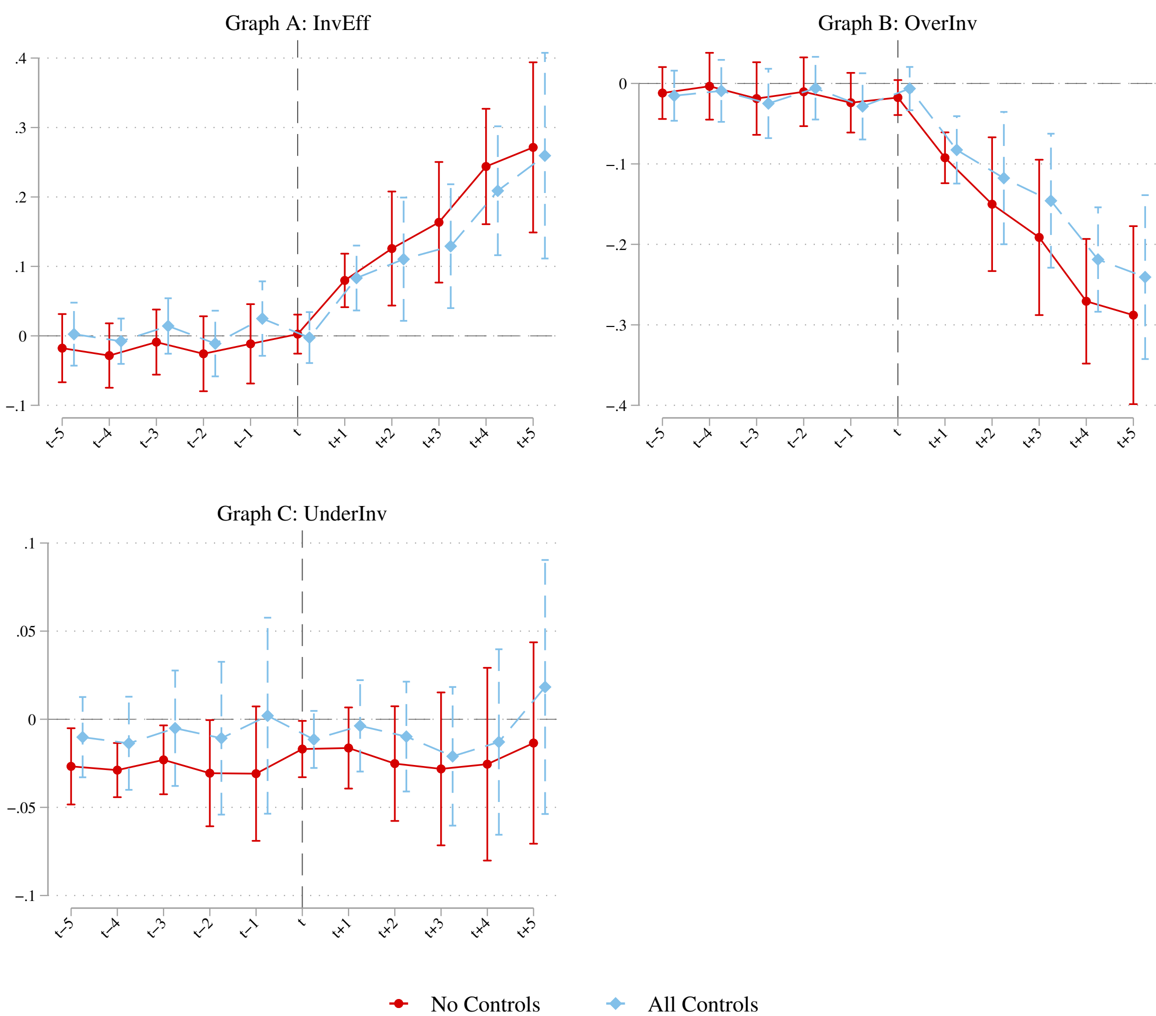

Figure 3: DiD Dynamics for Perfromance

These figures present the results of the effect of SHBs on firm performance over the period 2012 to 2023, estimated using the DiD imputation method. The dependent variables include *OROA*, *TFP*, *Ln(Sales/Emp)*, and *Ln(MVE)*. The red line represents estimates without control variables, while the blue line represents estimates with lagged firm- and state-level control variables. Controls include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. 95% confidence intervals based on standard errors clustered by state are reported.

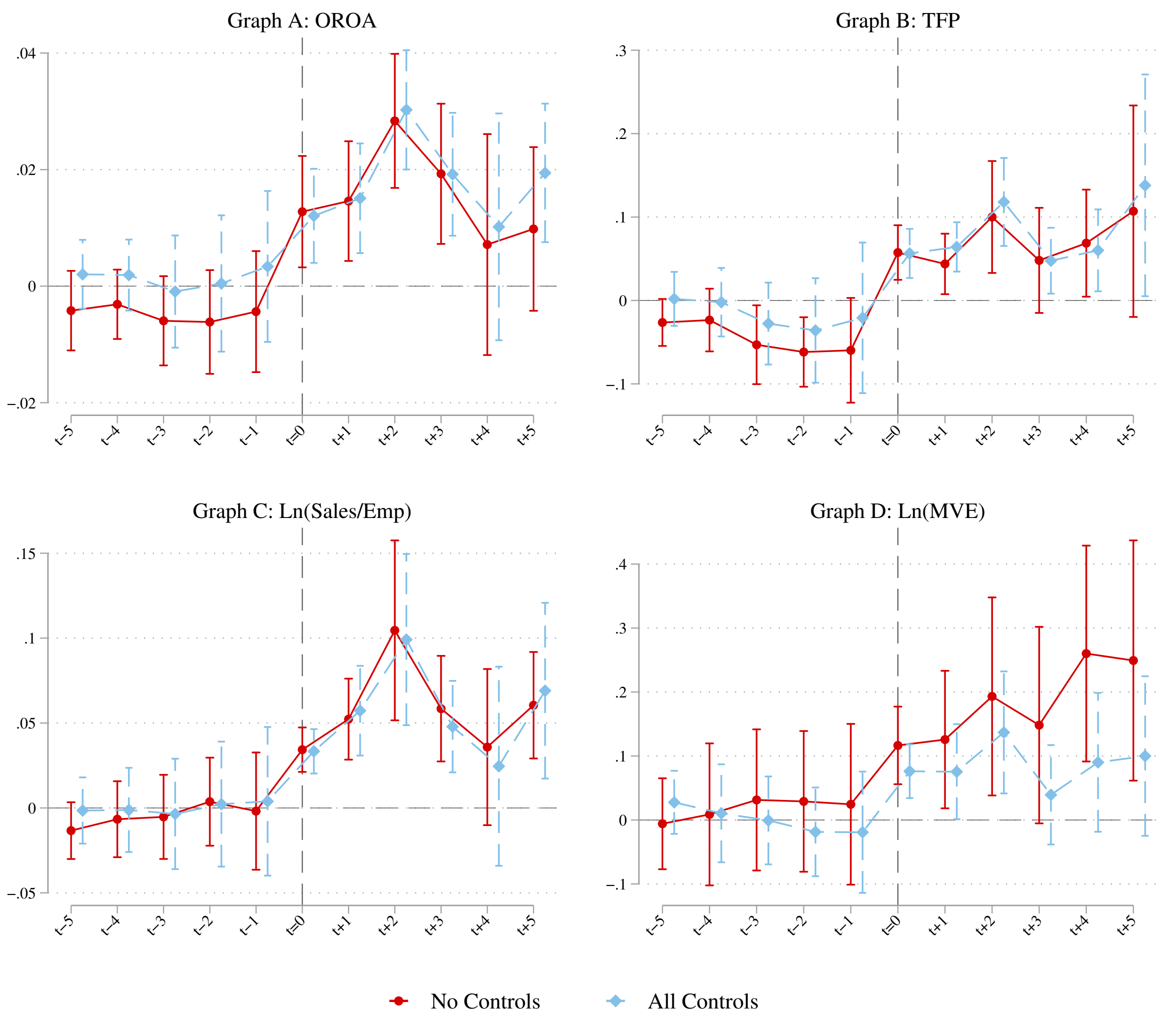

Table 1: Dates of Salary History Bans

This table presents the years when a state's salary history ban became effective. The dates are sourced from https://www.hrdive.com/news/salary-history-ban-states-list/516662/ and state websites.

State	Abbreviation	Effective Year
Alabama	AL	2019
California	CA	2018
Colorado	CO	2021
Connecticut	CT	2019
Delaware	DE	2017
District of Columbia	DC	2024
Hawaii	HI	2019
Illinois	IL	2019
Maine	ME	2019
Maryland	MD	2020
Massachusetts	MA	2018
Minnesota	MN	2024
Nevada	NV	2021
New Jersey	NJ	2020
New York City	NY	2017
New York (Albany County)	NY	2017
New York (Suffolk County)	NY	2019
New York (Westchester County)	NY	2018
New York (State-Wide)	NY	2020
Ohio (Columbus)	OH	2024
Ohio (Toledo)	OH	2020
Oregon	OR	2017
Pennsylvania (Lehigh County)	PA	2024
Pennsylvania (Philadelphia)	PA	2020
Rhode Island	RI	2023
Vermont	VT	2018
Washington	WA	2019

Table 2: Summary Statistics

This table reports summary statistics for the variables used in the regressions over the period 2012 to 2023. The sample consists of 26,211 firm-year observations. All variables are defined in the Appendix.

	Mean	SD	P25	P50	P75
CapEx	0.207	0.165	0.097	0.164	0.270
R&D	0.533	1.268	0.000	0.002	0.354
TotalInv	0.799	1.942	0.128	0.251	0.646
InvEff	-0.513	0.817	-0.575	-0.175	-0.072
OverInv	0.254	0.759	0.000	0.000	0.072
UnderInv	-0.256	0.457	-0.240	-0.071	0.000
OROA	0.081	0.154	0.047	0.106	0.158
TFP	-0.201	0.606	-0.489	-0.216	0.100
Ln(Sales/Emp)	5.968	0.939	5.431	5.902	6.441
Ln(MVE)	7.251	2.096	5.878	7.323	8.636
Ln(Assets)	7.281	1.818	6.016	7.272	8.498
MB	2.137	1.605	1.179	1.601	2.431
CashFlow	0.031	0.175	0.012	0.069	0.115
CashHolding	0.174	0.187	0.039	0.105	0.239
BLEV	0.301	0.246	0.108	0.270	0.430
Ln(GDP)	13.706	0.923	13.029	13.572	14.506
%ΔGDP	0.023	0.027	0.008	0.025	0.039
TaxRate	6.107	3.326	4.900	7.000	8.840
UnempRate	5.605	1.947	4.100	5.100	6.800
DemGov	0.359	0.480	0.000	0.000	1.000
Ln(UnempBen)	9.555	0.444	9.421	9.578	9.805
Age	38.577	1.681	37.572	38.689	39.570
Educ	0.361	0.061	0.321	0.354	0.398
%Female	0.514	0.008	0.510	0.514	0.520
Marijuana	0.631	0.483	0.000	1.000	1.000

Table 3: Covariate Balance

This table reports results from tests examining the covariate balance between treated and control firms. Treated firms are those in states that will implement SHBs in year t, while control firms are from states that never passed or have not yet legalized an SHB before a legalization event occurs. The columns show the mean values for the treatment and control groups, the difference between the two groups, and the corresponding t-statistics. The left panel shows results before weighting, while the right panel shows results after weighting the observations by the inverse probably of being treated. All variables are defined in the Appendix. t-statistics for the differences in means are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Before Weighting				After Weighting			
	Treat	Control	Diff	t-stat	Treat	Control	Diff	t-stat
Ln(Assets)	7.242	7.359	−0.117	−1.24	7.124	7.235	−0.111	0.80
MB	2.445	1.984	0.461	2.38**	2.654	2.321	0.333	−0.05
CashFlow	0.015	0.046	−0.031	−3.10***	0.006	0.022	−0.016	0.73
CashHolding	0.215	0.137	0.078	2.89***	0.245	0.205	0.041	−0.66
BLEV	0.285	0.318	−0.033	−1.80*	0.269	0.285	−0.016	0.34
Ln(GDP)	11.352	11.156	0.196	6.37***	11.374	11.332	0.043	−0.20
%ΔGDP	0.020	0.009	0.011	2.26**	0.021	0.015	0.006	0.25
TaxRate	7.659	5.014	2.645	2.32**	7.856	7.252	0.605	0.02
UnempRate	4.731	4.518	0.213	0.62	4.605	4.715	−0.110	−0.20
DemGov	0.413	0.308	0.105	0.58	0.361	0.406	−0.046	0.23
Ln(UnempBen)	9.758	9.409	0.349	2.25**	9.792	9.736	0.055	−0.20
Age	39.028	38.729	0.299	0.49	38.958	38.913	0.045	0.40
Educ	0.399	0.354	0.045	2.77***	0.399	0.396	0.003	−0.29
%Female	0.515	0.514	0.001	0.39	0.516	0.515	0.001	0.08
Marijuana	0.992	0.556	0.436	3.36***	0.995	0.964	0.032	0.04

Table 4: Determinants of SHB Adoptions: Duration Model

This table reports the results from Cox proportional hazard models, where a failure event is defined as the year when a state adopts an SHB. The sample period is from 2012 to 2023, and states are excluded from the sample after adopting an SHB. The independent variables include the state-level economic variables and state average firm-level variables. For example, state average capital expenditures (*State CapEx*) is the median value of *CapEx* across all firms within a state in a given year. The coefficients are reported alongside their corresponding t-statistic in parentheses, with standard errors clustered at the state level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	SHB							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(GDP)	1.898	1.892	2.244	2.298	1.770	1.784	1.962	2.880
	(0.84)	(0.86)	(1.00)	(1.01)	(0.83)	(0.83)	(0.91)	(1.13)
%ΔGDP	-4.360	-4.150	-5.617	-3.803	-4.729	-5.353	-5.607	-6.451
	(-0.78)	(-0.71)	(-1.01)	(-0.70)	(-0.81)	(-0.85)	(-0.85)	(-0.76)
TaxRate	0.035	0.034	0.008	0.042	0.031	0.031	0.031	0.024
	(0.31)	(0.28)	(0.07)	(0.34)	(0.26)	(0.27)	(0.27)	(0.23)
UnempRate	-0.058	-0.096	-0.085	-0.086	-0.090	-0.109	-0.134	-0.147
	(-0.20)	(-0.35)	(-0.31)	(-0.32)	(-0.33)	(-0.42)	(-0.48)	(-0.54)
%DemGov	0.015	-0.073	0.005	-0.187	-0.058	-0.033	-0.097	-0.135
	(0.03)	(-0.10)	(0.01)	(-0.32)	(-0.10)	(-0.06)	(-0.17)	(-0.20)
Ln(UnempBen)	2.053***	1.960**	1.935***	2.026***	2.030***	2.112***	2.017***	2.067**
	(2.88)	(2.25)	(2.64)	(2.92)	(2.96)	(2.90)	(3.03)	(2.15)
Age	0.193	0.235	0.191	0.240	0.221	0.183	0.227	0.192
	(0.91)	(1.12)	(0.97)	(1.16)	(1.07)	(0.90)	(1.10)	(0.84)
Educ	-6.063	-5.117	-6.160	-6.368	-5.084	-4.573	-5.782	-8.451
	(-0.72)	(-0.58)	(-0.73)	(-0.75)	(-0.65)	(-0.57)	(-0.69)	(-0.88)
%Female	23.634	25.226	27.611	30.991	24.776	27.798	19.036	24.810
	(0.97)	(0.96)	(1.23)	(1.30)	(1.05)	(1.18)	(0.73)	(0.93)
Marijuana	1.441	1.543	1.456	1.427	1.556	1.661	1.690	1.619
	(1.30)	(1.39)	(1.32)	(1.32)	(1.41)	(1.47)	(1.41)	(1.34)
State CapEx	4.394							4.968
	(1.28)							(0.73)
State R&D		0.275						-0.143
		(0.15)						(-0.08)
State InvEff			-0.931*					-0.168
			(-1.83)					(-0.17)
State OROA				-5.089				-7.017
				(-0.97)				(-0.80)
State TFP					0.320			-1.528
					(0.32)			(-0.90)
State Ln(Sales/Emp)						-0.496		-0.415
						(-0.73)		(-0.59)
State Ln(MVE)							0.249	0.468
							(0.87)	(0.99)
N of Obs	499	499	489	499	494	498	499	487

Table 5: SHBs and Investment

This table presents results from the DiD imputation method examining the effect of SHBs on firm investment over the period 2012 to 2023. The dependent variables include *CapEx*, *R&D*, and *TotalInv*. *ATT* represents the average treatment effect of SHBs on investment. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, %Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CapEx		R&D		TotalInv	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.032***	-0.031***	-0.191***	-0.183***	-0.319***	-0.293***
	(-5.47)	(-4.59)	(-5.71)	(-7.44)	(-7.44)	(-5.14)
Controls		✓		✓		✓
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
N of Obs	22,367	22,367	22,386	22,386	22,367	22,367

Table 6: SHBs and Investment Efficiency

This table presents results from the DiD imputation method examining the effect of SHBs on firm investment efficiency over the period 2012 to 2023. The dependent variables include measures of investment efficiency (*InvEff*), overinvestment (*OverInv*), and underinvestment (*UnderInv*). *ATT* represents the average treatment effect of SHBs on investment efficiency. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, %Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	InvEff		OverInv		UnderInv	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.127***	0.112***	-0.148***	-0.118***	-0.022	-0.009
	(5.45)	(4.34)	(-5.69)	(-5.36)	(-1.60)	(-0.63)
Controls		✓		✓		✓
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
N of Obs	20,202	20,202	20,202	20,202	20,202	20,202

Table 7: SHBs and Performance

This table presents results from the DiD imputation method examining the effect of SHBs on firm performance over the period 2012 to 2023. The dependent variables include *OROA*, *TFP*, *Ln(Sales/Emp)*, and *Ln(MVE)*. *ATT* represents the average treatment effect of SHBs on performance. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, $\%\Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	OROA		TFP		$\text{Ln}(\frac{\text{Sales}}{\text{Emp}})$		Ln(MVE)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.016***	0.017***	0.066***	0.076***	0.058***	0.056***	0.167***	0.083**
	(3.23)	(3.81)	(3.46)	(4.77)	(5.07)	(4.78)	(2.77)	(2.47)
Controls		✓		✓		✓		✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,382	22,382	17,511	17,511	22,317	22,317	22,379	22,379

Table 8: The Effect of Automation and Labor Skill

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023. In Panel A, *ATT-Low* and *ATT-High* measure the average treatment effect of SHBs on firm outcomes based on the below- and above-median values of lagged substitutability of labor with automated capital (*SLAC*), as defined in Bates et al. (2025). In Panel B, the effect of SHBs is evaluated for firms with below- and above-median values of lagged industry skill (*Skill*), as defined in Belo et al. (2017). Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, %Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Split by Substitutability of Labor with Automated Capital (SLAC)

	CapEx	R&D	InvEff	OROA	TFP	$\text{Ln}(\frac{\text{Sales}}{\text{Emp}})$	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT - Low	-0.045***	-0.298***	0.163***	0.029***	0.102***	0.082***	0.117***
	(-6.58)	(-7.82)	(4.43)	(5.01)	(6.01)	(6.96)	(3.52)
ATT - High	-0.010	-0.015	0.030	0.001	0.045**	0.019	0.045
	(-1.27)	(-0.64)	(1.43)	(0.20)	(2.40)	(1.55)	(1.14)
Low = High *p*-val	0.067	0.000	0.000	0.000	0.000	0.000	0.000
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,155	22,174	20,017	22,170	17,348	22,106	22,167

Panel B: Split by Industry Skill (Skill)

	CapEx	R&D	InvEff	OROA	TFP	$\text{Ln}(\frac{\text{Sales}}{\text{Emp}})$	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT - Low	-0.016**	-0.003	0.023	-0.004	0.022	0.007	0.015
	(-2.00)	(-0.13)	(1.09)	(-0.89)	(1.27)	(0.59)	(0.41)
ATT - High	-0.041***	-0.302***	0.164***	0.031***	0.114***	0.088***	0.133***
	(-6.10)	(-8.59)	(4.79)	(5.09)	(6.81)	(7.68)	(3.84)
Low = High *p*-val	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,156	22,175	20,018	22,171	17,349	22,107	22,168

Table 9: The Effect of Union Membership

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023. *ATT-Low* and *ATT-High* measure the average treatment effect of SHBs on firm outcomes based on the below- and above-median values of lagged union membership (*Union*). Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, $\%\Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT - Low	-0.044***	-0.259***	0.147***	0.020***	0.074***	0.065***	0.117***
	(-5.44)	(-7.18)	(4.52)	(5.21)	(3.90)	(5.41)	(3.46)
ATT - High	-0.034***	-0.082***	0.070***	0.016***	0.054***	0.025**	0.051*
	(-6.73)	(-3.22)	(3.82)	(2.94)	(2.30)	(2.10)	(1.91)
Low = High p-val	0.042	0.000	0.004	0.226	0.457	0.000	0.000
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	21,059	21,078	19,062	21,074	16,504	21,011	21,071

Table 10: The Effect of Corporate Governance

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023. In Panels A and B, *ATT-Low* and *ATT-High* measure the average treatment effect of SHBs on firm outcomes based on the below- and above-median values of lagged analyst coverage (*Analysts*) and Herfindahl–Hirschman Index (*HHI*), respectively. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. t-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Split by Analyst Coverage (Analysts)

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT - Low	-0.012*	-0.107***	0.052***	0.011***	0.060**	0.041***	-0.009
	(-1.69)	(-3.33)	(2.58)	(2.81)	(2.36)	(3.18)	(-0.26)
ATT - High	-0.046***	-0.242***	0.156***	0.022***	0.087***	0.067***	0.154***
	(-6.83)	(-5.20)	(4.33)	(4.28)	(4.71)	(5.73)	(4.54)
Low = High p-val	0.000	0.042	0.002	0.000	0.352	0.002	0.000
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,367	22,386	20,202	22,382	17,511	22,317	22,379

Panel B: Split by Herfindahl–Hirschman Index (HHI)

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT - Low	-0.041***	-0.304***	0.171***	0.026***	0.090***	0.083***	0.102***
	(-5.08)	(-7.92)	(4.33)	(5.64)	(5.08)	(6.69)	(3.15)
ATT - High	-0.018	-0.020	0.022	0.005	0.056***	0.019	0.058
	(-1.63)	(-0.72)	(0.82)	(0.99)	(3.27)	(1.38)	(1.40)
Low = High p-val	0.070	0.000	0.004	0.000	0.029	0.000	0.088
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,366	22,385	20,201	22,381	17,511	22,316	22,378

Table 11: SHBs and Worker Income: Current Population Survey Data

This table presents results from the DiD imputation method examining the effect of SHBs on worker income over the period 2012 to 2023. The dependent variable is the natural logarithm of total income. Columns 1 and 2 use the full sample, and ATT measures the average treatment effect of SHBs on income. In columns 3-4, 5-6, 7-8, and 9-10, treatment effects are estimated by splitting the sample by gender (female vs. male) and by race (non-white vs. white), gender and race, and incumbent employees vs. new hires, respectively. All models include fixed effects for state, year, occupation, industry, race, education level, age, sex, and marital status. Lagged control variables include *Ln(GDP)*, $\%\Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All regressions are weighted using CPS household-level weights. All variables are defined in the Appendix. t-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Full Sample		Split by Gender		Split by Race		Split by Race & Gender		Split by New Hires	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ATT	0.029***	0.021***								
	(6.89)	(4.85)								
ATT - Male			0.014**	0.006						
			(2.37)	(0.97)						
ATT - Female			0.047***	0.039***						
			(11.20)	(10.20)						
ATT - White					0.025***	0.017***				
					(5.34)	(3.54)				
ATT - Non-White					0.040***	0.033***				
					(4.01)	(3.16)				
ATT - Male & White							0.018***	0.010		
							(2.95)	(1.51)		
ATT - Male & Non-White							0.002	-0.006		
							(0.13)	(-0.42)		
ATT - Female & White							0.033***	0.025***		
							(7.07)	(6.43)		
ATT - Female & Non-White							0.083***	0.076***		
							(8.96)	(7.79)		
ATT - Incumbent									0.027***	0.019***
									(5.23)	(3.50)
ATT - New Hires									0.069***	0.060***
									(4.26)	(3.57)
Difference in ATT p-val			0.000	0.000	0.189	0.161			0.021	0.022
Controls		✓		✓		✓		✓		✓
FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N of Obs	484,079	484,079	484,079	484,079	484,079	484,079	484,079	484,079	324,726	324,726

Table 12: SHBs and Employee Ratings: Glassdoor Data

This table presents results from the DiD imputation method examining the effect of SHBs on employee-level Glassdoor ratings over the period 2012 to 2023. The dependent variables are three dimensions of employee ratings, including ratings of compensation and benefits (*Compensation*), career opportunities (*Career*), and firm culture (*Culture*). *ATT* represents the average treatment effect of SHBs on ratings. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, $\%\Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. t-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Compensation		Career		Culture	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.103***	0.103**	0.091*	0.129**	0.077*	0.102**
	(6.85)	(2.22)	(1.86)	(2.01)	(1.83)	(2.14)
Controls		✓		✓		✓
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
N of Obs	363,574	363,574	361,391	361,391	364,458	364,458

Silencing Salary Talk: How Salary History Bans Affect Corporate Investment and Performance

Online Appendix

Appendix A. A Model of How Salary History Bans Affect Corporate Investment and Performance

We present a model that rationalizes three empirical patterns following SHBs: (i) capital expenditures and R&D decline, (ii) overinvestment falls and investment efficiency improves, and (iii) short-run productivity and profitability rise, with potential attenuation over time.

A.1 Environment

Time is discrete with three dates $t \in \{0, 1, 2\}$. At $t = 0$, the firm chooses hiring and screening intensity as well as irreversible investment. Production occurs at $t = 1$, and long-run productivity evolves between $t = 1$ and $t = 2$.

A.2 Projects

The firm has a continuum of potential investment projects indexed by $j \in [0, 1]$. Each project requires irreversible investment $I_j \geq 0$ at $t = 0$ and generates payoff at $t = 1$:

$$\Pi_j = A\,\theta\,q_j\,f(I_j) - I_j. \tag{1}$$

Here, $A > 0$ is baseline firm productivity, $\theta > 0$ is effective labor quality, $q_j \in [0, \bar{q}]$ is project quality drawn independently from cumulative distribution function $F(q)$, and $f(\cdot)$ is increasing and concave with $f(0) = 0$, $f'(I) > 0$, and $f''(I) < 0$.

Intuition. Projects differ in inherent quality q_j, but all projects require capital and effective labor input to generate value. Investment therefore scales the productivity of labor, making labor quality a first-order determinant of investment returns. Because investment is irreversible, committing to low-quality or poorly matched labor can generate losses.

A.3 Hiring, wages, and the salary history information shock

At $t = 0$, the firm hires a worker (or representative unit of labor). True worker productivity is

$$p \sim \mathcal{N}(\mu, \sigma_p^2). \tag{2}$$

where μ is mean productivity and σ_p^2 is productivity variance.

Salary history as a signal

Absent SHBs, the firm observes a salary history signal

$$s = p + \varepsilon, \qquad \varepsilon \sim \mathcal{N}(0, \sigma_\varepsilon^2), \tag{3}$$

which is independent of p. With SHBs, the firm cannot observe s, which we model as $\sigma_\varepsilon^2 \to \infty$.

Under normality, the posterior mean of productivity conditional on observing s is

$$\mathbb{E}[p \mid s] = \mu + \kappa(s - \mu), \qquad \kappa \equiv \frac{\sigma_p^2}{\sigma_p^2 + \sigma_\varepsilon^2}. \tag{4}$$

Intuition. Salary history provides a low-cost signal of worker productivity. Removing this signal does not mechanically reduce expected productivity, but it weakens inference about productivity at the hiring stage and increases uncertainty about labor input.

Reservation wages and bargaining

The worker's reservation wage is

$$r = \bar{r} + \eta, \qquad \eta \sim \mathcal{N}(0, \sigma_r^2). \tag{5}$$

where $\bar{r}$ is the mean reservation wage. Salary history also conveys information about outside options, including alternative job offers and labor market conditions.

Without SHBs, observing salary history also reduces uncertainty about reservation wages:

$$\operatorname{Var}(r \mid s) = (1 - \lambda)\sigma_r^2, \qquad \lambda \in (0, 1). \tag{6}$$

SHBs reduce λ, increasing uncertainty about outside options.

Wages are determined by Nash bargaining:

$$w = (1 - \beta)\mathbb{E}[r \mid \mathcal{I}] + \beta\mathbb{E}[p \mid \mathcal{I}], \tag{7}$$

where $\beta \in (0, 1)$ denotes worker bargaining power and $\mathcal{I}$ is the firm's information set.

Intuition. Salary history informs not only productivity but also outside options and bargaining positions. By removing this information, SHBs increase uncertainty about wages and can shift bargaining power toward workers, raising expected labor costs and their dispersion.

Effective labor quality

Effective labor quality is defined as

$$\theta \equiv p - w. \tag{8}$$

Intuition. Effective labor quality captures the net contribution of labor after accounting for wages. Even if expected productivity is unchanged, greater uncertainty about wages or match quality increases uncertainty about θ, which directly affects investment payoffs.

A.4 Investment under labor-related uncertainty

Expected project payoff conditional on information $\mathcal{I}$ is

$$\mathbb{E}[\Pi_j \mid \mathcal{I}] = A\, q_j\, f(I_j)\, \mathbb{E}[\theta \mid \mathcal{I}] - I_j. \tag{9}$$

We introduce a reduced-form uncertainty cost:

$$C(I_j) = \frac{\phi}{2}\,\mathrm{Var}(\theta \mid \mathcal{I})\, I_j^2, \qquad \phi > 0. \tag{10}$$

The firm solves

$$\max_{I_j \geq 0}\; A q_j f(I_j)\mathbb{E}[\theta \mid \mathcal{I}] - I_j - \frac{\phi}{2}\mathrm{Var}(\theta \mid \mathcal{I}) I_j^2. \tag{11}$$

Proposition 1 (Investment declines under SHBs) *If SHBs increase* $\mathrm{Var}(\theta \mid \mathcal{I})$*, then the optimal investment level* I_j^* *is weakly lower for all* q_j*, and strictly lower for interior solutions.*

Intuition. Because investment is irreversible and complementary to labor, uncertainty about effective labor quality acts like an additional cost of scale. Larger investments magnify the consequences of unfavorable realizations of labor quality, inducing firms to scale back investment when uncertainty rises.

A.5 Managerial overinvestment and efficiency

Managers receive private benefits from investment scale equal to $(1-g)bI_j$, where $b > 0$ and $g \in [0, 1]$ measures governance strength. The manager chooses projects and investment to maximize

$$V_M = \sum_{j \in \mathcal{J}} \left[A q_j f(I_j)\mathbb{E}[\theta \mid \mathcal{I}] - I_j - \frac{\phi}{2}\mathrm{Var}(\theta \mid \mathcal{I}) I_j^2 \right] + (1-g)b \sum_{j \in \mathcal{J}} I_j. \tag{12}$$

Proposition 2 (SHBs reduce overinvestment) *If SHBs increase* $\mathrm{Var}(\theta \mid \mathcal{I})$*, then:*

1. *the project acceptance cutoff* $q^*(\mathcal{I}, g)$ *rises;*
2. *investment declines disproportionately in low-q (marginal) projects;*
3. *measured investment efficiency improves.*

Intuition. Private benefits tilt managers toward excessive investment, particularly in marginal projects. However, the uncertainty penalty is convex in investment, so increased uncertainty disproportionately discourages low-return, agency-driven projects. As a result, SHBs reduce overinvestment and improve investment efficiency.

A.6 Screening and match quality

The firm can invest in screening intensity $x \geq 0$ at $t = 0$ (e.g., structured interviews, tests, or standardized pay bands). Screening costs are

$$k(x) = \frac{c}{2}x^2, \qquad c > 0. \tag{13}$$

Screening improves the expected productivity of the hired worker and reduces mismatch risk:

$$\mathbb{E}[p \mid x] = \mu + \delta x, \qquad \delta > 0, \tag{14}$$

$$\operatorname{Var}(p \mid x) = \sigma_p^2 - \nu x, \qquad \nu > 0. \tag{15}$$

Proposition 3 (SHBs increase screening and match quality) *The removal of salary history increases the optimal screening intensity x^*. As a result, effective labor input improves through better matching, increasing short-run productivity and operating profitability.*

Intuition. SHBs force firms to hire under greater informational uncertainty about worker productivity and outside options. In response, screening becomes a substitute for lost information: by investing in more structured and deliberate evaluation, firms can better identify high-quality worker-firm matches and avoid extreme mismatches. Although screening is costly, its marginal value rises when salary history is unavailable, leading firms to optimally increase screening intensity. The resulting improvement in match quality raises the effective labor input entering production, increasing short-run productivity and operating profitability even as overall investment declines.

A.7 Dynamics and attenuation

Aggregate productivity evolves according to

$$A_{t+1} = A_t(1 + g_0) + \psi \, \overline{I}_t, \tag{16}$$

where:

- $A_t > 0$ denotes the firm's productivity level at time t;
- $g_0 \geq 0$ is the exogenous baseline growth rate of productivity (e.g., economy-wide or industry-wide trends);
- $\overline{I}_t \geq 0$ is the firm's investment intensity at time t (e.g., average investment across projects or total investment scaled by assets);
- $\psi > 0$ measures how investment translates into future productivity (through capital accumulation, process improvements, organizational capabilities, or technology adoption).

Proposition 4 (Short-run gains, long-run attenuation) *If SHBs increase screening and reduce overinvestment but persistently reduce investment, then productivity and profitability may rise in the short run but attenuate over time.*

Intuition. Productivity growth has an exogenous component, captured by g_0, and an endogenous component driven by investment. SHBs can improve short-run performance by tightening project selection and improving match quality, raising the level of productivity. However, if SHBs reduce investment persistently, the firm accumulates productive capacity and capabilities more slowly, lowering future productivity growth and causing initial performance gains to attenuate.

Table A1: Robustness to Using Alternative Investment Efficiency Measures

This table presents results from the DiD imputation method examining the effect of SHBs on firm investment efficiency over the period 2012 to 2023 using alternative investment efficiency measures. The dependent variables include measures of investment efficiency (*InvEff*), overinvestment (*OverInv*), and underinvestment (*UnderInv*). *ATT* represents the average treatment effect of SHBs on investment efficiency. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	InvEff		OverInv		UnderInv	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.108***	0.103***	-0.103***	-0.086***	0.007	0.018
	(3.78)	(2.58)	(-3.10)	(-2.66)	(0.45)	(1.02)
Controls		✓		✓		✓
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
N of Obs	20,202	20,202	20,202	20,202	20,202	20,202

Table A2: Robustness to Controlling for Industry×Year Fixed Effects

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023, and all regressions include two-digit SIC industry fixed effects interacted with year fixed effects. *ATT* represents the average treatment effect of SHBs on the specified outcomes. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%ΔGDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CapEx	R&D	InvEff	OROA	TFP	$\text{Ln}(\frac{\text{Sales}}{\text{Emp}})$	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.027***	-0.164***	0.072***	0.018***	0.086***	0.063***	0.074***
	(-4.36)	(-5.66)	(2.72)	(3.84)	(4.93)	(4.82)	(2.79)
Controls	✓	✓	✓	✓	✓	✓	✓
SIC2 × Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,323	22,342	20,162	22,338	17,488	22,273	22,335

Table A3: Robustness to Excluding the COVID-19 Period

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023, excluding the COVID period (2020). *ATT* represents the average treatment effect of SHBs on the specified outcomes. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, %Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.032***	-0.167***	0.108***	0.011**	0.052***	0.040***	0.045
	(-4.94)	(-7.12)	(3.48)	(2.32)	(2.75)	(3.80)	(1.25)
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	20,429	20,447	18,454	20,443	16,060	20,384	20,440

Table A4: Robustness to Using Alternative DiD Estimators

This table reports results from alternative DiD methods examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023. Panel A uses the Wooldridge (2021) approach, while Panel B uses the stacked DiD method. *ATT* represents the average treatment effect of SHBs on the specified outcomes. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, $\%\Delta GDP$, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics in parentheses are calculated from standard errors clustered by state. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively

Panel A: Wooldridge (2021) DiD

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.026***	-0.169***	0.105***	0.014***	0.063***	0.040***	0.074***
	(-4.52)	(-9.14)	(4.28)	(3.97)	(3.20)	(2.89)	(3.20)
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	22,558	22,576	20,430	22,573	17,677	22,508	22,569

Panel B: Stacked DiD

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.023***	-0.156***	0.089***	0.015***	0.052**	0.028*	0.049
	(-3.11)	(-3.88)	(3.54)	(3.41)	(2.37)	(1.94)	(1.15)
Controls	✓	✓	✓	✓	✓	✓	✓
Year × Cohort FE	✓	✓	✓	✓	✓	✓	✓
Firm × Cohort FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	72,762	72,826	65,749	72,800	57,987	72,595	72,808

Table A5: Robustness to Excluding Firms with Dispersed Operations

This table presents results from the DiD imputation method examining the effect of SHBs on firm outcomes. The sample period is from 2012 to 2023, and the regressions exclude firms with dispersed operations. In Panel A, firms in industries with dispersed operations include retail, wholesale, and transportation, defined by two-digit SIC codes of 52–59, 50–51, and 40–48, respectively. In Panel B, we restrict the sample to firms that have at least 75% of their reported establishment-level number of employees located in their headquarters state. *ATT* represents the average treatment effect of SHBs on the specified outcomes. Lagged control variables include *Ln(Assets)*, *MB*, *CashFlow*, *CashHolding*, *BLEV*, *Ln(GDP)*, *%*Δ*GDP*, *TaxRate*, *UnempRate*, *DemGov*, *Ln(UnempBen)*, *Age*, *Educ*, *%Female*, and *Marijuana*. All variables are defined in the Appendix. *t*-statistics, calculated from standard errors clustered by state, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Exclude Firms in Industries with Dispersed Operations

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.031***	-0.207***	0.121***	0.022***	0.107***	0.057***	0.098***
	(-3.75)	(-7.13)	(3.99)	(3.87)	(6.19)	(3.90)	(3.03)
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	17,857	17,873	15,728	17,870	13,903	17,818	17,869

Panel B: Exclude Firms with Dispersed Operations using Establishment-Level Data

	CapEx	R&D	InvEff	OROA	TFP	Ln($\frac{\text{Sales}}{\text{Emp}}$)	Ln(MVE)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ATT	-0.070***	-0.385***	0.185**	0.046***	0.288***	0.160***	0.249***
	(-3.67)	(-6.05)	(2.26)	(3.99)	(4.15)	(4.65)	(3.61)
Controls	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
N of Obs	5,364	5,365	4,884	5,365	3,911	5,346	5,365

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