

The Price of Knowing: Pay Transparency, Labor Market Tightness, and Aggregate Wage Effects^{*}

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Abstract

While pay transparency policies have gained popularity as tools to address pay disparities, little is known about their broad effects on average wage levels, and no evidence exists on whether these effects vary with labor market conditions. I develop a novel theoretical framework demonstrating how pay transparency shifts relative bargaining power in favor of firms or workers, depending on the availability of outside options. Exploiting the staggered adoption of the Right of Workers to Talk (ROWTT) laws across U.S. states and applying modern event-study methods, I corroborate these predictions and show that increased pay information sharing among employees raises average wages in tight labor markets, but reduces wages in slack markets. Taken together, these findings reveal previously overlooked tradeoffs essential to a comprehensive cost-benefit analysis of pay transparency policies.

Keywords: Pay Transparency; Labor Market Conditions; Wage Levels
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Large-scale pay transparency policies have become increasingly popular worldwide and in the U.S., particularly since 2010. The promotion of gender pay equity is typically cited as the primary policy goal. Consequently, a growing body of research has studied how transparency affects gender pay gaps. However, with one exception, these studies do not explicitly evaluate the aggregate impact on average wage levels. Furthermore, many of these studies analyze policies that simultaneously encourage across-firm pay information sharing and are therefore unable to isolate the wage effects solely due to internal information flow (Baker et al. 2023; Mas 2017; Obloj and Zenger 2022). Studies that do investigate policies aimed exclusively at improving within-firm pay transparency are restricted to mandatory internal reports of relative earning statistics by gender, a context that enhances pay comparisons along only a single dimension and undermines the full potential of pay transparency (Bennedsen et al. 2022; Böheim and Gust 2022; Blundell et al. 2025; Gulyas, Seitz, and Sinha 2023).

This paper examines how pay transparency affects average wage levels in distinctive ways depending on labor market conditions by assessing a class of within-firm pay transparency policies that allow employees to share any pay information with one another. Extending the pioneering model in Cullen and Pakzad-Hurson (2023), which describes the general effect of pay transparency on average wage levels, I introduce a novel theoretical framework that incorporates worker turnover and captures how bargaining dynamics between firms and workers shift differently in stronger and weaker labor markets. Consistent with my proposed hypotheses, event studies using U.S. state-level pay transparency laws show that increased transparency raises average wage levels in tight local labor markets, where labor demand exceeds supply, and reduces wage levels in slack markets. This finding challenges the current understanding¹ that pay transparency always leads to

¹ Based on Cullen and Pakzad-Hurson (2023), the only existing evidence on this topic to my best knowledge.

the unintended outcome of lower average wages in equilibrium, attributing the decline to the monopsony power firms gain after pay standardization. It also highlights the non-wage implications of pay regulations.

In the U.S., state-level within-firm pay transparency policies, referred to as the “Right of Workers to Talk” (ROWTT) laws, can significantly reshape labor market outcomes. As of 2010, 66% of private-sector workers reported being formally or informally discouraged by their employers from discussing pay (Institute for Women’s Policy Research 2017). Firms’ efforts to inhibit such conversations can have serious consequences. In the landmark case *Ledbetter v. Goodyear Tire & Rubber Co.* (2007), Mrs. Ledbetter discovered after 19 years through an anonymous note that she had been paid significantly less than her male colleagues. Because Goodyear prohibited pay discussions, the Supreme Court ruled against her for missing the filing deadline. Although Congress passed the Lilly Ledbetter Fair Pay Act of 2009 to extend filing windows (2009), pay secrecy itself was not resolved. Workers, especially those exempt from the National Labor Relations Act (NLRA), still lack protection when speaking out.

Whether pay transparency results in average wage increases or decreases has a complex relationship with the economic and policy environment. This paper takes the first step in evaluating the heterogeneous wage level effects by considering endogenous worker turnover and changes in relative firm-worker bargaining power across varying levels of labor market tightness. Previous empirical evidence suggests that workers often respond to discovering their colleagues’ pay with higher realized turnover or separation intent (Alterman et al. 2021; Card et al. 2012; Cullen and Perez-Truglia 2022). Standard search models agree that turnover decisions are, at baseline, a function of the arrival rate and quality of outside options, both of which fluctuate over the business cycle (Rogerson, Shimer, and Wright 2005). It follows that workers’ ability to credibly leverage

the threat of turnover in wage renegotiations under increased transparency hinges on the economic conditions that they face. Since pay standardization initially transfers price-setting power to firms, the availability of workers' outside options becomes a key constraint on whether firms compress wages from the top or the bottom of the distribution among similar employees in response to internal equity pressures.

To test these theoretical predictions, this paper exploits the staggered implementation of the ROWTT laws across U.S. states and applies Sun and Abraham (2021) event-study methods to estimate dynamic wage responses across different labor market conditions. First-stage evidence from secondary sources shows that between 2010 and 2018, states that enacted the ROWTT laws experienced three times the decline in pay secrecy from the baseline compared to other states (Cullen and Pakzad-Hurson 2023). Using over 10 million full-time private-sector prime-age workers in ACS waves from 2000 to 2023, I estimate the effects of ROWTT laws on log wage income separately for treated states with high versus low state-level unemployment rates at the time of the policy's effective date. The control group consists of states that never enacted the policy by 2024. Results indicate a decline of about 2% in average wage levels for states where the policy became effective in slack local labor markets, and contrasting dynamic average effects ranging from 1% to 4% for states where the policy was first enforced in tight local labor markets.

Causal identification of each subgroup-specific effect requires that, in the absence of the ROWTT laws, the average wage levels would have evolved similarly across treated states and control states. To interpret any potential difference in treatment effects between the two subgroups of treated states, there cannot be any subgroup-specific shocks correlated with the identifying variations. I present a series of systematic empirical tests that rule out key potential concerns, including endogenous policy timing, mean reversion from labor market differences across states,

differential selection or sorting, and responses along the extensive margin of employment. Moreover, I counter two common competing narratives in this context, specifically overlapping pay transparency measures and contemporaneous political campaigns, which could otherwise explain the observed patterns. Taken together, the evidence tells a consistent story: the opposing effects on average wage levels under pay transparency arise from differences in the associated labor market conditions at enactment, being positive in strong markets and negative in weak markets.

To the best of my knowledge, this paper is the first to examine how the effects of pay transparency on average wage levels vary with labor market conditions, offering both theoretical and empirical contributions. It shifts the focus away from wage disparities, which almost all prior research has concentrated, toward understanding whether firms or workers bear the economic incidence of pay transparency as reflected in average wage levels. This broader perspective has profound implications for a comprehensive cost-benefit analysis of pay transparency policies. While these policies have been shown to increase gender pay equity in most large-scale implementations², they also alter the aggregate welfare of firms and workers in ways not directly intended by design. Decision-makers should weigh these tradeoffs when drafting or evaluating pay transparency policies.

The remainder of this paper is organized as follows. Section 1 provides background information on the ROWTT laws and establishes their first-stage effectiveness. Section 2 reviews the existing theories linking pay transparency to wages and introduces a new framework that generalizes across different labor market conditions. Section 3 describes the empirical strategy and

² Studies that find reduced gender pay gaps include Baker et al. (2023); Bennedsen et al. (2022); Blundell et al. (2025); Mas (2017); and Obloj and Zenger (2022). Studies that find widen gender pay gaps include Böheim and Gust (2022) and Gulyas, Seitz, and Sinha (2023).

identification assumptions. Section 4 presents the results along with supporting evidence. Section 5 concludes the paper.

1. Institutional Details

1.1. What is a Right of Workers to Talk (ROWTT) Law

Within-firm pay transparency policies aim to increase pay information sharing among employees at the same firm. It is important to distinguish these from other types of pay transparency policies, such as requiring salary information in job postings. Depending on the policy type, the flow of additional pay information can be across or within firms and the nature of information shared can vary, both resulting in differential predicted impacts on labor outcomes (Cullen 2024). Broadly speaking, however, all pay transparency policies are primarily motivated by closing pay gaps, with no direct consideration of changes in aggregate average wage levels.

Specifically, the laws referred to as the “Right of Workers to Talk” (ROWTT) laws in the pay transparency literature (Cullen and Pakzad-Hurson 2023; Cullen 2024) protect workers’ right to disclose and inquire about pay among employees. These laws typically establish that this right is inherent, and firms shall not take away the right or retaliate against workers for exercising it. Sharing salary and wage information is always protected, and many recent ROWTT laws use the word “compensation” to allow for more comprehensive information sharing.

For individuals who are interested in learning their fair or expected potential pay, the pay of their colleagues provides the closest comparisons. Moreover, within-firm pay sharing is the foundation for organized campaigns among workers and can trigger broader group actions. Despite declining unionization, similar workers still come together to collectively bargain leveraging shared pay information, as evidenced by recent headlines such as “Why a Google spreadsheet was the most powerful tool for labor in 2019” (Reyes 2019).

1.2. History of the Right of Workers to Talk (ROWTT) Laws

In the U.S., workers' right to disclose and inquire about pay within firms was poorly protected historically. Though the National Labor Relations Act (NLRA) enacted in 1935 was passed to protect workers' "concerted activities", it took many landmark cases over time before the interpretation of the statute gradually shifted toward pay-information sharing among employees as a covered right (Green 2014). Even under today's interpretations, many groups of workers are still exempt from the NLRA. The NLRA statute uses language that was once ambiguous, and the punitive measures remain relatively weak, with additional procedural limitations that jointly make law enforcement under the NLRA difficult (Green 2014; Stansbury 2021).

Many state-level ROWTT laws were passed in recent years to fill this gap, with noticeable differences from the NLRA that enhanced right protections. The state-level statutes explicitly name pay information sharing among employees as a protected right, and they are applicable to almost all workers. For example, Illinois' law covers "...any individual for inquiring about, disclosing, comparing, or otherwise discussing the employee's wages or the wages of any other employee..." (Illinois Equal Pay Act of 2003). The most common exemption among the state-level ROWTT laws is to prohibit human resources staff who readily have access to pay information of other employees from sharing that information. In addition, harsher financial penalties are imposed, and specific state agencies are dedicated to violation detection and enforcement.

Led by Illinois, where the ROWTT law went into effect in 2004, a total of 21 states and the District of Columbia (DC) had the law in effect by 2024. Table 1 displays the year when the ROWTT law became applicable for each state, and Figure 1 visualizes this timeline. Darker blues represent later effective years, and gray means no ROWTT was ever in place. One thing that immediately stands out is that states with ROWTT laws are geographically concentrated on the

West Coast, Northeast, and the Midwest. The implications are discussed in later sections. At the federal level, an executive order by the Obama administration extended analogous protections to employees working for federal contractors, starting in 2016. Nonetheless, due to the narrow scope of workers protected under the executive order, many states continued to pass their own ROWTT laws that are more universal.

1.3. First-stage Evidence of the Right of Workers to Talk (ROWTT) Laws

Nationally, private-sector workers lack pay transparency. Unlike in the public sector, wage and salary information often is not easily accessible, and firms are known to intentionally silence pay conversations. The Institute for Women's Policy Research/Rockefeller Survey of Economic Security in 2010 reports that 25% of private sector workers at the time worked for employers where discussion about pay is formally prohibited or could be punished, and another 41% reported that their managers discouraged them from discussing wage and salary (Institute for Women's Policy Research 2017). For comparison, in the same survey, only 6% and 9% of the workers in the public sector reported that they are formally prohibited and informally discouraged from pay discussion, respectively.

States that passed the ROWTT laws on average experienced a larger increase in the likelihood that employees learned about each other's pay compared to the states that did not (Cullen and Pakzad-Hurson 2023). While there is no known national data measuring the levels of direct pay information sharing among employees, Cullen and Pakzad-Hurson (2023) gained access to two waves of repeated surveys in 2010 and in 2017/2018 that ask private-sector workers if their employers formally prohibited pay discussion. Figure A1 in the Appendix shows their estimated differential changes in the share of workers reporting pay secrecy across the two survey waves. The average share dropped from 33.2% to 10.1% for states where a ROWTT law went into effect

between the survey waves, and from 24.3% to 18.7% for all other states. This translates to a decline of 70 percent in worker perceived pay secrecy for the treated states from the 2010 baseline, compared to a reduction of merely 23 percent for the other states. The trajectories for the two groups of states are statistically different at the 0.001 level.

2. Theoretical Framework

2.1. Wage Differentials and Information Value of Pay

To predict the potential effects of pay transparency on wages, we need to understand what factors contribute to the dispersion in pay in the first place. Pay differences can be decomposed into two broad elements: the fundamental variations in people's marginal product of labor and any deviation due to bargaining. Even in a perfectly competitive labor market, some levels of pay dispersion are anticipated, owing to factors such as experience, education levels, and efforts.

Yet, the dispersion in wages net of the marginal product of labor is not negligible and is persistent in the U.S. labor market. Descriptive evidence documents a steady rise in residual wage inequality since the 1970s (Autor, Katz, and Kearney 2008). More recent structural estimates suggest that at least 21% of the cross-sectional variation in offered wages is not explained by individual productivity (Hall and Mueller 2018). A systematic calibration of search models to observed U.S. worker flows identifies on-the-job search and firms' ability to make counteroffers as the primary drivers of large residual wage dispersions (Hornstein, Krusell, and Violante 2011). These results highlight the important role of bargaining in wage determination.

The overall residual wage dispersion can also be decomposed into across and within firm variations, though empirical evidence is comparatively more limited. Descriptively, based on a large payroll dataset that covers over half a million firms, in 2022, the average pay gap between any two employees with the same position title is 46.7% and restricting the comparisons to people

with identical titles at the same company still yields an average pay gap of 10.7% (Cullen 2024). Asymmetric information in wage-setting is thought to be an important contributor to within-firm pay dispersions among similar employees. Often, firms approximate the true productivity of their workers, and workers don't know the full range of wages offered by their firms. Differential pay, thus, can arise due to uncertainty about both productivity and bargaining.

Documented pay dispersion among comparable employees at the same company makes it possible for increased within-firm pay transparency to trigger new bargaining and search behavior that will reflect in wage changes. Experimental evidence suggests that, on average, at the baseline workers accurately perceive their relative performance but not the relative pay, so learning about their colleagues' pay updates workers' own expected future pay (Cullen and Perez-Truglia 2022). This finding demonstrates that workers often misjudge the market value of their labor, and benchmarking to other similar workers helps to mitigate the information gap. Importantly, how workers react to the new information is not symmetric across the pay distribution. Workers who are paid above others typically won't take immediate actions, whereas workers who are paid below others will disproportionately renegotiate within-firm for higher pay or leave for better outside offers (Card et al. 2012; Cullen and Perez-Truglia 2022).

2.2. Existing Framework on the Aggregate Wage Effects of Within-firm Pay Transparency

To date, Cullen and Pakzad-Hurson (2023) is the only known study that focuses on policies solely targeting within-firm transparency and allows for the sharing of all pay information. More importantly, the authors are the first to attempt modeling the aggregate average wage responses to pay transparency. Their headline conclusion is that, though a subset of workers can secure a pay raise, increased pay transparency will result in average wage declines. Firms are pressured to move toward more standardized pay formulas, but this change effectively transfers the de-facto

bargaining power to the firms. Acting like monopsonists to maximize profit, firms set wages at lower levels and can reasonably refuse raises to avoid costly renegotiations with other employees. The shift in bargaining power in their model can reduce workers' demanded levels of initial pay to increase their chance of matching to a firm as well.

One key component of the Cullen and Pakzad-Hurson (2023) model is that there is no voluntary turnover or dynamic matching. Each worker has a constant private outside option randomly drawn from a distribution, and this outside offer bounds the minimum initial wage they are willing to accept working for a firm. It is assumed that there are no productivity differences between workers. Accordingly, each firm will employ workers at a wide range of wage levels, so long as a worker's initial offer is below the firm's pre-determined wage ceiling. With increased pay transparency, a subset of workers learns about the pay of their colleagues, and among them some will renegotiate and secure a higher pay up to the firm's maximum wage allowed. Regardless, once matched to a firm, any worker who is employed will always remain at the same firm since their outside options do not update. In this stylized model, foreseeing the labor cost spillovers, profit-maximizing firms rationally scale down the highest wage offered, and the transfer of the bargaining power due to pay standardization is one-directional in favor of firms.

However, voluntary turnover is one of workers' primary levers in wage negotiations, and empirically, workers who gain access to other employees' pay info are more likely to leave the firm or express an increase in the intent to look for new jobs (Alterman et al. 2021; Card et al. 2012; Cullen and Perez-Truglia 2022). Noticeably, turnover threat is not confined to the low earners within similar positions. When increased pay transparency reveals poor distributive justice in a firm, workers overall reduce their trust in the organization, and this doubt can manifest in higher separation intentions for all workers (Alterman et al. 2021).

Therefore, increased pay transparency likewise can shift the de-facto bargaining power toward the workers and, in turn, limit firms' ability to scale down the wage offers. High turnover is costly to the firm. At a minimum, extra resources need to be allocated to search efforts. In many contexts, there will be other incurred costs. For example, it is widely acknowledged that there is firm-specific human capital. Longer average employee tenures can increase a firm's rent seeking ability. Hence, when turnover threat is high, strategically firms are more inclined to satisfy workers' needs in bargaining. Exploring how turnover fits into the existing framework will help clarify our understanding of the aggregate average wage responses of pay transparency.

2.3. New Predictions under Varying Turnover by Labor Market Conditions

Workers' perceptions of pay equity are shaped by comparisons with one another, and their relative pay levels in the distribution influence their happiness, even if absolute pay levels remain constant (Luttmer 2005). In principle, to attend to increased demand for pay equity, firms can choose between giving slower raises to the high-earners or elevating the pay for the low-earners among comparable employees. Absent constraints, profit-maximizing firms will always choose the former option because it reduces current labor costs and lowers the benchmark for future pay. Though, in practice, firms' decisions on how to narrow pay distributions are confined by workers' turnover behavior.

In search models, voluntary turnover is typically a function of the arrival rate and the value of outside offers, so its level varies across different labor market conditions (Rogerson, Shimer, and Wright 2005). It follows that when expanding our within-firm bargaining model to account for turnover, we need to separately consider the effects of pay transparency on average wages in economic booms and busts. Depending on the availability of outside options for workers, pay

transparency in net can enhance the relative de-facto bargaining power for the firms or the workers, correspondingly to realized declines or raises in average wages respectively.

The case of tight markets is more intuitive. Since workers have abundant outside options, with pay transparency, in general there will be a higher level of realized turnover when pay inequity is left untreated. Firms overall have a stronger incentive to respond than in slack markets, but they are expected to react differently. If firms try to cut raises for the high-earners among comparable employees to cap the maximum pay, they will face stronger pushback. While higher perceived pay fairness may mitigate separation intent from the low-earners, the high-earners are unlikely to compromise given that in tight markets they have plentiful outside options and can just leave for better offers elsewhere. Firms likely have no choice but to instead raise the pay for the low-earners to address equity concerns and keep the high-earners' pay stable at already satisfactory rates. When the pay compression comes from the bottom of the distributions, the average wages will increase.

In slack markets, workers have fewer outside options and lower ability to credibly use turnover as a leverage in wage bargaining with firms. We move closer to the Cullen and Pakzad-Hurson (2023) model predictions conceptualized in a state of the world where there is no turnover. In anticipation of costly renegotiations and with newly granted monopsonist price-setting power, firms will reduce the maximum wage offered to cap the additional labor costs that can result from more frequent pay information exchanges. Pay equity is achieved primarily through shrinking raises for the high earners.

Critically, a question left unanswered by their model is that with low or no turnover threat, why do firms still feel pressured to move toward more standardized pay formulas and not just ignore renegotiations to curb the spillover concerns? I provide several potential explanations. Regardless of labor market conditions, the preservation of organizational harmony and company

reputation will hold firms somewhat accountable in closing pay gaps after pay revelations. Firm management faces significant challenges under large pay discrepancies, including increased employee grievance (Cullen and Perez-Truglia 2022) and diminished productivity (Bennedsen et al. 2022). Potentially more costly, firms' reactions to pay transparency is capable of shaping workers' trust in the organization, and failures to improve distributive justice will lead to higher separation intent (Alterman et al. 2021). The loss of trust is independent of the business cycle and is expected to persist. As a result, even in the extreme case when workers are completely locked into their matched firms in a recession, firms still have the incentive to send out signals that they care about equity, especially when they have the bargaining power to achieve equity through lower average wage levels.

In summary, I predict that pay transparency will increase average wages during tight labor markets but will decrease average wages in slack markets. Pay transparency nudges firms to move towards more standardized pay in response to equity concerns. This process results in an initial transfer of bargaining power to the firms who set the pay formulas. On the other hand, pay transparency arms workers with additional information that they can leverage in wage negotiations, albeit the strength of this leverage is contingent on whether their turnover threat is credible if the deal fails. Thus, when there are fewer outside options in the labor market, firms are free to reduce the raise sizes at the top of the wage distribution among comparable employees. Whereas when the outside options are plentiful, the net change in bargaining power favors the workers, and pay equity is achieved through larger raises at the bottom of the wage distribution.

3. Empirical Strategy

3.1. Data, Samples, and Summary Statistics

Large sample sizes and representative coverage of the entire U.S. make the American Community Survey (ACS) the ideal dataset to use in this study. Survey waves from 2000-2023 provide consistent data on individuals' state of work, demographics, and labor market outcomes. The primary outcome of interest is measured by taking the log of reported annual wage income by individuals. The effective dates of state-level ROWTT laws are sourced from Cullen (2024) and the U.S. Department of Labor (2025). Since the ROWTT laws regulate workplace practices, the treatment is assigned to individuals by the state of work.

This study focuses on individuals who are full-time, private-sector, and prime-age workers, where the impact from increased within-firm pay transparency is expected to be most pronounced. This choice aligns with the ACS sample criteria in Cullen and Pakzad-Hurson (2023). The reasoning is straightforward. Within-firm pay transparency has greater potential to benefit workers who at baseline have lower protections, yet who have high exposure to their firms to participate in increased pay discussion and bargaining with their firm. All states except for Virginia, Rhode Island, and Hawaii are included in the analyses. These three states are dropped because their ROWTT laws were enacted too recently³, so there is insufficient data to estimate the dynamic effects post the treatment. These states arguably do not belong in the never-treated states either. Later evidence demonstrates that the sample choices of individuals and states both are not influential in determining the results.

Table 2 presents the individual-level summary statistics for the sampled states across all time periods, weighted by person weights in the survey. Over 10 million full-time, private-sector, and

³ ROWTT is effective in 2020 for Virginia, in 2023 for Rhode Island, and in 2024 for Hawaii.

prime age workers form the sample of interest. States where the ROWTT law was in effect by 2019 are in the treated group, and the states that do not have an applicable ROWTT law as of 2024 are in the control group. It should be emphasized that balance is not needed for the empirical design. The demographic characteristics and labor market outcomes are, for the most part, similar across the two groups. The treated states are more racially diverse and educated than the control states on average. Noticeably, the average inflation-adjusted annual wage income is almost \$12,000 higher for the treated states. This general pattern is also observed for all prime age individuals more broadly. Unsurprisingly, the focal workers in this study are positively selected among the prime age population.

State-level unemployment rates in the effective year of the ROWTT law serve as proxy for the local labor market conditions, and this metric allows for split sample analyses of the theorized opposing effects of pay transparency on the average wage levels in aggregation. The grand average of all state-level employment rates between 1976 and 2024, or across the full panel dataset available, is used as the threshold. If the annual state unemployment rate is above the specified threshold, it is said that the state is in a slack labor market that year. Vice versa, if the state rate is below the threshold, then the local labor market is considered tight.

To maximize statistical power and examine heterogeneity in average wage effects, states that enacted a ROWTT policy are divided into two groups by the binary unemployment rate threshold. The 29 never-treated states⁴ are retained as a single group to serve as a baseline. Figure 2 is a kernel density plot of the state-level unemployment rates in the policy effective year for the sampled 19 treated states including DC. The wide overall dispersions, ranging roughly from 3 to

⁴ Although the state of Louisiana passed a law labeled as ROWTT in 2013, it is grouped with the never-treated states because the scope of its law falls short of typical ROWTT-style laws. The statute applies only to public employees and does not explicitly guarantee the core right of pay discussions among employees. Its substance closely resembles public-sector pay transparency provisions already common in non-treated states.

8.5 percentage points, permit informative analyses of the association between varying local labor market conditions and the average wage level effects of pay transparency. As shown in the figure, the binary threshold for unemployment rates separating the slack labor markets from tight markets is set at 5.75 percentage points. About one third of the treated states in the sample fall above the threshold and implemented the policy in slack markets.

3.2. Main Methodology and Identification Assumptions

Leveraging the staggered implementation of ROWTT laws across the U.S. states, event study designs are employed to isolate the causal effects of increased within-firm pay transparency on average wage levels. To minimize contamination, the control group consists exclusively of states that never enacted ROWTT laws. The primary assumption is that, between 2000-2023 and in the absence of the ROWTT law, the underlying average wages for full-time private-sector prime-age workers would have trended similarly in the states that had active ROWTT laws by 2019 compared to the states that never passed the law. This assumption is empirically tested through pre-trend analyses and supplementary event studies that use sample selection flags and proxies of potential wages as outcomes.

Furthermore, modern difference-in-difference methods are necessary in this setting to properly adjust for biases introduced by staggered adoptions. Given that the effect of pay transparency on average wages in aggregate is predicted to vary by labor market conditions, heterogeneous treatment effects across states and over the business cycle are anticipated. Traditional two-way fixed effects models, therefore, will produce misleading estimates. This study implements the Sun and Abraham (2021) method, which is designed to correct biases in event studies under heterogeneity and produces easily interpretable dynamic treatment effects over time.

Using this approach also maintains consistency with the estimation methods employed by Cullen and Pakzad-Hurson (2023) and allows for direct comparison of results on average wage changes.

Given the lack of consensus on best practices for staggered adjustments in triple-difference designs, this study evaluates the theorized diverging wage effects of pay transparency under different labor market conditions using split-sample analyses. The parallel trend assumption for causal identification is modified in a triple-difference setting. Ideally, treated and control states still share overall parallel trends on average, ensuring estimates of wage effects for each treated group are accurate. This part of the assumption is not required for causal interpretation of any gaps in post-treatment trends between the two groups of treated states, provided any baseline trend deviations from the control group equally apply to both groups. In other words, the split itself needs to be exogenous to the timing and adoption of ROWTT law.

The general event study setup is illustrated in Equation (1). While the data are at the individual level (i), treatment varies at the levels of the current state of work (s) and the calendar year (t). Relative time (j) is defined as the number of years since the ROWTT first went into effect in a given state. The never-treated states establish baseline comparisons. The year immediately preceding implementation ($t - 1$) is omitted as the reference period. Although dynamic effects are estimated for all available relative periods for flexibility, all analyses focus on the window from four years before to four years after the law's effective date to balance the number of treated states included with consistency in sample composition. All regressions are weighted by individual-level ACS survey weights to help with representativeness and precision. Standard errors are two-way clustered by state and year to account for within-state and within-year correlations.

$$Y_{s,t,i} = \alpha + \sum_{j=-19}^{-2} \beta_j D_{s,t+j} + \sum_{j=0}^{19} \beta_j D_{s,t+j} + \mu_s + \lambda_t + \varepsilon_{s,t,i} \quad (1)$$

The key outcome of interest is logged annual wage income. Measurement errors due to self-reporting are unlikely to bias the results, as such errors are expected to be differenced out, assuming no systematic deviations between the comparison groups. Inflation is measured consistently only at the national level and is captured by the year fixed effects, so there is no explicit need to convert nominal incomes to real terms. Individuals with no wage income drop out of the sample due to the log transformation, and potential extensive margin responses are separately examined in robustness checks. Wage income is top-coded in the raw data⁵ and is left unchanged. To further reduce the influence of outliers, the bottom 1st percentile is winsorized.

3.3. Replication of Cullen & Pakzad-Hurson (2023)

The primary goal of this study is to demonstrate that increased pay transparency has heterogeneous effects on aggregate average wage levels depending on local labor market conditions, and that the only existing theoretical model, introduced by Cullen and Pakzad-Hurson (2023), does not fully capture these dynamics and can benefit from further extension. Replicating their results using this study's regression setup helps rule out methodological differences as the source of any diverging findings. The core specifications are shared, though this study relaxes several restrictions imposed in Cullen and Pakzad-Hurson (2023)'s preferred model⁶: the control variables are omitted, the never-treated states replace the late-treated states to form the control group, and each relative time indicator outside of the main analysis window is left disaggregated rather than being grouped into an early-pre-period dummy and a later-post-period dummy.

⁵ For ACS in 2000-2002, wage income is top-coded at the state-mean above \$200,000. For ACS in 2003-onward, wage income is top-coded at the 99.5th percentile in state.

⁶ I first followed the exact steps of Cullen and Pakzad-Hurson (2023) from data extraction to results and successfully replicate their preferred estimates, as reported in Column 1 of Table 1 and plotted in Panel A of Figure 3. All subsequent modifications are introduced incrementally from this baseline.

Figure A2 plots the event study estimates based on Equation (1) using only states with a ROWTT law effective by 2013, the implicit treated sample that contributes to the balanced-panel Sun-Abraham dynamic effects reported in Table 1, Column 5 of Cullen and Pakzad-Hurson (2023). For this subset of treated states, both studies find a similar post-treatment decline of approximately 2% in average wage income that persists for at least three years. This comparison reinforces that methodological differences are unlikely to explain any divergence in broader results. In addition, since Cullen and Pakzad-Hurson (2023) draw their sample from primarily recessionary years, their results agree with my theoretical predictions that pay transparency decreases average wage levels in slack labor markets.

4. Results

4.1. Opposing Aggregate Wage Effects by Labor Market Conditions

Figure 3 presents split sample estimates for the treated states with high versus low state-level unemployment rates at $t = 0$, based on Equation (1), and this is my preferred specification. The diverging post-treatment trends are striking and persistent. For the subset of the treated states where the ROWTT law is enacted in weak local labor markets, a stable reduction of about 2% in the average wage levels is observed compared to all the never-treated states. This magnitude aligns with findings in Cullen and Pakzad-Hurson (2023), whose non-split sample analysis based on data from mostly recessionary periods also documented a decline in average wages following the policy. In contrast, when the policy became binding in stronger local labor markets, there is a growing effect of pay transparency on the average wage levels, starting with an immediate 1% increase and eventually reaching about 4%. The confidence intervals at the 95% level do not overlap between the two groups of treated states, indicating a statistically significant difference in post-policy trends. These opposing effects extend the pioneering work on this topic (from Cullen & Pakzad-Hurson

2023), which previously concluded that pay transparency will always lead to the unintended outcome of lower average wages in equilibrium. Including the three latest treated states to create a complete but unbalanced panel does not alter the results (see Figure A3).

My findings uncover factors previously overlooked in the literature: whether the firms or the workers bear the economic incidence for pay transparency depends on the economic and policy environment. This set of event studies provides the first causal evidence supporting my hypothesis that the effect of pay transparency on average wage levels is contingent on the tightness of the associated labor markets. These patterns are consistent with the predictions of the theoretical framework developed in this paper. In tight labor markets, firms face heightened competition for workers, and employees can more credibly leverage the threat of turnover in wage negotiations under pay transparency. Consequently, pay discrepancies among comparable employees tend to shrink as the low-earners secure higher wages, increasing average wage levels in the economy. In slack labor markets, general concerns of pay equity still prompt the firms to act. However, since firms can exercise their monopsonist power to a greater extent, narrower dispersion is more likely achieved through slower raises for the top-earners, reducing average wage levels in aggregate.

The absence of differential pre-trends for both groups of treated states affirms the key assumption that the timing of ROWTT law effective dates is exogenous. For instance, this rules out the possibility that states selectively enacted the ROWTT law in response to pre-existing economic conditions, which could have driven changes in average wage levels irrespective of the policy. Another plausible story that can be discounted is that workers with different wage trajectories or firms with distinct wage-setting strategies had been sorting towards states with more labor-friendly environments, potentially generating changes in the average wage trends that coincide with the ROWTT law.

Particularly relevant to the divergence in post-treatment effects, one may be worried that the partitioning of treated states based on state-level unemployment rates at $t = 0$ could induce mechanical mean reversion that might also explain the observed patterns. Marginal hires, who tend to earn lower wages, disproportionately exit the labor market during downturns and re-enter during recoveries. These cyclical composition shifts can mechanically raise average wages following tight labor markets and lower them after slack markets, effects that may not be fully captured by the annual trends estimated from the never-treated states. To address this concern, Equation (1) is re-estimated by splitting treated states instead based on the national unemployment rates at $t = 0$. The downside of this approach is that it provides a less granular measure of labor market conditions, which diminishes our ability to distinguish effects along this spectrum. The estimates are noisier, but the gap in post-treatment trends remains evident between states that enacted the ROWTT law in slack versus tight national labor markets (see Figure A4). The next section directly investigates differential trends in non-wage labor market outcomes, further strengthening the argument that mean revision is unlikely to be a major confounding factor.

4.2. Minimal Differential Sample Selections

Causal interpretations of wage trend comparisons require the absence of differential sample selection between the treated and control states, as well as between the two subgroups of treated states. The former ensures accurate estimation of individual effects, and the latter is required for causal interpretations of any divergence between the two treated subgroups. Age is treated as an exogenous characteristic. That said, because the focal workers of interest are also selected based on labor market outcomes, namely their employment status, hours worked, and private-sector choices, it is necessary to rule out the possibility that these selection margins themselves respond to the ROWTT laws. Compositional shifts along those margins can change observed average wage

levels independent of true wage adjustments. For example, if pay transparency makes hiring additional workers more expensive, such that new hires are disproportionately high- or low-ability individuals, the average wage levels in treated states may mechanically increase or decrease due to changes in average worker ability in the sample.

To assess this possibility, the first set of supporting evidence consists of event-study estimates that replace log wage income with various binary indicators of sample selection margins as the outcome in Equation (1). These indicators are constructed unconditionally: if a criterion is met, the variable is coded as 1, and all other cases including missing values are coded as 0. An overall inclusion indicator, capturing whether a prime-age worker is part of the final analyzable sample, shows no evidence of differential selections. This fact is reflected in the flat and non-diverging trends in Figure 4. To further rule out selection on specific non-wage labor market outcomes, the same exercise is repeated for the probability of being in the labor force, being employed, working full-time, and working in the private sector. All exhibit flat pre-trends, and no post-treatment differences are significant enough to explain the observed wage patterns.⁷ (see Figure A5).

Another potential source of sample selection is that workers may systematically sort into positions with different pay levels in a way that could have generated the observed wage gaps between groups even in the absence of ROWTT laws. To investigate this scenario, potential income by industry is constructed using state-level weighted average wage income from years before any ROWTT law was active. These pre-treatment values are then assigned to individuals based on their current industry, isolating the component of pay plausibly unaffected by the treatment itself. This approach assumes that workers' industry choices are not directly influenced

⁷ While there may be a slight post-treatment divergence in employment probability across the two types of treated states, the estimated differences are only marginally significant and unlikely to account for the full magnitude of the observed opposing effects of the ROWTT law on average wage levels.

by increased pay transparency within firms. If the observed post treatment divergence in actual average wages reflects true transparency effects rather than sorting, no comparable divergence should appear in potential income trends when estimating Equation (1). Consistent with this expectation, Figure 5 shows null results. A similar analysis using pre-period occupation-level wages (see Figure A6) reveals minor post-period divergence across treated subgroups, but the magnitudes are far too small to account for the main results.

4.3. Ruling out Potential Confounders Arise from the Institutional Context

The key identification assumption of a difference-in-differences design is parallel trends across the full study period. While this assumption can be empirically tested in the pre-treatment period, it can at best be inferred for the post-treatment period. Any simultaneous policies or shocks that coincide with the enactments of the ROWTT laws and influence wage dynamics will prevent causal interpretations, and any observed post-period trends can no longer solely be attributed to the ROWTT laws. For interpreting heterogeneous treatment effects, there can be no subgroup-specific shocks correlated with the way treated states are split.

Drawing on institutional context, two primary sources of concerns are the passage of other pay transparency measures around the same time and broader political economy effects associated with campaign activity. Notably, states that adopted ROWTT laws in later periods are disproportionately exposed to these factors, during a period when local labor markets were typically stronger. Carefully ruling out these competing narratives is essential to establish that the observed opposing post-period divergence in average wage levels truly arises from differences in labor market dynamics as predicted by the expanded bargaining framework I propose.

Legislatures often bundle multiple provisions into one bill or pass them in close succession, making it hard to identify the effects of any one component. Controlling for such “tagged-on”

provisions via stacked dynamic estimates is impractical, and dropping affected states entirely is a more feasible solution. The updated event study excludes treated states with other major pay transparency measures, namely salary history bans and mandatory salary postings, taken effect within four years of $t = 0$ ⁸. In all cases, the ROWTT law predated the other measures. Only one never-treated state passed a separate pay transparency measure⁹, and it is removed from the control group. Unfortunately, this strategy comes at the cost of a drastically smaller sample. Nevertheless, Figure 6 reveals a qualitatively similar divergence in post-treatment average wage trends between states that implemented the ROWTT law during weak versus strong labor markets.

In 2015-2016, a prominent national political campaign promoted broader labor movements¹⁰, and if states adopted the ROWTT law in parallel to other agendas that impact wage dynamics, the role of pay transparency would become ambiguous. The campaign could interact with the ROWTT law in two ways. First, it could make the existing law more salient and serve as an alternative explanation of why there are heterogeneous treatment effects. More problematically, it might introduce confounding policies that mask the true effect of the ROWTT law. An adapted event study using calendar year 2016 as the treatment provides evidence against year-specific differential effects. In Equation (2), the relative time (j) is redefined as years since 2016. Restricting to states that already have active ROWTT laws before 2016, any interaction between the campaign and the policy will manifest as abnormal spikes in the event study around 2016. The flat estimates throughout the event window in Figure 7 imply no such interactions.

$$Y_{s,t,i} = \alpha + \sum_{j=-16}^{-2} \beta_j D_{s,t+j} + \sum_{j=0}^7 \beta_j D_{s,t+j} + \mu_s + \lambda_t + \varepsilon_{s,t,i} \quad (2)$$

⁸ Effective dates are based on information provided in Table 1 of Cullen (2024).

⁹ Alabama does not have a ROWTT law in place by 2024, but the state enacted a salary history ban in 2019.

¹⁰ Obama's "Strengthening American's Voice in the Workplace" campaign promoted multiple interventions including the executive order extended ROWTT analogous protections to employees working for federal contractors (White House 2016).

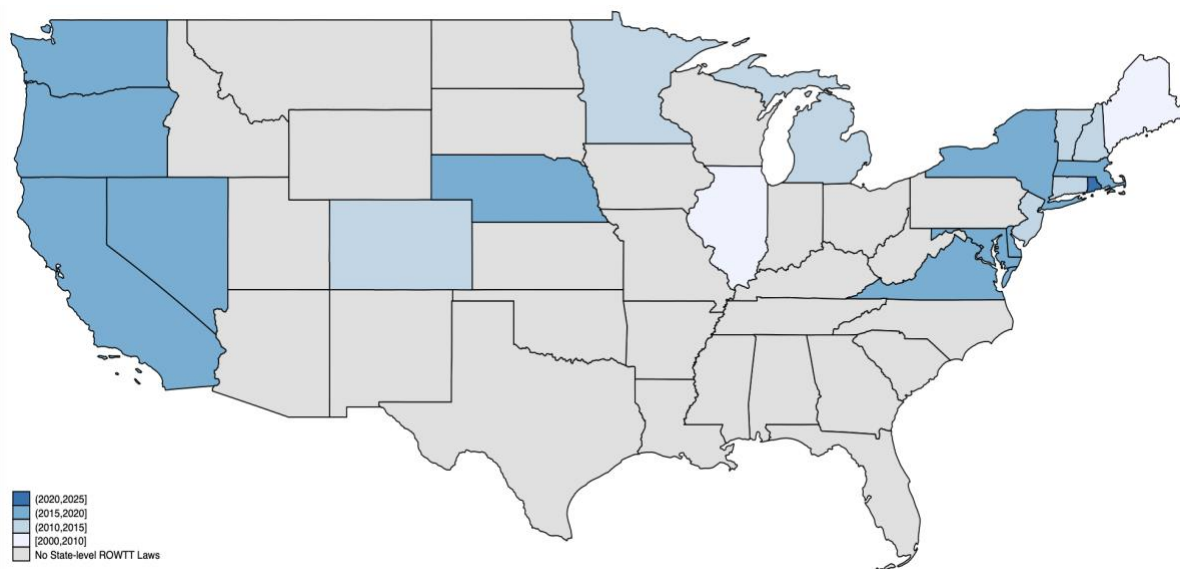
5. Conclusion

This paper provides new theoretical and empirical evidence on how pay transparency affects average wage levels, establishing heterogeneous effects by labor market conditions at the time of policy enactment. By focusing on the Right of Workers to Talk (ROWTT) laws, which promote within-firm pay transparency, I isolate the wage-level impacts of internal pay information sharing and document opposing effects that vary by relative market tightness. In tight markets, where labor demand exceeds supply, transparency leads to increases in average wages. In slack markets, where job opportunities are scarce, it results in average declines. These findings challenge the existing understanding that workers always bear the economic incidence of transparency, reflected in lower wage levels, and underscore the importance of labor market context in shaping policy outcomes. More broadly, the results highlight that policies aimed at improving pay equity may carry unintended welfare consequences for firms and workers. Recognizing the full picture is essential for policy evaluation and design.

Several important questions remain for future research. First, this paper focuses on wage income, but transparency may also affect non-wage components of compensation, such as benefits, bonuses, or other job amenities. Second, while the current framework centers on the bargaining process of workers who remain at their firms, further work could examine how pay transparency influences outcomes for job switchers or new hires, whose negotiations may follow different dynamics. Exploring these extensions would deepen our understanding of the broader consequences of transparency and help refine the welfare analysis across a wider range of labor market experiences.

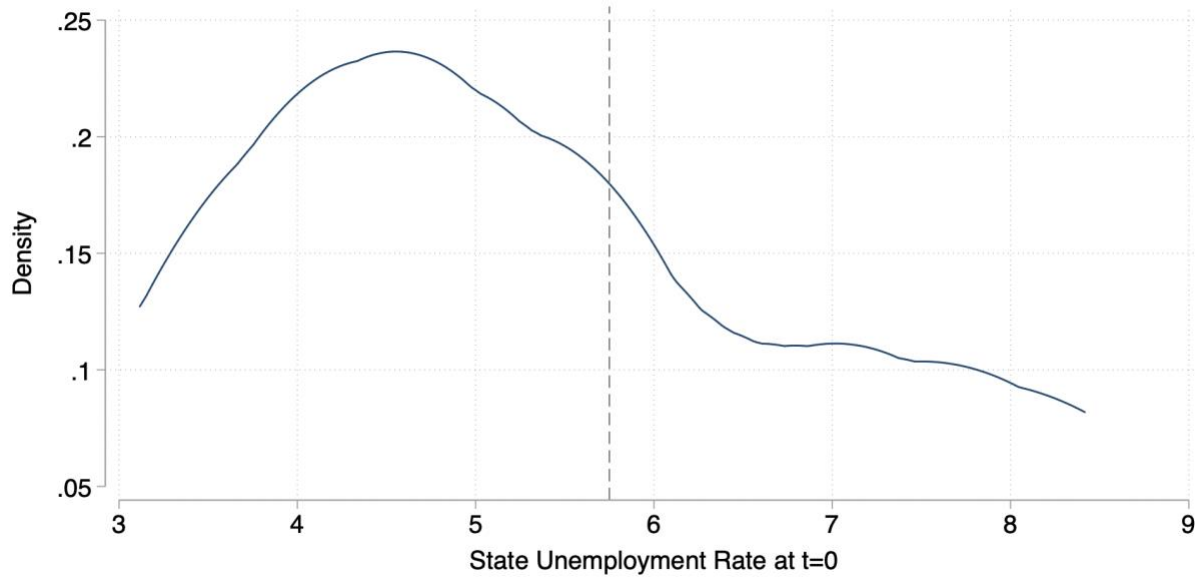
Figures

Figure 1: Map of ROWTT Effective Year for Each U.S. State



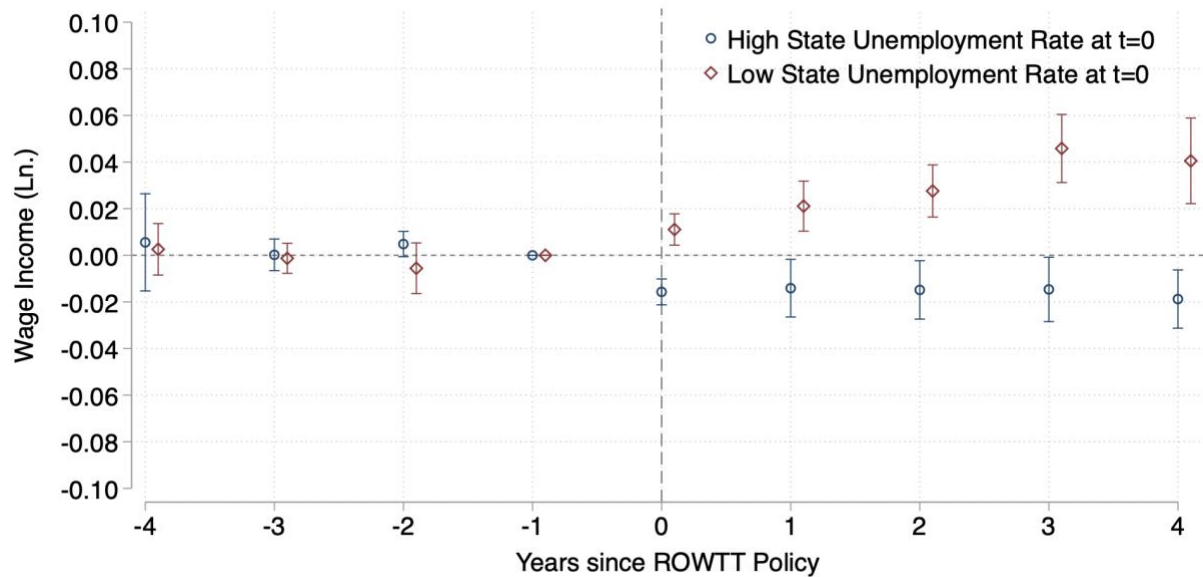
Notes: Alaska and Hawaii are not mapped. Alaska never passed a ROWTT law, and Hawaii is the latest state to pass the ROWTT law with an effective data in 2024.

Figure 2: Kernel Density of the State-level Unemployment Rates at Policy Enactment for Treated States



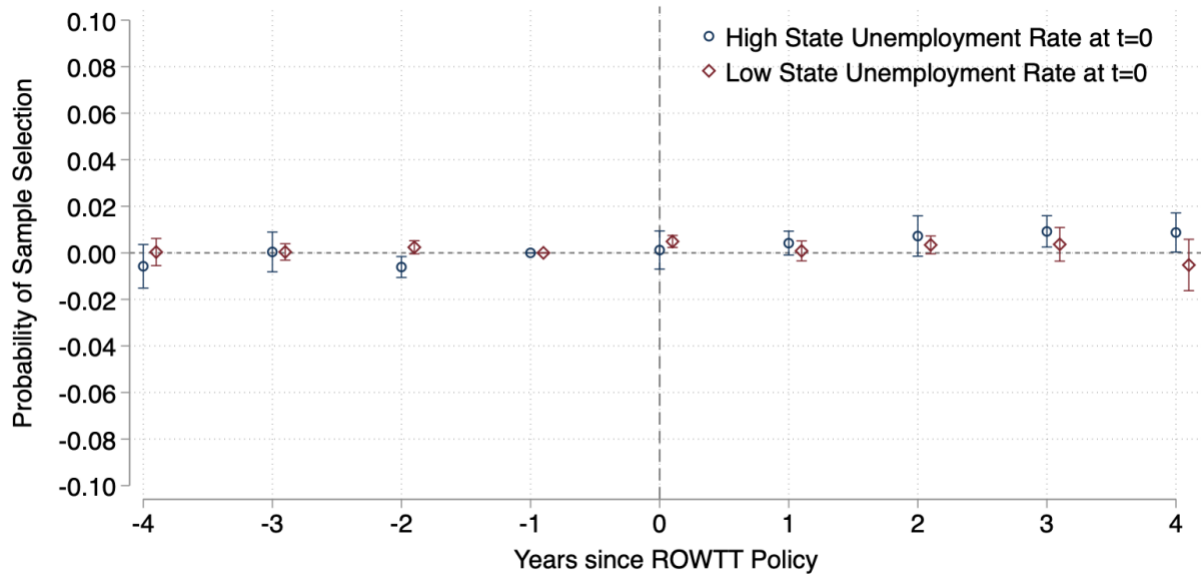
Notes: Including all treated states except for Virginia, Rhode Island, and Hawaii. Their ROWTT laws are enacted too recently for estimation of dynamic effects so are excluded from the preferred sample. State-level unemployment rates are calculated annually and matched to the ROWTT effective year. The vertical line is the binary threshold separating slack from tight local labor markets, and it's the grand average since 1976 (5.75 p.p.).

Figure 3: Main Results on Average Wage Level Effects by State Labor Market Conditions at Policy Enactment



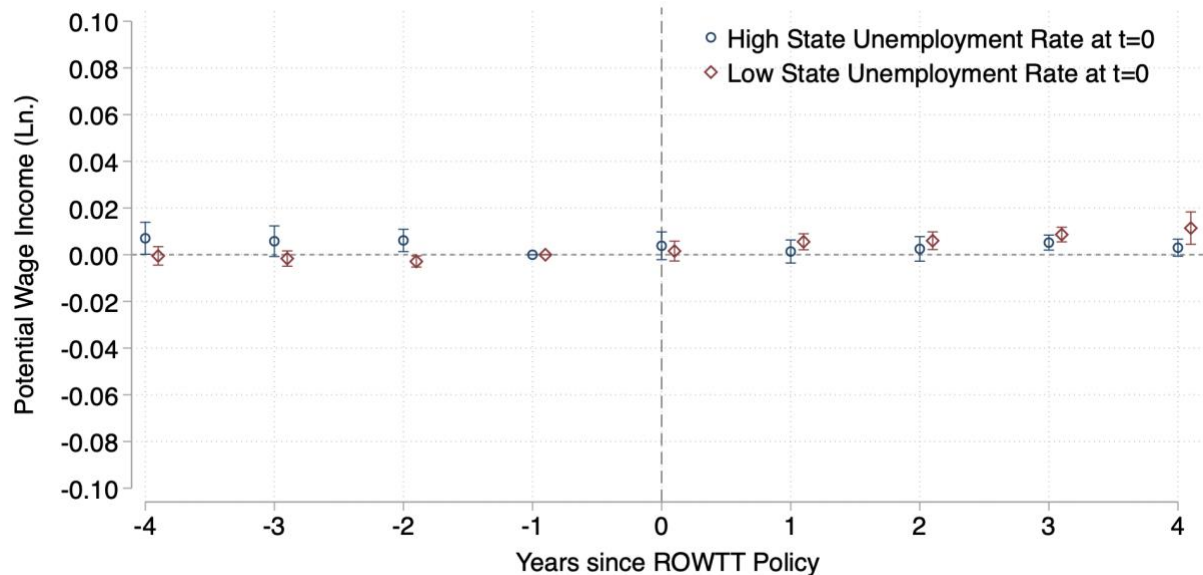
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel.

Figure 4: Sample Selections by State Labor Market Conditions at Policy Enactment



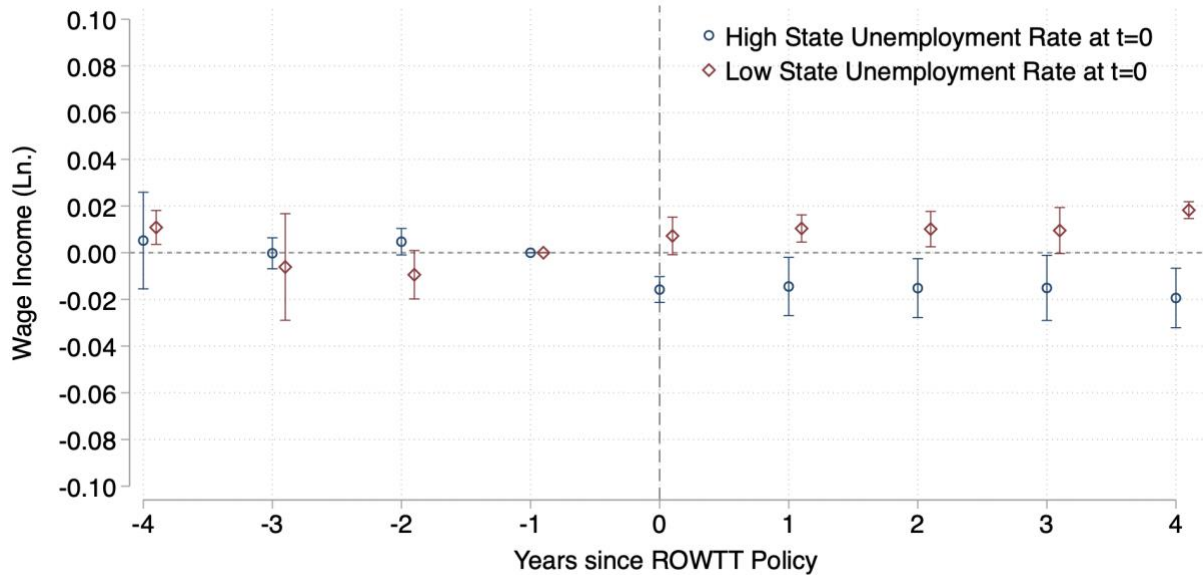
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel.

Figure 5: Selection on Potential Wage Incomes (Industry) by State Labor Market Conditions at Policy Enactment



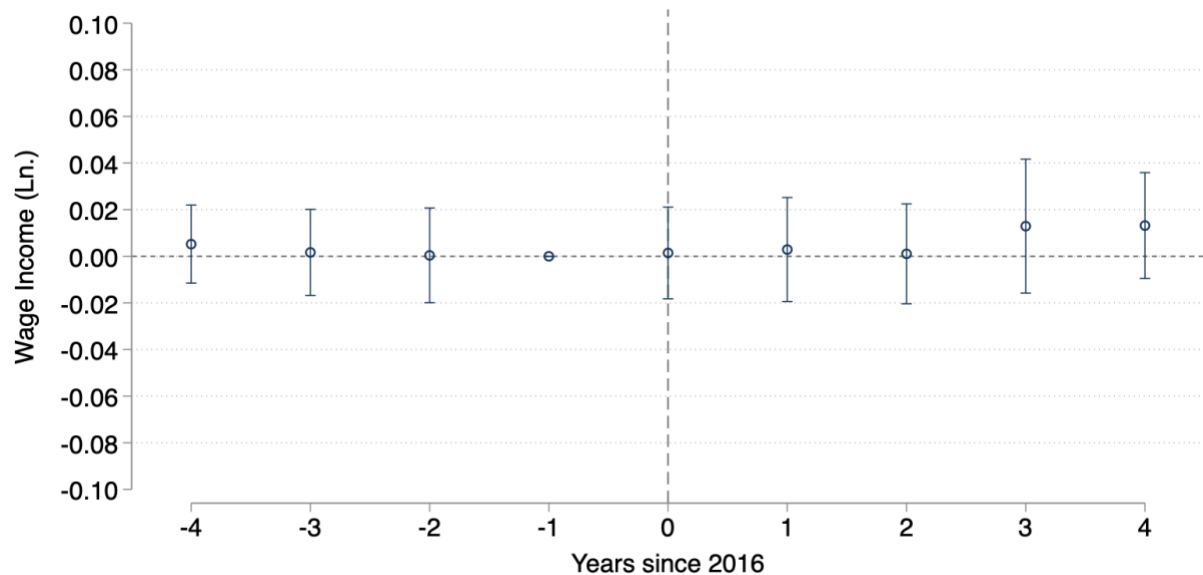
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel. Potential wage incomes are approximated by matching individuals to the pre-period industry weighted average income by their current industry.

Figure 6: Average Wage Level Effects by State Labor Market Conditions at Policy Enactment, Independent of Other Pay Transparency Measures



Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. In addition to the three most recent treated states, any treated states that enacted other pay transparency measures within 4 years of $t = 0$ are dropped, along with one control state that independently passed another pay transparency measure without ever enacted ROWTT.

Figure 7: Alternative Event Study Using Year=2016 as the Treatment to Rule out Confounding Effects of the National Labor Movement



Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. $t = 0$ is redefined to be $Year = 2016$, when a prominent national labor movement promoted multiple agendas. Only states that passed the ROWTT laws before 2016 are compared to the never-treated states. The purpose of this graph is to show that there are no differential effects from the campaign by treatment status defined in this study. Since the early-treated states already have the ROWTT laws in place, there should be no more sudden changes expected. Flat trends would suggest that the national movement did not interact with the ROWTT laws.

Tables

Table 1: Effective Dates of the ROWTT Laws for U.S. States

| State | Effective Year | State | Effective Year |
|----------------------|----------------|---------------|----------------|
| Illinois | 2004 | Delaware | 2016 |
| Maine | 2009 | Maryland | 2016 |
| Colorado | 2012 | New York | 2016 |
| New Jersey | 2013 | Oregon | 2016 |
| Vermont | 2013 | Nevada | 2017 |
| Michigan | 2014 | Massachusetts | 2018 |
| Minnesota | 2014 | Washington | 2018 |
| Connecticut | 2015 | Nebraska | 2019 |
| District of Columbia | 2015 | Virginia | 2020 |
| New Hampshire | 2015 | Rhode Island | 2023 |
| California | 2016 | Hawaii | 2024 |

Notes: Dates sourced from Cullen (2024) and the U.S. Department of Labor (2025). Louisiana is not listed because the law it enacted in 2013 falls short of typical ROWTT-style laws. The statute applies only to public employees and does not explicitly guarantee the core right of pay discussions among employees. Its substance closely resembles public-sector pay transparency provisions already common in non-treated states.

Table 2: Summary Statistics (Averages)

| Panel A: Full-time Private-Sector Prime-Age Workers (Sample of Interest) | | | |
|---|-----------------------|-----------------------|-------------------|
| | Treated States | Control States | All States |
| | (1) | (2) | (3) |
| Age | 39.11 | 39.27 | 39.20 |
| Male | 0.58 | 0.58 | 0.58 |
| Married | 0.57 | 0.59 | 0.58 |
| White | 0.69 | 0.76 | 0.73 |
| Education (Year) | 13.77 | 13.46 | 13.60 |
| Real Wage Income (Annual) | 61,628.12 | 49,757.90 | 55,199.27 |
| Observations | 4,782,151 | 5,666,886 | 10,449,037 |

| Panel B: All Prime-Age Individuals | | | |
|---|-----------------------|-----------------------|-------------------|
| | Treated States | Control States | All States |
| | (1) | (2) | (3) |
| Age | 39.44 | 39.50 | 39.47 |
| Male | 0.50 | 0.50 | 0.50 |
| Married | 0.57 | 0.58 | 0.57 |
| White | 0.68 | 0.74 | 0.71 |
| Education (Year) | 13.53 | 13.25 | 13.38 |
| In Labor Force | 0.83 | 0.81 | 0.82 |
| Employed | 0.78 | 0.77 | 0.77 |
| Work Full-time | 0.60 | 0.61 | 0.61 |
| In Private Sector | 0.68 | 0.68 | 0.68 |
| Real Wage Income (Annual) | 41,116.79 | 33,260.43 | 36,878.32 |
| Observations | 10,822,996 | 12,776,804 | 23,599,800 |

Notes: Based on individual-level data from ACS survey waves 2000-2023, for 19 states including DC that enacted ROWTT laws by 2019 (treated states), and all 29 never-treated states (control states). These are weighted statistics using provided individual-level survey weights.

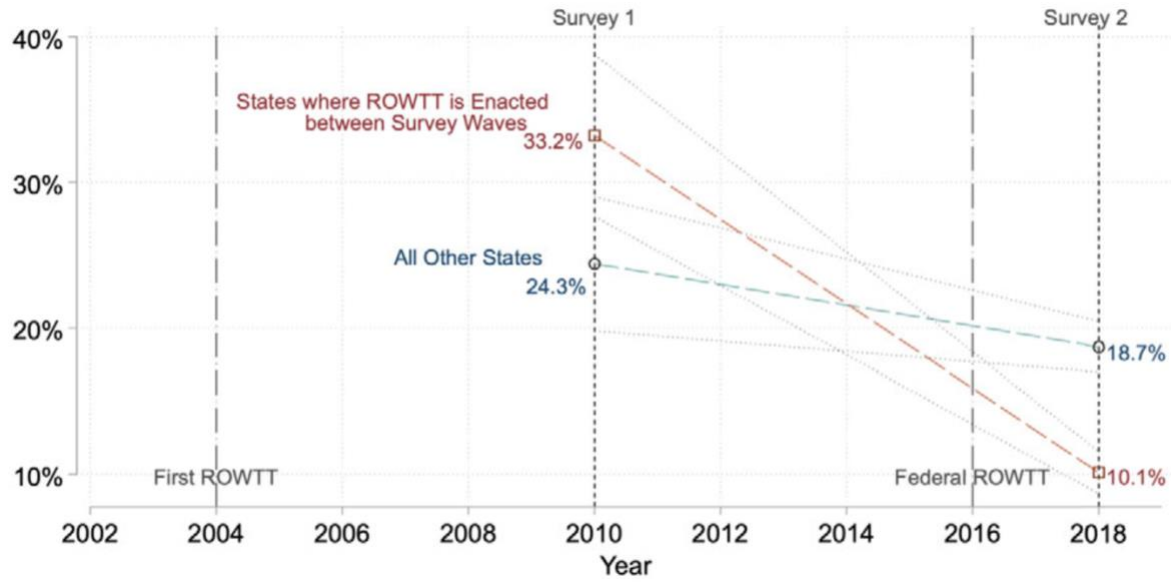
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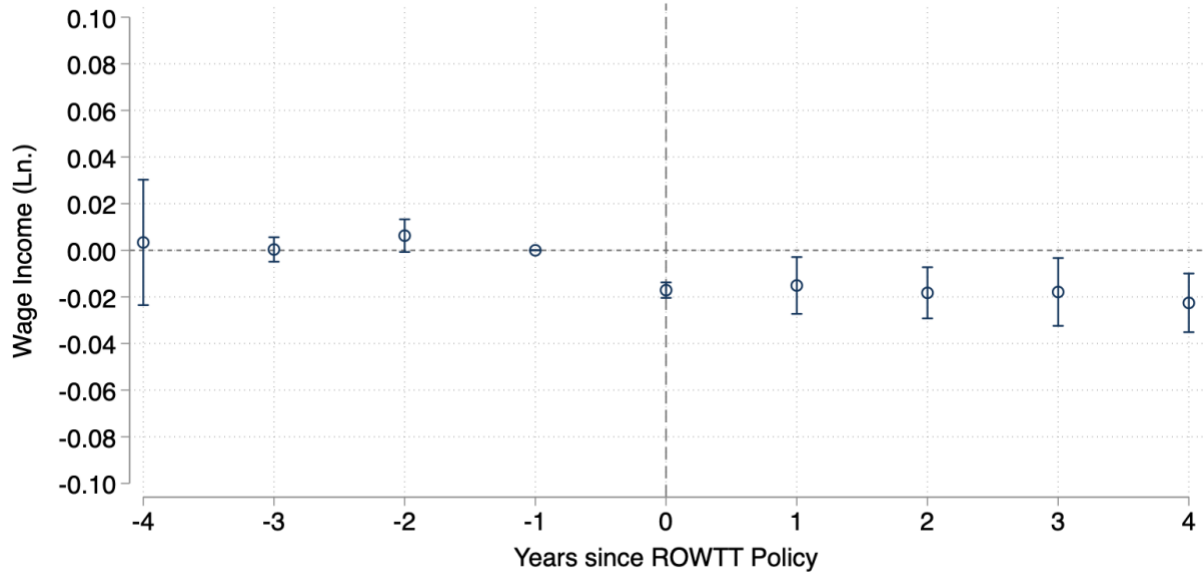
Appendix

Figure A1: Change in Pay Secrecy for ROWTT and Non-ROWTT States



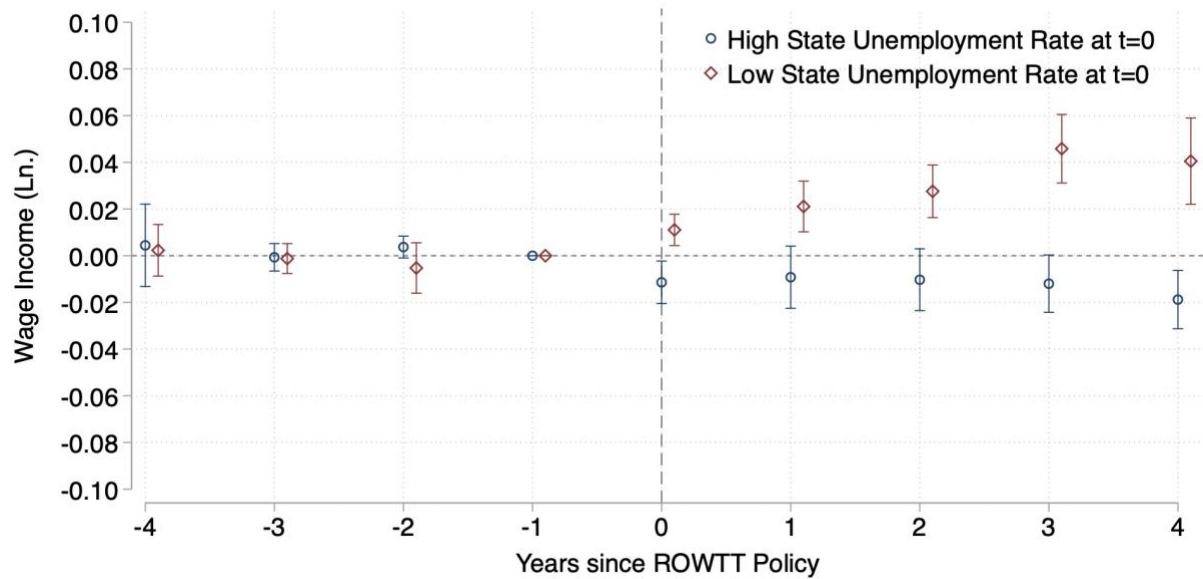
Note: This is an exact replication of Cullen and Pakzad-Hurson (2023) Figure 2

Figure A2: Event Study Using Only States that Enacted the ROWTT law by 2013 (Cullen and Pakzad-Hurson 2023 Sample)



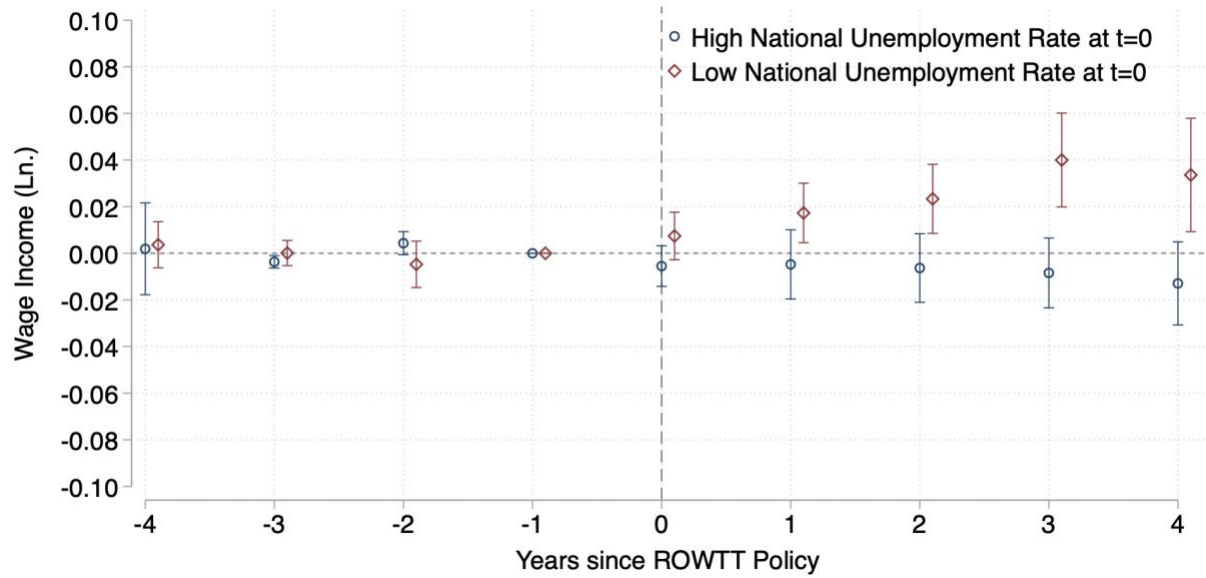
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. These results are replications of the balanced-panel Sun-Abraham dynamic effects reported in Table 1, Column 5 of Cullen and Pakzad-Hurson (2023).

Figure A3: Average Wage Level Effects by State Labor Market Conditions at Policy Enactment Using All States



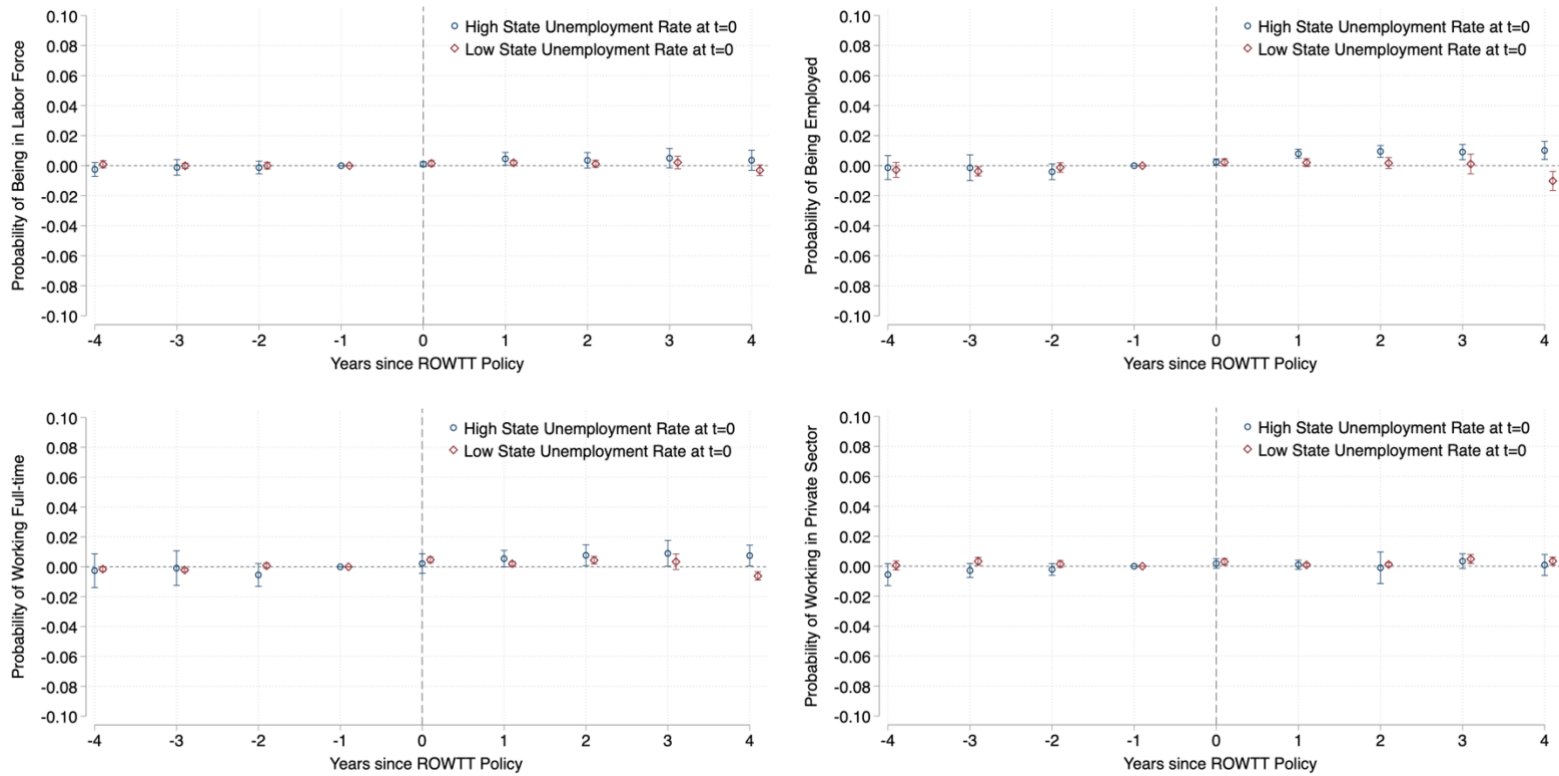
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are included, so the panel is not fully balanced in the post-period.

Figure A4: Average Wage Level Effects by National Labor Market Conditions at Policy Enactment



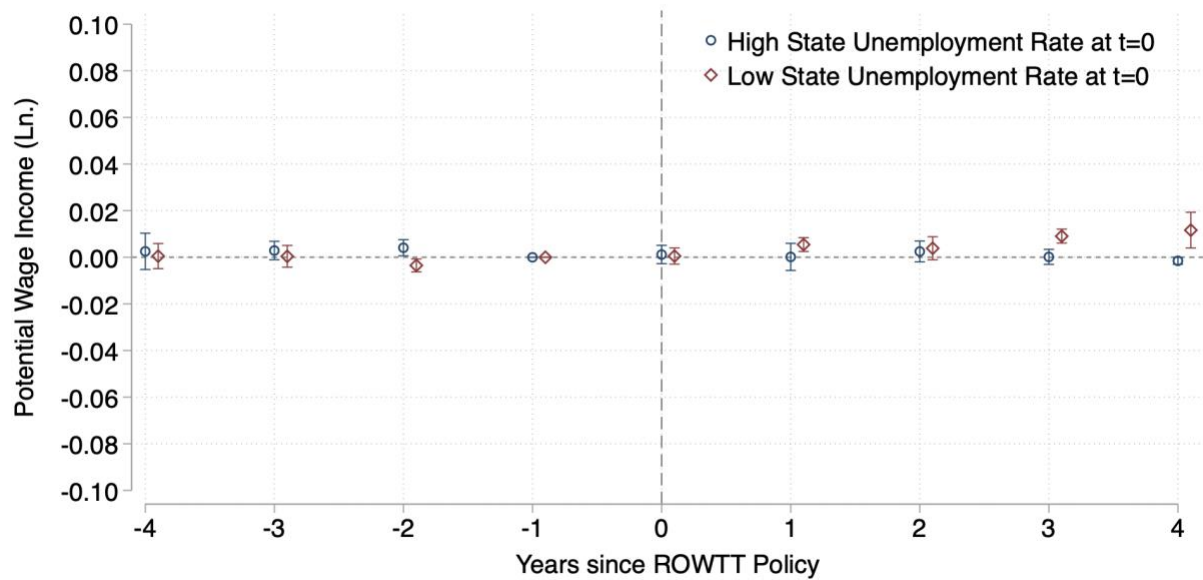
Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. Annual U.S. unemployment rates in the state policy enactment year are used instead to split treated states and are considered high if they are above the grand average of all national unemployment rates since 2000. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel.

Figure A5: Sample Selection on Employment Margins by State Labor Market Conditions at Policy Enactment Using All States



Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel.

Figure A6: Selection on Potential Wage Incomes (Occupation) by State Labor Market Conditions at Policy Enactment



Notes: Adjusted for staggered adoption using the Sun and Abraham (2021) method. Weighted by individual-level ACS survey. Standard errors are two-way clustered by state and year. State unemployment rates are calculated annually and are considered high if they are above the grand average of all state-level unemployment rates since 1976. Only the treated states are split into subgroups and all never-treated states serve as baseline comparisons for both treated subgroups. Three most recent treated states are dropped to create a balanced panel. Potential wage incomes are approximated by matching individuals to the pre-period occupation weighted average income by their current occupation.